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Abstract

Protected areas are employed world-wide as a means of conserving biodiversity. Unfortunately, restricting access to such areas imposes opportunity costs on local people who have traditionally relied on access to obtain resources such as fuelwood and bushmeat. We use contingent valuation to estimate the local benefits forgone from loss of access to a number of protected area types in Uganda. Methodologically, we innovate by implementing a "provision point" mechanism to estimate Willingness to Accept compensation (WTA) for loss of access to protected areas. We show that the provision point reduces mean WTA by a significant degree.

Keywords: conservation costs, protected areas, Uganda, willingness to accept, provision point mechanism, WTA.

I. Introduction

The establishment of protected areas (PA) such as national parks and game reserves is a common means of protecting biodiversity from habitat loss and hunting pressures in many developing countries. Protected areas enhance conservation by applying land use restrictions such as banning bush meat hunting and the collection of fuel-wood or timber for construction, and by restricting the conversion of land into agriculture. Unfortunately, properly enforced land use restrictions impose potentially high costs (opportunity costs) on local resource users (Norton-Griffiths and Southey, 1995).

Communities adjacent to protected areas in Uganda normally consume, exchange, or sell timber and non-timber forest products (NTFP) sourced locally as part of their livelihood strategies. In National Parks however, legislation precludes the hunting of wild animals and the extraction of timber and NTFP. Poor enforcement by under-resourced management authorities often translates on the ground into illegal exploitation of protected areas, especially those immediately adjacent to communities. Indeed, the use of PA resources such as fuel wood has increased dramatically in Uganda in recent years (NEMA, 2001; Bush et al., 2004; ITFC, in prep). Without access to PA resources or an alternative source of revenue, many rural households face increased levels of impoverishment[i].

As user pressures on protected areas increase, so do efforts to put in place more effective management strategies to control access, curb illegal hunting and provide communities with alternative means of ensuring their welfare. Unfortunately, exclusionary management practices tend to create tensions between local people and the authorities (Hulme and Murphee, 2001; Plumptre et al., 2004). If local communities wish to receive direct benefits from protected areas in the future, and if park authorities wish to see more stringent enforcement of unpopular management rules, it seems essential to put in place a management regime that promotes their acceptance by local communities. Information on the costs of lost access is a key indicator of how much effort must be deployed in order to mitigate the effect of losses by local people. Such information can be used to devise mechanisms that, perhaps in conjunction with integrated conservation and development projects, can maintain the well-being of local residents whilst also securing conservation objectives.

This paper reports the results of a research project that aimed to measure the value of a complete loss of access to PAs under a (hypothetical) scenario in which local users would receive monetary compensation for those losses. We aim to quantify the local economic (opportunity) cost of conservation to communities adjacent to four protected areas in western and south western Uganda. The household survey (conducted from January to July 2006) employed contingent valuation as part of a broader questionnaire survey on household socio economic characteristics to estimate the market and economic values of goods currently taken from the local PA. Whilst market prices are useful in measuring some aspects of the losses deriving from land use restrictions – where losses can be substituted by market purchases, or where losses are in terms of foregone sales - they cannot capture the full economic values of access to such resources to local people (Campbell et al., 1997; Godoy et al., 1997; Godoy et al., 2002a; Vedeld et al., 2004; Godoy et al., 2009) . Access to PAs can be viewed by local users as providing an insurance policy, given fluctuations in agricultural incomes both within and between years (Pattanayak and Sills, 2001). Access also has cultural and social values which are not captured by market prices for products such as fuel wood and bushmeat. Finally, losses of non-traded (subsistence) resources also impose costs on local people due to absence of substitutes on local markets, or poor market access (Geist and Lambin, 2002; Soares et al., 2006) . In such circumstances, contingent valuation methods are useful for measuring the economic value of the loss of access by local households to protected areas.

Since stricter access restrictions would deprive locals of traditional resources over which they consider themselves as having *de facto* property rights, willingness to accept compensation (WTA) for those losses is a more appropriate measure of costs than Willingness to Pay. Despite WTA often being the more appropriate measure, WTA studies are comparatively rarely performed in contingent valuation since they are suspected of systematically over-stating welfare losses (Rowe et al., 1980); Arrow et al., 1993; List and Shogren, 2002). We attack this vexing problem by presenting survey respondents with a payment method constructed from a Provision Point Mechanism (PPM) (Rondeau et al, 1999 and 2005; Poe et al, 2002). To our knowledge, this is the first application of a PPM in a WTA survey. The second objective of this paper is therefore to investigate how the PPM affects WTA value estimates within the context of contingent valuation. We show that the PPM significantly reduces the number of high value outliers and the resulting estimate of average welfare loss.

In what follows, Section 2 introduces the idea of a PPM for WTA. Section 3 describes the survey implementation, whilst results follow in Section 4. Conclusions are drawn in the final section.

II. Measuring WTA using contingent valuation – The Provision Point Mechanism.

The use of Contingent Valuation (CV) in developing countries is now widespread (Whittington, 2002; Whittington, 2004). CV has an established history in developing countries for the valuation of environmental amenities arising from national parks (Kramer et al., 1992; Kramer et al., 1995; Willis and Garrod, 1996; Whittington, 1998;

International Institute for Environment and Development, 2003). Mekonnen (1997) applied WTP elicitation formats to obtain the economic value of community forestry in Ethiopia, whilst Lynam et al. (1994) valued trees on communal lands in Zimbabwe. Ruitenbeek (1992) valued rainforests in Cameroon using a WTP scenario. In contrast, Smith et al (1998) use a WTA approach to analyse potential compensations payments required to induce land use changes among farmers in Peru with the objective of CO_2 sequestration. Other studies address the problem of compensation payments that are required in order to induce land use change by local farmers for watershed protection (Kramer et al 1992; 1995).

As is the case for studies from developed countries, the Willingness to Pay (WTP) format has indeed been largely favoured over WTA in developing country applications of CV, even when the objective of the research is to assess welfare losses (Smith et al., 1998) . Harrison and Rustrom (2005) discuss the historical side-stepping of WTA measures, attributing this to the difficulties researchers have encountered in their attempt to control scenario rejection, and the strong hypothetical bias that the WTA format appears to create. This bias relates to a tendency of respondents to overstate their true welfare loss from a change in environmental quality or access: in other words, WTA estimates of welfare change obtained using CV are thought to be "too big", with a greater degree of bias present than for equivalent WTP formats (this is independent of differences between *true* WTP and WTA for a given environmental or access change: see Plott and Zeiler, 2005). WTA scenarios also typically produce more protest bids due to scenario rejection by respondents. As a result, there is a general avoidance of WTA format surveys, even when it is theoretically the more appropriate welfare measure to use (Knetsch, 2005).

In this paper, we are interested in whether a Provision Point Mechanism (PPM) can reduce WTA over-statement. In its usual configuration, the PPM is a mechanism to facilitate voluntary contributions towards the provision of a discrete public good. We are not aware of any previous implementation of this mechanism in a WTA setting. It is therefore necessary to adapt the procedure to a WTA scenario and, accordingly, to rework the theory first laid out by Bagnoli and Lipman (1989), Marks and Croson (1998) and Rondeau et al. (1999) to the WTA context.

In the WTA setting, the Provision Point (PP) is the total amount of money available (e.g. in a trust fund) for compensating all affected individuals in a group for the loss of access rights to protected areas. Individuals are asked to make a claim or bid (*B*) for compensation from this fund. If the sum of all claims exceeds the money available in the fund, no compensation payments are made and the status quo is maintained (no access restriction is imposed). If the sum of claims is less than or equal to the PP, individual claimants receive their claim plus a share of the remaining portion of the total funds remaining after all claims are paid. Access regulations are then imposed.

To be specific, define B_i, B_j as individual claims on the compensation fund, PP as the total amount available in the fund and N as the number of claimants. It follows that

- $if \sum_{j=1}^{N} B_j > PP$ the sum of claims exceeds the available funds, no new regulation or enforcement is put in place and no compensation is paid;

- $if \sum_{j=1}^{N} B_j = PP$ the sum of claims exactly equals the amount available. Enforcement is put in place, compensation is paid and people receive exactly the individual amount of their claim;

- $if \sum_{j=1}^{N} B_j < PP$ the sum of claims is less than the funds available for compensation. In this case, enforcement is put in place, the compensation scheme goes ahead and people receive their individual compensation claim plus a share of the unclaimed funds available. In our application, the share to claimant *i* is simply equal to the proportion of *i*'s claim relative to the sum of all *N* claims.

The PPM gives rise to strategic incentives. Assume individual *i* maximizes the utility derived from the value of having access to the protected area (V_i) and income from other sources (I_i), subject to the external constraint imposed by the PP. The claim B_i represents the individual's revealed WTA for accepting the enforcement policy. The utility of individual *i*, contingent on the all participant's claims is given by[ii]

$$U_i = \begin{cases} u\left(I_i + V_i\right) & if \sum_{j=1}^{N} B_j > PP \\ u\left(I_i + B_i\right) & if \sum_{j=1}^{N} B_j = PP \\ u\left(I_i + B_i + \dfrac{B_i}{\sum_{j=1}^{N} B_j}\left(PP - \sum_{j=1}^{N} B_j\right)\right) & \sum_{j=1}^{N} B_j < PP \end{cases} \quad (1).$$

It is relatively straightforward to extend the game theoretic equilibrium prediction for a PPM in the WTP context (Bagnoli and Lipman, 1989; Rondeau et al. 1999; (Marks and Croson, 1998) to a WTA scenario. Our characterization of the equilibria relies only on the utility function of claimants being increasing in its argument, and players being rational. The analysis provided here is also meant to be a simple baseline illustrating the basic incentives for the case where there is no uncertainty about the benefits and costs of the conservation areas or that the program will be implemented and compensation paid. Both the value of conservation and the compensation payments are annual and remain constant over time. Two types of pure strategy Nash equilibria exist. In both cases, rationality imposes that any individual claim be at least as large as the individual's loss: $B_i \geq V_i$. Otherwise, the imposition of land use restrictions would necessarily result in a net loss to an individual, even with the compensation payment.

Imposing this rationality condition and maintaining as a working assumption that the amount available for compensation (PP) is sufficient to compensate all losses (i.e. the benefits of compensation could exceed the loss of value from restricting access), one finds that one set of equilibria is inefficient (the program is not implemented despite the fact that the compensation fund is large enough to compensate all losses) and is characterized by the following conditions:

$$\begin{aligned} &\sum_{j=1}^{N} B_j > PP; \text{ and} \\ &\left(\sum_{j=1}^{N} B_j - PP\right) > \left(B_i - V_i\right) \forall_i \end{aligned} \qquad (2).$$

The first condition states that the sum of claims is greater than the amount available for compensation. Therefore, no compensation is paid and no policy is implemented. For this to be a Nash equilibrium, it must also be the case that no individual is in a position to decrease his claim (in a rational manner) such that the sum of claims would become equal to or lower than the PP. This is the meaning of second component of Equation 2. If no rational revision of a single bid can be made that would result in the provision of compensation, then no one has an incentive to deviate from their original claim and the vector of claims is an equilibrium in which compensation is not paid.

There is also a set of efficient equilibria in which the proposed policy is implemented and compensation is paid. This set is made up of any combination of claims such that

$$\sum_{j=1}^{N} B_j = PP \qquad (3),$$

together with the rationality condition that no claim is smaller than the value of the lost access rights. In such cases, no single individual would have an incentive to deviate from their claim since increasing the claim would lead to the regulation not being imposed (with no compensation received), while decreasing one's claim would simply lower the compensation received. This set of equilibria is potentially very large. Any distribution of claims amongst all participants that meets condition (3) is an equilibrium. The presence of multiple equilibria also implies that there will also exist mixed strategy equilibria, potentially adding a substantial amount of noise to the data.

While efficient equilibria Pareto-dominate inefficient ones, it is important to realize that, in general, this mechanism is not theoretically incentive compatible. Everyone making a claim equal to their individual value (the value to them of loss of access to the PA) is only an equilibrium under the unlikely scenario that $\sum_{j=1}^{N} V_j = PP$. Whenever $\sum_{j=1}^{N} V_j < PP$, this game provides incentives for the sum of claims to be equal to the PP, and thus, for individual claims to exceed the true value of losses on average (that is, for WTA bids to be "too large"). The advantage of using the PPM is that, compared to an elicitation mechanism without a maximum level of compensation, the incentives of individual claimants are radically changed in the direction of more truthful revelation of real losses. An open-claims game without a PPM (and, by extension, a CV-WTA survey) is in fact a degenerate game since without a threshold, individual bids are not bounded from above at all. If individuals truly believed that their claim might be paid with some positive probability (however small) when there is no limit to the amount that can be paid, their optimal claim will be infinitely large. This could explain in part why the standard WTA format produces very large bids. The fact that CV-WTA bids are not typically infinite may stem from the fact that individuals in these studies realize that there must be an *implicit* maximum to the amount of compensation that can realistically be paid out. The PPM has the advantage of making the existence of a limit explicit, and of clearly stating the consequences of exceeding it for everyone.[iii]

Despite the fact that multiple equilibria make for weak theoretical predictions, laboratory experimentation with the PPM in the WTP context and some field applications in CV surveys provide useful insights on the empirical properties of the mechanism. Initial experimentation with threshold public goods in a WTP setting is attributed to Dawes et al. (1986), followed by Rapoport and Eshed–Levy (1989), Isaac et al. (1989), Suleiman and Rapoport (1992) and Rapoport and Suleiman (1993). Each of these papers presents results demonstrating that it is possible to significantly increase voluntary contributions to a public good by adding a minimum threshold of contributions required before any good is provided. In these papers, results from base PPM treatments (the addition of a threshold only) are somewhat mitigated by the absence of either a money back rule if a group fails to reach the threshold, or of a "rebate" rule defining how contributions in excess of the threshold are used (other than they are simply lost).[iv]

Isaac et al. (1989) test a money back rule in experiments where subjects could contribute any amount and Rapoport and Eshed–Levy (1989) run similar experiments where participants can only make a binary choice of contributing a given amount or nothing. Both report significant increases in contribution rates when the money back rule is put in place. Only Dawes et al. (1986) fail to report increases in contributions after the addition of a money back rule. The only exhaustive study of alternative ways to employ excess contributions was conducted by Marks and Croson (1998) using a PPM with a money back rule. In the absence of a rebate rule, excess contributions are lost. This provides a deterrent to contribute large amounts. Marks and Croson (1998) report that extending benefits (using the added contribution to increase the size of the public good) has the greatest positive influence on contributions. A proportional rebate of excess contributions leads to increased contributions over a no rebate rule, but does slightly worse than the extension of benefits. In our study, we adopt a proportional increase in claims because it is neither practical nor desirable to modify the size of the program based on the level of claims (i.e. imposing fewer restrictions on access when claims are lower than the funds available).

Another set of experimental papers has focused on the relationship between individual contributions and the participant's underlying value for the public good (i.e. at the threshold). While the PPM is not incentive compatible, the empirical evidence strongly suggests that the PPM can radically curtail free-riding, provide a more efficient level of public good provision, and improve demand revelation (Rondeau et al. 1999, Cadsby and Maynes, 1998; Rose et al., 2002; Messer et al., 2005). In perhaps the most extensive study of the relationship between contributions and underlying value, Rondeau et al (2005) point out that while the PPM produces contributions that are substantially closer to demand revelation than the equivalent voluntary contributions mechanism without a threshold, the PPM is not empirically demand revealing. If it was, a value of zero for the public good should produce no contribution, and bids should be perfectly correlated with value. Instead, they find that 1) a value of zero is predicted to yield strictly positive contributions; and 2) that increases in value result in a proportional but less than perfectly correlated increase in contributions. Such results are consistent with some form of warm glow, other-regarding preferences, or errors by subjects (Ferraro et al., 2003).

Despite promising experimental evidence that the PPM results in more efficient voluntary provision of public goods, few have introduced it to hypothetical WTP surveys. Murphy et al. (2005) compare real and hypothetical payments to a land conservation organisation using a PPM in a laboratory environment. They observe a difference in amounts contributed between the two treatments and conclude from an analysis of additional survey questions that it stems from hypothetical bias rather than from free-riding. In other words, the PPM appears to perform well when real money is involved, but some hypothetical bias remains. In the only other field work we are aware of, Poe et al. (2002) and Ethier et al. (2000) compare the real sign-up rates to an environmental program against hypothetical open-ended and referendum format CV surveys. They find a weak hypothetical bias in the open-ended format, and a strong bias in the dichotomous choice referendum format.

It is important to note that the PPM poses a difficult coordination problem (in equilibrium, aggregate contributions equal the PP). For this reason, some of the research on the PPM reported above implements a version of the mechanism where one or both of the PP or number of participants (N) is not specified and where subjects do not know if they are all identical (Rondeau et al, 1999, 2005). The logic behind this design feature is that it prevents respondents from making contribution decisions influenced by some notion of equal cost sharing (PP/N). Such influence has the potential to distract participants away from a more careful consideration of their own gains from the public good. Whilst withholding information does not make the PPM incentive compatible, it may in practice bring individual claims closer to their true value. The evidence on whether or not this matters is inconclusive (Rondeau et al., 2005), although Bagnoli and McKee (1991) present results that clearly demonstrate how participants can coordinate on an equal cost sharing equilibrium when all subjects have identical preferences. It is nonetheless fortuitous that most previous experiments were conducted without the information required for the implementation of this focal point since, in the field survey reported here, it was not possible to specify with precision at the beginning of the study how many households would be eligible for compensation.

One potential shortcoming of the PPM in the WTA context is that the mechanism gives each participant the power to veto the entire scheme by claiming an amount greater than the PP. There is no equivalent veto power in the PPM for WTP. As previously mentioned, protest bids are a common problem in WTA valuation exercises, and the PPM cannot be expected to eliminate true protest claims. The reasons behind such a protest response in any study may be difficult to ascertain. In cases such as that considered here, it may be due to cultural factors i.e. opposition to any form of control over local land access, or due to people having a grudge about being displaced from former traditional lands. However, we posit that a credible PPM scenario should curtail the number of high claims that are not meant to be true protest bids or fundamental rejections of the scenario. At a minimum, the limit on the total amount that can be paid out in compensation provides a strategic incentive to bring one's claim closer to the true value of anticipated losses. If anything, PPM results might also make it easier to screen for true protest bids by comparing the distributions of bids across payment scenarios[v].

Summing up, if our understanding of the PPM in WTP context carries over to the WTA setting, the establishment of a PP should decrease the likelihood of individuals making bids that overstate their value by a large amount, giving a more accurate measure of true WTA. In our policy context, this would provide a useful tool for improving accuracy in the measurement of the economic costs of changes in local access arrangements to protected areas. In the next sections, we test this proposition empirically.

III. Case Study Design

A contingent valuation survey was administered in villages located in the vicinity of four protected areas in Uganda. Data were collected from households in communities around each of four different PAs. The surveys collected not only WTA responses, but also information on education level, household size, income sources and income group of each respondent. Table 1 provides details on the sampling regime. The four protected areas are ecologically different (tropical closed canopy rainforest, afro-montane forest and a savannah woodland and tropical closed canopy mix), implying that the value of access V_i (in terms of timber, NTFPs, fuelwood and bushmeat extracted) was expected to vary considerably both across and within areas.

Sampling methods

Site selection was based on finding PAs with similar historical levels of illegal park use as well as differing examples of existing governance strategies to resolve illegal use issues. The sample population at each site is defined as all probable users of the PA. In practice this meant we focussed on parishes (Local Council II) with a political boundary bordering the park. The boundary defining potential users was identified as households that were at a maximum of two hours walk to the PA: that is, within around 5km of the PA. Beyond this distance, households were unlikely to view the PA as a potential resource for exploitation (Bush et al 2004). At the site level, stratification reflected socio-economic and topographic differences around the sites. Typically every n^{th} parish adjacent to the site was sampled at random (around 50% of parishes). A village (local council I) was then selected from within each parish at random from sub-strata of those villages adjacent to and non-adjacent to the park. Within each village a stratified (according to wealth ranking) proportionate random selection of around 30 households was drawn for interview, with half having the PPM treatment and the other half without. From these 30 households the application of the PPM treatment was at random selecting every n^{th} household in the stratified list. Finally heads of households were selected to act as respondents. However, usually all available members of the household were in attendance during the interview. This sampling regime was possible due to relatively recent census data available from the national statistics office and good information available in village level administrations[vi].

The cross-sectional data were collected during the period January-July 2006, with the survey team typically spending 3 days per village and six days in a parish. For example in Queen Elizabeth National Park the total time spent on the survey was of 36 days in and around the villages of the park. Surveying was conducted by trained research assistants, themselves from rural parts of the west and south western parts of Uganda, supervised full time by the lead researcher. The first day of the survey method also involved a participatory rural appraisal exercise exploring various household and institutional aspects of resource use. Finally, every community was re-visited as part of a wider study funded by CARE International on rights and equity in PA management.

Fuel wood was the main resource extracted by local households, followed by timber for construction, and bushmeat. In all sites, alternative sources of such goods were few e.g. natural forests resources or woodlots not privately owned or protected (Bush, 2009). The history of protection also varies across the case study sites, with a much more recent introduction of access restrictions in Tengele than at the other three sites.

Hypothetical scenario

The payment scenario in the survey sets up a framework for the implementation of a hypothetical community-based park management scheme in collaboration with park management authorities, aimed at improving the conservation status and resource condition of protected areas. Discussion with respondents during the survey highlighted the costs and benefits to local people of their local PA, and the problems caused by current rates of illegal resource extraction such as declining stocks of fuelwood and declining populations of animals hunted for bushmeat. It was pointed out to respondents that such a rate of use was unsustainable, and would lead to future problems for local people in terms of sustaining their well-being. Respondents were then asked to state the minimum level of compensation (WTA) they required to forgo access to all resources from their local Protected Area for a period of one year, under a scheme in which surveillance and enforcement of access restrictions was implemented by a newly-formed community group. Examples were provided of possible actions by local people to aid enforcement, such as the reporting of illegal snares. Absent the implementation of such a compensation scheme, current over-use and illegal access would continue, with no compensation being paid to households.

Two separate payment scenarios were employed in the study. The control treatment is an open-ended CV format in which respondents were simply asked to state their WTA compensation to forgo the benefits from the PA for one year. This provides a basis for comparing the results of the second treatment, which included a PPM. In each of the communities surveyed, respondents were randomly assigned to one of the two treatments (i.e. with or without the PPM). An open-ended CV format was chosen based on the experience gained in an extensive pilot testing of the study. As noted by many authors (e.g. Champ and Bishop, 2006), no elicitation method is problem free. Of direct relevance to this paper is the work by Poe et al. (2002), who found an open-ended design WTP with a PPM resulted in lower levels of hypothetical bias than a referendum format WTP with a PPM. In a comparable study to ours using a WTA format in Nepal, Shrestha et al (2007) also use at open-ended payment mechanism. They find that this open-ended design works well in estimating the costs to local people of access restrictions in the Koshi Tappu Wildlife Reserve. Whilst it would have been interesting to include alternative payment elicitation in the present study, resource constraints and pre-testing results meant that we focussed on an open-ended payment design alone.

In the PPM version of the questionnaire, the PPM was explained in the following fashion:

"..The community is being asked to make monetary bids to assess the demand for such a scheme and estimate the level of compensation. Only a limited amount of funds are available for such a scheme. If the sum of all the household compensation bids is **less than** or **equal to** the money available then the scheme would **go ahead** as described, and a proportional share of any surplus funds between the communities' bids and the compensation fund will be made.

If the sum is **more** than the money available then such a scheme **would not** go ahead and it is likely that the current situation would continue..."

Training and pre-testing was carried out at Queen Elizabeth National Park, and involved a week of theoretical and practical training on the survey methods for the survey personnel followed by a full pilot run of the entire survey method in a park-adjacent community. A full debriefing covering every aspect of the pilot survey was conducted every evening, and any ambiguities in the survey questionnaires were addressed. Surveying was rigorously supervised to ensure that enumerators complied with established procedures, and that communities were visited according to the random sample selection made from the site sampling frame. Pre-testing was conducted to identify weaknesses in the presentation and comprehension of the questionnaire by both the enumerators and respondents, and to determine the most appropriate response formats to different questions. In general there was consensus from enumerators that the scenario was found to be credible by respondents. The scenario addressed both a real conservation issue (illegal use) and an appropriate response to resolving it (direct payments for conservation in response to community-led enforcement).

It typically took the team of 5 enumerators about 3 days in each community to complete the interviews. During this period the research team either found local lodgings or camped within the community. The extended period of contact with local people allowed the team to develop a high degree of familiarity with the social and natural environment of each community. This often gave opportunities to discuss responses and resolve sampling problems. For example, amongst some of the diverse local cultures in which the survey was administered, it was culturally taboo to tell strangers how many children or livestock the household has for fear of bringing bad luck. However it is not a social taboo for neighbours or other local key informants to divulge information about neighbours' situations, so information could be gathered in this way. An estimate of total household income (adjusted per adult equivalent unit) was made so that households could be allocated to income quartiles as a basis for comparison. An assessment was made of the demographic composition of each household, level of education, and employment. Data was also collected on total household income from the sale of PA and non-PA goods. Further information on the survey procedures and market price results can be found in Bush et al (2011).

IV. Results

A total of 690 households were contacted. Ten questionnaires were discarded as incomplete. A further 5 questionnaires were discarded because respondents did not want to answer the WTA question, leaving 675 usable surveys included in the analysis (Table One).

Descriptive Analysis

Table 2 shows descriptive statistics of the variables collected by group: the first panel shows statistics related to households treated with the PPM (N = 338) while the second panel shows statistics for the control group of respondents (N = 337).[vii] A definition of each variable can be found in Table 3.

In general, treated households (those given the PPM version of the survey) are more educated than the control group (those not given the PPM version). For example, around 27% of the treated group received primary education compared to 23% of the non-treated. Mean household size is 6.3 persons in the treated group (with a standard deviation of 2.7), while it is slightly lower in the non-treated group (around 6 with a standard deviation of 2.5). Net annual adjusted household income does not seem to be different among the groups, but is more dispersed in the PPM than in the control sample. Distances between the household's dwelling and a market or the PA do not seem to be different across groups (3.17 Km and 3.09 Km for the PPM and control samples respectively).

Households in the study area are highly reliant on natural resources. Fewer than 2% of interviewed households have sources of income other than agriculture, livestock or the PA. Here, PA related income is defined as the market value of resources extracted from protected areas, whether consumed within the household or sold. Across the different regions, the largest fraction of household income derived from PAs in the last twelve months was 5% in Tengele. Almost no income from use of the PA is reported for Bwindi. As noted above, however, the economic value of access is likely to be larger than income earned since access to the PA offers other indirect benefits such as access to resources at times when other income sources are low or unexpectedly reduced, whilst cultural importance may also be attached to access

Analysis of Contingent Valuation responses

WTA bids were solicited in the local currency (Ugandan Shillings, UGS), but results are presented here in US$[viii]. A total of four zero bids were recorded out of the 675 completed surveys. Where such a zero response to the WTA question was received, clarification was sought from the respondent. All four zero bids were consistent with a zero value attached to PA access and were coded accordingly.

As argued earlier, introducing the PPM is expected to lower mean WTA. We also expect the variance to fall if the main effect is to reduce WTA claims without drastically modifying the overall shape of the distribution. Table 4 shows the descriptive statistics for the two treatments. The mean WTA with the PPM is US$354 per household (with a standard deviation of US$320), compared to US$482 without the PPM (with a much larger standard deviation of US$541). As expected, the PPM lowers the standard error of the mean (US$17 compared to US$29) and results in a lower maximum bid value (US$1,579 compared to US$3,158) than the control treatment. The difference between the mean WTA across the two groups is statistically significant using different tests (see Table 3). One-way ANOVA shows that the equality among groups can be rejected at 1% significance level. However, one-way ANOVA assumes equal population variance. In a Bartlett's chi-squared test, the hypothesis that variances are equal across treatments is rejected at the 1% significance level (χ^2 = 88.5). We therefore applied a non-parametric analogue to the one-way ANOVA, namely the Kruskal–Wallis test. In this case, we once again reject the null hypothesis of equality of means across groups at the 1% significance level (Newbold et al. 2003).

Initial evidence therefore strongly hints at a significant difference between the two distributions of WTA in the expected direction. These results could, however, be driven by outliers. Figure 1 presents two histograms of the claims using all observations in the sample (N=675). Twelve observations are above US$2,000 and they all belong to the control group. More specifically, three individuals report a WTA equal to US$2,105, one individual is willing to accept US$2,526, seven respondents report US$2,631 and one reports a bid of

US$3,157. In order to identify whether the effect of introducing a PPM is more pervasive than simply affecting the upper tail of the distribution, it is useful to run WTA tests on samples from which the “outlier” observations have been eliminated. Even when all 12 potential outlier observations are eliminated, the probability that the mean WTA of the two groups are equal is well below 10% in a two-sided test[ix]. These results also show no hint that the PPM induces respondents to increase their bid in an effort to get authorities to put in place a larger compensation fund.

Since we believe that using the PPM sample claims are closer to true values than control treatment responses, our best estimate of the mean welfare loss to households from access restrictions to protected areas is therefore $354/household/year. In the next section, we will employ a more formal analysis using regression (with sampling weights) and matching estimators, but this will not change this figure substantially. The figure can be interpreted as an upper bound on the per-household “cost of conservation” for access to the protected areas studied. Our estimate of WTA is quite high relative to the mean annual income of $1,011 per household. However, despite obvious difficulties involved in comparing across similar studies undertaken by other authors, we also note that our mean WTA estimate is not out of line with comparable estimates of WTA to forego access to resources in other developing countries. A study in Madagascar in 1995 (Shyamsundar and Kramer, 1996) valued local access forgone form a forest in a similar scenario at $108.34/hsld. A more recent study in Nepal (Shrestha et al., 2007) valued mean local losses from foregone access to Koshi Tappu Wildlife Reserve at $238/hsld. Allowing for a mean annual global inflation figure of 3.8% and purchasing power parity differences between countries this gives adjusted values of approximately $213/hsld. (Madagascar) and $280/hsld. (Nepal)[x].

The analysis so far has focused on direct comparisons of WTA distributions across groups without accounting for underlying differences in the socio-demographic characteristics of the respondents. Table 2 clearly indicated that some differences do exist across groups. A probit regression can provide a diagnostic on whether the probability of belonging to the treated group is correlated with one or more household characteristics. This shows that the treatment and control groups are statistically different in the size of their arable land holding (see Table 5). However, the size of the effect on the probability of being in the treated group does not seem substantial. For example, a one standard deviation change in hectares of land around the mean will increase the probability of being in the treated group by only 5 percentage points. Note that none of the other variables which might indicate an undesirable selection effect operating (such as distance to market, or protected area income) in being in the PPM treatment are statistically significant. Given this, the next section is devoted to a more formal analysis of WTA by regressing it on a dummy variable TREATED (associated with the PPM sample) and a set of controls in order to account for variables that help determine household WTA.

Econometric analysis, basic models

A WTA bid curve was estimated using different specifications (Table 6). These provide more formal tests of the impact of the PPM on WTA, and more precisely identify the determinants of the costs of conservation measures. The basic econometric specification is given by the following equation (4):

$$WTA_i = \beta_0 + \beta_1(TREATED_i) + f(X_i) + \varepsilon_i \qquad (4)$$

where TREATED is the dummy variable taking the value of 1 if the individual belong to the group subject to the PPM and 0 otherwise; $f(X_i)$ includes the following set of control variables: education level, household size, the size of the household arable land holding, the value of total assets owned by the household, household income and an estimate of PA-related income, distance to nearest market, distance to PA, and three site dummies to represent un-observed variations in access benefits unique to each PA. The list of variables included in our survey is thought to cover the variables most likely to explain the compensation required by households. The loss of access to resources within PAs might depend on household's ability to collect resources from the PA (proxied here by household size), on alternative (substitute) sources of income (measured here by the amount of agricultural land cultivated by the household), and on how much income the household reports that it collected from being able to access the PA in the past. The area of agricultural land owned by a household might also partly determine the damages they would suffer from crop raiding from wild animals, thus the sign on this variable is hard to determine a priori. We also included variables measuring how far the household lives from the nearest market, and how far they live from the PA (as a measure of accessibility), since other studies have shown these to be important drivers of income from protected areas (Vedeld et al., 2004; Foerster et al, 2011; Sukanda et al, 2010), whilst distance to market may also explain the ability to substitute away from PA income. Dummies are included to represent either the un-observed characteristics of each sampling location (site) or different management regimes (since to each site corresponds a different governance regime), which might impact on the value of access to PAs. The omitted site is Queen Elizabeth National Park. Standard errors have been adjusted for clustering within village to account for possible intra-correlation at village level in the WTA responses (see, e.g., William, 2000), whilst we use sampling weights based on (i) the probability of a particular village being selected for sampling, (ii) the probability of each household in any village being selected and (iii) the probability of being in the control group rather than the treated group. Including all the variables above without imputing missing values would have reduced the sample to 470 observations. This is because the variable "distance to nearest market" has many missing values. Excluding the distance variable from the regressors leaves 620 usable observations. For this reason, the OLS model in the third column of Table 6 excludes the distance from market variable – this is re-introduced in Table 7 as part of robustness checks.

In order to verify whether PPM has a significant effect on the value of bids the following null hypothesis is tested:

$$H_0: \beta_1 = 0 \,. \quad (5)$$

where β_1 is the parameter estimate on the variable TREATED. The first column of Table 6 shows the results of a simple OLS specification in which the bid is regressed against the variable TREATED without controls. The null hypothesis in (5) is rejected at 1% significance level. The second column shows the results of a model that controls for individual variables (education, income, etc.) while in the third column location variables have been added. Table 6 is useful because it clearly shows that the coefficient on the variable TREATED is negative and strongly significant no matter what controls have been added, confirming the previous findings that the PPM reduces WTA bids. In addition, we reject the null hypothesis at the 1% significance level that the PPM and control group surveys yield the same WTA claims. After controlling for confounding factors and systematic differences among the groups, a household in the PPM treatment group would bid on average approximately US$119 less than a comparable household in the control group. This analysis confirms that the PPM has a considerable effect on stated WTA.

Households with a larger area of agricultural land to cultivate state significantly higher WTA amounts. Respondents with more agricultural land to cultivate may be considering compensation in terms of potential reparations against losses from crop raiding, rather than just the opportunity cost of reduced access to the PA. Indeed, crop raiding was an issue in all sites and in participatory exercises was always amongst the top three qualitative costs of living adjacent to the PA. Annual total household income and asset variables do not have a statistically significant relationship with WTA. However, annual income from the protected area has a positive and statistically significant impact on WTA, which makes intuitive sense. The number of occupants in a household is correlated with lower WTA bids, although the effect is not statistically significant. No significant effects are observed for distance to the protected area, a result that could be attributed to insufficient variability in the data for this variable.

The last rows provide the mean predicted value of treated and control groups. These values have been estimated by taking the average of the linear prediction of each regression when TREATED takes the value of 1, yielding the mean predicted WTA of the treated, and when TREATED takes the value of 0, providing the mean predicted WTA of the control group. As expected, adding controls lead to a lower value of the (mean predicted) WTA. The value corresponding to the treated group is our preferred estimate of the cost of conservation of the average household and is in the range of US$340-$370.

Tests of misspecification, omitted variables and robustness.

A battery of tests was run to verify that our findings are robust under different hypothesis, assumptions and specifications. These are reported in Table 7. The severity of multi-collinearity among explanatory variables was checked using the Variance Inflation Factor (VIF) comparison. VIF estimates for the regressors showed that there were no serious multi-collinearity problems. All the variables have a value lower than 2.5. [xi] Although the low R-squared of our basic models does not invalidate the mean WTA measure, it can be argued that it may have consequences in term of the statistical significance of the coefficient on TREATED if relevant variables have been omitted. In our study households were randomly assigned to the PPM treatment. A formal test of omitted variables (known as the RESET test) can be conducted by adding the powers of the predicted WTA to the set of regressors and checking for the statistical significance of these power terms. The null hypothesis of no omitted variable cannot be rejected at any level standard level of significance also when adding up to three powers of predicted WTA ($F(3, 605) = 168$), *p-value* =0.17). The RESET test also suggests that a linear model is the correct specification.

As a further test of specification of our WTA model, a Box-Cox model that includes linear and log-linear models as special cases was run:

$$(WTA^{\theta} - 1)/\theta = \beta_0 + \beta_1(TREATED_i) + f(X_i) + \varepsilon_i \quad (6)$$

and the results are reported in the first column of Table 7. The Box-Cox model with general θ is difficult to interpret and use. However, the TREATED variable is negative rejecting the null hypothesis (4) once again, and the estimate of θ is 0.06 is not statistically significant, which gives more support for a log-linear model ($\theta = 0$) than the linear model ($\theta = 1$).

Because of this, the second column of Table 6 reports the result of a log-linear WTA model (see Cameron and Trivedi, 2009). The log-linear specification does not affect the previous findings. For the sake of simplicity we therefore keep a linear specification for what follows. We also allow for the true distribution of WTA amounts being censored at zero by using Tobit regression (Halstead et al., 1991). The third column of Table 7 shows that the effects of TREATED and the rest of the variables do not vary significantly when modelling the data using such a Tobit regression.

What if the conditional tests just run are affected by the presence of a few individuals who mistakenly over state their WTA? The fourth column of Table 7 shows the effect of TREATED on WTA when dropping the single observation in which bid was greater than US$ 3,000 (as we did in the simple unconditional test). Dropping that observation has the expected effect of lowering the difference between the WTA stated by the treated and non-treated group by about US$5: an individual participating in a PPM survey states a claim that is on average US$84 smaller than somebody answering the control survey (not a substantial difference from our baseline difference of US$89). The fifth column brings this concept a bit further and considers as “outliers” all the claims in the regular scenario that are higher than the largest claim in the PPM group, i.e., we drop all the bids higher than US$1,900. Even in the very unlikely scenario that all these observations are “outliers”, the coefficient on the PPM is still negative and significant at 5% level. Rejecting these eleven highest claims in the control group halves the difference in WTA between the two groups but it still averages US$38 suggesting that part of the difference might be inflated because of these 11 observations.

The sixth column of Table 7 extends the concept of outliers to all observations that may have unusual "influence" in determining coefficient estimates. Influential observations can be detected using several measures. A common measure is dfits (see Cameron and Trivedi, 2009). Large absolute values of dfits indicate an influential data point. A rule of thumb suggested by Cameron and Trivedi (2009) among others is that the observations with the absolute value of dfits greater than 2*(square root of (number of parameters/sample size)) may be worthy of further investigation. The model presented in column six shows that even eliminating all these observations does not change our key result on the effects of the PPM treatment. The next column shows the estimates from OLS regression when distance to market is added as an independent variable. Although we have formally tested for omitted variables, distance to market might be an important determinant of WTA, but this variable was dropped from baseline models because of many missing values. When included in the regression, the coefficient on distance to market is not statistically significant. More importantly, adding this variable to the model does not have any significant impact on the other coefficient estimates, and does not affect the conclusions we have already reached on the effect of the PPM. [xii]

Finally, we have used a different econometrics technique, propensity score matching estimator (Leuven and Sianesi, 2003). The propensity score was computed by estimating a probit regression of being TREATED on the same covariates used in the previous regressions. This estimator then compares the WTA of households who based on our covariates have a very similar probability of receiving treatment (similar propensity score). The difference between WTA between households who received the PPM and who did not can be interpreted as average treatment effects. None of the observations fall off the region of common support when matching on 2, 4 or 6 nearest neighbours. The difference between treatment and control group varies between 121, 97 and 100, respectively and is always statistically significant at 1% level ; values that are comparable to the OLS estimates. Table 7 shows the coefficient of one of the models estimated where the observations where matched on propensity scores paired to the closest 4 matches[xiii]. The difference between treated and un-treated WTA is $97 in this matching model.

V. Discussion and Conclusions

This paper investigates the use of a Provision Point Mechanism (PPM) to estimate the opportunity costs of conservation actions to local people, using Willingness to Accept (WTA) contingent valuation approach. As we noted earlier, researchers have become reluctant to use WTA approaches even when the distribution of property rights (whether *de facto* or *de jure*) suggests that compensation-based welfare measures are more appropriate than payment-based measures. This has been attributed to the tendency of WTA questions in hypothetical markets to lead to the over-statement of true losses, and to encourage protest bidding. Our main focus in this paper was a methodological one, to devise a strategy (the PPM) which reduces the problems of applying WTA-measures to estimates of the costs of conservation.

By extending the basic idea of the PPM from a willingness to pay context, we show that a WTA-PPM can significantly reduce the magnitude of mean hypothetical WTA in a way consistent with theoretical predictions that the PPM improves demand-revelation. Empirically, the most notable difference between the distributions of claims under the two payment mechanisms are at the upper end of the distribution, although our analysis demonstrates that use of the PPM has a statistically significant effect throughout the distribution. The significant decrease in the number of claims that could be considered protest bids or outliers suggests that the PPM mechanism could facilitate the applications of WTA designs in contingent valuation (or, indeed, in choice modelling) when compensation is the appropriate welfare framework to adopt.

A wide range of factors, including historical variation in conflicts and participation, will impact on peoples' perceptions of the value of losses in access to PAs. A more complete understanding of what drives differences in perceptions of losses across people would need to take into account this wider range of factors. However, from a sample selection perspective, the variation of the impacts of such issues are likely to be homogeneous between the treatment groups (PPM and no PPM), and thus not likely to bias the findings of our study in terms of the effects of the PPM.

The choice of the WTA approach is policy relevant in that compensation for loss of access to forest resources (based on rights to use) is becoming more widely discussed in policy and management circles, especially conserving forests in the context of carbon financing and REDD+. Here we use the term "compensation" in the sense of providing some benefit specifically to offset loss of access (opportunity cost), rather than to offset costs such as crop raiding due to proximity to a PA, be they at an individual or group e.g. community level. In terms of local level compensation, a tourism revenue sharing program was explicitly set up in Uganda to provide community level benefits from the national parks to offset such costs of living next to protected areas as an incentive to promote good will towards the PA. Anecdotal evidence and research in the grey literature suggests that what heterogeneous park adjacent households need are interventions that directly affect the most high risk (in terms of dependence on illegal PA use) households, in terms of offsetting opportunity costs of reduced resource access through effective implementation of management regulations. The pre-testing and focus group discussion work carried out in our study in every village prior to survey administration helped to develop a comprehension of the plausibility of the compensation scenario.

For the case of protected areas in Uganda, use of the use of a WTA format is more appropriate than asking a WTP question. Local people living in and around the four protected areas (PAs) in Uganda have depended on access to these areas for fuelwood, bushmeat and non-timber forest products, particularly at times of the year when other sources of subsistence are very limited. Exploiting such resources is a livelihood strategy which these very poor households employ irrespective of current legal restrictions. Since international treaties such as the Convention on Biological Diversity have stated that use of protected areas as a way of safeguarding biodiversity should not come at the cost of perpetuating or worsening poverty, finding ways of measuring the true opportunity costs of conservation to communities in developing countries is important. Determinants of WTA here were found to include access to agricultural land and household size; whilst mean compensation demanded also varied to a degree across protected areas. This variation across PAs could be due to a variation in both the productivity of the four different ecosystems, and the governance arrangements currently in place in each.

We have also argued that measuring the financial cost of loss of access to PAs would under-estimate the welfare loss to such households, and indeed found that our WTA estimates were much higher than the financial value of lost access, which averaged US$21/household, varying from almost zero (Bwindi) to $44 (Tengele), with higher absolute values for richer households (Bush et al. 2011). Whilst there is good reason to suppose that the WTA estimate in the PPM treatment is still biased upwards due to hypothetical market effects, a strong argument can still be made that the true costs of loss of access to PAs is greater than $21 per household for the reasons explained above.

To date, much of the environmental economics literature has been devoted to estimating the benefits of conserving the world's biodiversity and its most valuable ecosystems (Kontoleon et al., 2007). Establishing protected areas is a dominant means of achieving such conservation world-wide. However, the costs of conserving biodiversity in developing countries can fall disproportionately on poor households. As an imperative the full range of costs of conservation need to be quantified, and ways sought to mitigate these, if effective levels of conservation are to be achieved without exacerbating poverty. On the basis of work reported here, we suggest that a WTA approach incorporating a PPM is worthy of further investigation as part of the economist's toolbox. However, many important methodological questions remain unanswered. Future experimental work could investigate the role of the PPM in mitigating hypothetical bias relative to its effect on pure strategic behaviour. It is also unclear how important the use of a PPM is in moderating protest bids, since very low levels of protesting were found in our survey. Finally, the extent to which respondent behaviour is motivated by an attempt to influence the level of an eventual compensation fund is also an issue that should be investigated

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Table 1 Data collection sample frame by protected area and treatment application

Protected Area	**Bio Type**	**Governance Type**	***No of Households in survey***	***Treatment applications***		
				With PPM	***Without PPM***	***No Bid***
Queen Elizabeth National Park	Savannah Woodland & Grassland	Strict National Park (no community co-management)	*329 (11 communities)*	*167*	*162*	*0*
Bwindi Impenetrable Forest National Park	Afromontane Forest	National Park with some community co-management	*232 (8 communities*	*114*	*117*	*1*
Budongo Forest Community Forest Reserve (Masindi District)	Tropical High (Closed Canopy) Forest	Forest on private land, community owned and managed	*60 (2 communities)*	*31*	*29*	*0*
Tengele Forest, Collaborative Forest Management	Tropical High (Closed Canopy) Forest	Forest Reserve (public land), with community co-management	*59 (2 communities)*	*26*	*29*	*4*
		Total households	***680***	***338***	***337***	***5***

(Data collected January to July 2006)

Table 2 Sample descriptive characteristics (Variable descriptions in table 3)

Variable	N	Mean	St Dev	Min	Max	%
		Treated with PPM				
WTA	338	354	320	0	1,579	-
EDGROUP1	92	-	-	-	-	27.3
EDGROUP2	90	-	-	-	-	26.71
EDGROUP3	75	-	-	-	-	22.26
EDGROUP4	80	-	-	-	-	23.74
HHTOTALO	338	6.33	2.68	1	15	-
AGRILAND	314	5.24	9.38	0	80	-
NTHIUS	338	658.77	2,346.64	0	36,812.79	-
NPAIUS	338	5.67	23.93	0	263.16	-
ASSETVALUE	338	189,401	769,871	0	8,222,000	-
DISTMARK	264	3.17	3.14	0	10	-
DISTPA	334	1.55	1.74	0	14	-
		Not treated with PPM				
WTA	337	482	541	0	3158	
EDGROUP1	75	-	-	-	-	22.19
EDGROUP2	77	-	-	-	-	22.78
EDGROUP3	95	-	-	-	-	28.11
EDGROUP4	91	-	-	-	-	26.92
HHTOTALO	337	5.98	2.55	1	16	-
AGRILAND	314	3.99	5.56	0	55	-
NTHIUS	337	613.87	1879.76	0.00	27500.00	-
NPAIUS	337	36.69	304.15	0.00	4547.37	-
ASSETVALUE	337	145,186	689,657	0	10,730,000	-
DISTMARK	254	3.09	3.45	0	10	-
DISTPA	333	1.55	1.67	0	14	-

Table 3 - Variable descriptions for determinants of bid value

Variable	**Description**
WTA	Willingness to Accept variable in US dollars per household
TREATED	The group treated with PPM takes value of 1, while group not subject to PPM takes value of 0.
EDGROUP1	Education level that corresponds to no formal education
EDGROUP2	Education level that corresponds to primary education
EDGROUP3	Education level that corresponds to secondary education
EDGROUP4	Education level that corresponds to tertiary education
HHTOTALO	Household total occupants; total number of individuals in the household irrespective of age/sex class
AGRILAND	Agricultural land (Ha.); area of agricultural land cultivated by the household (arable land holding)
NTHI	Net annual total household income
NPAI	Net annual protected area income
DISTMARK	Distance to market (Km); distance from households dwelling to travel to nearest market
DISTPA	Distance to PA (Km); distance from household's dwelling to the protected area boundary
BWINDI	Dummy variable that takes the value of 1 if the respondent lives in Bwindi site, = 0 otherwise
BUDONGO	Dummy variable that takes the value of 1 if the respondent lives in the Budongo site, = 0 otherwise
TENGELE	Dummy variable that takes the value of 1 if the respondent lives in the Tengele site, = 0 otherwise

Table 4 Impacts of the PPM treatment on mean WTA and statistical tests of WTA differences across treated and control groups

Groups	N	Mean	St Dev	Min	Max
Treated with PPM	338	354	320	0	1,579
Control	337	482	541	0	3,158

One-way ANOVA

Source	SS	df	MS	F	Prob > F
Between groups	2770480.52	1	2770480.52	14.05	0.0002
Within groups	132691875	673	197164.748		
Total	135462356	674	200982.724		

Bartlett's test for equal variances: chi2(1) = 88.4971 Prob>chi2 = 0.000

Kruskal-Wallis non-parametric equality of population rank test

Groups	N	Rank sum
Treated with PPM	338	107688
Control	337	120462

chi-squared = 6.698 with 1 d.f. probability = 0.0097

Notes: (i) all monetary amounts are in US dollars.

Variable definitions and unit of measures given in Table 3

Table 5 Probit analysis of belonging to the treated group

DEP VAR = TREATED	Coefficients	Standard errors
EDGROUP	0.138	(0.084)
HHTOTALO	-0.012	(0.029)
AGRILAND	0.016*	(0.009)
NTHI in thousand US$	-0.004	(0.024)
NPAI in thousand US$	-2.575	(1.866)
DISTMARKET	-0.001	(0.020)
DISTPA	0.023	(0.034)
BWINDI	0.210	(0.218)
BUDONGO	0.051	(0.219)
TENGELE	0.338	(0.233)
Constant	-0.404*	(0.227)
N	470	

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

Variable definitions given in Table 3

Table 6 Econometric analysis of determinants of WTA bid value(basic models)

VARIABLES	(1) bid	(2) bid	(3) bid
Treated	-82.555***	-95.634***	-95.722***
	(22.871)	(25.015)	(24.268)
EDGROUP		24.508	27.144
		(18.251)	(16.419)
HHTOTALO		4.257	1.218
		(7.683)	(6.984)
NTHI in thousand US$		-3.468	-3.199
		(9.068)	(8.707)
NPAI in US$		0.238***	0.247***
		(0.046)	(0.036)
ASSETVALUE		0.003	0.010
		(0.016)	(0.018)
AGRILAND		5.125**	5.460**
		(2.028)	(2.238)
DISTPA			4.791
			(9.687)
BWINDI			10.829
			(44.619)
BUDONGO			26.669
			(32.892)
TENGELE			-136.167***
			(27.735)
Constant	453.417***	320.711***	324.481***
	(30.197)	(49.596)	(54.344)
Observations	675	628	620
R-squared	0.010	0.052	0.064
Mean predicted WTA of treated (US dollars)	370.8	342.1	338.7
Mean predicted WTA of control (US dollars)	453.4	431.8	427.3

Note: Standard errors clustered at village level, and are shown in parentheses. Key: *** p<0.01, ** p<0.05, * p<0.1 Mean predicted WTA of the treated is the linear prediction of WTA when TREATED takes the value of one, while mean predicted WTA of the control group is the linear prediction of WTA when TREATED takes the value of zero. Variable definitions are given in Table 3. Sampling weights were used to account for selection probability. See text for details.

Table 7 Robustness checks on regression models of determinants of WTP

VARIABLES	Box Cox	Log-linear	Tobit	OLS Eliminating bids>US$3000	OLS Eliminating bids>US$1900	OLS Eliminating abs(dfits)<2*threshold	OLS adding distance to market	Nearest Neighbour Matching
Treated	-0.348	-0.210**	-97.172***	-90.205***	-53.036*	-82.821***	-88.104***	-97.03***
		(0.079)	(32.836)	(24.105)	(27.074)	(26.905)	(26.256)	
EDGROUP	0.19	0.100**	26.351	25.982	29.811*	32.838**	25.960	Yes
		(0.043)	(22.371)	(16.548)	(14.387)	(15.496)	(18.530)	
HHTOTALO	-0.004	0.008	1.502	1.952	6.700	4.148	-2.890	Yes
		(0.014)	(7.437)	(6.958)	(5.164)	(5.477)	(8.840)	
NTHI in thousand US$	-0.003	-0.014	-2.994	-2.660	-0.880	-14.717***	-3.135	Yes
		(0.019)	(8.196)	(8.649)	(8.469)	(4.164)	(8.505)	
NPAI in US$	0.001	0.000***	0.247***	0.249***	0.029	0.050	0.250***	Yes
		(0.000)	(0.079)	(0.035)	(0.031)	(0.052)	(0.032)	
ASSETVALUE	-0.001	0.000	0.010	0.011	0.012	0.013	0.019	Yes
		(0.000)	(0.017)	(0.018)	(0.018)	(0.019)	(0.074)	
AGRILAND	0.021	0.014**	5.552**	5.221**	4.664**	7.148***	5.217*	Yes
		(0.005)	(2.422)	(2.211)	(1.966)	(1.806)	(2.488)	
DISTPA	0.005	0.013	4.347	4.891	2.843	4.705	3.898	Yes
		(0.025)	(10.275)	(9.704)	(9.241)	(7.985)	(12.723)	
DISTMARKET							4.282	Yes
							(8.496)	
BWINDI	0.008	0.002	3.896	14.826	22.712	10.937	96.204	Yes
		(0.119)	(44.044)	(43.946)	(43.524)	(41.126)	(76.160)	
BUDONGO	0.047	0.012	25.727	31.492	-24.553	-52.262	32.396	Yes
		(0.124)	(77.695)	(31.654)	(35.513)	(32.808)	(32.326)	
TENGELE	-0.548	-0.377***	-137.055***	-131.442***	-99.790***	-122.988***	-170.019***	Yes
		(0.093)	(51.611)	(26.070)	(27.150)	(23.952)	(37.240)	
Constant	6.39	5.316***	325.882***	315.583***	246.844***	284.576***	337.537***	Yes
		(0.131)	(77.033)	(51.324)	(45.930)	(47.281)	(56.043)	
θ	0.064							
	-0.043							
σ			413.9***					
			-34.81					
Number of matches								4
Number of observations off the common support								0

Observations	616	616	620	619	609	611	470	620
R-squared		0.067		0.065	0.053	0.061	0.075	
Mean predicted WTA of the treated		238.4	335.8	338.8	342.5	339.9	354.1	347.6
Mean predicted WTA of the control		285	425.8	422.1	380.3	405.8	442.1	441.6

Note: Standard errors clustered at village level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Mean predicted WTA of the treated group is the linear prediction of WTA when TREATED takes the value of one, while mean predicted WTA of the control group is the linear prediction of WTA when TREATED takes the value of zero. The values appearing in the second column referring to the log-linear model refers to the exponential transformation of the predicted WTA. “Yes” in the last column indicates the variables over which the matching occurred.

Sampling weights were used to account for selection probability. See text for details.

Variable definitions given in Table 3.

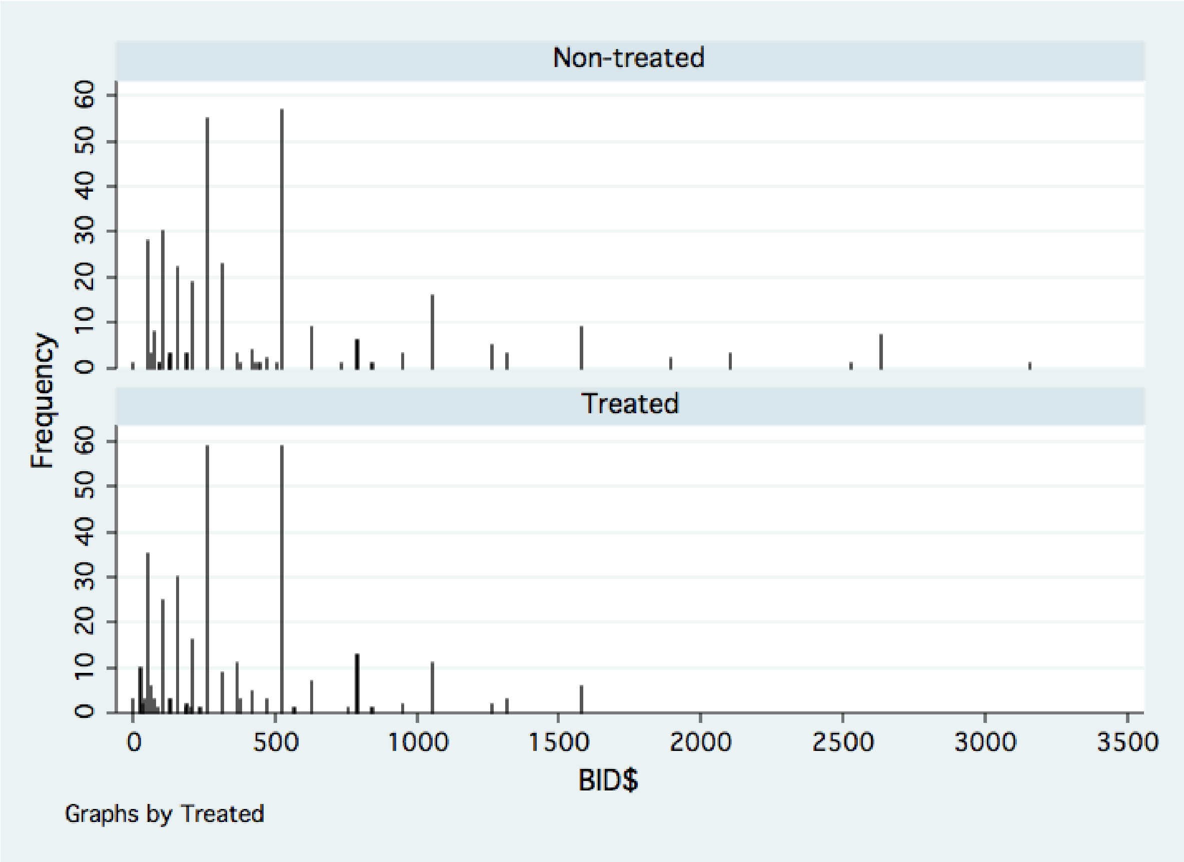

Figure 1 – Distribution of WTA bids among treated and non-treated groups

ENDNOTES

[i] In addition to the loss of access, communities also face considerable threats from crop raiding animals residing within protected areas. This can impose significant costs on local communities. In some cases these losses may be offset by the benefits of bushmeat supply (Hulme and Infield, 2001). Losses due to crop damages was reported by participants of our study as a significant issue (Bush and Mwesigwa, 2007).

[ii] Without loss of generality, $V_i = v(Y;X)$ is taken to represent the actual value lost by an individual following the imposition of access restrictions. This value can depend on a number of determinants (Y) such as the proximity to the area, household access to substitute resources or markets, etc; and on households characteristics (X). Since some individuals may plan to violate those restrictions and continue with some level of illegal resource extraction, V_i (and therefore the individual's claim on the compensation fund) may not represent the full value of current resource usage.

[iii] Of course, individuals also could have claims of losses that are genuinely close to infinity if the PA's provide life sustaining resources and have no substitutes. In this case, compensation fund would not be sufficient to pay claims and the no-policy outcome would be efficient.

[iv] In the WTA context, the money back rule corresponds to the fact that all claims are ignored when the PP is exceeded (i.e. all claims are "returned"). The rebate rule is the proportional formula determining that households would receive a share of the unclaimed funds if the sum of claims is less than the amount of funds available.

[v] It is worth noting that if respondents think that the size of the compensation fund is endogenous to the responses in the survey, they might increase their bids in an attempt to increase the size of the compensation fund. Which of the two opposite effect (if any) dominates is therefore an empirical question.

[vi] Note that in the regression results reported in Tables 6 and 7, we use a weighed regression procedure based on sampling probabilities.

[vii] From here on, we will refer to the "treated" group as the group of respondents that were subjected to PPM, and the "control" as the group that was not treated with the PPM.

[viii] Exchange rate in 2008 was 1900 UG shillings = 1 US$.

[ix] One-way ANOVA reports F =3.78 with probability greater than F = 0.052. Kruskal-Wallis reports Chi-squared equal to 3.373 with probability = 0.066.

[x] 3.8% is the IMF mean rate of inflation since 1980, PPP conversions are made using the World Bank GDP (PPP) per capita values for 2008.

[xi] A VIF greater than 10 is usually taken as indicative of a problem (Kennedy 2003).

[xii] A Tobit model similar to this OLS, including distance to market, was estimated too. As expected, the results between OLS and Tobit are identical.

[xiii] [xiii] T-tests were run to check differences before and after the matches. The null hypothesis that the mean values of the two groups do not differ after matching cannot be rejected for any variable. Moreover we checked for "bad matches" as follows. We impose the condition to discard those treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls. This yields leaving out 3 observations. However, the results do not change substantially from the one reported in the text. Propensity score matching has been estimated using the Stata command –psmatch2-. Notice that we have always found a statistically significant difference between treatment and control group using different matching techniques, including matching on covariates instead of propensity scores using the –nnmatch- command (Abadie et al. 2004).