Measuring the Local Costs of Conservation: A Provision Point Mechanism for Eliciting Willingness to Accept Compensation

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ABSTRACT. *Protected areas are employed worldwide as a means of conserving biodiversity. Unfortunately, restricting access to such areas imposes opportunity costs on local people who have traditionally relied on them 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.* (JEL Q51, Q56)

I. INTRODUCTION

The establishment of protected areas 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 bushmeat hunting and the collection of fuelwood or timber for construction, and by restricting the conversion of land into agriculture.

Unfortunately, properly enforced land use restrictions impose potentially high 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 nontimber forest products (NTFPs) 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 NTFPs. Poor enforcement by underresourced management authorities often translates on the ground into illegal exploitation of protected areas, especially those immediately adjacent to communities. Indeed, the use of protected area resources such as fuelwood has increased dramatically in Uganda in recent years (NEMA 2001; Bush et al. 2004; Blomley et al. 2010). Without access to protected area resources or an alternative source of revenue, many rural households face increased levels of impoverishment.[1]

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

[1] 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 were reported by participants of our study as a significant issue (Bush and Mwesigwa 2007).

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a management regime that promotes their acceptance by local communities. Information on the costs of lost access is essential to the development of mitigation programs at the local level. 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 while 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 protected areas under a (hypothetical) scenario in which local users would receive monetary compensation for those losses. We aim to quantify the economic opportunity cost of conservation to communities adjacent to four protected areas in western and southwestern Uganda. A broad household socioeconomic survey was conducted that included a contingent valuation (CV) component to estimate the market and economic values of goods currently taken from the adjacent protected area.

While 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 such resources to local people (Campbell, Luckert, and Scoones 1997; Godoy et al. 1997, 2002, 2009; Vedeld et al. 2004). Access to protected areas can be viewed by local users as providing an insurance policy against fluctuations in agricultural incomes both within and between years (Pattanayak and Sills 2001). Access also has cultural and social values that are not captured by market prices for products such as fuelwood and bushmeat. Finally, losses of nontraded (subsistence) resources also impose costs on local people, due to absence of substitutes in local markets, or poor market access (Geist and Lambin 2002; Soares-Filho et al. 2006). In such circumstances, CV methods are useful for measuring the economic value of the loss of access by local households to protected areas.

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 therefore a more appropriate measure of costs than willingness to pay (WTP) to gain access to the protected area. Despite WTA often being the more appropriate measure, WTA studies are comparatively rarely performed in CV, since they are suspected of systematically overstating welfare losses (Rowe, d'Arge, and Brookshire 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, Schulze, and Poe 1999; Rondeau, Poe, and Schulze 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 CV. We show that the PPM significantly reduces the number of high-value outliers and the resulting estimate of average welfare loss.

II. MEASURING WTA USING CV: THE PPM

The use of CV in developing countries is now widespread (Whittington 2002, 2004). In particular, CV has an established history for the valuation of environmental amenities arising from national parks in developing countries (Kramer et al. 1992; Kramer, Sharma, and Munasinghe 1995; Willis, Garrod, and Yiew 1998; Whittington 1998; IIED 2003). Mekonnen (1997) applied WTP elicitation formats to obtain the economic value of community forestry in Ethiopia; Lynam, Campbell, and Vermeulen (1994) valued trees on communal lands in Zimbabwe; and Ruitenbeek (1992) valued rainforests in Cameroon, also using a WTP scenario. In contrast, Smith et al. (1998) used a WTA approach to analyze the potential compensation payments required to induce land use changes among farmers in Peru. Kramer et al. (1992) and Kramer, Sharma, and Munasinghe (1995) estimated the compensation payments required to modify land use by local farmers for watershed protection in Madagascar.

As is the case for studies from developed countries, the WTP format has indeed been largely favored 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 Ruström (2005) discuss the historical sidestepping of WTA measures, attributing it 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 PPM can reduce WTA overstatement. In its usual configuration, the PPM is a mechanism to facilitate voluntary contributions toward 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, Schulze, and Poe (1999).

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 or enforced). If the sum of claims is less than or equal to 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, so enforcement is put in place, compensation is paid, and people receive exactly the individual amount of their claim; and
- if $\sum_{j=1}^{N} B_j < PP$, the sum of claims is less than the funds available for compensation.

In the last 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. Thus, the additional payment to individual i is

$$\left(B_i \Big/ \sum_{j=1}^{N} B_j\right)\left(PP - \sum_{j=1}^{N} B_j\right).$$

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 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[2]

[2] Without loss of generality, $V_i = v(Y;X)$ is taken to represent the forgone benefits of protected area use by a household 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 household characteristics

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

It is relatively straightforward to extend the game theoretic equilibrium prediction for a PPM in the WTP context (Bagnoli and Lipman 1989; Rondeau, Schulze, and Poe 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:

$$\sum_{j=1}^{N} B_j > PP; \text{ and} \quad [2]$$
$$\left(\sum_{j=1}^{N} B_j - PP\right) > (B_i - V_i)\forall_i.$$

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 PP. This is the meaning of the second item in 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 more interesting 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

$$\sum_{j=1}^{N} B_j = PP, \quad [3]$$

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 his 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 among 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 his individual value (the value to an individual of loss of access to the pro-

(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.

tected area) is an equilibrium only 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 *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, an open-ended 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, the optimal claim of an expected utility maximizer should indeed 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.[3]

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, Schmidtz, and Walker (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).[4]

Isaac, Schmidtz, and Walker (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 make only 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).

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

[4] 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 "refunded"). 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.

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 it can radically curtail free-riding, provide a more efficient level of public good provision, and improve demand revelation (Rondeau, Schulze, and Poe 1999; Cadsby and Maynes 1998; Rose et al. 2002; Messer, Schmit, and Kaiser 2005). In perhaps the most extensive study of the relationship between contributions and underlying value, Rondeau, Poe, and Schulze (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 were, 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) 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, Rondeau, and Poe 2003).

Despite promising experimental evidence that the PPM results in more efficient voluntary provision of public goods, few have introduced it to CV surveys. Murphy, Stevens, and Weatherhead (2005) compare real and hypothetical payments to a land conservation organization 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 *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, Schulze, and Poe 1999; Rondeau, Poe, and Schulze 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. While 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, Schulze, and Poe 1999), 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 *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, in other words, 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.[5]

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 CV survey was administered in villages located in the vicinity of four different protected areas in Uganda. 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, afromontane 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 protected areas with similar historical levels of illegal park use, as well as differing examples of existing governance strategies to resolve illegal use issues. The sampling universe at each site is defined as all probable users of the protected area. In practice this meant we focused on parishes (Local Council II) with a political boundary bordering the park. The boundary defining potential users was identified as households residing at a maximum of two hours' walk to the protected area, that is, within around 5 km of the protected area. Beyond this distance, households were unlikely to view the protected area as a potential resource for exploitation (Bush et al. 2004). At the site level, stratification reflected socioeconomic 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) was then randomly selected from within each parish. For each selected village, approximately 30 households were randomly selected from a list of all households, stratified by wealth level. The sampling was proportional to the size of wealth groups within the population, and half were given the PPM treatment. Heads of households were selected to act as respondents. However, all available members of the household were usually 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.[6]

The CV data were collected during the period January–July 2006, with the survey team typically spending three days per village and six days in a parish. For example, in Queen Elizabeth National Park the total time spent on the survey was 36 days. Surveying was

[5] 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 effects (if any) dominates is therefore an empirical question.

[6] Note that in the regression results reported in Tables 6 and 7, we use a weighed regression procedure based on sampling probabilities. Selection probabilities have been computed as follows: (Probability of selecting village/parish v) $\times$ (Probability of selecting household in each village v) $\times$ (Probability of being in treated group (or being in control group for nontreated)).

conducted by trained research assistants, themselves from rural parts of western and southwestern Uganda, who were supervised full time by Bush. 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 revisited as part of a wider study funded by CARE International on rights and equity in protected area management. Fuelwood was the main resource extracted by local households, followed by timber for construction, and bushmeat. At all sites, alternative sources of such goods were few and typically limited to natural forest resources or woodlots not privately owned or protected (Bush 2009).

Policy Scenario

The payment scenario in the survey sets up a framework for the implementation of a 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 protected area, 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 overuse 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 to forgo the benefits from the protected area for one year. This provides a basis for comparing the results of the second treatment, which included 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 an 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. While it would have been interesting to include alternative payment elicitation methods in the present study, resource constraints and pretesting results meant that we focused 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 is 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 pretesting was carried out at Queen Elizabeth National Park and involved a week of training in the survey method 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. Pretesting was conducted to identify weaknesses in the presentation and comprehension of the questionnaire by both the enumerators and the respondents, and to determine the most appropriate response formats for 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 five enumerators about three days to complete the interviews in each community. 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, among 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 neighbors or other local key informants to divulge this information, so information could be gathered indirectly. An estimate of total household income 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 protected area and non-protected-area goods. Further information on the survey procedures and market price results is provided by Bush, Hanley, and Rondeau (2011).

IV. RESULTS

A total of 690 households were contacted. Ten questionnaires were discarded as incomplete. A further five questionnaires were discarded because respondents did not want to answer the WTA question, leaving 675 usable surveys included in the analysis (Table 1).

TABLE 1
Data Collection Sample Frame by Protected Area and Treatment Application

Protected Area	Bio Type	Governance Type	Number of Households in Survey	Treatment Applications: With PPM	Without PPM	No Bid
Queen Elizabeth National Park	Savannah Woodland and Grassland	Strict national park (no community comanagement)	329 (11 communities)	167	162	0
Bwindi Impenetrable Forest National Park	Afromontane Forest	National park with some community comanagement	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 comanagement	59 (2 communities)	26	29	4
Total Households			680	338	337	5

Note: PPM, provision point mechanism.

Descriptive Analysis

Table 2 shows descriptive statistics of the variables collected by group: Panel A shows statistics related to households treated with the PPM ($N = 338$), while Panel B shows statistics for the control group of respondents ($N = 337$).[7] 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 nontreated. 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 nontreated group (around 6, with a standard deviation of 2.5). Net annual 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 protected area 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 protected area. Here, protected-area-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 protected areas in the last 12 months was 5% in Tengele. Almost no income from use of the protected area 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 protected area offers other indirect benefits such as access to resources at times when other income sources are low or unexpectedly reduced, while cultural importance may also be attached to access. Since extraction of resources from protected areas is also illegal, we cannot exclude the possibility that households underreport their use or the value of access to them.

Analysis of CV Responses

WTA bids were solicited in the local currency (Ugandan shillings, UGS), but results are presented here in U.S. dollars.[8] 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 protected area 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, or if the PPM disproportionally affects large values. Table 4 shows the descriptive statistics for the two treatments. The mean WTA with the PPM is \$354 per household (with a standard deviation of \$320), compared to \$482 without the PPM (with a much larger standard deviation of \$541). As expected, the PPM lowers the standard error of the mean (\$17 compared to \$29) and results in a lower maximum bid value (\$1,579 compared to \$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 the 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 ($\chi^2 = 88.5$). We therefore applied the Kruskal-Wallis test, a nonparametric analogue to the one-way ANOVA (Newbold, Carlson, and Thorne 2003). In this case, we once again re-

[7] 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.

[8] The exchange rate in 2008 was 1,900 Ugandan shillings = 1 U.S. dollar.

TABLE 2
Sample Descriptive Characteristics (Variable Descriptions in Table 3)

Variable	*N*	Mean	Std. Dev.	Min.	Max.	%
A. 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	—
B. Not Treated with PPM (Control Group)						
WTA	337	482	541	0	3,158	
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	1,879.76	0.00	27,500.00	—
NPAIUS	337	36.69	304.15	0.00	4,547.37	—
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	—

Note: PPM, provision point mechanism.

ject the null hypothesis of equality of means across groups at the 1% significance level.

Initial evidence therefore strongly hints at a significant difference (in the expected direction) between the two distributions of WTA. 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 \$2,000 and they all belong to the control group. More specifically, three individuals report a WTA equal to \$2,105, one individual is willing to accept \$2,526, seven respondents report \$2,631, and one reports a bid of \$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, we also run tests on samples from which 12 "extreme" observations have been removed. In this case, the probability that the mean WTA of the two groups are equal is well below 10% in a two-sided test.[9] These results, therefore, show no hint that the PPM could induce respondents to increase their bid in an effort to get authorities to put in place a larger compensation fund.

Since it is not rational for a household to claim less compensation than the true loss, the lower PPM claims should be closer to true values than the control treatment responses. Our raw 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. This figure can be interpreted as an upper bound of the

[9] 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.

TABLE 3
Variable Descriptions for Determinants of Bid Value

Variable	Description
WTA	Willingness to accept variable (U.S. 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
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 protected area (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

Note: PPM, provision point mechanism.

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	2,770,480.52	1	2,770,480.52	14.05	0.0002
Within groups	132,691,875	673	197,164.748		
Total	135,462,356	674	200,982.724		

Kruskal-Wallis Nonparametric Equality of Population Rank Test

Groups	*N*	Rank Sum
Treated with PPM	338	107,688
Control	337	120,462

Note: In one-way ANOVA, Bartlett's test for equal variances: $\text{chi}^2(1) = 88.4971$, Prob > $\text{chi}^2 = 0.000$. All monetary amounts are in U.S. dollars. MS, mean sum of squares; PPM, provision point mechanism; SS, sum of squares.

opportunity cost to households of losing 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 Mada-

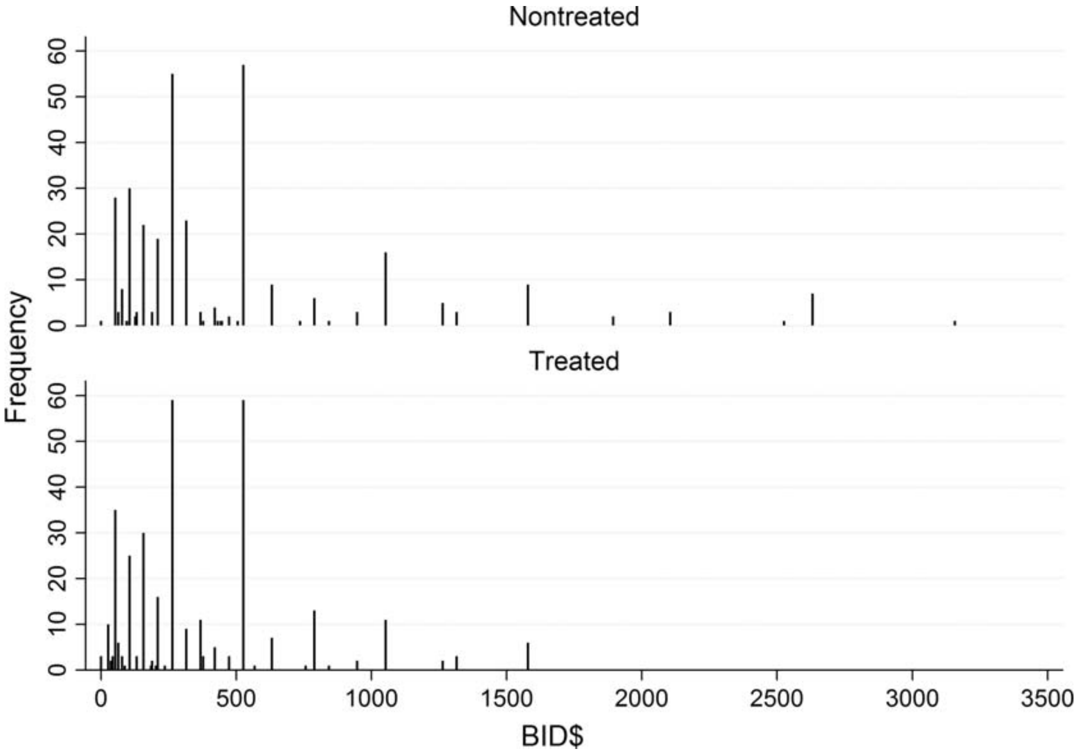

FIGURE 1
Distribution of WTA Bids among Treated and Nontreated Groups

gascar in 1995 (Shyamsundar and Kramer 1996) valued local access forgone form a forest in a similar scenario at $108.34/household. A more recent study in Nepal (Shrestha et al. 2007) valued mean local losses from foregone access to Koshi Tappu Wildlife Reserve at $238/household. 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/household. (Madagascar) and $280/household. (Nepal)[10]

The analysis so far has focused on direct comparisons of WTA distributions across groups without accounting for underlying differences in the sociodemographic characteristics of 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 agricultural land holding (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 are significantly correlated with the probability of being in the PPM treatment. Nonetheless, 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.

[10] 3.8% is the IMF mean rate of inflation since 1980, and PPP conversions are made using the World Bank GDP (PPP) per capita values for 2008.

TABLE 5
Probit Analysis of Belonging to the Treated Group

Variable (Dep. Var. = TREATED)	Coeff.	Std. Err.
EDGROUP	0.138	0.084
HHTOTALO	− 0.012	0.029
AGRILAND	0.016*	0.009
NTHI (thousand U.S. dollars)	− 0.004	0.024
NPAI (thousand U.S. dollars)	− 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

Note: $N = 470$.
* $p < 0.1$.

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]:

$$WTA_i = \beta_0 + \beta_1(TREATED_i) + f(X_i) + \varepsilon_i, \quad [4]$$

where *TREATED* is a dummy variable taking the value of 1 if the individual received the PPM questionnaire, 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 protected-area-related income, distance to nearest market, distance to the protected area, and three site dummies to represent unobserved variations unique to each protected area. In addition, we should expect the value of access losses to depend on household's need and ability to collect resources from the protected area. We therefore include household size, the availability of substitute sources of income (measured by the amount of agricultural land cultivated by the household), and past park area–related income as additional explanatory variables. The area of agricultural land owned by a household might also partly determine the damages it would incur as a result of crop raiding by wild animals. Therefore, the direction of the effect of this variable cannot be determined a priori.

We also include in our regressions variables measuring how far the household resides from the nearest market, and from the protected area (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; Jotikapukkana, Berg, and Pattanavibool 2010). Dummies are included to represent either the unobserved characteristics of each sampling location (site) or different management regimes (the two effects are confounded since each site corresponds to a different governance regime). The omitted site is Queen Elizabeth National Park. Standard errors have been adjusted for clustering within village to account for possible intracorrelation at village level in the WTA responses (see, e.g., Williams 2000).

We use sampling weights based on (1) the probability of a particular village being selected for sampling, (2) the probability of each household in any village being selected, and (3) the probability of being in the control group rather than the treated group (see also footnote 6). 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 ordinary least squares (OLS) model in the third column of Table 6 excludes the distance from market variable; this is reintroduced 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

TABLE 6
Econometric Analysis of Determinants of WTA Bid Value (Basic Models)

	(1)	(2)	(3)
Variable	Bid	Bid	Bid
TREATED	− 82.555*** (22.871)	− 95.634*** (25.015)	− 95.722*** (24.268)
EDGROUP		24.508 (18.251)	27.144 (16.419)
HHTOTALO		4.257 (7.683)	1.218 (6.984)
NTHI (thousand U.S. dollars)		− 3.468 (9.068)	− 3.199 (8.707)
NPAI (U.S. dollars)		0.238*** (0.046)	0.247*** (0.036)
ASSETVALUE		0.003 (0.016)	0.010 (0.018)
AGRILAND		5.125** (2.028)	5.460** (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*** (30.197)	320.711*** (49.596)	324.481*** (54.344)
Observations	675	628	620
R-squared	0.010	0.052	0.064
Mean predicted WTA of treated (U.S. dollars)	370.8	342.1	338.7
Mean predicted WTA of control (U.S. dollars)	453.4	431.8	427.3

Note: Standard errors clustered at village level are shown in parentheses. 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. Sampling weights were used to account for selection probability. See text for details.
** $p < 0.05$; *** $p < 0.01$.

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 $95 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 protected area. Indeed, crop raiding was an issue in all sites and in participatory exercises was always among the top three qualitative costs of living adjacent to the protected area. 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 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 $338–$370.

TABLE 7
Robustness Checks on Regression Models of Determinants of WTA

Variable	Box-Cox	Log-linear	Tobit	OLS Eliminating Bids > $3,000	OLS Eliminating Bids > $1,900	OLS Eliminating abs(dfits) < 2*threshold	OLS Adding Distance to Market	Nearest Neighbor 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 (thousand U.S. dollars)	−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 (U.S. dollars)	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)	

(table continued on following page)

TABLE 7
Robustness Checks on Regression Models of Determinants of WTA *(continued)*

Variable	Box-Cox	Log-linear	Tobit	OLS Eliminating Bids > \$3,000	OLS Eliminating Bids > \$1,900	OLS Eliminating abs(dfits) < 2*threshold	OLS Adding Distance to Market	Nearest Neighbor Matching
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. All monetary amounts are in U.S. dollars. 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. WTA, willingness to accept.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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 multicollinearity among explanatory variables was checked using the variance inflation factor (VIF) comparison. VIF estimates for the regressors showed that there were no serious multicollinearity problems. All the variables have a value lower than 2.5.[11] 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, we note that the *TREATED* variable is negative, rejecting the null hypothesis [5] once again, and that the estimate of θ is 0.06 is not statistically significant. This suggests that a log-linear model ($\theta = 0$) may be more appropriate than the linear model ($\theta = 1$). We therefore report in the second column of Table 7 the result of a log-linear WTA model (see Cameron and Trivedi 2009). The log-linear specification does not modify our previous findings. For the sake of simplicity we therefore keep a linear specification in what follows. We also allow for the true distribution of WTA amounts being censored at zero by using Tobit regression (Halstead, Lindsay, and Brown 1991). The third column of Table 7 shows that the effects of *TREATED* and the rest of the variables do not vary significantly when modeling the data using such a Tobit regression.

The fourth column of Table 7 shows the effect of *TREATED* on WTA when dropping the single observation in which bid was greater than \$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 nontreated group by about \$5: an individual participating in a PPM survey states a claim that is on average \$84 smaller than somebody answering the control survey (not a substantial difference from our baseline difference of \$89). The fifth column expands the data truncation and considers as "outliers" all the claims in the regular scenario that are higher than the largest claim in the PPM group, that is, we drop all the bids higher than \$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 11 highest claims in the control group halves the difference in WTA between the two groups, but it still averages \$38, suggesting that part of the difference might be inflated because of these 11 observations. However, we must not lose sight of the fact that the PPM is employed specifically with the intent of providing incentives to eliminate such large claims. Overall, the analysis leads us to believe that the long tail of the nontreated distribution represents this lack of incentive to curtail one's claim, rather than the presence of statistical outliers.

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

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

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 the sixth column 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 coefficients and does not affect the conclusions we have already reached on the effect of the PPM.[12]

Finally, we have used a different econometrics technique, the propensity score matching estimator.[1] 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 being in the PPM sample (a similar propensity score). The difference between WTA between households who received the PPM and who did not can be interpreted as an average treatment effect. None of the observations fall off the region of common support when matching on 2, 4, or 6 nearest neighbors. The difference between treatment and control group varies between 121, 97, and 100, respectively, and is always statistically significant at the 1% level. These values are comparable to earlier 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 four matches.[13] The difference between treated and nontreated WTA is $94 in this matching model.

V. DISCUSSION AND CONCLUSIONS

This paper investigates the use of a PPM to estimate the opportunity costs of conservation actions to local people, using a WTA CV 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 CV markets to lead to the overstatement of true losses, and to encourage protest bidding. Our main focus in this paper was a methodological one, to devise a strategy (the PPM) that reduces the problems of applying WTA-measures to estimates of the costs of conservation.

By extending the basic idea of the PPM from a WTP 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 out-

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

[13] *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 results in us leaving out three observations. However, the results do not change substantially from those reported in the main text. Propensity score matching has been estimated using the Stata command –psmatch2-. Notice that we have always found a statistically significant difference between the treatment and control groups using different matching techniques, including matching on covariates instead of propensity scores using the Stata –nnmatch- command (Abadie et al. 2004).

liers suggests that the PPM mechanism could facilitate the applications of WTA designs in CV (or, indeed, in choice modeling) when compensation is the appropriate welfare framework to adopt.

A wide range of factors, including historical variation in conflicts and participation, has the potential to affect peoples' perceptions of the value of losses of access to protected areas. 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 variations 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 in the context of carbon sequestration through forest conservation and REDD+. Here we use the term "compensation" in the sense of providing payments to offset the foregone benefits of park access, rather than to offset costs such as crop raiding due to proximity to a protected area. At the community level, this may not be the only benefit flowing from restricting access. Programs such as the tourism revenue sharing scheme that was set up in Uganda to provide community-level benefits from the national parks also promote goodwill toward the protected area and its conservation objectives. The distribution of such benefits may, however, leave those who stand to lose most from access restrictions without compensation or incentives to curtail their resource extraction. Anecdotal evidence suggests that to be effective, interventions must carefully target households that are the most dependent on park-area resources.

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 in Uganda have depended on access to these areas for fuelwood, bushmeat, and NTFPs, particularly at times of the year when other sources of subsistence are very limited. Exploiting such resources is a livelihood strategy that 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 the area of agricultural land cultivated by households and the income obtained from protected area access; while mean compensation demanded also varied to a degree across protected areas. This variation across protected areas 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 protected areas would underestimate the welfare loss to such households, and we indeed found that our WTA estimates were much higher than the financial value of lost access, which averaged \$21/household, varying from almost zero (Bwindi) to \$44 (Tengele), with higher absolute values for richer households (Bush et al. 2011). While there is good reason to suppose that the WTA estimate in the PPM treatment is still biased upward due to hypothetical market effects, a strong argument can still be made that the true costs of loss of access to protected areas is greater than \$21/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, Pascual, and Swanson 2007). Establishing protected areas is a dominant means of achieving such conservation worldwide. However, the costs of conserving biodiversity in developing countries can fall disproportionately on poor households, who often have the most impact on natural resources. Quantifying the full range of conservation costs offers hope that they can be mitigated without exacerbating poverty. The results of our study suggest that a WTA approach incorporating a PPM, while not theoretically incentive compatible, can help reign in excessive valuation estimates and is worthy of further investigation. 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 behavior. 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 behavior 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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