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# **‘When to Take “No” for an Answer’? Using Entreaties to Reduce Protests in Contingent Valuation Studies**

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## **Abstract**

We report the results of two field experiments to investigate the usefulness of entreaties in reducing protest zero responses in contingent valuation (CV) studies. These two experiments estimate willingness to pay for tropical biodiversity amongst distant beneficiaries and for reductions in water supply risks, respectively. The entreaties in both contexts, in essence, entailed an additional text to ‘talk people out of their protests’ using, respectively, a split sample test and a within sample test. Results indicate that, in both cases, these scripts were effective in significantly reducing protest zeros, with one experiment reducing protests at the payment principle stage of the valuation scenario and the other reducing protests at the payment elicitation stage. Using entreaties in this way tentatively may be a useful contribution to the existing CV literature where protests rates are high and, moreover, appear to ‘defy’ efforts to address the issue through best practice in the design and testing of survey instruments. However, while protests were reduced by about a third in both cases, the entreaties clearly did not eliminate a majority of protest zeros. Moreover, as we discuss, there are good reasons why the responses of ‘reclaimed’ protestors remain open to scrutiny.

## **Acknowledgements**

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## 1. Introduction

Many contingent valuation (CV) studies experience a high level of protest responses. Such responses are problematic since they do not represent ‘true’ economic values (Jorgensen *et al.* 1999). The most common type of protest response occurs where a respondent does not provide his or her genuine willingness to pay (WTP) but instead states a zero value, referred to as a protest zero (Mitchell and Carson, 1989). This presents a crucial problem for the analyst since there is no way to impute the true value held for the good (Bateman *et al.* 2002; Mitchell and Carson, 1989). Consequently, the typical approach to dealing with protest zeros is to remove them from the sample. This can leave the resulting welfare estimates open to bias and reduce survey efficiency.

In this paper, we focus on tackling the problem of protest zeros, through an empirical investigation of whether the number of protest zeros reported might be reduced using an entreaty. This builds, in particular, on a number of studies which have recently used entreaties such as ‘cheap talk’ scripts to deal with the different problem of hypothetical bias (see, for example, Brown *et al.* 2001; Bulte *et al.* 2005; Carlsson *et al.* 2005; Cummings and Taylor, 1999, List, 2001; Lusk, 2003; Murphy *et al.* 2005; Poe *et al.* 2002). Despite some success at reducing hypothetical bias, the potential of these scripts for dealing with other types of contingent valuation bias largely remains unexplored. In this paper, we present two applications that explore the use of such scripts as one means to deal with, and understand, protest responses. The first estimates WTP for tropical biodiversity amongst distant beneficiaries and the entreaty is tested using a split-sample procedure. The second estimates WTP for reductions in household water supply risks and the entreaty is administered in a within-sample context.

These are, on the face of it, very different policy contexts. However, the problem of protest zeros in evaluating preferences for proposed changes in provision is, as we find, common to both. In turn, the two experiments that we outline in this paper are linked by the shared object to see how entreaties might ‘talk people out of their protests’ and so uncover WTP where a respondent otherwise might have registered a protest zero. This, we argue, could represent a useful contribution to the existing

literature on contingent valuation where protests rate are high and, moreover, appear to ‘defy’ efforts to address the issue through best practice in the design and testing of survey instruments. However, we do this with a note of caution about how responses elicited following an entreaty might be interpreted as a valid measure of preference.

The remainder of the paper is organised as follows: the next section reviews the literature on protest responses and developments in using entreaties in stated preference studies particularly, but not exclusively, in the use of ‘cheap talk scripts’ to deal with hypothetical bias. We then introduce the experimental design used in each of our case studies including the criteria used to classify protest responses and the research hypotheses. Following this, we present and critically reflect upon our results and analysis. We conclude with a summary of our main findings and recommendations.

## **2. Literature Review**

In this paper we are specifically concerned with the problem of protest zeros, a situation which arises where a zero value is reported for a good even though a respondent truly values its provision (see, for example, Mitchell and Carson, 1989). Such responses are particularly evident in situations where the good in question has traditionally been provided free of charge (Strazzera *et al.* 2003; Jakobsson and Dragun, 1996). There are many reasons for this kind of behaviour but typically it is the result of an objection to some aspect of the valuation process or contingent market: for example, the payment vehicle, the policy intervention, the institutional setting, lack of comprehension of the task, insufficient information, ethical objections or it may simply reflect some form of strategic behaviour such as free-riding (Boyle *et al.* 2001; Jorgensen *et al.* 1999, 2001; Morrison *et al.* 2000; Strazzera *et al.* 2003).

Since protest responses do not reflect the true value of a good including these responses in the final welfare analysis may lead to biased estimates. There is a long-standing debate about how to react to this insight and, in turn, whether to retain or discard such responses for the sake of analysis (see, for example, Halstead *et al.* 1992). But, in general, these data are typically excluded from subsequent consideration of valuation responses. In order to do this the analyst is required to

identify which responses are true zeros and which are protest zeros. The standard procedure is to use follow-up questions to distinguish between those that state zero because they genuinely do not value the good and those that state zero for some other reason (Mitchell and Carson, 1989).<sup>1</sup>

Clearly, this procedure is reliant on self-reporting and there may be many reasons for a respondent's stated response. For example, Jorgensen and Syme (2000) argue that respondents not wishing to pay for valid reasons such as budget constraints may also object on the basis of some aspect of the contingent market.<sup>2</sup> The waters are further muddied by the lack of an established protocol on how to identify protest responses. Indeed, a handful of contributions suggest controversy exists over how to define or identify protest responses (Boyle and Bergstrom, 1999; Jorgensen *et al.* 1999; Meyerhoff and Liebe, 2006) with some authors considering only payment vehicle rejections as protest zeros while others use wider criteria including insufficient information and ethical objections (Jakobsson and Dragun, 1996). In effect, two separate analyses of the same good could provide widely different welfare estimates depending on how the analyst interprets these aspects of the data (Jorgensen *et al.* 1999).

There is also debate over the efficacy of censoring protest bids once identified. This stems from concern that the exclusion of protest responses may affect the validity and potential for generalisation of the results if there is evidence of systematic bias amongst those protesting (Bateman *et al.* 2002; Jorgensen *et al.* 1999; Mitchell and Carson, 1989; Morrison *et al.* 2000; Strazzera *et al.* 2003a). For example, Jorgensen and Syme (2000) find that censoring protests would bias CV results towards those individuals who favour paying for environmental goods or with higher income. Schlapfer *et al.* (2004) found that most of their protest zeros were articulated by 'disappointed voters'.

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<sup>1</sup> Meyerhoff and Liebe (2009) have argued that these debriefing questions are also important in choice experiment (CE) settings. While far less frequently employed than in its CV counterpart, these authors present evidence that protest attributes are related significantly to status quo effects (i.e. choosing the baseline) in CE formats.

<sup>2</sup> Meyerhoff and Liebe (2006) investigate possible determinants of protest behaviour and conclude that it is related to a range of factors including notably ethical perceptions about norms of provision for the sorts of (public) good being valued in these studies. Haddad and Howarth (2006) discuss more broadly how ethical considerations and preferences might shape this debate.

As a result of the challenging issues that surround the identification and treatment of protest responses there has been some exploration of alternative ways to deal with protest zeros. Strazzera *et al.* (2003a), for example, suggest a sequential procedure, to deal with the sample selection problem which can occur if protestors are significantly different from the censored sample, in CV models with open-ended WTP data. Strazzera *et al.* (2003b) and Calia and Strazzera (2001) also pursue this important line of inquiry with regards to the statistical challenge posed by this potential sample selection problem. Morrison *et al.* (2000) use follow-up questions to recode protestors, at the model estimation stage, as being in support of the proposed change if their concerns could be dealt with: for example, if "... an alternative, acceptable way of collecting money could be found ..." (p419) and they can afford to pay. In the current study, we take a rather different approach to dealing with the problem of protest responses by exploring a means of reducing the propensity to protest through the use of an entreaty.

Entreaties, such as cheap talk scripts, have been previously used to deal with hypothetical bias. The term 'cheap talk' itself arises from its use in the information, bargaining and game theory literature where it typically refers to the "... cost-less transmission of information and signals ..." (Cummings and Taylor, 1999, p650). In the contingent valuation literature, the term usually refers to a script, typically quite lengthy, that is added to the valuation scenario in order to directly draw respondents' attention to the problem of mis- or over-stating true values. As this could occur in a hypothetical setting, the script directly asks respondents to frame their response as if they were in a real-life setting. For the most part its application has been successful – in terms of its impact on hypothetical bias – albeit with some sensitivity to differing script lengths, respondent experience and payment levels (see, for example, Brown *et al.* 2003; Cummings and Taylor, 1999; List, 2001; Lusk, 2003; Murphy *et al.* 2005).

Previous empirical work has involved both laboratory and field experiments. Each has its merits. The chief virtue of the former lies in its use of a closely-controlled environment allowing an in-depth investigation of behavioural responses such as in Cummings and Taylor's (1999) test of variations of an eight-paragraph script. By contrast, field experiments – as we use in the current paper – mean less control over

participant behaviour and, typically, shorter (and less varied) scripts. Some studies have indicated caution might be the watchword in using such scripts in the field (Poe *et al.* 2002; Aadland and Caplan, 2006). Other contributions have concluded more positively (Aadland and Caplan, 2003; Bulte *et al.* 2005; Carlsson *et al.* 2005) including Lusk (2003) which uses a self-administered format (in a mailed survey) and, in doing so, offers support for the use of cheap talk even in the absence of a moderator to ensure the script is fully read and assimilated. More generally, if the policy relevance of entreaties is to be fully explored it seems important also to test efficacy in the field as well as in the laboratory.

Exploring the merits of entreaties also strikes us as worthwhile in other apparent problem areas in CV applications. Specifically, in this paper, we investigate whether an analogous type of entreaty, i.e. a protest-correcting entreaty, might persuade protestors to reveal their true preferences for a good. Interestingly, this speculation was anticipated in a study of US water quality by Mitchell and Carson (1984) albeit with little divergence being found there between pre- and post-entreaty protest rates. More recently, Carlsson and Martinsson (2006) find that an entreaty script reduced the proportion of zero WTP responses in their sample, in a CV application to power outages in Sweden. In the current paper, we build on this small but interesting body of research by focusing on entreaties designed to self-correct for protest zeros in two CV applications.

### **3. Study Design**

We conducted two distinct CV experiments in the United Kingdom (UK). The main objective of Experiment 1 was to test the use of an entreaty to reduce protest zero bids using a split-sample procedure. The survey elicited WTP for protecting tropical biodiversity amongst distant beneficiaries. The proposed policy change was adapted from Bruner *et al.* (2004). Respondents were asked to consider a programme which would expand the existing global network of protected areas by 30% (about 3.5 million km<sup>2</sup>) and further that this additional conservation would consist of 'high priority sites' in developing countries. Experiment 2 similarly investigated the effectiveness of protest-correcting entreaties but in a within-sample context. This second survey explored preferences for reducing the risks of interruptions to

household water supply. The geographical area of interest was the Thames Water catchment which includes households in much of London as well as towns such as Oxford and Swindon.<sup>3</sup> Within this water catchment, respondents were informed (with visual aids) of the risk of water use restrictions (including severe rationing involving, for example, rotating cuts to household water supply, and use of standpipes or hydrants on streets) with and without a package of investments, the most prominent feature of which was the construction of a large storage reservoir (WRSE, 2006).

The payment mechanism for eliciting WTP in Experiment 1 was an international fund supported in the UK via increases in income tax. Similar scenarios have been used in, for example, Kramer and Mercer (1997) and Horton *et al.* (2003) and have demonstrated WTP values that, in principle, could be captured for biodiversity conservation. Nevertheless, there remain legitimate and interesting practical questions about the implementation of such measures. Indeed, in initial focus groups for Experiment 1, a number of participants expressed concerns about possible management problems and difficulties perhaps involving overt corruption that might hamper any such large expansion in the protected area network. Whilst CV practitioners try to ensure that credible information is provided that allays these concerns, Carson (1998) comments on the difficulty of designing tropical forest protection scenarios where respondents' concerns with outright corruption and general inability of developing countries to provide the requisite conservation loom large. These concerns have a genuine basis in the real world. A key reason why conservation programmes involving payment for environmental services are rare in many areas particularly Africa is precisely because of such implementation problems. Furthermore, there is debate amongst conservation experts that protected areas are frequently little more than 'paper parks' where protection is provided in name only (see, for example, Pearce, 2007).

In Experiment 2, the payment vehicle was the respondents' annual household water bill. It is, in effect, an institutional given that it is the industry that will initiate investments in improving water quality and, as in this case, additions to the water supply (or management of demand) in the UK. The cost of these improvements is then

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<sup>3</sup> Specifically this includes all those households whose domestic water is supplied by Thames Water plc.



passed on to consumers, subject to regulatory approval, in the form of higher water bills. Nevertheless, despite its realism, using this payment vehicle has been found to create anxieties in respondents related to perceived excessive profits within the industry, controversial privatisation (albeit some years ago), combined with resentment about the current level of (and other changes in) household water bills (e.g. Bateman *et al.* 2006).

To reiterate, from the standpoint of eliciting WTP values, these concerns boil down to either uncertainty that the good in question will be provided (Experiment 1) or objections to the payment mechanism itself (Experiment 2). For the CV practitioner the answer to these problems should be to ensure that credible information is provided in the valuation scenario that allays these fears. Such avenues, of course, should be explored fully as part of the design process and may involve conveying, for example, details that might reassure respondents about good project management or monitoring in the case of biodiversity conservation (Experiment 1) or a complementary package of measures such as fixing leaky pipes to reduce the prospect of water use restrictions (Experiment 2). In fact, in the latter case, this was one element of the scenario presented to respondents alongside the reservoir which was presented as the main investment.<sup>4</sup> Dealing, in this same way, with broader scepticism about the water industry is arguably more difficult to conceive of. Yet without doing this it is arguable that respondents – implicitly – are being asked simply to suspend their disbelief. To the extent that respondents are not willing to ‘play along’ in this way, then one outcome might be protest responses (particularly protest zeros) or heavily discounted WTP values. That such values might not reflect respondents’ true preferences is an understandable artefact of the inability of a CV scenario to resolve problems that emanate from elsewhere and, moreover, because of this cannot be easily addressed through the valuation scenario design.

With this in mind, the scenarios in Experiments 1 and 2 were constructed with the explicit introduction of an *additional* entreaty. The entreaties themselves were designed specifically to address any general real world concerns relating to the provision of the proposed programmes that were legitimately not part of the scenario

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<sup>4</sup> This itself is apparently a reasonable assertion given that fixing leaks and upgrading pipes in itself does not augment available water hugely (WRSE, 2006).

description (for the reasons already discussed). In other words, the purpose of these scripts was to provide respondents with an additional appeal based on residual concerns they might have that the proposed programme was not possible (as in Experiment 1) or aversion (for example) to the way in which it would be financed (as in Experiment 2). In each case, the entreaty text amounted to about half a page of the survey<sup>5</sup> and invited respondents to set aside their concerns either about ineffectual institutions and corruption in developing countries (Experiment 1) or about water companies and ‘the government’ more broadly (Experiment 2). The full entreaties are presented in an Appendix to this paper.<sup>6</sup>

In particular, we are interested in the impact that these entreaties might have on the numbers of respondents that would otherwise state a zero WTP value which can be construed as a protest response. Given that the script, in effect, might ‘talk people out’ of their protest bids, the treatments could be said potentially to provide more accurate estimates of the true benefits attached to achieving the principle of the proposed policy change.<sup>7</sup> This emphasis on potential accuracy is important as there are undoubtedly important questions to ask about the interpretation of responses elicited in the face of an entreaty as we discuss later. We acknowledge that the entreaty approach might itself have negative consequences for the quality of subsequent WTP responses. For example, this procedure might introduce an element of hypothetical bias to the extent that asking explicitly for respondents to concentrate on project outcomes divorces their thinking from issues of substance about the processes by which the outcomes are delivered. In this sense then, using entreaties might be viewed

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<sup>5</sup> Our entreaties are shorter than Cummings and Taylor (1999) 8-paragraph long lab-based cheap talk script but are significantly longer than several of the one-paragraph field-based cheap talk entreaties that have been used (Carlsson, Frykblom and Lagerkvist, 2005; Bulte et al 2005). Shorter entreaties have been found to be less effective (see Poe *et al.* 2002).

<sup>6</sup> It should be noted that most CV studies contain some text which could be construed as an entreaty, typically urging respondents to focus solely on the scenarios proposed and realistically evaluate their ability to pay. However, this normally amounts to a line or two of text only and is therefore rather different in scope and content to the more detailed and substantial half page entreaties used in our experiments. As noted, shorter entreaties have been found to be less effective (Poe *et al.* 2002). We are grateful to an anonymous referee for pointing this out.

<sup>7</sup> It can of course be argued that objections to institutional settings or payment vehicles are a true reflection of people’s preferences for the bundled good on offer in CV studies, encompassing the outcome of interest and the specific way in which it is to be delivered. However, in both our experiments, we are mostly interested in valuing particular *outcomes* (i.e. biodiversity conservation and security of water supply). The provision mechanisms were specified in the scenarios for credibility but attempts were made to make them as realistic and as neutral as possible, to minimise their influence on the stated preferences for the outcomes themselves.

better as a ‘last resort’ and certainly not as a substitute for prior reflection and testing of descriptions of scenarios to be valued.

Willingness to pay, in the valuation scenarios of both Experiments 1 and 2, was assessed in two stages. The first stage was a payment principle question, in which respondents were asked whether in principle they would be willing to contribute to the specified programme. Those responding YES or DON’T KNOW went on to the second stage of the valuation scenario (and those stating NO went straight to follow-up questions). At the second stage, respondents were asked to report their maximum WTP per annum. The elicitation format in both experiments was a payment ladder.

Protests were identified at both stages of the valuation process. At the first stage, respondents stating NO to the payment principle were asked to choose their main reason for being unwilling to do so, in Experiment 1, from a list of reasons (including an open-ended ‘other’ category) and, in Experiment 2, from an open-ended follow-up question.<sup>8</sup> At the second (payment card) stage, in both experiments, respondents were asked to provide a brief explanation for a zero WTP response in a separate open-ended follow-up question.

[FIGURE 1 ABOUT HERE]

The experiments differed in the way the entreaty was administered to respondents. This is described in Figure 1. In Experiment 1, we used a split-sample design with the additional script inserted immediately after the valuation scenario and prior to the payment principle question in one version of the questionnaire. The other questionnaire version was identical except for the absence of the entreaty. In Experiment 2, a within sample test was employed. If a respondent gave a protest zero, in response to the payment principle question, that respondent was then presented with the entreaty text. The respondent was then asked if, in the light of this additional information, he or she would like to re-consider their initial answer to the payment principle question. If the answer was in the affirmative then that respondent was asked – in the second payment ladder stage of the valuation scenario – to report his or her

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<sup>8</sup> The reason for this difference in approach was simply that Experiment 1 was administered in a mail survey and Experiment 2 by in-person interview.

maximum WTP. Those respondents who had *originally* stated YES (or DON'T KNOW) in response to the payment principle question but were not prepared to pay anything at the second stage were also asked to state the *main* reason for this. Again, to the extent that this answer could be interpreted as a protest zero, the respondent was presented with the entreaty text and then asked if he or she wished to make a revised WTP response. Note that the entreaty was administered only once to any one respondent.

Broadly speaking, a protest in the case of either experiment was defined as any zero response given on the basis of: (i) concerns over the credibility of the proposed scenario or objections to the process whereby the specified change would be provided; (ii) objection to some aspect of the payment vehicle, for example, 'too high water bills' or 'lack of trust in the institution', 'excessive profits' or 'government should pay'; and, (iii) objection to some other aspect of the valuation scenario such as, for example, 'I need more information'. In large part, the classification of protests relies on a strong element of subjectivity with different studies reporting different methods of classification. Nevertheless, the latter two categories are frequently identified in the CV literature as protest responses. The decision also to include the first type of response was due to our contention that these types of respondents were not revealing their true preferences for the *achieved outcome* of the proposed programme. Responses which were considered to be valid zeros were those that reflected a genuine lack of economic value for the good, for example, 'I don't care about this issue', 'I prefer to spend my money on other things' or 'I can't afford it'.

The inclusion of the entreaties was examined primarily in terms of its effect on the incidence and likelihood of protest zero responses. We speculate therefore that the inclusion of the entreaty would result in: (i) in Experiment 1, a lower number of protests in the entreaty survey version than in the non-entreaty version; and (ii) in Experiment 2, a lower number of protests post-entreaty than pre-entreaty. Although the main purpose of the entreaty scripts was to examine their effectiveness in reducing the number of protest responses, we are also interested in examining whether these would have any effect on WTP. This allows us to comment subsequently on whether WTP values, given in the light of the entreaties, are significantly different to the WTP

of either the without entreaty sample (Experiment 1) or the pre-entreaty sample (Experiment 2).

#### **4. Results**

In Experiment 1, the survey was administered using a drop-off and mail-back procedure and was delivered to a total of 1836 households in six randomly selected areas of London in January 2006. In total 601 usable questionnaires were returned. In Experiment 2, the main survey took place in July and August 2007 across the London and Swindon/ Oxford region of England. It consisted of 746 in-person interviews, using a quota sample.

[TABLE 1 ABOUT HERE]

A summary of socio-demographic characteristics is provided in Table 1. This describes, in the case of Experiment 1, these characteristics across valid responses (i.e. positive WTP and valid zeros) and protest responses in both treatments (i.e. no entreaty and with entreaty). The table shows that, with the exception of sex, which differs between the valid zeros sub-samples in both treatments (but is not used in any further analysis), all the observable characteristics of the sub-samples who gave valid answers and who protested are not statistically different between treatments. For Experiment 2, these same characteristics are illustrated again for those respondents classified as giving a valid WTP value (positive or zero) or a protest response. In addition, the final column gives the characteristics of those protestors in Experiment 2 who subsequently stated a positive WTP value in the light of the entreaty administered to them. That is, these are respondents who have changed their minds about their protest following the entreaty. In the table and, in what follows, we refer to these respondents as “new entrants” (to the contingent market) in order to distinguish them from the initial valid positive or zero WTP (of “original” or “old entrants”).

##### *Zero Willingness to Pay Responses*

We report the main reasons for zero WTP categorized into valid and protest responses in Table 2. In Experiment 1, on average, 23% of respondents with the additional script and 32% without it were not willing to pay anything towards the proposed change.

The main reasons for zero WTP responses were found to be roughly similar between treatments with, for example, the most frequently cited reason – ‘cannot afford to contribute’ – accounting for 27% and 25% of all zero responses with and without the entreaty respectively.<sup>9</sup> Of the valid reasons for giving a zero WTP, the most notable difference between treatments is the higher proportion reporting ‘prefer to spend money on other things’ which was 1% under the entreaty treatment compared to 7% without it. On average, in both treatments, just over half of the reasons given for zero WTP consisted of protest responses.

In the case of Experiment 2, column 4 in Table 2 indicates that 224 respondents stated a zero WTP (30% of this portion of the sample). Out of these respondents, protest zeros clearly predominate and represent 25% of our total sample. These protest responses were motivated by a number of objections, most prominently ‘paying higher water bills’. The data in column 4 of the table illustrate the incidence of reasons for protest zeros before any of these protesting respondents received the entreaty text. All those respondents identified as protestors then received the entreaty. The final column of the table focuses on “new entrants” only, that is, those who changed their mind (56 respondents in all) after the entreaty. The column depicts the original reason “new entrants” gave for their initial (pre-entreaty) protest zero. Amongst “new entrants”, the largest *single* protest category is also objecting to paying higher water bills, which accounts for almost one third (29%) of total “new entrants” in this experiment. However, looking across other protest reasons, 40% of “new entrants” originally had given reasons for their initial protest based on some form of antipathy towards the water industry (whether this be perceived excessive profits, ownership, responsibility or efficiency), while another 24% had expressed an initial dissatisfaction with the specified project itself.<sup>10</sup>

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<sup>9</sup> In Experiment 1, some 10% and 4% of protestors (in the “no entreaty” and “entreaty” respectively) stated that they were not willing to pay for a range of (self) articulated reasons which did not fall exactly within the key categories outlined in Table 2. We therefore grouped them under ‘Other reasons’ for simplicity. For the most part, these broadly relate to protests connected to specific aspects of the payment vehicle and miscellaneous concerns about the way in which any such conservation scheme would work. The incidence of these miscellaneous concerns appears to be lower in the entreaty version of the questionnaire.

<sup>10</sup> There is some ambiguity, as we noted earlier, about the classification of responses such as these as protests. Thus, if there were credible alternative options to the ones presented in Experiment 2 that might deliver the same reductions in the risk of water use restrictions, then it could well be argued that at least two of the protest categories in Table 2 could equally be construed as valid zeros. In the project presented to respondents, however, a reservoir was presented as the main solution to water supply

[TABLE 2 ABOUT HERE]

*Effect of Entreaty on Incidence of Protest Zeros*

The top half of Table 3 indicates that, in Experiment 1, at the first stage of the valuation exercise only a small number of respondents protested to the payment principle question. In the entreaty version only 2.7% of the respondents answering the payment principle question provided a protest NO compared to 4.0% without the additional script. The entreaty, however, had a much more noticeable effect at the second stage of the valuation exercise (payment ladder) which received a higher proportion of protest responses. Hence, the additional script was associated with a rate of protest of 8.9% compared to 13.7% in the version of the questionnaire without the script. Overall, the entreaty treatment reduced protests by 34%.<sup>11</sup> One interpretation of these results is that the entreaty had a reasonably large effect in reducing protest zeros in Experiment 1. However, confidence in this interpretation must be qualified. One reason for this is that, across the two treatments, the distribution between protest and valid zeros (i.e. each as a proportion of all zeros) is very similar. This is particularly so at the payment principle stage. We cannot therefore discount the possibility that the entreaty had a blunter impact than intended in terms of influencing the tendency to state both protest and valid zeros (and thus affect the readiness of respondents to state a positive WTP more generally).<sup>12</sup> We return to this issue later.

[TABLE 3 ABOUT HERE]

The bottom half of Table 3 indicates that, in Experiment 2, the clear majority of respondents stating protest reasons for not being prepared to pay anything did so at the payment principle stage of the valuation exercise (about 24%). Very few respondents either answered DON'T KNOW or YES to the payment principle

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issues, together with a wider suite of complementary measures that included fixing leaky pipes. Evidence suggests that (whatever its merits or otherwise in other respects) the reservoir is in fact crucial to the risk reductions we specified being realised (WRSE, 2006).

<sup>11</sup> Chi-squared tests revealed no significant difference at this payment principle stage between the two treatments. For the payment ladder stage, however, chi-squared tests indicate that protest responses were statistically significantly lower (5% sig. level).

<sup>12</sup> We are grateful to an anonymous reviewer for pointing this out.

question and then subsequently stated a zero WTP when presented with the payment ladder (less than 1%). This is in contrast to Experiment 1 where the majority of protest responses occurred at the second stage of the valuation exercise, i.e. at the payment ladder stage.

The reason for the differential occurrence of protests between experiments is likely due to the diverse valuation contexts they cover. Experiment 2 involves paying higher household water bills to water companies for reducing the risk of water use restrictions. The piloting stages of our research showed that some people, for example, were very unhappy with what they perceived as water companies' excessive profits. We would expect some of those feelings to translate into protests at the payment principle stage of the valuation exercise as people take the first available opportunity to vent their discontent about paying higher water rates, companies making high profits, not paying for improvements out of these profits and so on. Conversely, Experiment 1 dealt with protection of tropical biodiversity via an international fund, which our preliminary research showed most people tended to support in principle. Here the objections started when people considered more closely the details of the programme being offered in order to value its benefits. Hence, it is arguably then at the second (payment ladder) stage of the valuation exercise that we would expect some people to start worrying about effectiveness of the proposed programme, or about possible corruption among other reasons.

Recall that the entreaty, in Experiment 2, was designed to see if respondents registering an initial protest zero might reflect on this response, in the light of some additional information about why the valuation question was being asked, and revise whether they would be willing to pay. This design means that the WTP data, in effect, can be analysed with and without the revised WTP responses: respectively, 'after entreaty' and 'before entreaty'. Hence, in the bottom half of Table 3, under Experiment 2, the data in the columns 'no entreaty/ pre-entreaty' takes a respondent's first valuation answer as final. By contrast, entries in the columns 'entreaty/ post entreaty' take into account the revised valuation answers of protestors (as well as taking the first answer of everybody else). Notably, the entreaty had the effect of reducing the ultimate number of protests quite substantially, with about 30% of the initial protestors (56 respondents in total) changing their minds in the light of the



entreaty. As a result, following the entreaty being administered, protest responses are about 17.5% of the overall sample.<sup>13</sup>

To further explore in a multivariate context the effect of the entreaties on the probability of protesting we ran logit models for both datasets. In Experiment 1, the logit model explores the likelihood of protesting at the second (payment ladder) stage of the valuation where most of the protests occurred. The dependent variable was coded 1 if the respondent protested and 0 if not. The results are reported in Table 4. As expected, the entreaty dummy variable was found to be a significant determinant of the probability of protesting (at the 5% significance level).<sup>14</sup> The negative sign on the coefficient confirms the findings of the non-parametric results, namely that the inclusion of the script significantly reduces the likelihood of protesting. Furthermore, the probability of protesting was found to increase significantly with income and amongst respondents who considered the environment to be the least important priority for additional government funding.<sup>15</sup> By contrast, respondents with degree-level education or above and respondents who stated they undertook routinely ‘environmental-friendly’ tasks (as proxied by recycling often or always) were significantly less likely to protest. Protest responses also declined with the perceived likelihood that the protected areas would result in long-term conservation, thus suggesting that belief in the efficacy of the scheme is a critical bridge between preferences being translated into positive WTP amounts for specific conservation schemes.

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<sup>13</sup> Note that in Experiment 2 (and unlike Experiment 1) the proportion of valid zeros is invariant between treatments by design, i.e. those stating valid zeros did not receive the entreaty.

<sup>14</sup> In this experiment, we also investigated whether our entreaty was more effective at reducing the propensity to protest amongst certain types of individuals. This follows on from the literature using cheap talk to deal with hypothetical bias, where there has been some evidence to suggest that cheap talk is more effective at reducing stated WTP for certain types of individual (see Aadland and Caplan, 2003; List, 2001; Lusk, 2003). To investigate this, we re-examined the entreaty effect on protestors by comparing the characteristics of those protesting with and without the additional script (using student t-tests, Mann-Whitney tests and chi-square tests). Overall, the key socio-demographic and attitudinal variables were not significantly different for protestors between treatments suggesting that the entreaty was similarly effective for most respondent types. These summary statistics can be found in Table 1 and Table A.2 (in the Appendix). However, given the relatively small number of protestors we would suggest treating these outcomes with caution.

<sup>15</sup> This involved a ranking exercise of five general issues: crime, education, the National Health Service, environment and poverty. This finding itself raises the possibility that, in some degree, protests might be related to (conventionally) ‘valid’ reasoning after all. We have taken, however, the standard practice in identifying protestors using a debrief response question immediately following the WTP question. It is plausible though that digging further than this convention entails might lead one to speculate that – for some respondents – protest motives might sit alongside valid reasons for not being willing to pay.

[TABLE 4 ABOUT HERE]

Interestingly, given its within-sample format, Experiment 2 permits us to examine, in particular, the characteristics of those respondents who initially protested (N=187 from Table 3) but then subsequently stated a positive WTP amount (N=56 from Table 3) in the light of the entreaty text being administered. Table 5 presents results from a logit model which includes only those respondents who initially protested (either at the first or second stage of the valuation exercise). The dependent variable is coded 1 if a protestor subsequently changed his or her mind following the additional script and 0 if otherwise (i.e. the respondent kept to his or her initial zero payment decision and so the final WTP amount remained therefore zero). The results indicate that the probability of reversing an initial protest zero increased significantly if the respondent was female although did not change significantly (at the 5% significance level or lower) with any other demographic variable. The experience of household water supply problems – specifically, low water pressure, discoloration and supply interruptions – is also found to have a mixed effect on the probability of a change of mind. Whilst experiencing low water pressure increases the probability of revising the original protest answer, water supply interruptions have no effect and water discoloration significantly reduces the likelihood of a revised answer.

[TABLE 5 ABOUT HERE]

What does emerge as significant is whether or not respondents found elements of the questionnaire difficult, although again the direction of the effect is not uniform. In all, three such variables are included. The first is the respondent's own rating of how difficult he or she found understanding the visual and textual information and preliminary questions relating to changes in water supply disruption risk reduction.<sup>16</sup> The second is again based on the respondent's own assessment but this time relates to whether, in a concluding question, the respondent stated he or she found the survey difficult overall. The third is based on the interviewer's assessment of how difficult

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<sup>16</sup> In total there were two risk questions of this type. Both questions were designed to familiarise respondents with, and test understanding of, a visual representation (10×10 grids) of 'before' and 'after' water supply interruption risks along with numerical (risk) information.

the respondent found, in particular, the valuation exercise. In the case of the first indicator, the finding is apparently that expressing difficulty with the risk (change) aspect of the valuation scenario is associated with less likelihood of revising a protest response in the light of the entreaty. However, in the case of the latter two variables, experiencing, or being perceived to experience, difficulty in the survey and the valuation questions increased the likelihood of a respondent, registering a protest zero, revising his or her response following the entreaty text being administered.

### *Effect of Entreaty on WTP Estimates*

While the main purpose of the entreaty, in both experiments, is to reduce protests, potential effects on WTP are also of interest. The results of the non-parametric estimation of mean and median WTP using the raw data, with protestors and outliers removed, are reported in Table 6 (with further details on the frequency distribution of responses to the payment ladder being reported in Tables A.1(a) and A.1(b) in the Appendix, for Experiments 1 and 2 respectively). Notably, in both experiments, there are no statistically significant differences in mean WTP between treatments in Experiment 1 or between the samples with- and without those protestors who subsequently stated a positive WTP amount in the light of the entreaty in Experiment 2.<sup>17</sup> But as mean WTP is based both on *positive* WTP bids and on *valid* zeros, we conducted some further analysis to see if the entreaty had discernible effects on any of these components.

[TABLE 6 ABOUT HERE]

Firstly, in the context of Experiment 1, we might ask what is the effect of the entreaty on the proportion of respondents reporting *valid* zeros? The answer to this question indicates a borderline significant difference (10% level) in the number of valid zeros reported between the two treatments at the payment principle stage ( $\chi^2 = 2.5939$ ,  $p$  value = 0.107). No differences were detected at the payment ladder stage. The results of the comparison of valid zeros are reported in Table 3 (top half). It thus appears that

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<sup>17</sup> Moreover, as depicted in Table 1 and Tables A.2 and A.3 (in the Appendix) there were practically no significant differences in the key socio-demographic variables, attitudes and behaviours between the censored sub-samples across the treatments, in both experiments.

the entreaty, in addition to reducing the number of protest zeros, has also had a minor reducing effect on the number of valid zeros in this experiment. In other words the additional script appears to have encouraged some respondents whom would otherwise have reported a ‘valid’ zero to instead report positive WTP bids.

It is interesting to ask why we might have fewer valid zeros under the entreaty treatment. A candidate answer might start with the observation that, as discussed, the standard procedure for identifying protests is crude. This, in the case of Experiment 1, may have resulted in some respondents in the ‘no entreaty’ treatment being incorrectly identified as ‘valid’ zeros. If so, then it is reasonable to assume that, under the entreaty treatment, incorrectly identified ‘valid’ zeros would be swayed to report their true WTP rather than a zero bid, explaining the lower proportion of ‘valid’ zeros. Alternatively, as also noted previously, it could be that the entreaty itself had a blunter impact than intended in terms of influencing the tendency to state both protest and valid zeros, and therefore increasing the likelihood of a positive WTP answer. Note that the within-sample design of Experiment 2 avoids this reduction in valid zeros (or, for that matter, any potential effects on initial positive WTP amounts) given that it is focused only on those respondents identified as stating protest zeros.<sup>18</sup>

Finally, to explore the influence of the entreaties on the *positive* WTP estimates, we ran interval regression models (Cameron and Huppert, 1989; Haab and McConnell 2002), for each experiment, using maximum likelihood estimation procedures to identify the main determinants of positive WTP values. The results of the regression model, for the pooled data (entreaty and non-entreaty treatments) for Experiment 1, are reported in Table 7.<sup>19</sup> Willingness to pay is found to increase with: income, membership of environmental organizations and ranking of environment as a priority. A key finding is for the variable ‘Entreaty(1)’. This is a dummy variable taking a value of 1 if a respondent received the treatment that included the entreaty and 0 otherwise. It appears that (positive) WTP is not influenced significantly by the

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<sup>18</sup> However, the issue of possible misidentification remains.

<sup>19</sup> Detailed information on the distribution of all covariates used in this regression across all relevant sub-samples and treatments can be found in Table A.2 in the Appendix. With the exception of the knowledge variable, the distribution of covariates does not differ across treatments for any of the sub-samples considered (full sample, valid responses, valid positives, valid zeros and protest zeros).

inclusion of the additional script given the insignificant coefficient on this dummy variable.

[TABLE 7 ABOUT HERE]

In the corresponding interval regression of determinants of positive WTP responses for Experiment 2, however, the entreaty is found to influence WTP significantly. Here, the influence of the entreaty is captured by a dummy variable ('Entreaty(2)') which takes a value of 1 if a respondent's positive WTP was their answer following the entreaty text (i.e. the respondent initially stated a protest zero but changed their answer after the entreaty, becoming what we labelled as a "new entrant") and 0 otherwise. Table 8 reveals that the coefficient on this variable is both negative and highly significant.<sup>20</sup> In fact, the parametric estimates of WTP that can be calculated from this model indicate that inclusion of the (revised) positive WTP values of this former group reduces the mean (positive) WTP by a little under £5 (from £30.17 to £25.60). This indicates, in this experiment at least, that respondents who initially protest then waver and state a revised positive WTP value take an implicit relatively conservative approach. In other words, the revised positive WTP amounts are, on average, significantly lower, than the positive WTP amount of respondents who answered thus at the first time of asking. Inspection of the frequency distribution of WTP over the payment ladder in Experiment 2 (Table A.1(b) in the Appendix) supports this finding.

[TABLE 8 ABOUT HERE]

In order to give a little more context to this result, we ask the following question. What would we 'predict' the average WTP of "new entrants" to be based on relevant socio-economic and demographic characteristics as well as other observed responses? We provide an approximate answer to this firstly by running an interval regression (using the same explanatory variables as in Table 8) on those respondents only who

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<sup>20</sup> Detailed information on the distribution of all covariates used in this regression across all relevant sub-samples and treatments can be found in Table A.3 in the Appendix. With the exception of Swindon/Oxford and water conservation, the distribution of covariates does not differ between protestors and those expressing a valid WTP, nor between new entrants and the group originally expressing a valid positive WTP.

stated a positive WTP at the first time of asking (i.e. “original entrants” who initially did not protest and thus did not receive the entreaty). We then use the results of this regression to calculate what WTP would have been expected to be for “new entrants”: specifically based on the characteristics of those respondents in this group.

This implied mean WTP value as *predicted* for these 56 respondents – making up our group of “new entrants” – is £33.77. This value is somewhat higher than the corresponding WTP of those who stated a positive value at the first time of asking (i.e. £30.17) and those protestors who (received the entreaty but) did not change their minds (i.e. 29.76). More significantly, this value (£33.77) is considerably higher than the estimate of WTP based on the *actual* (revised) responses of these “new entrants” (£25.60). Put another way, the WTP of these entrants is not what we would expect based on their characteristics alone. Differences across characteristics and responses – between valid positives (“old entrants”) and “new entrants” are illustrated in Table A.3 in the Appendix to this paper. We reflect further on this finding below in the discussion that follows this section.

## **5. Discussion**

Taken as a whole, our results appear to provide evidence that entreaties can be partially effective in dealing with the problem of protest zeros, reducing their occurrence by about a third in both our experiments. Moreover, this is achieved at little cost in terms of over-burdening the valuation scenario given the relative brevity of each of the additional texts. In both cases, the entreaty texts were designed explicitly to allay the concerns of potential (Experiment 1) and actual (Experiment 2) protestors with a focus on reasons that, we knew from earlier stages of the research, commonly underpinned protest answers in both of the policy contexts examined.

The question remains as to whether an entreaty is actually needed at all. Surely, it might be argued, any well-devised valuation scenario is the end-product of a research process, involving qualitative testing, in-depth interviews and so on. As such the final design will have minimised problems such as ‘high’ protest rates. This is an entirely reasonable argument and is not nullified by the inclusion of entreaty texts such as in this paper. In large part, motives expressed in justification of protest zeros, in both of

our experiments, were connected to beliefs about the *process* of how a policy change is provided rather than the *outcome* of the policy change itself. To the extent that such beliefs cannot straightforwardly be allayed within a conventional valuation scenario, the entreaty text serves a useful purpose. Moreover, using a within-sample approach as in the case of Experiment 2, it is possible to analyse the data with or without the (positive) WTP values of those who initially protested but subsequently revised their answer.

Both entreaties appear to have led to a significant reduction in protests. Nevertheless, it is evident that, in Experiment 2 for example, only a minority (about a third) of protestors changed their mind. By this same token, however, this also means that the majority of protestors stuck to their original response even in the light of the entreaty text. In this sense, the entreaty appears to be only a partial answer at best. In addition, there may be reasons to be cautious in any interpretation of the WTP responses of the one-third or so respondents that the entreaty appears to ‘return’ to the sample.

A positive construal (of these responses), for example, in the case of Experiment 2, is that the entreaty text could have given respondents an opportunity to ‘pause for breath’ and further reflect on the specified policy change. This might have been particularly useful for those protestors who stated, precipitously, that they would not even pay in principle. Indeed, one could query the efficacy of a payment principle question that arguably permits ‘easy protest’. We can only speculate, in Experiment 2, about how many protests there would have been if respondents were routed straight to the second stage valuation question. Evidence from Experiment 1, however, indicates that a significant proportion of protest responses can also occur at the payment stage as well. Less positively, and again in Experiment 2, we cannot rule out the possibility that respondents – on being taken through the entreaty text – may have understood it as a form of suasion or tacit cue that their initial (considered) answer was ‘wrong’. By implication then, these respondents may have reasoned that they simply should revise their response accordingly.

In this context, there are divergent interpretations of our finding, from the parametric analysis, that the mean of the positive WTP values of “new entrants” (in Experiment 2) was about one third lower than the corresponding mean for non-protestors (who

had a positive WTP for the specified change). On the one hand, one reading of this finding is that it could be taken as an indication that the former group did not appear to have been tempted, on average, to ‘over-correct’ (which is also apparently the case for Experiment 1, where the entreaty did not lead to an inflation of positive WTP values). On this view, the lower (parametric) mean of this group could be – on the face of it – plausible given the earlier reservations of these respondents about paying for the policy change.

On the other hand, and by contrast, it could be that this lower WTP is symptomatic of a reaction to the implied suasion in an entreaty. Put another way, those who stop their protest – i.e. our “new entrants” – might do this for genuine reasons related to their preferences (and which the entreaty uncovers) or they could feel ‘obligated’ to offer a positive WTP response which, on average, they keep – for motives that we do not observe – low. While, it is worth pointing out that we did seek to make the entreaty text in both experiments as neutral as possible, we cannot dismiss completely rule out the latter conjecture.

We can state, however, that the actual WTP of these “new entrants” is rather lower than what would be predicted based on e.g. the characteristics of these respondents. Depending on the extent to which we are prepared to assume that WTP primarily is determined by these observed influences, this might give cause for reflection about the efficacy of these revised WTP responses. Alternatively, it could reinforce the case for getting such respondents (i.e. “new entrants”) back within the analysis rather than disregarding their initial protest responses (as they apparently think rather differently about the policy change than inspection of observable predictors would suggest). Clearly, we are in danger of raising more questions than we are able to answer, given our available data. Yet this ambiguity itself perhaps urges some caution about drawing overly optimistic and premature conclusions about using entreaties to talk respondents ‘out of their (actual or potential) protests.

Moving beyond claim and counter-claim in order to understand more directly why a protestor may or may not revise a protest response could suggest the need for detailed qualitative testing as a future contribution. Perhaps more useful still could be the testing of different variations of an entreaty in order perhaps to get at the issue of the



impact of underlying incentives (conveyed within the text). Arguably, this is something that would be best undertaken in an experimental setting in the laboratory rather than the field.

## **6. Conclusions**

While drawn from distinct policy contexts, the purpose of both the CV experiments outlined in this paper was to test the usefulness of entreaties as a means to tackle protest zeros. In our first experiment (Experiment 1), the primary reason that respondents used to justify a protest zero response can be summed up as an inability to believe that the requisite tropical forest conservation can be achieved in practice. In our second experiment (Experiment 2), protesting respondents tended to state that antipathy towards paying higher water bills and the privatised water companies more generally determined their reaction to a proposed scheme to reduce the risk of household water supply interruptions.

In both cases, we explored whether an entreaty text can influence the likelihood that a respondent will protest. We did so, in Experiment 1, using a split sample approach. Almost half of respondents received a treatment that included an additional text designed to anticipate protest motives in eliciting WTP for conservation schemes. The remaining respondents received an otherwise identical treatment which did not include this additional text. In Experiment 2, we used a within-sample approach. Respondents stating a protest zero were read an additional entreaty text and then asked if they wished to revise their WTP response.

In both experiments, roughly the same proportions – about one-third – of protest respondents possibly were swayed by the entreaty text. In the case of Experiment 2, a total of 56 respondents who stated an initial protest zero opted to change their mind and so subsequently stated a positive WTP amount following the administration of the entreaty text. In Experiment 1, however, this inference about the influence of the entreaty is more indirect and is based on the difference in protest rates between the samples receiving the entreaty and non-entreaty treatments.

Notably, in both cases, the entreaty appears to have little effect on (non-parametric) mean WTP, calculated after removal of protestors. In other respects, there are notable differences in the results. In Experiment 1, respondents tended to protest at the payment elicitation stage of the valuation scenario rather than the payment principle stage. In Experiment 2 the opposite was found. Furthermore, in Experiment 2, mean WTP of those who state positive amounts at second time of asking, on average, was significantly lower than the analogous mean positive WTP of those who stated these amounts at the first time of asking. In Experiment 1, however, the entreaty had no significant effect on stated positive WTP amounts although it did seem to lead to a reduction in the number of valid zeros (and reflected in the fact that both protest and valid zeroes were reduced in rough proportion between non-entreaty and entreaty versions).

Interpretation of these findings involves some ambiguity. We appear to ‘get back’ (Experiment 2) or avoid antagonising (Experiment 1) a number of respondents who otherwise might have been discarded from the analysis. However, there remain legitimate questions regarding the quality of the responses gained as a result of this process. Where entreaties are administered to all respondents (as in one of the treatments in Experiment 1) it is possible that this text is a rather blunt instrument in that it influences responses beyond only the rate of protest zeroes. Furthermore, while we sought to keep our entreaty text as impartial as was feasible, it still may be construed as say a form of suasion. This problem might be particularly acute in within sample treatments (such as Experiment 2) where there is a target respondent for the entreaty text (i.e. a protestor). Any conclusion, therefore, that entreaties can be effective in dealing with the problem of protest zeros must be tempered by such uncertainties in interpreting the worth of responses made in the light of this additional text. In future work, it might be that some of these ambiguities could be reduced by testing different variations of an entreaty in order perhaps to get at the issue of how the underlying text might influence responses (or otherwise).

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## Appendix:

### Figure A.1(a): Entreaty used in Experiment 1

Some people have stated that they would not be willing to pay anything toward the protected area programme, **not because they do not value it** but because they **do not really think the proposed change is possible**.

For example, some people felt that **such a large expansion is not possible**; some **did not believe local institutions would be able to effectively manage the areas**; whilst others **simply did not trust** institutions in the developing countries to put all the money into the protected areas due to **corruption**.

These are **all valid** concerns however they do **unfortunately mean that it is not possible for us to tell whether or not these people place any value** on ensuring these tropical species and ecosystems are conserved.

**To do this we need estimates from people about how much it is worth to them, if anything, to ensure the protected area programme takes place.**

**So, for the purposes of this valuation we ask that you set aside any doubts you may have about the plausibility of the proposed expansion and focus instead on how much it would mean to you if these outcomes were truly in place.**

### Figure A.1(b): Entreaty used in Experiment 2

In this study we have found other people such as yourself say that they would not be prepared to pay anything for reducing the risk of future water use restrictions. This is not because they don't care about this risk or cannot afford to contribute something to reduce it. Rather it is because these people think, for example, that the Government should pay or that they have lost trust in the water companies.

It is understandable that people sometimes feel like this. But it does mean unfortunately that it is not possible for us to tell whether or not such people think that reducing the risk of water use restrictions is important or not. For that, we need to know whether it really is worth anything at all to these people to reduce the chance of the water shortages – in other words, whether you believe you would be better off or not.

**So in answering this next question, please put aside any concerns you may have about water companies, how well regulated they are, or your opinion of the Government. Instead, please focus only on what it would be really worth to you and your household to reduce the risk of future water use restrictions. I know this is not an easy thing to do but it is really important for us to try to find out about the benefits of reducing these risks for everyone.**



**Table A.1(a) Payment Ladder Experiment 1**

WTP (£)	No Entreaty			Entreaty		
	Freq.	Percent.	Cum.	Freq.	Proport.	Cum.
0	43	17.99	17.99	33	12.69	12.69
0.50	9	3.77	21.76	10	3.85	16.54
1	12	5.02	26.78	4	1.54	18.08
2	6	2.51	29.29	8	3.08	21.15
5	19	7.95	37.24	19	7.31	28.46
7	1	0.42	37.66	1	0.38	28.85
10	25	10.46	48.12	28	10.77	39.62
12	7	2.93	51.05	10	3.85	43.46
15	4	1.67	52.72	11	4.23	47.69
20	9	3.77	56.49	29	11.15	58.85
30	12	5.02	61.51	10	3.85	62.69
50	33	13.81	75.31	49	18.85	81.54
60	7	2.93	78.24	7	2.69	84.23
100	43	17.99	96.23	28	10.77	95.00
120	1	0.42	96.65	0	0.00	95.00
250	7	2.93	99.58	13	5.00	100.00
300	1	0.42	100.00	0	0.00	100.00
Total	239	100		260	100	

**Table A.1(b) Experiment 2**

WTP (£)	All valid responses (Pre-entreaty)			New Entrants		
	Freq.	Percent	Cum.	Freq.	Percent	Cum.
0	38	6.80	6.80	0	0.00	0.00
1	3	0.54	7.33	0	0.00	0.00
5	86	15.38	22.72	9	16.07	16.07
10	66	11.81	34.53	15	26.79	42.86
15	40	7.16	41.68	5	8.93	51.79
20	61	10.91	52.59	6	10.71	62.5
25	49	8.77	61.36	5	8.93	71.43
30	51	9.12	70.48	2	3.57	75
35	25	4.47	74.96	3	5.36	80.36
40	23	4.11	79.07	3	5.36	85.71
45	15	2.68	81.75	0	0.00	85.71
50	44	7.87	89.62	5	8.93	94.64
55	5	0.89	90.52	0	0.00	94.64
60	19	3.40	93.92	2	3.57	98.21
65	1	0.18	94.10	0	0.00	98.21
70	33	5.90	100.00	1	1.79	100.00
Total	559	100		56	100	

**Table A.2: Mean Values for Interval Regression Co-variates in Experiment 1 (Table 7)**

	No Entreaty					Entreaty				
	Full sample	All valid responses	Valid positives	Valid zeros	Protest zeros	Full sample	All valid responses	Valid positives	Valid zeros	Protest zeros
Income	£59,341	£57,258	£61,626	£37,916	£67,142	£55,853	£55,383	£56,784	£44,375	£62,132
Kids	0.5	0.5	0.5	0.7	0.6	0.7	0.7	0.7	0.8	0.7
Degree	70.5%	71.5%	77.0%	46.5%	64.2%	72.1%	71.9%	74.4%	54.5%	74.3%
Knowledge	90.9%	90.4%	93.4%	76.7%	96.2%	95.1%	96.2%	97.4%	87.9%	85.7%
Environmental Membership	14.1%	14.6%	16.8%	4.7%	13.2%	13.8%	13.5%	14.5%	6.1%	17.1%
Environmental Goods	3.2	3.3	3.3	2.9	3.1	3.1	3.2	3.2	2.8	2.9
High Environment Rank	11.7%	12.1%	14.3%	2.3%	11.3%	13.4%	13.5%	15.0%	3.0%	14.3%
Developed Country Help	85.6%	86.6%	91.3%	65.1%	84.9%	86.6%	86.2%	89.4%	63.6%	91.4%
No. of observations	298	239	196	43	53	298	260	227	33	35

**Table A.3: Mean Values for Interval Regression Co-variates in Experiment 2 (Table 8)**

	Full sample	Valid	<i>Of which:</i>		All protests	<i>Of which:</i>	
			Valid positives	Valid zeros		“New entrants”	Remaining protests
Swindon/Oxford	0.314	0.317	0.288	0.500	0.348	0.464	0.298
Income (£)	£31926.6	£32049.1	£33089.1	£15579.8	£32009.6	£33549.7	£31351.2
Sex	0.476	0.452	0.468	0.316	0.529	0.393	0.588
Impact_A	1.595	1.600	1.647	1.211	1.529	1.429	1.573
Impact_B	2.043	2.037	2.090	1.632	1.995	1.821	2.069
Impact_C	4.340	4.337	4.324	4.135	4.422	4.589	4.351
Bill Problem	0.055	0.054	0.050	0.108	0.059	0.054	0.062
Boil Water	0.007	0.003	0.004	0.000	0.016	0.000	0.023
Leakpipes	0.390	0.384	0.385	0.270	0.428	0.446	0.420
Environmental Membership	0.209	0.207	0.212	0.053	0.231	0.268	0.215
Supply Interruption	0.157	0.152	0.158	0.053	0.173	0.161	0.178
Water Conservation	0.895	0.914	0.925	0.711	0.850	0.946	0.809
Water Butt	0.245	0.245	0.252	0.108	0.254	0.268	0.248
Garden	0.698	0.693	0.693	0.553	0.743	0.786	0.725
Garden and Water Butt	0.245	0.245	0.250	0.105	0.258	0.291	0.244
No. of observations	746	559	521	38	187	56	131

**Table 1: Socio-Demographic Characteristics**

	Experiment 1				Experiment 2		
	No Entreaty		Entreaty		Valids	Protests	New entrants
	Valids	Protests	Valids	Protests			
Sex (% male)	47.5	37.1	39.8	50.1	45.8	52.9	39.3
Age (mean years)	44.2	43.3	44.2	45.4	43.4	45.0	45.0
Education: Degree level or above (%)	71.9	74.3	71.5	64.2	27.2	25.8	25.0
<i>Employment Status (%):</i>							
Employed (>30 hrs/week)	45.2	45.7	46.0	37.3	47.1	54.0	48.2
Employed (<30 hrs/week)	10.0	5.7	8.8	9.8	13.8	12.8	19.6
Unemployed	1.9	2.9	2.1	3.9	8.2	6.4	8.8
Retired	11.2	17.1	13.8	13.7	15.2	20.3	17.9
Gross annual household income (mean £)	55,383	62,132	57,258	67,142	31,898	32,010	33,550
Number of Observations	260	35	239	53	559	187	56

Note: Age/Income taken as mid-point of category/ band.

**Table 2: Reasons for Zero WTP**

STATED REASON FOR ZERO WTP	Experiment 1 (Split Sample)		Experiment 2 (Within Sample)		
	No Entreaty	Entreaty	Pre- Entreaty	Post- Entreaty: New Entrants	
Protest zeros	I don't believe the conservation programme would work	3%	4%		
	I'm concerned about corruption	3%	0%		
	Any payment for this conservation scheme should be voluntary OR funded in some other way than through taxes OR by reallocating revenues from existing taxes	16%	20%		
	I do not pay taxes/ not applicable	5%	5%		
	I object to the proposed solution (reservoir)			8%	11%
	I don't trust government	4%	3%		
	The water company should address leaky pipes first			15%	13%
	I object to paying higher taxes <u>OR</u> water bills	10%	10%	27%	29%
	Water companies make enough profits as it is			12%	18%
	I object to water companies being privatised			1%	3%
	The water company should pay for this			4%	7%
	The water company is inefficient			8%	12%
	The government should pay for this	6%	5%	3%	3%
	I do not believe the water supply risk reductions would actually happen			2%	1%
	I'd like to have more information before making a decision	2%	3%	3%	5%
	Other reasons	10%	4%	0%	0%
Valid zeros	I cannot afford to pay	25%	27%	11%	
	I prefer to give to causes other than conservation	8%	12%		
	I prefer to spend my money on other things than conservation	7%	1%		
	This conservation scheme is not a problem	2%	4%		
	I don't care about the issue of conservation	1%	0%		
	The reductions in risk of restrictions are not important			1%	
	The risk of restrictions is too low for me to care			1%	
	The benefits are too far off in the future			0%	
	I will not be living here in when the investment is completed			1%	
	Water use restrictions would not affect me			1%	
Total number of zero responses	96	68	224	56	

Note: Expressed as % of zero responses.  
The columns may not sum to 100% due to rounding error.  
n.a. = not applicable

**Table 3: Incidence of Protest and Valid Zeros With and Without Entreaty**

	No Entreaty <sup>b/</sup>		Entreaty <sup>b/</sup>	
	Pre-Entreaty <sup>c</sup>		Post Entreaty <sup>c</sup>	
	Protest zeros N (%)	Valid zeros N (%)	Protest zeros N (%)	Valid zeros N (%)
<b>Experiment 1</b>				
Payment Principle	12 (4.0%)	40 (13.4%)	8 (2.7%)	28 (9.3%)
Payment Ladder	41 (13.7%)	3 (1.0%)	27 (8.9%)	5 (1.7%)
<b>Experiment 2</b>				
Payment Principle	180 (24.1%)	36 (4.8%)	124 (16.6%)	36 (4.8%)
Payment Ladder	7 (0.9%)	1 (0.1%)	7 (0.9%)	1 (0.1%)

Notes:

- Protest and valid zeros expressed as % of total responses. Only respondents stating YES or DON'T KNOW went onto the second, payment ladder, stage.
- In Experiment 1, "Entreaty" refers to the additional (entreaty) text being present after the valuation scenario and immediately prior to the payment principle question being asked. "No Entreaty" refers to the version of the questionnaire with no additional text at all.
- In Experiment 2, the entries in the columns under the "No Entreaty" heading refer to the initial numbers and frequencies of protest and valid zeros at either the payment principle or payment ladder stages. After administration of the Entreaty some respondents changed their original response from a zero to a positive WTP. The entries in the columns under the "Entreaty" heading refer to those protest and valid zeros which still remain *after* the administration of the entreaty text (i.e. from people who did not change their mind). The Entreaty text was administered after the valuation question, either after the payment principle question or the payment ladder. This depended on at what stage the respondent protested. Protest zeros can differ, therefore, between the before and after entreaty administration at either the payment principle or ladder stages. Valid zeros do not differ by design (i.e. these respondents did not receive the entreaty text).

**Table 4: Logit Regression on Probability of Protesting at 2<sup>nd</sup> Stage (Experiment 1)**

<b>Variables</b>	<b>Coeff.</b>	<b>z-stat</b>	
Income (divided by 1000)	0.005	1.68	*
Degree	-0.561	-1.72	*
Long-term Conservation	-0.419	-2.40	**
Low Environment Rank	0.762	2.35	**
Recycle	-1.088	-2.54	**
No Developed Country Help	0.635	1.08	
Knowledge	-0.488	-0.86	
Entreaty(1)	-0.679	-2.32	**
Constant	1.232	1.38	
LR Chi <sup>2</sup> (8)	30.39		***
Pseudo R <sup>2</sup>	0.08		
N	471		

Notes: \*\*\*, \*\*, and \* represent statistical significance at 1%, 5% and 10% levels respectively. Variables are defined as follows: *Income*: midpoint of annual household income band (divided by 1000); *Degree*: dummy variable, coded 1 if degree level education or above; *Long-term Conservation*: attitude towards the likelihood that the new protected areas will lead to long-term conservation of species and ecosystems from 1 (very unlikely) to 5 (very likely); *Low Environment Rank*: dummy variable, coded 1 if respondents rank environment as lowest priority amongst 5 general issues; *Recycle*: dummy variable, coded 1 if recycles often or always; *No Developed Country Help*: dummy variable, coded 1 if respondents do not believe developed countries should help pay the costs of conserving tropical biodiversity; *Knowledge*: dummy variable, coded 1 if respondent has some knowledge of causes of biodiversity loss; *Entreaty(1)*: dummy variable, coded 1 if respondent received the additional script.

**Table 5: Logit Regression on Probability of a Protestor Changing Mind Following Entreaty Stage (Experiment 2)**

Variable	Coeff.	z-stat	
Sex	-1.298	-2.91	***
Swindon/Oxford	0.657	1.38	
Age	0.138	1.67	*
Age <sup>2</sup>	-0.001	-1.58	
Environmental Membership	-0.064	-0.14	
Income (divided by 1000)	-0.010	-0.88	
Low Education	-0.587	-1.34	
Risk Questions Difficulty	-0.486	2.15	**
Survey Difficulty	2.952	2.5	***
WTP Difficulty	0.303	2.07	**
Strong Views	0.814	1.14	
Process	-1.087	-1.78	*
Pay Bills, Why Conserve	-3.804	-1.6	
Water Discolouration	-2.570	-2.88	***
Supply Interruption	-0.248	-0.41	
Low Water Pressure	1.272	1.98	**
Constant	-5.794	-2.68	***
LR Chi <sup>2</sup> (15)	32.36		
Pseudo R <sup>2</sup>	0.234		
N	174		

Notes: \*\*\*, \*\*, and \* represent statistical significance at 1%, 5% and 10% levels respectively. Variables are defined as follows: *Sex*: dummy variable, coded 1 if male; *Swindon/Oxford*: dummy variable, coded 1 if respondent is from Swindon/Oxford, and coded 0 if from London; *Age*: age (years); *Age2*: age squared; *Environmental Membership*: dummy variable, coded 1 if member of environmental or conservation group/ organization; *Income*: mid-point of household annual income band (divided by 1000); *Low Education*: dummy variable, coded 1 if respondent left school at 16 or younger; *Risk Questions Difficulty*: coded from 1 (if stated that warm-up risk questions were very easy) to 5 (if stated that risk questions were very difficult); *Survey Difficulty*: dummy variable, coded 1 if stated, in response to a final debrief question, that the survey was overall difficult; *WTP Difficulty*: coded from 1 (if the interviewer assessed that the respondent found it very easy to state their WTP) to 5 (if the respondent was assessed to have found it very difficult to state their WTP); *Strong Views*: dummy variable, coded 1 if stated strong views about how to reduce water supply interruption risks; *Process*: dummy variable, coded 1 if stated that the ways in which water security is achieved are as important as having a secure water supply; *Pay Bills, Why Conserve*: dummy variable, coded 1 if respondent stated that s/he pays bill so why should s/he conserve water; *Water Discolouration*: dummy variable, coded 1 if experienced water supply discolouration in the past 12 months; *Supply Interruption*: dummy variable, coded 1 if experienced an interruption to water supply in the past 12 months; *Low Water Pressure*: coded 1 if experienced low water pressure (upstairs) in the past 12 months.



**Table 6: Non-parametric Lower-bound Mean and Median WTP (Protestors and Outliers Removed)**

	<b>N</b>	<b>Median WTP</b>	<b>Mean WTP</b>	<b>SD</b>
<b>Experiment 1</b>				
No Entreaty	239	£12.00	£40.19	£54.73
Entreaty	260	£20.00	£40.35	£57.11
<b>Experiment 2</b>				
Pre-Entreaty	559	£20.00	£25.30	£19.80
Post-Entreaty	615	£20.00	£25.00	£19.60

Note: These estimates are based on the raw data using lower bound of interval only. In Experiment 1, the Entreaty split had 3 missing observations while the No Entreaty had 5 missing observations.

**Table 7: Interval Regression Model on Positive WTP Only (Experiment 1)**

<b>Variables</b>	<b>Coeff.</b>	<b>z stat</b>
Income (divided by 1000)	0.009	5.61 ***
Kids	-0.083	-1.13
Degree	0.159	0.94
Knowledge	0.424	1.08
Environmental Membership	0.567	3.48 ***
Environmental Goods	0.060	0.73
High Environment Rank	0.524	2.78 ***
Developed Country Help	0.454	1.82 *
Entreaty(1)	0.073	0.52
Constant	1.500	3.26 ***
Sigma	1.353	
Log pseudo-Likelihood	-1103.72	
Wald chi <sup>2</sup> (9)	91.89	***
McKelvey & Zavoina's R <sup>2</sup>	0.15	
N	404	

Notes: \*\*\*, \*\*, and \* represent statistical significance at 1%, 5% and 10% levels respectively. Variables are defined as follows: *Income*: midpoint of annual household income band (divided by 1000); *Kids*: number of children <16 years of age in household; *Degree*: dummy variable, coded 1 if degree level education or above; *Environmental Membership*: dummy variable, coded 1 if member of environmental organization; *Environmental Goods*: frequency of choosing environmental goods over regular products from 1 (never) to 5 (always); *Knowledge*: dummy variable, coded 1 if respondent has some knowledge of causes of biodiversity loss; *Developed Country Help*: dummy variable, coded 1 if respondents believe developed countries should help pay the costs of conserving tropical biodiversity; *High Environment Rank*: dummy variable, coded 1 if respondents rank environment as top priority amongst 5 general issues; Entreaty(1): dummy variable, coded 1 if respondent received the additional script.

**Table 8: Interval Regression Model on Positive WTP Only (Experiment 2)**

Variable	Coeff.	z-stat	
Swindon/Oxford	0.316	5.10	***
Income (divided by 1000)	0.007	5.68	***
Sex	0.098	1.79	*
Impact_A	-0.023	-0.78	
Impact_B	-0.020	-0.65	
Impact_C	0.087	2.64	***
Bill Problem	-0.205	-1.75	*
Boil Water	0.390	4.97	***
Leakpipes	-0.103	-1.87	*
Entreaty(2)	-0.265	-2.93	***
Environmental Membership	0.219	3.21	***
Supply Interruption	0.054	0.71	
Water Conservation	0.229	2.35	**
Water Butt	-0.132	-1.10	
Garden	0.015	0.23	
Garden and Water Butt	0.170	1.39	
Constant	2.324	13.26	***
Sigma	0.621		
Log pseudo-Likelihood	-1373.43		
Wald chi <sup>2</sup> (16)	182.55		
McKelvey & Zavoina's R <sup>2</sup>	0.21		
N	564		

Notes: \*\*\*, \*\*, and \* represent statistical significance at 1%, 5% and 10% levels respectively. Variables are defined as follows: *Swindon/Oxford*: dummy variable, coded 1 if respondent is from Swindon/Oxford, and coded 0 if from London; *Income*: midpoint of household annual income band (divided by 1000); *Sex*: dummy variable, coded 1 if male; *Impact\_A*: coded from 1 (if Level A water use restrictions have no impact on day-to-day activities) to 5 (if restrictions have a very large impact); *Impact\_B*: coded from 1 (if Level B water use restrictions have no impact on day-to-day activities) to 5 (if restrictions have a very large impact); *Impact\_C*: coded from 1 (if Level C water use restrictions have no impact on day-to-day activities) to 5 (if restrictions have a very large impact); *Bill Problem*: dummy variable, coded 1 if had a water/ sewerage billing related problem in the last 12 months; *Boil Water*: dummy variable, coded 1 if received a notice to boil water in the last 12 months; *Leakpipes*: dummy variable, coded 1 if believed that fixing leaky pipes was a priority; *Entreaty(2)*: dummy variable, coded 1 if the final stated positive WTP was a revised value following the entreaty; *Environmental Membership*: dummy variable, coded 1 if member of environmental or conservation group/ organization; *Supply Interruption*: dummy variable, coded 1 if experienced an interruption to water supply in last 12 months; *Water Conservation*: dummy variable, coded 1 if stated a positive attitude to conserving water; *Water Butt*: dummy variable, coded 1 if has a water butt; *Garden*: dummy variable, coded 1 if has a garden; *Garden and Water Butt*: dummy variable, coded 1 if has a garden and a water butt.

**Figure 1: Structure of the Valuation Scenario and Entreaty Administration**

<b>Experiment 1 (split-sample)</b>		<b>Experiment 2 (within-sample)</b>		
<b>Version 1 (No Entreaty)</b>	<b>Version 2 (Entreaty)</b>	<b>If does not protest:</b>	<b>If protests at payment principle stage:</b>	<b>If protests at WTP/ payment ladder stage:</b>
	<b>ENTREATY</b> ↓			
<i>Payment principle</i>	<i>Payment principle</i>	<i>Payment principle</i>	<i>Payment principle</i> ↓	<i>Payment principle</i>
			<b>ENTREATY</b> ↓	
			<i>Payment principle (repeat)</i> ↓	
↓	↓	↓	↓	↓
<i>Payment ladder</i>	<i>Payment ladder</i>	<i>Payment ladder</i>	<i>Payment ladder</i>	<i>Payment ladder</i> ↓
				<b>ENTREATY</b> ↓
				<i>Payment principle (repeat)</i> ↓
				<i>Payment ladder (repeat)</i>