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The Effect of Varying the Causes of Environmental Problems on Stated WTP Values: Evidence from a Field Study[*]

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Abstract

Standard applications of utility theory assume that utility depends solely on outcomes and not on causes. This study uses a field experiment conducted in the Netherlands to determine if alternative causes of an environmental problem affect willingness to pay to ameliorate it. We find evidence supporting the hypothesis that people are willing to pay significantly more to correct problems caused by humans than by nature (the "outrage effect"), but find no support for the hypothesis that "moral responsibility" matters. We also find support for the hypothesis that stated willingness to pay values obtained via "cheap talk" and "consequential" treatments are lower than without inclusion of these protocols.

Key words: field experiment, endangered species, non-market valuation

1. Introduction

Outcomes of environmental valuation studies frequently are interpreted through the lens of a 'purchase model' [13] in which utility is a function of outcomes or consequences only. This perspective has been challenged both by psychologists and economists who have recognized the importance of context and attributes of the good being valued. This work implicitly provides a transition from the notion of pre-existing well-behaved preference orderings toward theories of 'constructed preferences', where *true values* of goods and services might not exist, but rather are conditional upon context. For example, Kahneman *et al.* [13], Kahneman and Ritov [14], DeKay and McClelland [8], and Brown *et al.* [1] provide evidence that people are willing to pay (WTP) more to avoid an environmental problem if they think it is man-caused than if they think it is an outcome of nature. Kahneman *et al.* [13] refer to this as the "outrage effect". Their empirical results suggest that intentional harm caused by humans is considered more upsetting than unintentional harm, and therefore triggers a larger WTP response to ameliorate the problem.

These findings have not settled the debate; rather, they have created another one. Walker *et al*. [21] dispute the suggestion that people are more upset about man-made disasters than about natural ones, and are hence willing to contribute more to undo them. In a study on how the cause of an environmental problem affects the disparity between WTP and willingness to accept (WTA), they find the *opposite result* for their WTP values – WTP to undo harm caused by humans was lower than WTP for natural damages. Rather than an "outrage effect" triggered by the distinction between human versus natural causes *per se*, Walker *et al.* [21] hypothesize that WTP is driven by the degree of responsibility that people feel for the damages.[1] Building on this hypothesis, Brown *et al.* [1] postulate that "the identification of a

negligent party that has the ability to pay for some sort of restitution (such as a corporation) will lower the general public's WTP, possibly even below WTP if the loss were caused by a natural process" ([1], p.490].

In this paper, we use responses from 1335 Dutch households to sort out the various arguments. The specific case we consider is conservation of a locally threatened species (seals in the Netherlands). Our first objective is to test whether WTP indeed increases when humans instead of nature (a virus) cause the harm. To disentangle outrage and responsibility effects, we distinguish between two (unintentional) types of human causes: (i) global warming, where society at large is responsible (and arguably everyone, albeit only minimally), and (ii) drilling for oil and gas, where in their pursuit of profits, industry causes the damage. The outrage effect predicts that WTP for both human causes exceeds WTP for the natural cause. The responsibility effect predicts that WTP to undo harm caused by oil- and gas-drilling firms will be lower than WTP to undo harm caused by global warming (and possibly even lower than WTP to undo damages caused by a natural virus).

Our second objective is to investigate the effects of varying the framing of the WTP question in three alternative ways. Cummings and Taylor [6, 7], List [15], and Carson *et al.* [4] find that people make lower bids in hypothetical valuation exercises when reasons for hypothetical bias are explicitly discussed or when they are told that their responses to valuation questions will have real consequences. We test these issues in our field study and elicit WTP with a hypothetical valuation question, with a hypothetical question combined with a form of cheap talk, and finally with a hypothetical question indicating that the study results will be considered by policy makers. The factorial experimental design employed allows causes to be crossed with scripts so that both types of treatments can be independently varied.

Several interesting insights are obtained. First, we find evidence supporting the outrage hypothesis. People are willing to pay significantly more to protect seals when they appear to be threatened by an act of mankind (oil and gas drillers, greenhouse effect), rather than an act of nature (virus). Second, we fail to find evidence in support of the responsibility hypothesis. Third, we find that: (i) stated values obtained using cheap talk and consequential devices are significantly lower than comparable values obtained using a hypothetical question without these treatments, and (ii) stated values across the cheap talk and consequential treatments are statistically indistinguishable.[2] A fourth result potentially sheds light on another ongoing argument in non-market valuation. Cummings *et al.* [5] compare the outcomes of actual and hypothetical bids and find that hypothetical referenda are not incentive compatible. Haab *et al.* [11] debate this conclusion, contending that it may be due to heteroskedasticity. Because we vary the bid presented across panellists, we can test for heteroskedasticity using the method developed by Cameron and James [3]. While the script affects bid levels, we do not find support for the hypothesis that the variance is affected when we vary the phrasing of the script. This outcome provides weak support for the position of Cummings *et al.*, but of course does not imply that heteroskedasticity will not be a problem in cases where real payments are considered – exploring this issue is left for future work.

2. Data and experimental design

Data were obtained from a survey of participants in the CentERpanel, which consists of more than 2,000 households in the Netherlands. Panel members are selected to be representative of the Dutch population.[3] Panellists receive a "netbox" from CentER, Tilburg University, so that they can retrieve and return questionnaires via a television. To ensure a good response rate, before panellists are selected, they are interviewed to investigate their

commitment to completing questionnaires to be sent each week. In practice, when given the chance, a large majority of households agree to be part of the CentERpanel.

The analysis focused on declines in the seal population in the Waddenzee (an estuary in the North of the Netherlands), a problem that has been widely publicized in the Netherlands for many years. The seal population reached a low point of about 300 animals in the 1970s. Currently the number of animals has recovered to some 2,000 seals, but that number is still much lower than the 18,000 seals that lived in the Waddenzee in the beginning of last century. The seal population is threatened by three possible and distinct developments. First, new diseases (especially certain viruses) have taken a severe toll on the population in the past and continue to pose a serious threat.[4] Second, climate change and the associated rise of the sea level might trigger the disappearance of the seal's breeding grounds. Third, commercial oil and gas drilling may have the same effect, not because the sea level rises, but because the land level falls. The threat from viruses represents the case in which the seal population may be harmed by natural causes for which no societal group is responsible, whereas with oil and gas drilling, actions taken by a comparatively small group of people for private gain contribute directly to the species hardship. Climate change represents an intermediate situation in which virtually everyone is to some extent responsible for the problem.

The survey began with a brief introduction (common to all groups), in which attention was directed to the declining seal population. Then, panellists were presented with a description of one of the three types of threats along with a plausible mitigation measure. These scripts, labelled virus, climate change, and oil and gas drilling, are shown below in translation to English from Dutch.

1. Virus: A number of factors continues to threaten the seal population. One important threat is a new virus that undermines the species' resistance to various diseases. The

origins of the virus are unknown, but it is regarded as a "natural enemy" of the seal population. The spreading of the virus is a natural process, independent of human actions. It is possible that, without any preventive actions, the seal population in "the Waddenzee" falls by some 50%. An effective preventive measure would be a vaccination program.

2. Climate change: A number of factors continues to threaten the seal population. One important threat is climate change, mainly caused by burning of fossil fuels. Climate change (or the greenhouse effect) is a global problem because all people using fossil fuels are responsible for the emissions of carbon dioxide in the atmosphere (and not simply people in the Netherlands). An important risk of climate change and the associated rise of the sea level is that breeding grounds will be submerged for longer periods. This will negatively impact on the ability of female seals to deliver and feed young seals. It is possible that, without any preventive actions, the seal population in "the Waddenzee" falls by some 50%. An effective preventive measure would be elevating the existing sand banks by adding sand to them.

3. Oil and gas drilling: A number of factors continues to threaten the seal population. One important threat is drilling for gas by oil and gas companies in "the Waddenzee." An important risk of gas exploitation is that the land level will fall so that breeding grounds will be submerged for longer periods. This will negatively impact on the ability of female seals to deliver and feed young seals. It is possible that, without any preventive actions, the seal population in "the Waddenzee" falls by some 50%. An effective preventive measure would be elevating the existing sand banks by adding sand to them.

After the threat was described, panellists were asked to value conservation measures (vaccinations or elevating sand banks, depending on treatment type) to protect seals from further harm.[5] In each case, panellists were asked one discrete choice valuation question using one of three scripts.[6] Scripts used (labelled hypothetical, hypothetical/cheap talk and hypothetical/consequentialism) are shown below, again in translation to English from Dutch. A WTA script also was included in the survey. Responses to this script are analysed in Bulte *et al.* [2].

1. Hypothetical: The government can decide to take special measures to protect the seal population from the above-mentioned threat. Such measures, however, are costly. Would you be willing to pay a one-time amount of DFL X to support protection of the seal population? All the money would be used to finance a vaccination program/elevation of sand banks (choose appropriate case).

"yes"
"no"

2. Hypothetical/Cheap Talk: The government can decide to take special measures to protect the seal population from the above-mentioned threat. Such measures, however, are costly. Would you be willing to pay a one-time amount of DFL X to support protection of the seal population? All the money would be used to finance a vaccination program/elevation of sand banks (choose appropriate case).

   Note: this is a hypothetical question! You don't have to actually pay the money. In general, people experience difficulties answering hypothetical questions. People typically bid more money then they are really willing to pay.

   One reason why people might be tempted to bid too much is as follows. People try to accept or reject a bid based on their evaluation of the "true value" of the commodity (in this case, seal conservation in "the Waddenzee"). But if people should actually make the payment, they also consider that they can spend their money only once and that money spent on seal conservation is not available for other purchases.

   When answering the bid question below, try to think whether you are really willing to pay this amount for the conservation of seals. Try to imagine that this amount of money is no longer available to finance other purchases.

   Would you be willing to pay a one-time amount of DFL X to protect the seal population?

   "yes"
   "no"

3. Consequentialism: The government can decide to take special measures to protect the seal population from the above-mentioned threat. Such measures, however, are costly. Would you be willing to pay a one-time amount of DFL X to support protection of the seal population? All the money would be used to finance a vaccination program/elevation of sand banks (choose appropriate case).

   Note: the results of this study will be made available to policy makers, and could serve as a guide for future decisions with respect to taxation for this purpose. It is important that you think before answering the question.

   Would you be willing to pay a one-time amount of DFL X to protect the seal population?

   "yes"
   "no"

The survey concluded with an open-ended de-briefing question that invited panellists to provide comments about any of the questions. About 80% of panellists did so. Demographic information (income, schooling, age, gender, marital status, family size, province of residence) about panellists was not collected in the survey because it already was available from CentERpanel.

Regarding the elicitation of values, the cheap talk script is a shortened and revised version of the scripts used by Cummings and Taylor [7] and List [15]. While their scripts were longer, orally presented, and describe hypothetical bias in detail, a much shorter version was used here to reduce the amount of material presented to panellists. Our script could be thought of as similar to the shorter cheap talk scripts used by Cummings and Taylor [7] and Poe *et al*. [17], which *failed to* eliminate hypothetical bias. Also, use of consequentialism followed as closely as possible Carson *et al.* [4] and Cummings and Taylor [6], who use randomization devices to provide subjects with uncertainty about whether the exercise will actually be economically binding.[7] In theory, use of consequentialism should provide panellists with incentives to state their true preferences.

Our 3x3 experimental design crossed the three causes (virus, climate change, and oil and gas drilling) with the three valuation scripts (hypothetical, hypothetical/cheap talk, and consequentialism). Each panellist was randomly assigned to one of the nine treatment cells. Within each cell, each panellist was presented with a randomly drawn bid from the set (DFL 10, 40, 80, 120, where DFL 2.2 ≈ Euro 1≈ US$1). A preliminary version of the questionnaire was administered to 100 panellists who were randomly selected for a pilot test. Among other things, the pilot was used to establish that the questions were understandable and that the bids presented in the experiment roughly spanned the range of values expressed by CentER

panellists. The instrument was then revised and in September 2001, it was sent to all panellists who did not participate in the pilot experiment. Panellists had 5 days to complete the survey 'on line,' and no subject was assigned to more than one treatment. In total, 1819 panellists responded--a response rate of more than 95%; and all panellists who responded answered all of the questions presented. Eliminating the 433 panellists that received WTA valuation question reduces the sample to 1386. After reviewing responses to the de-briefing question, the sample was further reduced to 1335 because 51 panellists said either that the threat to seals was not real or that the solution proposed would not work. The relatively low number of objections to the proposed method of intervention is consistent with Kahneman *et al*. [13], who argue that outcomes are likely insensitive to the types of intervention (also note that both elevation of sand banks and vaccination programs are "plausible" interventions; both have been mentioned in newspapers). Nevertheless, whether these 51 panellists are eliminated from the data set has virtually no effect on the results presented in the next section.[8]

3. Analysis

This section analyses whether panellists said they would pay the bid presented to reverse damage to the seal population. Analysis begins by comparing fractions of panellists across treatments that stated they would pay the amount presented in the survey to prevent further harm to the seal population in the Waddenzee. These comparisons, presented in Table 1, should be considered only as suggestive because the percentage of responding panellists that were shown the four bid values differs between treatment cells. As demonstrated momentarily, the likelihood that a panellist would offer to pay the amount presented is smaller for larger bid values. Nevertheless, it is useful to obtain at least a rough idea about treatment effects before proceeding with a more detailed analysis. In pairwise comparisons of

proportions shown in Table 1, panellists in the virus/hypothetical with cheap talk cell are significantly less likely at the 1% level to agree to pay the stated bid amount than panellists in all other cells. Pairwise comparisons of proportions setting oil and gas drilling/hypothetical with cheap talk against climate change/consquentialism, virus/consequentialism, and climate change/hypothetical with cheap talk also were significantly different at the 5% level.[9]

The dichotomous choice responses obtained can be modeled parametrically by specifying the linear WTP function shown in equation (1).[10]

$$Y=\beta_0+\beta_1 DRILL+\beta_2 CLIMATE+\beta_3 CHEAPTALK+\beta_4 HYPOTHETICAL+Z\gamma+u \qquad (1)$$

WTP to protect seals (Y) is expressed in terms of treatment effects (*DRILL*, *CLIMATE*, *CHEAPTALK*, *HYPOTHETICAL*), a vector of controls for panellist characteristics (Z), and an additive stochastic preference term (u). In equation (1), the β_j and the elements of γ are coefficients to be estimated and u is assumed normally distributed with variance σ^2. The constant term, β_0, is interpreted as WTP to avoid the virus threat when values are elicited using consequentialism.

Equation (1) is estimated using binomial probit because values of WTP are latent. Estimates of β and γ in equation (1) are recovered using methods developed by Cameron and James [3]. Results are shown in Table 2. Standard errors of coefficient estimates were computed via Taylor series expansion. Covariates include treatment cell dummy variables and controls for gender, gross household income, schooling, years of age, and whether the panellist lives in Friesland or Groningen (the nearest provinces to the Waddenzee). Other controls, such as panellists' marital status and number of children were included in unreported regressions but coefficients of these variables never differed significantly from zero at the 5% level. Because treatments were randomly assigned to panellists, they are orthogonal to panellist

characteristics. Thus, provided that the true model of WTP is linear as shown in equation (1), alternative choices of controls have little effect on the estimated coefficients of the treatment variables.

Six aspects of Table 2 are noted prior to discussing the main results. First, estimates of expected WTP for each panellist, computed by inserting coefficient estimates into equation (1) are always positive, ranging from 0.41 Dfl to 156.76 Dfl, with an average of 81.08 Dfl across the entire sample.[11] Second, the null hypothesis that all nine treatments have the same effect on WTP (H_0: $\beta_1=\beta_2=\beta_3=\beta_4=0$) is rejected at the 5% level using a likelihood ratio test. Third, panellists are significantly less likely at the 5% level to say they would pay higher bid values than lower bid values. Fourth, women's WTP is larger than WTP among men. Fifth, gross household income, years of age, schooling, and proximity to the Waddenzee are not significant determinants of WTP.[12] The result for the variable measuring proximity to the Waddenzee suggests that the use value of seals is unimportant. Sixth, the coefficient of the bid value is a point estimate for $-1/\sigma$, so that an estimate of σ across all treatments (see Column 4) is (1.0/0.00717)=139.47. In this context, a question arises whether σ varies across different treatments, creating the type of heteroskedasticity problem highlighted by Haab *et al*. [11] in their comment on Cummings *et al*. [5]. Additional regressions were run (not presented here), specified in the same manner as equation (1), except that the price variable was interacted with the four treatment variables. A likelihood ratio test indicates that the null hypothesis that coefficients of these interaction terms are jointly zero is not rejected at the 5% level; thus values of σ across all treatments are statistically indistinguishable.

Does the source or cause of an environmental problem affect WTP or, in other words, does the outrage effect exist? As typically applied, standard utility theory holds that utility

depends solely on outcomes, and that the cause is unimportant. If people value only outcomes and do not care about the cause, then in equation (1), $\beta_1=\beta_2=0$. This joint hypothesis is rejected at the 5% level using a likelihood ratio test. Also, t-tests reject the individual null hypotheses that $\beta_1=0$ and $\beta_2=0$ at the 5% significance level. Thus, the oil and gas drilling and the climate change treatments differ from the virus treatment: people are willing to pay more to avoid man-caused environmental harm than if the harm occurs by an act of nature. These results broadly support the existence of an outrage effect and are consistent with earlier work using museum visitors and undergraduates as subjects ([13], [14], [8], [1]). [13]

Interestingly, and in contrast to the proposed "moral responsibility effect" advanced by [1] and [21], results in Table 2 do not reject the null hypothesis that $\beta_1=\beta_2$ at the 5% level. Thus, WTP is not significantly different at conventional levels when the harm is caused by a specific group of people (oil and gas firms), as compared with when it is caused by society as a whole (climate change from greenhouse gas emissions). In fact, contrary to predictions of the moral responsibility hypothesis, we find that WTP to undo harm caused by corporations does not differ significantly at the 5% level from WTP to undo harm caused by society. An alternative explanation for our result could be that people consider harm caused for a corporation's profits to be more "unfair" than harm caused by society at large, triggering more "outrage" and a larger contribution to offset the damages. Analysing this issue in greater detail is left for another occasion, but see [9] and [10] for a discussion of "fairness" in economic thinking.

Does the method of eliciting values affect panellists' WTP? If elicitation method does not matter, then in equation (1) $\beta_3=\beta_4=0$. Using a likelihood ratio test, this joint hypothesis is rejected at the 5% level. Also, in individual t-tests at the 5% level, the null hypothesis that

$\beta_3=0$ is not rejected and the null hypothesis that $\beta_4=0$ is rejected, implying that responses in the hypothetical treatment differ from those in the consequentialism treatment, but there is no such difference between responses in the cheap talk and consequentialism treatments. Further, the null hypothesis of no difference between the hypothetical treatment and the cheap talk treatment (H_0: $\beta_3=\beta_4$) is rejected using a likelihood test at the 5% level. Because heteroskedasticity does not explain these differences, the logical conclusion is that the hypothetical treatment yielded the largest WTP values. Whether these results suggest that value statements in cheap talk and consequentialism treatments map into actual preferences is open for debate; yet, combined with findings from previous studies, these results support the notion that both the cheap talk and consequential treatments can provide plausible value estimates (e.g., Cummings and Taylor [6], Cummings and Taylor [7], List [15], Carson *et al.* [4]).[14]

The previously discussed probit estimates are obtained under the assumption that the error term, u, in equation (1) is normally distributed. To examine the possible contribution of this assumption to our results, we considered two alternative tests that do not impose this distributional assumption: (1) Chi-square tests for response differences between cause and elicitation scripts and (2) Turnbull lower bound estimates of WTP (Haab and McConnell [12], pp. 72-78). First, 72 Chi-square tests for differences in treatment effects (available from the authors on request) were carried out using the frequencies of yes/no responses to the valuation question in 2x2 contingency tables for each bid point in the experimental design (see Sheskin [18]).[15] Eight tests identified significant differences at the 5% level that are consistent with the probit results discussed above (differences identified are between the hypothetical and hypothetical/cheap talk scripts, the hypothetical and consequentialism scripts, the oil/gas

drilling and virus scripts, the oil/gas drilling and climate change scripts, and the climate change and virus scripts. Two of the Chi-square tests show a significant difference between the hypothetical/cheap talk and consequentialism scripts. However, in one case, the fraction of "yes" answers to the hypothetical/cheap talk script exceeded that for the consequentiaism script, whereas in the other case the reverse outcome obtained. This outcome may be responsible for the result reported in Table 2 indicating no significant difference between these two treatments. Overall, the comparatively small number of significant differences found in the Chi-square tests suggest that differences between treatment effects may not be large and the probit analysis may be allowing a few design points to dominate the results.

Second, the Turnbull estimates (available from the authors on request) are roughly the same order of magnitude but exhibit less variation across treatments than do those based on the probit estimates. Consistent with the results above, using standard differences in means tests we find that the Turnbull estimates are significantly larger at the 5% level for treatments using the hypothetical question than for those using either cheap talk or consequentialism. Estimates are generally larger for the treatment cells involving oil and gas drilling, but the differences are not always statistically significant at the 5% level. In any case, the Turnbull estimates are broadly consistent with those obtained from the probit analysis.

4. Conclusions

We conducted a field experiment using a large Dutch panel to examine the effects of context and framing on the WTP to conserve seals in the Netherlands. Based on our probit analysis, we cautiously draw several conclusions. We find that WTP to protect seals from harm is affected by the nature of the threat. The patterns of results suggest that the "outrage

effect" plays a role. We also find that framing matters – stated WTP is conditional on whether bid values are elicited with hypothetical questions, or using a hypothetical format that either contains "cheap talk" or mention of possible consequences of the survey. We find that stated values in "cheap talk" and "consequentialism" treatments are significantly lower than stated values in a hypothetical question without these additions. We also find that alternative tests that do not impose the normality assumption do not discriminate between these effects as sharply as those based on the probit analysis.

Consistent with earlier psychological work, we find evidence to support the hypothesis that preferences are conditional on context – they appear to be constructed on a case-by-case basis. From a normative perspective, if both outcomes and causes are important, then a good deal of economic theory, including CVM, should be reconsidered as utility is typically measured over levels, not over levels and what induced that level. In a positive sense, if antecedents are important, then policymakers must take into account this piece of information when crafting optimal policy.

On the brighter side, the finding concerning the method of eliciting WTP is encouraging. Since it is difficult to move beyond hypothetical surveys, it is of considerable interest to understand whether panellists take the valuation question seriously. Interestingly, the cheap talk and consequentialism treatments yield comparable value estimates. This suggests that CVM studies can be improved by including these devices.

One might speculate that providing additional information about the consequences of the stated bid (in terms of foregone consumption opportunities or impact on policy-making) triggers a move from one mental map to another—almost as if a switch is flipped. When responding to purely hypothetical questions, people may not provide their full attention. Yet

when reminded of the consequences of their actions, a 'serious' response is warranted. Our results indicate that exactly how people are reminded does not seem to matter much. We suspect this will be a topic of considerable future interest given that CVM remains the "only game in town" to gather total values of non-marketed goods and services.

Endnotes

[1] The idea for a moral responsibility effect originates from results in Kahneman and Ritov [14] where the human-versus-nature effect is large in case of general pollution but small in case of a human predator. Note, however, that in a similar setting Kahneman *et al.* [13] find, on the contrary, a large effect in case of a human predator, so that the issue seems far from settled.

[2]Of course, it is an open question how these hypothetical payments would compare to the case of real payments. Institutional restrictions on how the panel could be used prevented asking them to make actual cash payments.

[3]Additional information regarding the panel is available at www.centerdata.nl.

[4]As a matter of fact, one of these three threats did affect the seal population shortly after completing the data collection. A virus killed a significant share of the population and experts predict that as much as half of the population might die as a result.

[5] In order to get realistic cases, we have to vary not only the cause but also the solution (proposed intervention) – a vaccination program for the virus threat and elevation of sand banks for the climate change and gas drilling cases. Subramanian and Cropper [20] show that people could also care about the characteristics of regulatory programs, but Kahneman et al. [13] suggest that this effect is relatively unimportant. We ignore the effect of different interventions but, as described more fully below, delete 51 observations where panellists used the debriefing question to object to the proposed solution.

[6]The dichotomous choice approach was used in the valuation exercise because it appears to be incentive compatible [5], but it has the disadvantage that survey panellists' willingness to pay values are not directly revealed.

[7]To avoid misleading panellists, environmental policymakers in the Netherlands were informed of the study before execution and subsequently briefed on the major results.

[8]Another tabulation of responses to the de-briefing question suggests that some people are opposed to paying for environmental problems caused by oil and gas firms. One conjecture is that these panellists might provide a value because it is the only available way to register their unhappiness with the situation. However, only 33 of 450 panellists who received the oil/gas drilling script stated that they thought firms should pay and that the panellists themselves should not; and of these persons, 31 refused to pay the bid value presented. These 33 responses were retained in the data set. We thank Mark Dickie for encouraging the investigation of this point.

[9] Difference between means tests were performed to test for demographic differences between treatment cells. In the cases of age, whether a partner is present, and gender, the homogeneity null was never rejected at the 5% level assuming unequal population variances (Sheskin [18]). For gross income, the null hypothesis of no difference between cell means was rejected in one pair-wise comparison, but this outcome occurs because the income of one sample member was an outlier. Mean numbers of household members and mean numbers of children present in the household, however, exhibited more variation between treatment cells and the null hypothesis of no difference between means of these variables was rejected in a few instances. Details of these tests are available from the authors on request.

[10]A log-linear WTP model also was estimated. Results for both treatment effects and control variables tell essentially the same story as those for the linear WTP model estimates described below.

[11] Averages of panellists' expected WTP by cause and by elicitation method are available from the authors on request.

[12]Other income measures tried were net income of panellists and gross and net income of the panellist's household. When substituted for gross income of panellists in the Table 2 regression, these variables were also insignificant. Dummy variables defined on the quartiles of all four of these variables also were tried with no appreciable change in results. Details are available from the authors upon request.

[13] Now that we find that cause matters, it is of course important to rethink the formal model that underlies the CVM. Smith [19] introduces the idea to use a CES sub function in the indirect utility function, in case one wants to relax the standard assumption of perfect substitutability, in order to get a parameter for a degree of substitution. A similar line could be explored in this case but we leave that exercise for future work.

[14]See List and Shogren [16] for a literature review of the comparison between hypothetical and actual statements of value.

[15]Thirty-six (4x3x3) Chi-square tests compared responses to two elicitation scripts for given causes and bid values, and 36 additional tests compared responses to two causes for given elicitation scripts and bid values. Results of these tests are available from the authors on request.

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Table 1: Proportion of "Yes" Responses by Treatment

Treatment	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	0.526 (0.038)	0.356 (0.041)	0.501 (0.040)
Climate Change	0.597 (0.038)	0.503 (0.041)	0.488 (0.041)
Oil and Gas Drilling	0.593 (0.041)	0.616 (0.038)	0.558 (0.039)

[a]Standard errors in parentheses.

Table 2: Probit Estimates of Treatment Effects (n=1335)

Variable	Mean	Estimates of β and γ (std. error)
Constant	----	53.73* (27.78)
Bid Value	62.11	----[b]
Oil/gas drilling	0.338	43.82* (13.22)
Climate change	0.321	28.33* (12.75)
Virus	0.340	----[a]
Hypothetical/Cheap Talk	0.317	-4.44 (12.16)
Hypothetical	0.344	30.42* (12.71)
Consequentialism	0.339	----[a]
Panellist is male	0.546	-39.69* (11.21)
Household monthly gross income (in Dutch guilders)	14099.83	-0.667E-05 (1.41E-06)
Panellist has primary education	0.045	-1.78 (27.54)
Panellist has secondary education	0.363	7.80 (16.44)
Panellist has university education	0.120	----[a]
Panellist has vocational education	0.473	27.24 (16.21)
Years of age	46.12	0.03 (0.353)
Proximity to the Waddenzee	0.082	-10.48 (17.93)

[a]Denotes omitted dummy variable.
[b]The estimate of σ is -(-1/0.00717)=139.47, with standard error of 17.08.
*Denotes coefficient significantly different from zero using a t-test at the 5% level or lower.

TABLES AVAILABLE FROM THE AUTHORS
ON REQUEST

Table A1: Experimental Design: Sample Sizes by Treatment (N=1335)

	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	Dfl 10; n = 40 Dfl 40; n = 38 Dfl 80; n = 37 Dfl 120; n = 50	Dfl 10; n = 41 Dfl 40; n = 29 Dfl 80; n = 33 Dfl 120; n = 29	Dfl 10; n = 36 Dfl 40; n = 48 Dfl 80; n = 43 Dfl 120; n = 32
Climate change	Dfl 10; n = 32 Dfl 40; n = 44 Dfl 80; n = 45 Dfl 120; n = 35	Dfl 10; n = 29 Dfl 40; n = 33 Dfl 80; n = 39 Dfl 120; n =33	Dfl 10; n = 37 Dfl 40; n = 31 Dfl 80; n = 33 Dfl 120; n = 37
Oil/gas drilling	Dfl 10; n = 37 Dfl 40; n = 34 Dfl 80; n = 36 Dfl 120; n = 31	Dfl 10; n = 41 Dfl 40; n = 34 Dfl 80; n = 48 Dfl 120; n = 34	Dfl 10; n = 36 Dfl 40; n = 47 Dfl 80; n = 37 Dfl 120; n = 36

Notes: Each cell represents four unique treatments. For example, "Dfl 10" in row 1, column 1 denotes that one treatment had 40 subjects answering a dichotomous choice question on whether they would pay Dfl 10 to save the seals when they are threatened by nature and the question is hypothetical.

Table 2A: Mean Years of Age of Panellists by Treatment Cell[a,b]

Treatment	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	47.36 (1.12)	45.04 (1.23)	46.04 (1.15)
Climate Change	48.01 (1.17)	47.88 (1.29)	43.72 (1.13)
Oil and Gas Drilling	46.97 (1.25)	44.20 (1.15)	45.72 (1.05)

[a]Standard errors in parentheses.
[b]At the 5% level, mean age of panellists in the climate change/hypothetical treatment is different from mean age in both the climate change/consequentialism and the oil and gas drilling/hypothetical cheap talk treatments. Also, at the 5% level, mean age in the virus/hypothetical treatment is different from mean age in the climate change/consequentialism treatment Pairs of other means are not significantly different at the 5% level.

Table 3A: Proportion of Male Panellists by Treatment Cell[a,b]

Treatment	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	0.60 (0.04)	0.58 (0.04)	0.52 (0.04)
Climate Change	0.53 (0.04)	0.53 (0.04)	0.54 (0.04)
Oil and Gas Drilling	0.54 (0.04)	0.54 (0.04)	0.52 (0.04)

[a]Standard errors in parentheses.
[b]Difference between means tests show no significant differences between cells at the 5% level.

Table 4A: Mean Gross Household Income (in Dfl/month) of Panellists by Treatment Cell[a,b]

Treatment	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	27458.34 (3074.42)	12276.03 (2611.79)	12343.40 (2175.61)
Climate Change	13762.47 (3047.16)	12293.29 (2110.35)	11947.54 (2065.92)
Oil and Gas Drilling	13158.40 (2751.48)	11149.62 (1838.83)	13426.49 (1995.76)

[a]Standard errors in parentheses.
[b]At the 5% level of significance, mean gross monthly household income of panellists in the virus/hypothetical cell is different from the means of all other cells. Pairs of other means are not significantly different at the 5% level.

Table 5A: WTP by Treatment

Treatment	Hypothetical	Hypothetical/ Cheap Talk	Consequentialism
Virus	\$76.42 (1.72)	\$42.65 (2.00)	\$48.24 (1.69)
Climate Change	\$106.05 (1.87)	\$73.39 (1.93)	\$78.15 (1.92)
Oil and Gas Drilling	\$121.43 (1.93)	\$87.53 (1.79)	\$93.03 (1.76)

[a]Standard errors (in parentheses), which are computed based on the values of expected WTP in each treatment cell, understate variability of expected WTP because they do not account for shocks entering the model through the error term.

Table 6A: Chi-Square Tests of Differences Between WTP Elicitation Treatments Given Assignment of the Oil and Gas Drilling Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Hypothetical vs. Cheap Talk	0.60	1.07	0.01	0.15
Hypothetical vs. Consequentialism	0.76	1.77	0.66	0.03
Cheap Talk vs. Consequentialism	0.02	5.78*	0.65	0.05

*denotes significant at 5% level with one degree of freedom

Table 7A: Chi-Square Tests of Differences Between WTP Elicitation Treatments Given Assignment of the Climate Change Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Hypothetical vs. Cheap Talk	1.88	0.40	0.74	0.01
Hypothetical vs. Consequentialism	0.00	4.84*	2.81	0.04
Cheap Talk vs. Consequentialism	2.11	2.20	0.70	0.09

*denotes significant at 5% level with one degree of freedom

Table 8A: Chi-Square Tests of Differences Between WTP Elicitation Treatments Given Assignment of the Virus Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Hypothetical vs. Cheap Talk	7.41*	6.46*	0.80	2.05
Hypothetical vs. Consequentialism	0.06	2.75	0.55	1.95
Cheap Talk vs. Consequentialism	5.84*	1.33	2.66	0.01

*denotes significant at 5% level with one degree of freedom

Table 9A: Chi-Square Tests of Differences Between Cause Treatments Given Assignment of the Hypothetical/Cheap Talk Elicitation Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Oil/Gas Drilling vs. Climate Change	0.58	0.76	0.88	0.24
Oil/Gas Drilling vs. Virus	3.28	9.66*	5.31*	4.43*
Climate Change vs. Virus	0.81	5.25*	1.89	2.75

*denotes significant at 5% level with one degree of freedom

Table 10A: Chi-Square Tests of Differences Between Cause Treatments Given Assignment of the Hypothetical Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Oil/Gas Drilling vs. Climate Change	0.00	0.65	0.00	0.04
Oil/Gas Drilling vs. Virus	0.03	0.13	1.65	0.21
Climate Change vs. Virus	0.04	0.20	1.83	0.07

*denotes significant at 5% level with one degree of freedom

Table 11A: Chi-Square Tests of Differences Between Cause Treatments Given Assignment of the Consequentialism Treatment

	P=Dfl 10	P=Dfl 40	P=Dfl 80	P=Dfl 120
Oil/Gas Drilling vs. Climate Change	0.76	0.02	5.67*	0.33
Oil/Gas Drilling vs. Virus	0.64	0.01	2.08	3.60
Climate Change vs. Virus	0.00	0.06	1.18	1.86

*denotes significant at 5% level with one degree of freedom

Table 12A: Turnbull Estimates of Lower Bound Willingness to Pay by Treatment

Treatment	Willingness to Pay
Oil and Gas Drilling/Hypothetical with Cheap Talk	$37.04
Oil and Gas Drilling/Hypothetical	$66.80
Oil and Gas Drilling/Consequentialism	---[a]
Climate Change/Hypothetical with Cheap Talk	$53.62
Climate Change/Hypothetical	$68.40
Climate Change/Consequentialism	$52.20
Virus/Hypothetical with Cheap Talk	$36.70
Virus/Hypothetical	----[a]
Virus/Consequentialism	$59.80

[a]Could not be computed because the percentage of "no" responses presented did not increase monotonically with bid values.