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Preference Elicitation under Oath*

Nicolas Jacquemet† Robert-Vincent Joule‡ Stéphane Luchini§ Jason F. Shogren¶

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†Paris School of Economics and University Paris I Panthéon–Sorbonne. Centre d'Economie de la Sorbonne, 106 Bd. de l'Hôpital, 75013 Paris. Nicolas.Jacquemet@univ-paris1.fr

‡Laboratoire de Psychologie Sociale - EA 849, University of Provence, 29, avenue Robert-Schuman 13621 Aix-en-Provence cedex 1. joule-rv@up.univ-aix.fr

§GREQAM-CNRS, Centre de la Vieille Charité, 13236 Marseille Cedex 02. stephane.luchini@univmed.fr

¶Department of Economics and Finance, University of Wyoming, Laramie, WY 82071-3985, United States; and Department of Economics, Umeå University, S 901 87 Umeå, Sweden. JRamses@uwyo.edu

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Abstract

Eliciting sincere preferences for non-market goods remains a challenge due to hypothetical bias – the so-called gap between hypothetical monetary values and real economic commitments. The gap arises because people either overstate hypothetical values or understate real commitments or a combination of both. Herein we examine whether the traditional real-world institution of the solemn oath can improve preference elicitation. Applying the social psychology theory on the oath as a truth-telling-commitment device, we ask our bidders to swear on their honour to give honest answers prior to participating in an incentive-compatible second-price auction. Results from our induced valuation testbed treatments suggest the oath-only auctions outperform all other auctions (real, hypothetical, and real-with-oath). In our homegrown valuation treatments eliciting preferences for dolphin protection, the oath-only design induced people to treat as binding both their budget constraint (i.e ., lower values on the high end of the value distribution) and participation constraint (i.e., positive values rather than zero bids used to opt out of auction). Our oath-only results are robust to extra training on the auction and to consequential wording about the reason for the oath.

Keywords: Oath; Commitment ; Vickrey auction ; Hypothetical Bias ; Induced Values ; Homegrown Values.

JEL Classification: C9; H4; Q5

Résumé

La révélation des préférences des individus à l'égard des biens non-marchands reste une question largement ouverte en raison de l'existence d'un problème de biais hypothètique – qui désigne la différence généralement observée entre les valeurs monétaires déclarées dans un contexte hypothètique et les engagements financiers effectifs des individus. Ce biais apparaît à la fois parce que les individus sur-estiment leur vraie valeur dans un contexte hypothètique et parce qu'ils sous-estiment leur véritable engagement financier dans un contexte réel. Cet article cherche à évaluer dans quelle mesure une procédure de serment solennel peut permettre d'améliorer les mécanismes de révélation des préférences au regard de ces deux dimensions. En application des résultats de la théorie de l'engagement issue de la psychologie sociale, les participants aux expériences ont la possibilité de s'engager sur l'honneur à donner des réponses sincères avant de participer à une enchère au second prix. Les résultats issus de traitements en valeur induite suggèrent que les enchères précédées d'un serment dominent toutes les autres conditions expérimentales (incitations montaires, hypothètique et serment avec incitations). Un second traitement applique ce même mécanisme à la révélation des préférences à l'égard de la protection des dauphins. La signature d'un serment solennel conduit les participants à prendre en compte à la fois leur contrainte budgétaire (ce qui discipline le comportement de révélation dans le haut de la distribution) et leur contrainte de participation (ce qui les conduit à déclarer des valeurs positives plutôt que des valeurs nulles destinées à se soustraire à l'enchère). Ces résultats sont robustes à la familiarité des participants à l'égard du mécanisme d'enchère ainsi qu'à l'usage d'un vocabulaire qui met en évidence les conséquences du serment.

Mots-clés: Serment; Engagement ; Enchère à la Vickrey ; Biais hypothétique ; Valeurs induites ; Valeurs réelles.

What gives an oath the degree of efficacy it possesses, is, that in most points, and with most men, a declaration upon oath includes a declaration upon honor: the laws of honor enjoining as to those points the observance of an oath. The deference shown is paid in appearance to the religious ceremony: but in reality it is paid, even by the most pious religionists, much more to the moral engagement than to the religious.

Jeremy Bentham, *Rationale of judicial evidence* (1827).

1 Introduction

Eliciting sincere preferences for non-market goods remains a challenge due to hypothetical bias – the so-called gap between hypothetical monetary values and real economic commitment. Accumulating experimental evidence from lab and field reveals the average person tends to overstate real economic commitments by a substantial amount.[1] These results reinforce the long-standing explanation that the bias arises because the budget constraint is not binding in hypothetical valuation exercises (see Cummings, Brookshire, and Schulze, 1986; Cummings and Taylor, 1999; Harrison and Rutström, 2008, for a review). Other observers, however, have argued the opposite – real bids are "too low" as bidders shave bids downward, even to zero if they use the bid to exit the auction. Recall mechanism design requires a person to be no worse off by participating in the mechanism than otherwise, i.e., the participation constraint (see for instance Laffont and Martimort, 2002). Most lab or field experiments, however, do not provide people with a controlled "opt-out" mechanism to exit the auction. Pressing people to state a bid in an auction they do not want to be in could serve to pressure the person, thereby violating his or her participation constraint.[2] Hypothetical bids without binding budget constraints, or real economic commitments without binding participation constraints, both suggest we need to find another institution to commit the bidder to behaving sincerely when stating their preferences.

Faced with such revelation problems, real-world courts ask witnesses to take an oath "to tell the truth and nothing but the truth". Using the theory of commitment from social psychology (Kiesler, 1971; Joule and Beauvois, 1998), we herein examine whether this traditional real-world institution

[1] The concern over hypothetical bias comes from the large literature on valuation through stated preference surveys – see, among others, Cherry, Frykblom, Shogren, List, and Sullivan (2004); Smith (1994); Harrison (2006) for a summary. Based on eighty-three studies, Murphy, Allen, Stevens, and Weatherhead (2005) for instance found the ratio of hypothetical-real bids ranged from near unity to twenty-five for different deliverable goods like irradiated pork, watercolor paintings, and maps.

[2] Bidders can opt out of the experiment by bidding zero, as a result of this violation (Smith, 1994; Cherry, Frykblom, Shogren, List, and Sullivan, 2004, see for example). An over-abundance of zero bids then emerge as people use their low bid to opt out of what they consider to be an unacceptable auction environment. For example, in a field experiment auctioning off a valuable Cal Ripken Jr. baseball card, List and Shogren (1998) observed more than half the bidders bidding zero for the card immediately after submitting a positive hypothetical bid.

can induce subjects to reveal their preferences sincerely. We use a solemn oath as a truth-telling-commitment device, asking our bidders to swear on their honour to give honest answers prior to participating in an incentive compatible second-price auction. Bidders are free to sign the oath or not, and participation and earnings are not conditional on signing the oath. Our experimental evidence comes from two experiments, designed jointly: an induced value (IV) second-price auction and a homegrown value (HG) second-price auction. The design and rules implemented in both are as identical as possible. This allows us to build on the results from the controlled IV auction to interpret bidding behavior for a non-market good for which we do not know the homegrown demand curve. For each experiment, we run three main treatments: the baseline disconnects bidding decisions and subjects' payoffs from the experiment (the traditional hypothetical context) ; the real setting reintroduces monetary incentives ; and the oath treatment gives the subjects the option of signing an oath to tell the truth before participating in the baseline.

For induced values, our results show oath improved demand revelation – people bid more sincerely after taking an oath without any monetary incentives. Adding monetary payments to the oath, however, reduced sincere bidding, supporting the idea that external incentives crowd out the intrinsic motivation enhanced by commitment (see Bowles, 2008). For homegrown values, the oath elicited preferences that seemed to reduce violations both in the budget constraint (only 18.9% overbidding compared to 47.7% in the hypothetical-only auction) and in the participation constraint (only 1.1% zero bids compared to 26.7% in the real-only auction). Overall, our findings suggest the oath has potential to create the commitment needed to better link intentions and actions in demand revelation.

2 Background: Oaths, commitment and preference elicitation

Oaths are found in many cultures and societies. Sylving (1959) traced the familiar oath of the present-day courtroom back to pre-religious, pre-animistic cultures. Sylving argues that in the pre-animistic stage the oath, a primitive self-curse, was a meaningful expression of man's belief in his own magic power. Seton (1930) reports that in the Igala Tribe in Nigeria oaths were sworn to the Attah Ebbo, the father of magic, to ensure the fidelity of its members. The oath has taken on a more religious context in modern monotheist societies in which the oath-taker invokes a God as a witness to his or her sincerity (and offers him or herself as subject to divine vengeance in the case of perjury). In some modern Western countries, however, a person who refuses to swear on the grounds that he does not believe in an almighty omniscient God can swear upon his honor and conscience. The ancient Greek application of the oath, later adopted by Roman exponents of Greek thought, was based on moral, humanistic considerations (although it was believed in Greece and Rome that Zeus or Jupiter strikes perjurers with lightning). Taking an oath was the mark of proud, self-confident and free human beings: "[...] an oath is a kind of test to prove that men are free-born" (Plutarch, 40-120 AD, Roman Question ♯44).

This view has been disputed by philosophers such as Emmanuel Kant for whom the oath serves to challenge the view that truthfulness is an absolute duty (Sylving, 1959), and no oath is needed to incite people to tell the truth. Johann Gottlieb Fichte claims that one cannot force anybody to swear an oath because a person "who would have no scruple to affirm publicly a falsehood [...] will confirm this by an oath" (Fichte, 1797, p.139, translated by the authors). Invoking a God who will punish the perjurer would only be an act of superstition, which goes against the moral nature of religion according to Fichte. Fichte claims that an oath can only be seen as a solemn assurance "with the only aim to detain from all carelessness and to bring people to concentrate and to reflect on the importance of such assurance" (p.138) and asserts that only people who voluntarily agree to depend on an oath from another party must believe that he or she tells the truth: "volenti non fit injuria" (p.139). For us, this means that if we ask people to take an oath to tell the truth, we must then believe that they do so and cannot grumble if they don't. Said differently, if we ask people to truthfully reveal their preferences, we voluntarily accept the risk that they may be lying to us.

2.1 The oath as a commitment device

What the social psychology theory of commitment tells us is that the risk of lying is greatly diminished in an oath-taking context. This is because taking an oath can be understood as a strong commitment (see Joule and Beauvois, 1998, for an extended review).[3] Kiesler and Sakumura (1966) define commitment as a "binding of the individual to behavioral acts" (p.349). This means that, in a given course of action, one's past actions influence actions to come. Foot-in-the-door experiments are typical methods used in social psychology to produce commitment from people. This procedure first asks subjects to perform an initial request purposefully designed so nearly everyone will comply with the task. Subjects are then asked for a second request which is thematically in line with the initial one. A classic example is the panhandler who first asks a passer-by if he knows what time it is, and then asks him if he has any spare change. The standard result is that people are more likely to agree to perform the second request if they have already agreed to perform the initial one (see Burger, 1999, for a review). In their seminal experiment on foot-in-the-door devices, Freedman and Fraser (1966a) telephoned housewives in Palo Alto, California. They asked them if they would be willing to answer a few questions about the kind of soaps they use. Two or three days later, the subjects were asked if they would accept a visit from five or six men at their house for two hours or so, to classify the household products they use. In this foot-in-the-door situation, 52.8% agreed the second request. In the control group, where only the second request was made, only 22.2% agreed to it.

Further evidence from commitment-experiments in social psychology has shown that subjects comply with certain actions much more often when they have freely chosen to commit themselves

[3]The idea of commitment has been used in the context of "commitment device" in economics at least since Schelling (1960) and Elster (1977). Their individual commitment devices work to solve problems of self-control and intertemporal inconsistencies (see Brocas, Carrillo, and Dewatripont, 2003, for a review). This literature, however, has not put forward any direct link with the social psychological theory of commitment.

to doing them through a prior engagement or promise. In Kulik and Carlino (1987), for instance, parents of a child suffering from an inner ear infection (*otitis media*) were asked to express a verbal promise to give their child all prescribed antibiotic medication. Parents who were committed through this verbal promise showed significantly higher compliance than a control group of parents who had not been asked to promise. Interestingly, recent findings from experiments in economics support this evidence. In studies focusing on pre-play communication in games, people are observed to make promises to other players about what they are going to do later in the game. Moreover, those people who made promises in the communication phase are found to be likely to keep them when playing in both a hold-up game (Ellingsen and Johannesson, 2004) and in trust games (Charness and Dufwenberg, 2006).

The magnitude of the behavioral effects of committing oneself to a particular task (for example by making a promise or signing an undertaking) depends on the degree of commitment, commitment being "a continuous variable rather than a dichotomous one" (see Kiesler, 1971, p.30). Additional findings in social psychology have shown that commitment holds with force when it has been written down and signed or publicly announced (Pallack, Cook, and Sullivan, 1980; Katzev and Wang, 1994). For instance, Wang and Katsev (1990) asked people to sign an undertaking to recycle paper by throwing used paper in a specific bin. This small addition to the design increased the average weight of recycled paper by more than one hundred percent. Similar results were obtained through asking people to sign an undertaking to replace ordinary lamp bulbs at home by low consumption ones (Joule, Girandola, and Bernard, 2007) or to take a shower instead of a bath for a certain period of time (Joule, Bernard, and Halimi-Falkowicz, 2007). To sum up, commitment theory states that commitment is stronger if it has been made freely, was publicly expressed and/or has consequences for the subject ; as opposed to being obtained under pressure, made anonymously and/or being without consequences.

>From that point of view, an oath that is publicly expressed, taken freely and signed, appears an extreme and more accentuated commitment device than a verbal promise or a written undertaking. An oath should induce people to be consistent with their initial commitment in subsequent decisions. We apply this to elicitation methods based on stated preference, by having people swear to tell the truth.[4]

[4] As far as we know, the only explicit occurrence of the oath in the literature in economics is Braham and Bolle (2006). In a game theoretical framework, the authors consider institutional signals, such as an oath, as signal simpliciters, that is, as an unmistakable cue that serves to communicate information which can incite action or influence the behavior of others. As noted by Schlesinger (2008), however, the solemn oath is a serious promise made with the full intention of being kept. Beyond its informational content, an oath is above all a personal commitment. In that sense, the oath focuses a person's attention on the task at hand just like the cheap talk procedure used in some valuation exercises (e.g., see Cummings and Taylor, 1999). Unlike cheap talk scripts, though, our oath procedure does not provide information on how other people behave, but rather asks each person to comply with their signed statement – which is to tell the truth. Unlike the oath, moreover, cheap talk is only informative and does not rely on any kind of commitment from subjects.

2.2 Open questions in "real auction" bidding behavior

Preference elicitation methods are mechanisms to reveal individual preferences in contexts where the market cannot reveal them. While auctions with real economic commitments should provide – in theory – the incentive needed for sincerely bidding in a demand-revealing auction, evidence suggests this is not always the case. In induced value settings, for instance, experimental evidence suggests bidders off the margin of the market clearing price are less likely to bid sincerely in demand-revealing auctions (see e.g., Kagel, 1995). Here off-the-margin bidders tend to over-bid if they perceive their induced value is far removed from the potential market clearing price in a real auction (see for example, Shogren, Margolis, Koo, and List, 2001). Data suggests low-value bidders tend to bid a positive but excessive amount compared to the induced value in the second-price auction (see for example, Knetsch, Tang, and Thaler, 2001).

In contrast, within homegrown value settings, evidence suggests a different pattern of behavior – a set of bidders bid "zero", which suggests they are under-stating their preference in a real economic commitment context. These bidders seem to be using their low bid to opt out of what they may consider to be an unacceptable auction environment. This arises because most real bidding experimental designs do not provide people with an opt-out mechanism to exit the market. Pressing people to state a bid in an auction they do not want to be in could serve to pressure them, thereby violating their participation constraint.

Social psychology theory also helps explain some underbidding in real auction experiments. The psychological theory of reactance supports this point – people will find some way to opt out of a situation when pressured in an unappreciated or unacknowledged way (Brehm, 1966). The theory works in three steps. First, a person perceives an unreasonable or unfair restriction on his or her action; he fails to see why it is being applied, or judges that the context is too harsh, or feels that the restriction is unfairly limited to a few people. Second, the restriction induces an intense motivational state, called reactance. Reactance arises because people perceive themselves as wronged or misled and they want out of the situation. Third, the person acts to remove reactance. People with reactance try to get the unreasonable or unfair restriction removed, or else they try to subvert the restriction.[5]

As a result of reactance, a large number of zero bids can be observed within real bidding behavior in lab and field experiments. Consider the experimental design in List and Shogren (1998), for example. Today one of the authors is less convinced of the evidence supporting the case that real bidding behavior in lab or field experiments is the best benchmark of "true homegrown values". List and Shogren observed that 50 to 55 percent of all bids dropped to zero in the real auction (from a positive amount in the hypothetical auction), which translated into over one-third of the valuation gap between real and hypothetical bidding. At the time, hypothetical bidding was seen

[5]Reactance operates as a special form of cognitive dissonance (Festinger, 1957) but reactance has one distinctive feature: people experience reactance when someone else restricts them in an unreasonable or unfair way while dissonance is experienced when people act inconsistently, that is dissonance is induced by the people themselves.

Table 1: Experimental design

Experimental design				
Experiment	Commitment	Quiz	Vickrey with IV	Vickrey with HG
IV			×	
IV + Oath	×		×	
HG		×		×
HG + Oath	×	×		×
HG + Traning		×	×	×
HG + Training + Oath	×	×	×	×
HG + Consequential Oath	×	×		×
Main features of each core part				
	Commitment	Quizz	Vickrey with IV	Vickrey with HG
Number of repetitions	1	10 questions	9	5
Group sizes	1	1	9	9
Payments	-	Number of true answers	Sum of auction's earnings	Winner's earnings decreased by 2nd price
Experimental Treatments – Take-Home earnings from the experiment				
Baseline	All auctions are non-binding			
Monetary incentives	The last auction of the experiment is binding			
Show-up fee	In all experiments, set equal to 10 €			

as the culprit. Reflecting back, the experimental design most likely generated the large number of zero real bids observed. First, the monitors asked people to state a hypothetical bid for a baseball card; and then immediately asked each person for a bid with actual monetary consequences. A person first bid hypothetically and then was told the auction was now "for real". Given that this experiment was run in the field at a sports card show, many people could have seen this design as a "bait and switch" or "entrapment", and re-acted to this by opting out with a zero bid. People can use the zero bid option to exit a contrived market within which they are otherwise trapped. Many otherwise positive value bidders seemed to use the zero bid as a sure-fire way to exit the auction without playing – no pay, no play.[6] The aim of the oath is to induce these potential zero bidders to think about what positive value they actually put on the auction, and not to use their bid in protest or to opt out.

[6]The degree to which zero-bid reactance is at stake in experiments that use discrete choice or referendum mechanisms is unclear given the 0-1 nature of the bidding (Cummings and Taylor, 1999, see), i.e., when is a "no" a "no to the price," and when is it a "no to the participation itself"? One could expect some reactant zero answers if people felt pressure to participate and they had no other opt-out option in these experiments than to say "no". This reactance effect is similar to the idea of "protest bids" that emerged early on in the non-market valuation literature (see Cummings et al. 1986). Here a zero bid in a contingent valuation survey could be a "protest" against the survey exercise rather than a statement of zero value for the nonmarket good in question

2.3 Overview of the experimental design

In the light of all the above, the open question this paper addresses is whether an oath can induce more sincere bidding behavior, thanks to commitment, thereby improving the power of demand revelation and the accuracy of stated preference methods. Herein, we test whether a person taking an oath bids more sincerely in an incentive-compatible auction, both in induced and homegrown value auctions. The test is based on the set of experiments described in Table 1. Throughout the paper, we hold constant the demand revelation mechanism – the classic second price Vickrey auction. We start by testing the oath both with and without monetary incentives in an IV auction. These different treatments allow us to calibrate observed bidding behavior against theory and against each treatment. We then examine how the oath affects preference elicitation of HG values to protect dolphins compared to hypothetical and real bidding. Finally, we design two robustness treatments to test the reliability or fragility of our evidence: the first relies on the IV auction design to train subjects with the mechanism before entering the dolphin adoption auctions; the second implements an alternative procedure for the oath.

3 Testbed treatments: The oath in an induced value auction

The induced-value setting provides the highest level of control on preferences. We use this as a benchmark to assess how the oath affects preference elicitation.

3.1 Design of the IV-Experiment

Our first experiment implements a four-treatment design of an IV auction: (i) baseline hypothetical bidding, without oath or monetary incentives; (ii) baseline coupled with an oath ; (iii) bidding with binding monetary incentives and (iv) an oath coupled with monetary incentives.

IV-Baseline. We use a Vickrey second price auction, in which the highest bidder pays the second-highest bid. In relying on an auction rather than, for instance, a provision point mechanism, we depart from some previous studies aimed at investigating hypothetical bias in preference revelation (Cummings and Taylor, 1999; Brown, Ajzen, and Hrubes, 2003). The rationale for this choice is four-fold. First, Vickrey auctions are weakly demand-revealing: in theory, each bidder's weakly dominant strategy is to bid her or his induced value (Vickrey, 1961). Second, they are straightforward to explain. Third, market price is endogenous. Fourth, bidders announce their value for the good in a continuous manner and this allows for much more precision in the statistical analysis than with provision point mechanisms.

Each auction has 9 bidders, each endowed with a single induced value – i.e., the price at which the bidder can sell the good to the monitor after the auction. The induced demand curve is identical in all auctions and is defined by: $\{84; 76; 71; 68; 65; 63; 53; 38; 24\}$. All monetary values are expressed in

ECU (*Experimental Currency Unit*). The auction is repeated over 9 rounds, implementing all possible permutations among individual private values. Each bidder experiences each private value once, and the entire demand curve is induced in every period. Hypothetical profits equal the difference between the induced value and the price the winning bidder pays for the good (the second highest bid). If a bidder does not purchase the good, her profit is zero for that round. Only the winner sees the two highest bids at the end of the round.

Although the repetition is deterministic, we avoid end-game effect by providing the subjects with no information on that point – except for the repetition itself. Each bidder knows nothing about the other bidders' induced value or the induced demand curve. A bidding period ends when each bidder has chosen a bid between 0 and 100. After each round, bidders are informed privately about whether they won and, in this case only, the market clearing price and the hypothetical earnings for that period. Lastly, everybody is privately informed about whether a new auction period is about to start. Each bidder is paid a flat 10€ show-up fee for participating in the experiment.

IV-Monetary-incentives. This treatment makes bidding behavior binding – auction winners earn their take-home pay based on the difference in their induced value and the market-clearing price for each round. The common knowledge exchange rate is 3ECU for 1€. The monetary values in ECU accumulated across all auction periods are added to a 10€ show-up fee – in the event of negative total earnings, the show up fee could decrease to as low as 5€.[7] The monetary incentives are common knowledge, since we state explicitly that payments depend upon decisions made in each period at the beginning of the written instructions, and repeat this at the end.

IV-Oath. This treatment is identical to the baseline except each subject is asked to sign an explicit oath before entering the lab. The oath is implemented as follows: each subject enters alone and is directed to a monitor at the front of the laboratory. The monitor then offers each subject a form to sign entitled "solemn oath".[8] An English translation of the original oath form in French is provided in Figure 1. The *Paris School of Economics* logo on the top of the form and the address at the bottom indicate that it is an official paper; the topic designation and the research number were added so to ensure the credibility. The monitor explicitly points out to the subject before he or she reads the form that he or she is free to sign the oath or not, and that participation and earnings are not conditional on signing the oath (subjects are, however, not informed about the topic of the experiment when asked to take the oath). The subject reads this form, which asks whether he or she agrees "to swear upon my honour that, during the whole experiment, I will **tell the truth and always provide honest answers**" (emphasized in the original form). We chose the wording "solemn oath" and "upon honour" given the secularism of French modern society, in

[7] This lower results arises from participants recruitment conditions: we contractually commit ourselves to minimum earnings of 5€.

[8] The "Oath" is written on the form and read by the subject, but never said aloud.

Figure 1: Oath form used in the experiments

PARIS SCHOOL OF ECONOMICS
ÉCOLE D'ÉCONOMIE DE PARIS

SOLEMN OATH

Topic: "ATNoReC"; Research number 4523B

I undersigned ..... swear upon my honour that, during the whole experiment, I will:

Tell the truth and always provide honest answers.

Paris, ... Signature.....

Paris School of Economics, 48 Boulevard Jourdan 75014 Paris - France.

which law and political parties cannot be based on any religion.[9] The oath procedure could make a specific reference to the Bible, for example, if the experiments were run in the US. Regardless of whether the subject signs the oath, he or she is thanked and invited to enter the lab. The exact wording used by the monitors to offer the oath to respondents was scripted to standardize the phrasing of the oath. One monitor stayed in the lab until all subjects had been presented with the oath, to avoid communication prior to the experiment. Subjects waiting their turn could neither see nor hear what was happening at the oath-desk.

[9]The French Constitution is itself based on such an oath: the Tennis Court Oath (in French, *serment du Jeu de Paume*) which was an oath signed by the members of France's Third Estate to continue to meet until a constitution had been written, despite royal prohibition.

IV-Oath-Monetary-incentives. This treatment combines the IV-Oath procedures with the binding auction bidding in the IV-Monetary-incentives treatment.

3.2 Experimental procedures

Four experimental sessions – one per treatment – were conducted at University Paris I. Each session involved 18 subjects, providing two independent observations on a 9-times repeated Vickrey auction. The experiment was computerized using a software program developed under REGATE (Zeiliger, 2000) and participants were recruited based on ORSEE (Greiner, 2004).

On arrival, a monitor welcomes the participants and distributes an individual consent form. Unlike the oath form, the consent form is mandatory and publicly distributed. It also indicates that the subject will earn at least 5€ in the experiment. Participants hand in the form to the monitor before entering the lab. In the IV-Oath treatment, the consent form is picked up at the private desk, before the oath presentation. A computer is then randomly assigned to each subject and a monitor distributes and reads aloud the instructions, based on Cherry, Frykblom, Shogren, List, and Sullivan (2004)'.[10] To improve understanding of the game, a non-numerical example is developed covering all the instructions. However, the instructions do not indicate that bidding one's induced value is the dominant strategy. Participants are also asked to answer a short questionnaire highlighting the most salient features of the game. Once the questionnaire is orally corrected the experiment begins.

In each session, subjects begin by answering a short socio-demographic survey about individual characteristics. The auction rounds then start. After 9 rounds, the monitor pays each subject privately in cash – a fee of 10€ in the baseline and oath treatments and a sum computed from this same fee and the amount of ECU accumulated (or lost) in the monetary incentive treatments. Overall, 72 subjects participated, yielding 162 observations. The experiment lasted around half an hour and the average take-home earnings was about 10€.

3.3 Results

Table 2 provides raw data on observed behavior by treatment and round. Contrary to most studies run in North America (see Murphy, Allen, Stevens, and Weatherhead, 2005, for a survey) we do not observe a bias between the IV-Baseline and IV-Monetary-incentives treatments.[11] The average demand revelation is even slightly better when monetary incentives are dropped, falling from 106% to 104%. This feature has been replicated in valuation experiments in France (see e.g., Ehmke, Lusk, and List, 2007).

But this does not imply that our bidders perfectly revealed their true value for the good. In fact, past experimental evidence has shown that the second-price auction with monetary incentives only

[10] An English translation of the original instructions in French is available from the authors upon request.

[11] An unconditional test of equality between average bids under the two conditions leads to a t statistic equal to 0.4610, the p-value of which (0.6451, two-sided) unambiguously supports the claim.

Table 2: Aggregate bidding behavior by group and round

	Agreg. Demand	Round 1	2	3	4	5	6	7	8	9	Total
IV-Baseline	1084	938	992	1140	1103	1221	1153	1179	1142	1266	10134
		86.5	91.5	105.2	101.8	112.6	106.4	108.8	105.4	116.8	103.9
IV-Monetary	1084	949	1081	1245	1149	1062	1254	1290	1168	1130	10328
-incentives		87.5	99.7	114.8	105.9	98.0	115.7	119.0	107.7	104.2	105.9
IV-Oath	1084	977	1121	1117	1093	1214	1172	1155	1093	1186	10128
		90.1	103.4	103.0	100.8	112.0	108.1	106.5	100.8	109.4	103.8
IV-Oath+Monetary	1084	991	1072	1134	1097	1137	1189	1236	1258	1242	10356
-incentives		91.4	98.9	104.6	101.2	104.9	109.7	114.0	116.0	114.6	106.2

Note. The second column reports the aggregate induced demand, i.e., the sum of the induced values exogenously attributed to the buyers. For each treatment in row, the upper figure gives the aggregate revealed demand, i.e., the observed bids) in each round (in column) and summed over rounds (last column). The lower figure gives the ratio of this revealed demand to the aggregate induced demand, in %.

leads to mixed performance at the individual level – as bidders overbid or underbid relative to their induced value (see Kagel, 1995). This is particularly salient for off-the-margin bidders, i.e., those whose the private value for the good is at the lower end of the distribution (Parkhurst, Shogren, and Dickinson, 2004).

In line with this, we observe poor revelation of the whole demand curve for both treatments. Table 3 summarizes the aggregate bidding behavior by treatment and IV. Demand revelation almost monotonously decreases, from 159% under binding incentives – and 145% in IV-Baseline – for the lowest induced value, to rather satisfactory revelation (105% under monetary incentives and 101% in the baseline) for the highest induced value. If all subjects maximized their personal payoff, each bid should equal the induced value. In our setting, 5.5% of bids are perfectly demand-revealing in the IV-Baseline treatment; 70.3% of bids were within a ±10 percent interval centered on the induced value. Insincere bidders both inflated and shaved bids in near-equal percentages: 46.3% and 48.1%.

Result 1 *In an induced value second price auction, bidding behavior both with and without monetary incentives differs from perfect demand revelation.*

Support. We test the assumption of perfect revealing bids by specifying the true underlying bidding function as linear in induced value: $b^*_{it} = \beta\nu_{it} + \alpha + \phi_t + \alpha_i + \epsilon_{it}$, where b_{it} denotes subject i's ECU bid in trial t; ν_{it} denotes subject i's induced value in trial t; ϕ_t are fixed-round effects and the α_i's are zero mean subject-specific random variables with common variance σ^2_α. This last term accounts for individual heterogeneity in bidding behavior. The bids we observe, b_{it}, are censored at 0 and 100 due to the design of the experimental auction. We estimate the true underlying parameters by fitting

Table 3: Aggregate bidding behavior by group and Induced Value

	IV	24	38	53	63	65	68	71	76	84
	AD	432	684	954	1134	1170	1224	1278	1368	1512
IV-Baseline	RAD	626	808	1050	1193	1201	1192	1242	1290	1532
	RAD/AD	144.9	118.1	110.1	105.2	102.6	97.4	97.2	94.3	101.3
IV-Monetary-	RAD	687	735	1078	1045	1318	1259	1281	1334	1591
incentives	RAD/AD	159.0	107.5	113.0	92.2	112.6	102.9	100.2	97.5	105.2
IV-Oath	RAD	475	757	1046	1129	1261	1249	1331	1345	1535
	RAD/AD	109.9	110.7	109.6	99.6	107.8	102.0	104.1	98.3	101.5
IV-Oath+Monetary	RAD	710	807	1045	1185	1218	1256	1234	1381	1520
-incentives	RAD/AD	164.4	118.0	109.5	104.5	104.1	102.6	96.6	101.0	100.5

Note. The first row reports the induced values (IV) attributed to buyers. The second row reports the corresponding aggregate demand (AD) in each treatment, i.e., induced values × number of subjects. For each treatment (four remaining rows), the upper part of the row displays the revealed aggregate demand (RAD, i.e., the observed bids posted by buyers the induced value of whom are reported in column). The lower part reports the ratio of this revealed demand to the aggregate induced demand, in %.

the latent variable model: $b_{it} = \min[\max(b^*_{it}, 0), 100]$. Assuming normality of the idiosyncratic error term, this defines a panel Tobit model censored at both 0 and 100.

The model is implemented by assuming that the distribution of heterogeneity in the population, α_i, is normal and uncorrelated with the idiosyncratic error. Econometric results are given in Table 4. Based on the estimated parameters of the bidding function, we can test for perfect revealing bids for each treatment by considering $H_0 : \{\beta = 1, \alpha = 0, \phi_t = 0\ \forall t\}$ (see, for instance, Shogren, Margolis, Koo, and List, 2001). Results from Wald tests for the first two treatments are:

IV-Baseline	:	W=38.66	p =0.0000	H_0	Rejected
IV-Monetary	:	W=23.36	p =0.0095	H_0	Rejected

We reject the perfect revealing bids for both treatments. ∎

This leaves the door open for improvement, and we now look at whether there is mileage in asking bidders if they would be willing to swear to tell the truth and to give honest answers prior to participating in the auction without monetary incentives. We found that many subjects were happy to take the oath, and most of them decided very quickly to do so. Some even pointed out that responding truthfully during the experiment was natural and entirely normal– however, as we have seen above, telling the truth is not obvious at all in IV-baseline and IV-Monetary treatments. We had an acceptance rate of 94.5% as only one subject out of eighteen refused to take the oath. Self-selection is not an issue.[12]

The third line of Table 3 describes bidding behavior in the IV-Oath treatment. Aggregate bidding behavior exhibits satisfactory revelation over the whole demand curve. In particular, we do not observe over-bidding for the lowest induced value: the ratio of revealed aggregate demand

[12]Five percent refusal rate is standard for commitment experiments (Joule and Beauvois, 1998; Burger, 1999).

Table 4: IV bidding behavior – Panel Tobit estimations

Variable	IV-Monetary-incentives $n = 162$	IV-Baseline $n = 162$	IV-Oath-Monetary-incentives $n = 162$	IV-Oath $n = 162$
	Parameter estimates (p-value)			
ν_{it}	.855	.788	.756	.969
	(.000)	(.000)	(.000)	(.000)
Constant	.480	4.44	8.99	-6.1
	(.945)	(.394)	(.044)	(.182)
Round dummies	YES	YES	YES	YES
σ_u	2.40	9.19	4.88	8.72
	(.000)	(.000)	(.000)	(.000)
σ_ϵ	19.55	14.25	12.31	12.64
	(.000)	(.000)	(.000)	(.000)
Log-likelihood	-675.11597	-659.40375	-640.24359	-641.81659

Note. Individual random effects Tobit models. The random effects are assumed normal; round (fixed) effects are controlled for in the estimation, but omitted in the Table. The *endogenous* variable is the bid posted. ν_i denotes the induced private value. The columns report results from separate regressions on each treatment.

over aggregate demand is 110%. In addition, 16.0% of bids equal the induced value, and 70.4% are within 10 percent of this value. Insincere bidders tend to inflate rather than shave their bids: 50.7% of bids were higher than the private value and 33.3% lower.

Result 2 *In a hypothetical second price auction, asking a bidder to take an explicit oath pledging to tell the truth and always provide honest answers leads to sincere bidding behavior.*

Support. Based on the Tobit regression provided in Table 4, we apply the Wald test of perfectly revealing bids to the IV-Oath treatment.

$$\text{IV-Oath} \quad : \quad W{=}15.55 \quad p{=}0.1133 \quad H_0 \quad \text{Not rejected}$$

We cannot reject the null hypothesis that the subjects motivated by the oath bid sincerely. We used the treatment variable – each subject was asked to take an oath, and not whether or not he or she agreed to take the oath. Descriptive statistics and econometric tests lead to similar conclusions when the subject who did not sign the oath is dropped from the analysis.[13] ∎

Since we focus on the behavioral consequences of the oath, we do not provide here a direct test of the psychological motives underlying the observed change in bidding behavior when subjects have signed an oath. Various candidates can be found in the literature. A first possible explanation stems from the self-attribution interpretation put forward in the theory of commitment: people infer their

[13] Detailed results are available upon request from the authors.

attitudes from their own behavior (Bem, 1972; Kiesler, Nisbett, and Zanna, 1969).[14] In line with this, people deduce from their first behavior that they are the "kind of person [...] who does this sort of thing" (Freedman and Fraser, 1966b, p.101), and are subsequently more likely to perform the second request. Following this line of reasoning, the oath acts as a commitment device in our experiment because it puts people in a certain mind frame regarding the action that is going to follow. That is, when they bid in the auction, subjects may consider themselves as being someone who bids sincerely because they have already signed the oath that commits them to "tell the truth". The oath frames how each person thinks about his or her behavior in the task.

Two further interpretations come from the economic literature on pre-play communication in experimental games, which has shown that promises exchanged freely before the game starts enhance cooperative behavior. Ellingsen and Johannesson (2004) consider that communication in general, and promises in particular, create commitment because people have a preference for consistency. This implies individuals have a taste for keeping their word. Formally, this preference induces a cost of lying for those who do not respect their own promises. The authors show in a trust game that inequality aversion combined with a cost of lying strengthens the credibility of promises. Following this first line of reasoning, the oath can impose a cost on lying if bidders do not bid their induced value after having signed the oath. This in turn fosters truth-telling behavior, which in our case means disciplining off-the-margin bidders for whom the Vickrey second price auction does not perform well. The second explanation is based on guilt aversion, which happens to a player failing to meet the payoff expectations attributed to others (Dufwenberg and Gneezy, 2000; Battigalli and Dufwenberg, 2007). Avoiding such disappointments to others could then be the reason why people keep their promises in trust games (Charness and Dufwenberg, 2006). In our case, this would mean subjects bid sincerely because they do not want to disappoint the monitor. Vanberg (2008) presents an experiment that distinguishes between these two explanations, by comparing the effect of promises in trust games, depending on whether the player is the one who made them or not (some players are randomly switched after the pre-play communication phase). Results unambiguously favor the taste-based – which Vanberg also labels "commitment-based" (p.1467) – explanation.

One common feature of the first two explanations is that the oath enhances the intrinsic motivation of subjects to reveal their true value. In this context, social psychologists have shown that extrinsic motivation can be counterproductive: when people receive "extrinsic" rewards (often monetary) for doing an interesting task, their intrinsic motivation for doing the task is affected adversely. For instance, a person is more likely to stop an ongoing activity of his own accord if he has received monetary rewards to do it than if he has not received any monetary rewards (see Deci, 1975; Deci, Koestner, and Ryan, 1999, for a meta-analysis of 28 experimental results). This finding

[14] The self-attribution argument is also central to self-signaling models in economics. In self-signaling models, agents derive utility from the outcome of actions, *outcome utility*, but also derive *diagnostic utility* from the information that the action provides on some underlying trait or disposition in themselves (see for instance Bodner and Prelec, 2001). However, agents in self-signaling models are usually forward-looking whereas in commitment theory the focus is essentially backward-looking: people infer from past behavior beliefs and attitudes towards actions which are about to come.

is also common in foot-in-the-door experiments (Burger, 1999). For instance, Zuckerman, Lazzaro, and Waldgeir (1979) observed that housewives who were asked to fill-in a five-minute questionnaire and were paid for the task were less willing to agree to fill in a thirty-minute follow-up questionnaire three days later, compared to those who were not paid. In addition, these housewives were even less likely to fill in any questionnaire of any length compared to the other housewives. This led us to close our 2×2 design in such a way as to assess whether incentives can increase oath-based commitment, by including a fourth treatment which combines the two revelation tools.

Result 3 *Combining the oath with monetary incentives leads to less sincere bidding compared to bidding with oath-only, monetary-only, and the baseline treatments.*

Support. Again based on the Tobit regression model, perfect revelation is strongly rejected for the IV-Oath-Monetary-incentives treatment:

$$\text{IV-Oath-Monetary-incentives} \quad : \quad W{=}50.86 \quad p{=}0.0000 \quad H_0 \quad \text{Rejected}$$

This treatment leads to a flatter line compared with other treatments. Averaging the trial-specific effects ϕ_t and individual effects α_i leads to the following regression lines:

IV-Baseline	:	bid = 0.788 x Ind. Value	+	16.5
IV-Monetary-incentives	:	bid = 0.855 x Ind. Value	+	13.9
IV-Oath	:	bid = 0.969 x Ind. Value	+	3.7
IV-Oath-Monetary-incentives	:	bid = 0.756 x Ind. Value	+	19.1

The IV-Oath-Monetary-incentives condition exhibits the highest discrepancy with the perfect revealing bid behavior: the slope is lower and the intercept is dramatically larger. Split sample tests confirm the statistical significance of the result: bidding behavior in the IV-Oath-Monetary-incentives treatment is significantly different from IV-Baseline ($p = 0.05$), IV-Monetary-incentives ($p = 0.02$) and IV-Oath ($p < 0.01$). ∎

Crowding-out applies as well to our procedure: IV-Oath-Monetary-incentives induced people to bid even less sincerely than in other treatments. In line with the self-attribution interpretation, Deci (1975) and Staw (1976) argue that *oversufficient* rewards lead subjects to infer that their actions are motivated by extrinsic reward rather than by their intrinsic interest in doing the task, leading to *overjustification*. In our case, a person should have two "good" reasons to bid sincerely in the IV-Oath-Monetary-incentives. However, the monetary incentives undermine the commitment induced by signing the oath, because there are now external reasons to bid sincerely in the auction. Incentives crowd out commitment through the oath. This result supports the literature in economics suggesting that intrinsic motivation and extrinsic motivation can be substitutes (see Kreps, 1997; Frey and Jegen, 2001, for surveys). This also provides additional insights on potential explanations for the effects of the oath on bidding behavior. Guilt aversion would imply that the oath succeeds because subjects want to comply with what the experimenter expects from them – a special type

of Hawthorne effect, recently put forward by Carpenter, Liati, and Vickery, 2006. In line with this interpretation, however, adding an oath should improve revelation whatever the benchmark. In particular, this would imply that behavior under IV-Oath-Monetary-incentives would be closer to perfect revelation than IV-Monetary-incentives. But this is not what we observe. By contrast, commitment-based explanations can account for this further result, based on the crowding-out of the intrinsic motivation to tell the truth. In Section 5.2 we rely on an alternative oath procedure to provide further insights on this point.

To sum up, our experimental results are twofold: (i) the oath procedure significantly improves the power of the Vickrey mechanism to reveal true values for the good: this device notably manages to discipline the bidding behavior of off-the margin bidders; (ii) adding monetary incentives weakens the demand-revealing power of the oath. IV auctions provide persuasive results on the performance of our elicitation procedures, because preferences are perfectly observed and controlled. The second set of experiments applies the same experimental design to a setting closer to the field as regards the good sold in the auction.

4 Application treatments: The oath in Homegrown value auctions for a nonmarket good

We now consider preference elicitation of homegrown values for a real-world non-market good: adopting a dolphin through a monetary donation to the *World Wide Fund* (hereafter WWF), a well-known non-governmental organization devoted to "protecting the future of nature".[15] Subjects' homegrown values are elicited using the same elicitation mechanism, a second-price auction. The price for improved parallelism with decisions in the real world is the lack of control over true preferences in this experimental setting. We focus on three treatments: hypothetical bids, monetary-incentives bids, and oath-only bidding. We drop the monetary-oath treatment since it was the worst-performing treatment in the IV testbed experiment.

4.1 Design of the HG-experiment

The good. We focus on preferences for an environmental good, namely the protection of dolphins. To ensure the credibility of the procedure, we use a particular action offered by the WWF. Among a wide range of individual actions, the WWF offers the opportunity to "adopt" endangered animal species. This takes the form of an individual donation to a program aimed at fighting threats like habitat loss and poaching faced by endangered animals. Depending on the amount of the donation (among three possible values), donators are sent gifts such as an adoption certificate, a photograph of the animal, a cuddly stuffed toy dolphin, a gift box, and so on. For the purpose of our experiment,

[15] The WWF was formerly named the *World Wildlife Fund*, which remains its official name in the United States and Canada. Since 2001, the WWF has been named the *World Wide Fund* in all other countries. More information about the WWF can be found at `http://www.worldwildlife.org/about/`.

this procedure has the attractive feature of ensuring the credibility of the donation, thanks both to the WWF label and to the documentation associated with donation. We chose the entry-level offer, i.e., only the adoption certificate and photograph are sent. Since the photograph and the adoption certificate are essentially symbolic in nature, this reduces the risk of valuations being influenced by "by-product" goods, such as a cuddly stuffed toy or a gift box.

The adoption procedure is described to the subjects using a French-language, slightly modified version of the official web page set up by the WWF.[16] The page provides a short description of a dolphin's life and of the WWF and, more importantly, a detailed presentation of the donation program and the documentation (gifts) sent should a subject adopt a dolphin. The scroll bar used to choose a donation amount between 0 and 30 Euros, along with an "OK" button, appears directly on the page and the subjects see the good description until they confirm their choice.

The actual market price of the donation to the WWF is 25 USD (18.50 Euros when the experiments took place). Since we do not impose any reservation price in the provision rule (as reported above minimum bid is zero), we subsidize the winning donation to reach the market price when monetary incentives are binding. The good sold in the experiment is potentially cheaper in the lab than in the market. Once actual offers are below the market price, the observed values are independent of field opportunities. This protects our data from the censoring issue raised by, e.g, Harrison, Harstad, and Rutström (2004). Moreover, subjects are not told anything about this subsidy. Formally speaking, the discrepancy between in-the-lab and market prices is then influential on observed behavior if and only if subjects are actually aware of the donation procedure and the market price of the donation. Some items aimed at assessing subjects' knowledge are included in a debriefing questionnaire (see Section 4.2 below).

Elicitation mechanism. As in the IV-Experiment, individual valuations for the good are elicited thanks to a Vickrey (second-price) auction: each bidder privately posts a bid, the highest bid determining the winner of the auction, i.e., the subject entitled to adopt a dolphin, and the amount of donation is equal to the second highest bid. We also divide each 18-subject session into smaller groups of 9 bidders to avoid too much distortion of bidding at the bottom of the distribution of bids (see Kagel, 1995, and Section 3.3). Two groups in each session are involved in two independent adoption processes.

Show-up fee property rights. Our focus on donation behavior requires the subjects to enter the auctions with some positive experimental earnings, which may then be spent on the donation. This would mean giving subjects a rather large show-up fee for participating in the experiment. It is an increasing concern in laboratory experiments that behavior can differ according to whether one has to decide on the allocation of either windfall or earned wealth (sometimes called *endowment effect*, see, among others, Rutström, 1998; Cherry, Frykblom, and Shogren, 2002). In the specific context

[16] The original page in English is available at `https://secure.worldwildlife.org/ogc/ogcAC_speciesDetail.cfm?gid=8`, the page used in the experiment is provided in supplementary material.

of demand revelation using Vickrey auctions, Jacquemet, Joule, Luchini, and Shogren (2009) show that earned money does make a difference to bidding behavior as compared to windfall wealth. In line with these results, and to be as close as possible to actual stated preferences surveys in the field, we use an earned-wealth design. This also replicates a common feature of homegrown valuation experiments focusing on hypothetical bias (e.g., Cummings and Taylor, 1999; Cummings, Elliott, Harrison, and Murphy, 1997).

Earned wealth is implemented through a preliminary stage during which the subjects are asked to answer general knowledge questions. Accompanying each question is a list of four possible answers. The set of questions was taken from the annals of the "Concours de Catégorie B de la fonction publique" which is a civil service entry test for those who hold at least the French baccalaureate.[17] This is appropriate to discriminate between undergraduate students. Subjects are explicitly told that one and one only out of the four is true, and that monetary earnings labeled in ECU (*Experimental Currency Unit*) are proportional to correct answers. The position of the correct answer is randomized between questions and the ordering of questions is kept the same for all subjects in all treatments.[18]

Experimental treatments. We rely on three main treatments: *HG-Baseline*, *HG-Monetary-incentives* and *HG-Oath*. The first two treatments only differ as regards the monetary consequences of the adoption auction. The adoption is hypothetical in the *HG-Baseline* treatment while the donations are subtracted from subjects' earnings in the *HG-Monetary-incentives* treatment. This implies that donation is merely declarative in *HG-Baseline* and no funds are actually transferred to the WWF – no adoption certificate is sent to the adopter. All other experimental features are kept the same in these two treatments. The *HG-Oath* treatment adds an oath procedure to an *HG-Baseline* treatment before the experiment starts: each subject is asked to sign a "solemn oath" before entering the lab. The entire oath procedure is identical to that implemented in the *IV-Oath* treatment described in section 3.1. Earnings stemming from the quiz are real in all treatments to avoid unwarranted wealth differences between our treatments.

4.2 Experimental procedure

On arrival, each subject signs an individual consent form and enters the lab. This form is mandatory for participation in the experiment. In the HG-Oath treatment only, subjects are then asked to take a truth-telling oath. A computer is then randomly assigned to each subject and a monitor distributes and reads aloud the instructions.

The experiment begins by asking the subjects to fill out a computerized questionnaire about socio-economic characteristics (gender, sex,. . .). The instructions of each part of the experiment are

[17] Our source is `http://pagesperso-orange.fr/bac-es/qcm/annales_c02_r01.html`.

[18] The data on observed answers are not commented on here; the full list of questions and data are available from the authors upon request.

distributed and read aloud just before it starts. Before the game begins, participants are encouraged to ask clarifying questions, privately answered by the monitor. All experiments start with the quiz (questions are displayed one after the other along with the four possible answers). Subjects are provided information on their score only at the end of the quiz along with their corresponding earnings in ECU. The payment rate is 2 ECU *per* correct answer and the exchange rate is 3 ECU for 1 €. With an expectation of ten correct answers out of twenty, the average monetary earnings for the quiz would be 7 € (payment is rounded up to the next 50 cents), which makes 17 € in total added to the 10 € show-up fee.

The last part of the experiment in all three treatments is the adoption auction. The instructions first detail the WWF, the adoption procedure, and how the collected funds will be used. The auction is then described using the same instructions as in the IV experiment (same non-numerical example and same questionnaire to check subjects' understanding at the end of the instructions). The only difference is the good and its description. Subjects are grouped into two 9-bidder auctions. The wording of the instructions is slightly modified between the HG-Monetary-incentives and HG-Baseline treatments. We follow Cummings and Taylor (1999) in replacing the affirmative language used in real auctions ("you *will* participate in the adoption procedure", "you *will* adopt a dolphin", "we *commit* ourselves to sending your donation to the WWF") with a hypothetical language in the hypothetical auctions: "we want you to *suppose you were to* participate in the adoption procedure", "you *would* adopt a dolphin", "we *would commit* ourselves to sending your donation to the WWF" (italics added).

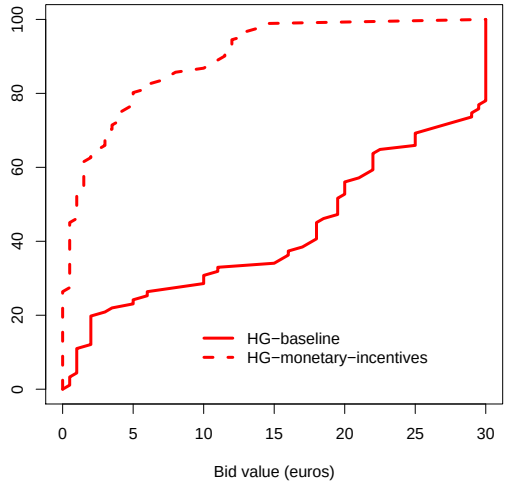
At the end of the experiment, subjects are asked to answer a computerized debriefing questionnaire. The aim of the questions is to assess the level of knowledge and the level of agreement of the subjects as regards the WWF and its actions, their knowledge of the WWF adoption procedure, their degree of familiarity with the auction mechanism through online auction websites and whether they have participated in other experiments or not. Finally, the monitor pays each subject privately in cash.

4.3 Results

Since subjects enter the lab with their own unobserved preferences, preference revelation can no longer be deduced by contrasting bidding behavior with true preferences. We rely on bidding behavior only. In Figure 2, we present the empirical distribution functions (EDF) of bids in HG-Baseline and HG-Monetary-incentives treatments.

Bids in HG-Baseline dominate bids elicited in HG-Monetary-incentives: the EDF of HG-Baseline bids is first order stochastically dominated by the EDF of bids elicited with HG-Monetary-incentives. This means that data exhibit a hypothetical-real gap for low bids as well as for high bids. A closer look at the data is provided in Table 5, where we compute average and median bids as well as the number of violations of the two constraints underlying truth-revelation: the number of zero bids, which includes participation constraint violations through opting-out behavior ; and the number

Figure 2: Empirical Distribution Functions of bids in real and hypothetical treatments



Note. Empirical distribution function of bids, pooling all five rounds of the HG experiment: each point along the curve gives the proportion of observed bids that are lower than the one in abscissa.

of bids above experimental earnings. Recall over-bidding means that the subject would pay out of hi/her pocket to adopt the dolphin, hypothetically in HG-Baseline and for real in HG-Monetary-incentive. This gives us a proxy to measure violations of the budget constraint. The Table highlights a substantial difference in the elicited preferences according to whether incentives are binding or not: mean and median bids in baseline are €17.43 and €19.5 as compared to €2.98 and €1 in monetary-only. This leads to an average hypothetical-real ratio of 584.9%. This means that bids in HG-Monetary-incentives are on average six times lower than in HG-Baseline.

Consider the low end of bids first. No bidders bid zero values in the hypothetical treatment; but we observe nearly 27% of bidders bidding zero in the monetary-only treatment. Our level of zero bids is about half that observed in List and Shogren (1998). The key difference here is that we used a between-sampling design, while they used a within-sample design in which a hypothetical bid was followed immediately by the real bid. Reactance theory would predict more recoil within the within-sample design. Now consider the high end of the distribution. No subject bid so high that he or she took the chance of paying out of his/her pocket to adopt a dolphin in the monetary incentives. In contrast, almost half the bid are outside the budget constraint in the HG-baseline treatment. Together, these two differences produce a significant gap between preferences elicited with or without monetary incentives. As a result, two distinct variations in behavior drive the result: the opting-out of the auction through zero bids when incentives are binding,[19] and the unreliable

[19]To further test this explanation, we conducted a companion treatment with monetary incentives identical to that of HG-Monetary-incentives but with one key change: subjects can choose to either participate or not in the auction prior to bidding. Interestingly, 3 out of 18 subjects declined to participate in all five auctions. Over all 5 rounds, 41.1% of subjects refused to participate in 1 round or more. We observe only one zero bid in the five rounds (1.1%) since the subjects choose to participate. This re-enforces our interpretation that zero bids in the real treatment are

Table 5: Homegrown bidding behavior in real and hypothetical treatments

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
	Mean bid (€)	15.36	18.19	17.33	17.38	18.9	17.43
HG-baseline	Median bid(€)	16.25	20.5	19.75	19.75	20.75	19.5
	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	7	9	8	9	10	43 (47.7%)
	Mean bid (€)	3.3	2.97	3.17	3.17	2.3	2.98
HG-monetary	Median bid (€)	1.25	1	1.25	1	0.75	1
-incentives	♯ zero bids	3	5	5	5	6	24 (26.7%)
	♯ bids > gains	0	0	0	0	0	0 (0.0%)
Average hypothetical-real ratio		465.5%	612.5%	546.7%	548.3%	821.7%	584.9%

Note. For each treatment (in *row*) and by round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings (*fourth row*). The last row of the table gives the ratio between average baseline bids and average monetary-only bids.

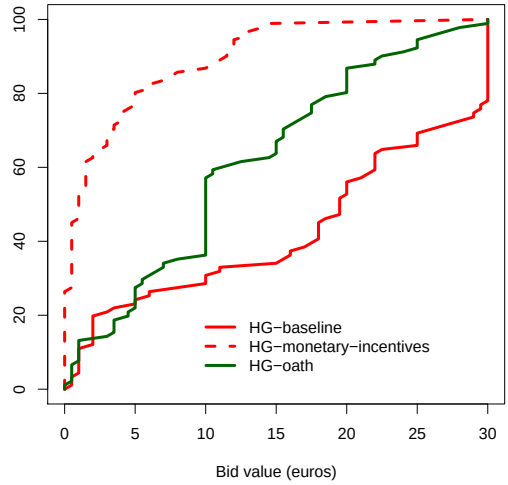
willingness to pay out of ones' own pocket when incentives are dropped.

Result 4 *Hypothetical bias exists in the homegrown second price auction without an oath. We observe significant differences in bidding behavior with and without monetary incentives. Bidders frequently violate (47.7%) their experimental earnings budget constraint without monetary incentives; bidders frequently (26.7%) violate the participation constraint with monetary incentives.*

Support. We statistically test the difference in mean bids using a two-sample mean difference test based on a non-parametric bootstrap procedure that accounts for potential correlation between the five bids of the same subject and for asymmetry in the empirical distribution of bids. The bootstrap procedure is based on bootstrapping subjects and their five bids in the sample (999 times), instead of considering independent bids, i.e., bootstrapping on bids. To account for asymmetry in the empirical distribution, we computed an equal-tail bootstrap p-value (see Davidson and MacKinnon, 2006). The two-sample bootstrap mean difference test is significant with $p < .001$. This significant gap between bids in HG-Baseline and bids in HG-Monetary-incentives is not likely to be explained by differences in total experimental earnings (earning from the quiz + show-up fee of 5 euros) between the two treatments: subjects earned on average €18.9 (s.d. 0.21) in baseline – arising from an average 13.3 correct answers out of 20 in the earned-money phase – and €18.6 (s.d. 0.25) with monetary incentives – 12.88 correct answers in average. A two-sample mean difference test leads to $p = 0.364$). Nor can this be explained by differences in respondents characteristics. Unconditional mean and proportion tests show that there are no significant differences in socio-demographic and

more likely to be reactance to the implied participation constraint in the auction. That is, zero bids suggest a 'no to the participation itself' rather than being a 'no to the price'.

Figure 3: Empirical Distribution Functions of bids in oath treatment



Note. Empirical distribution function of bids, pooling all five rounds of the HG experiment: each point along the curve gives the proportion of observed bids that are lower than the one in abscissa.

debriefing questions: gender ($p = 0.738$), knowing the WWF ($p = 0.614$), having already adopted dolphins (only one individual out of 18 had already adopted a dolphin, $p = 0.320$), level of agreement with WWF actions ($p = 0.508$), past experience with the auction mechanism proxied by stated number of purchases on auction websites ($p = 0.400$).

The large number of zero bids observed with monetary incentives is explained by: three subjects out of eighteen who bid only zero bids –16% of total elicited bids ; two subjects who bid zero four and three times and fifty cents otherwise, and the rest from one bidder with a maximum bid of 50 cents and one with a maximum bid of €10. ∎

We now turn to the way the oath deals with both violations. In HG-Oath, all subjects but one took an oath prior to participating in the auction.[20] Figure 3 provides the empirical distribution of bids from those subjects that participated in the oath treatment, along with a recall of the EDF of bids in the baseline and the monetary incentives treatments. Having subjects sign an oath before participating in the auction leads to a distribution that first order dominates the baseline. Quantitative differences can be deduced from Table 6. The mean bidding behavior is €11.46 after an oath was signed, as compared to €17.43 in HG-baseline, i.e., without monetary incentives (Table 5). Behavior in the oath treatment is also different from what we observe in the monetary incentives treatment. EDF of bids in HG-Oath first order dominates EDF of bids in HG-monetary-incentives. The difference is mainly driven by the decrease in the number of zero bids as compared to what we observe when incentives are binding. Comparing Table 6 to Table 5, the percentage of bids above subject's experimental earnings is 18.9% in the oath experiment and 47.7% in the baseline

[20] Again, the statistical analysis is conducted on the whole sample. Results are similar when excluding the subject who did not take the oath (detailed results available on request from the authors).

treatment without monetary incentives. The number of violations is more than twice less when subjects take an oath.

Result 5 *In a second price homegrown values auction with hypothetical decisions, asking a bidder to take an explicit oath that pledges him or her to tell the truth and always provide honest answers leads to (i) lower bids than those elicited using incentives and (ii) less violations of the budget constraint than those observed in the baseline.*

Support. The decrease in mean behavior as compared to the behavior we observe in the monetary treatment is significant at $p = 0.026$ according to our two-sample one-sided mean difference bootstrap test. We statistically test the decrease in the number of bids outside the budget constraint induced by the oath as compared to the behavior in HG-Baseline by a likelihood ratio (LR) proportion test that allows for potential correlation between the five bids of the same subject. The test is based on the comparison of three multinomial distributions each of which allows for 32 possible outcomes, and 31 parameters. Each outcome is a vector of dimension five (the five bids of a given subject –each subject counting as one observation) and a dimension is coded zero if the subject bids zero or one otherwise, that is $2^5 = 32$ possible outcomes.[21] We then compare the estimated loglikelihood of the pooled data HG-baseline and HG-Oath together) against the sum of loglikelihoods of HG-Baseline and HG-Oath considered separately using an LR test. The decrease in bids outside the budget constraint is significant with $p < 0.001$ according to our LR test. Again, these results are not likely to be explained either by differences in experimental earnings (mean experimental earnings is €18.88 in HG-Baseline and €18.13 in HG-Oath, $p = 0.275$ – corresponding on average to 12.2 correct answers out of 20 in the quiz) or differences in debriefing questions: gender ($p = 0.516$), knowing the WWF ($p = 0.990$), knowledge of the WWF wild animal adoption scheme (only one subject was aware of it), level of agreement with WWF actions ($p = 0.870$), past experience with the auction mechanism ($p = 0.210$).

We test the overall significance of results 4 and 5 using a random effects panel Tobit model pooling together data from HG-Baseline, HG-Monetary-incentives and HG-Oath. The left-censoring limit is 0 and the right-censoring limit 30. These limits are the bounds of the scroll bar used on the adoption screen when subjects bid their valuation (see section 4.1 and the screen shot of the adoption page provided in the supplementary material). Dummy variables are introduced to control for the HG-Monetary-incentives and HG-Oath treatments (HG-Baseline being the referent) as well as total earnings and individual's characteristics.[22] Results are presented in Table 7: as compared to behavior in HG-Baseline, monetary incentives drastically decrease revelation: associated parameter is -16.7 with $p < .001$. The oath also has a significant impact on bidding behavior as compared to bidding behavior in HG-Baseline: associated parameter is -7.41 with $p = .002$. This means that, on average, the decrease in bids is twice less in HG-Oath than in HG-Monetary-only as compared to

[21] In our sample, the total number of outcomes is 10 (some particular arrangements of zero bids are not observed).

[22] In the model, we used only variables with substantial variability. For instance, we excluded "knowledge of WWF wild animal adoption" because only one subject was aware of it.

Table 6: Homegrown bidding behavior in the oath treatment

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
	Mean bid (€)	9.42	10.72	11.28	11.83	14.02	11.46
HG-oath	Median bid (€)	7	10	10	10	13	10
	♯ zero bids	0	1	0	0	0	1 (1.1%)
	♯ bids > gains	3	2	4	2	6	17 (18.9%)
Average oath-real ratio		285.5%	360.9%	356.2%	373.2%	617.4%	384.9%
Average oath-hypothetical ratio		61.3%	55.9%	65.1%	68.1%	74.1%	65.7%

Note. For each round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings (*fourth row*). The last two rows of the table give the ratios between the average bids after an oath and: first the average baseline bids, second the average monetary-only bids.

HG-Baseline when accounting for observed heterogeneity of subjects. This difference is significant with $p < .001$. ∎

In short, for the IV treatments, the oath paid off – we could not reject the null hypothesis of sincere bidding in a second price auction under oath. For the homegrown value treatments, two lessons emerge: (1) hypothetical bias exists and is caused by violations in both budget and participation constraints, and (2) the oath seems to bind bidders to avoid both violations – people are less likely to overstate high bids and less likely to understate low bids. The next section describes two alternative designs which test the robustness of our main results.

5 Robustness: Training and Consequential wording

We widen the context, using two alternative designs to provide evidence on the effects of training and consequential wording. First, training is common in nearly all experimental auctions (Lusk and Shogren, 2007), and a substantial difference has been observed depending on whether subjects are familiar enough with the mechanism. We assess the robustness of our main results to exposure to a preliminary stage in which our subjects learn how to behave in a second price auction. Second, we try to gather further evidence on why the oath work, by intensifying the feeling of guilt associated with violating the oath signed.

5.1 Commitment and training

Our first robustness test involves training via practice rounds with the second price auction. Ideally, preference elicitation should be more accurate if bidders have no misconceptions about the operations and procedures of the auction mechanism (see for example Shogren, Shin, Hayes, and Kliebenstein, 1994; Plott and Zeiler, 2005; Lusk and Shogren, 2007). While straightforward in theory, the second

Table 7: HG bidding behavior – Panel Tobit estimations

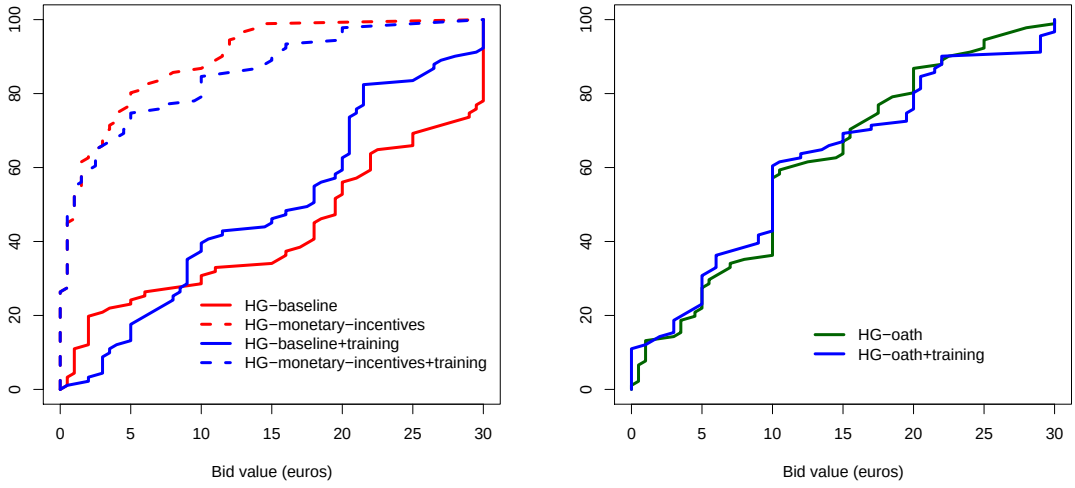
Variable	Parameter estimate	P-value
Treatment dummies		
Constant term	14.67	0.126
Monetary-incentives	-16.71	0.000
Oath	-7.41	0.002
Earnings (€)	-0.08	0.871
Round dummies	YES	
Individual's characteristics		
Age	-0.11	0.615
Male	-2.13	0.295
Participated to other experiments	-3.10	0.119
Experience with auctions websites	1.42	0.076
Knows WWF	-.243	.937
Agrees with WWF actions	3.23	0.001
Knows WWF's dolphin adoption programme	1.32	.722
σ_u (sd.)	6.48 (.69)	
σ_e (sd.)	3.69 (.19)	
Log likelihood	-747.15	

Note. Individual random effects Tobit models (random effects are assumed Gaussian), $n = 54$ and $T = 5$. The *endogenous* variable is the bid posted. Monetary incentives and Oath are dummy variables. Round (fixed) effects are controlled in the estimation but omitted; results are available upon request. Wald joint nullity test is 81.01 with $p < .0001$.

price auction used in our treatments is likely to be unfamiliar to many bidders. They might not immediately realize that bidding their true preferences is the weakly dominant strategy. By training via practice rounds, bidders can learn the potential consequences of under- and over-bidding one's preferences for the good.

We train subjects in the three HG treatments with an additional induced values hypothetical second-price auction, inserted between the quiz and the homegrown auction (see Table 1). Training auctions are identical to those conducted in the IV-Baseline treatment: the auction is repeated over 9 periods, implementing all permutations between private values and the whole demand curve being induced in every period (see section 3.1). We choose to run the training auctions under a hypothetical setting to avoid unwarranted wealth effects. Bidding is done in experimental currency, without conversion into Euros whatever the homegrown treatment that follows. The introduction of an IV hypothetical treatment between the quiz and the homegrown auctions is the only difference between training experiments and those conducted in the previous section. The description of the IV auction closely follows that of the homegrown auction, the only difference being: the repetition of the game – 9 periods for the IV auction to enhance learning of how auctions work *versus* a five-period HG auction – and the good sold. The wording of the auction, in particular, is identical, calling the bids "prices" and the amount of the donation "market price". The training phase is added to the three treatments presented in the previous section and three new treatments are implemented:

Figure 4: Empirical Distribution Functions of bids from trained bidders



(a) Baseline and Monetary incentives (b) Oath

Note. Empirical distribution function of bids, pooling all five rounds of the HG experiment: each point along the curve gives the proportion of observed bids that are lower than the one in abcissa.

HG-Baseline+training, HG-Monetary-incentives+training and HG-Oath+training.

5.1.1 Results

We look first at the effect of training in HG-Baseline+training and HG-Monetary-incentives+training. EDFs of bids are provided in Figure 4.(a), along with those associated with bidding behavior of untrained bidders observed in HG-Baseline+training and HG-Monetary-incentives+training. The curves are marginally different for the monetary incentives treatments with a greater but still small effect of training in the baseline treatments. Here, training seems to increase low bids but decrease higher bids. Examining the summary statistics provided in Table 8 shows again that training marginally affects bidding behavior. In HG-Baseline+training, the number of bids above experimental earnings is lower with training than without training (21.1% of bids compared to 47.7%). This in particular leads bidders to bid on average less (€15.31) than in *HG-baseline* (€17.43). With incentives, trained bidders bid more (mean is €4.23) than with monetary incentives only (mean is €2.98).

Result 6 *Our initial results on bidding behavior remain robust. Training bidders does not change bidding behavior in a homegrown value auction with and without monetary incentives.*

Support. As expected given the observed EDFs, the differences as regards training are not significant: the p-value of our two-sample bootstrap mean difference test for the hypothetical setting is : $p = 0.494$, and $p = 0.476$ for the real setting. ∎

Table 8: Homegrown bidding behavior after (IV) training

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
	Mean bid (€)	14.61	15.25	14.41	16.72	15.58	15.31
HG-baseline	Median bid (€)	13.25	18.25	16.75	19.75	14.75	17.75
+training	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	3	4	3	6	3	19 (21.1%)
	Mean bid (€)	3.33	5.08	4.42	4.17	4.17	4.23
HG-monetary	Median bid (€)	0.75	0.75	1.25	0.75	0.75	1
-incentives	♯ zero bids	5	4	5	5	5	24 (26.7%)
+training	♯ bids > gains	0	0	0	0	0	0 (0.0%)
Average hyp.-real ratio with training		438.7%	300.2%	326.0%	401.0%	373.6%	361.9%
	Mean bid (€)	10.86	10.97	11.25	12.00	12.17	11.51
HG-Oath	Median bid (€)	10.0	10.0	10.0	10.0	10.0	10.0
+training	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	3	3	3	3	4	16 (17.8%)
Average oath-real ratio with training		326.1%	215.9%	254.5%	287.8%	291.8%	272.1%
Average oath-hyp. ratio with training		74.3%	71.9%	78.1%	71.8%	78.1%	75.2%

Note. For each Treatment (in *column*) and round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment for trained subjects: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings (*fourth row*). The last row of the upper part provides the ratio between the average bids in the baseline and the average monetary-only bids. The last two rows of the lower part give the ratios between the average bids after an oath and: first the average baseline bids, second the average monetary-only bids.

We now turn to the effect of training combined with an oath and compare bidding behavior when subjects sign a truth-telling oath with and without training. As in HG-Oath, we found that subjects expressed no reluctance to take the oath. Again, all subjects but one took an oath prior to participating in the auction (94.5% acceptance rate).[23] EDFs of bidding behavior are presented in Figure 4.(b) (oath without training being in plain green and oath with training in plain blue). As seen in the figure, the two curves are almost indistinguishable, suggesting that in oath treatments training subjects with respect to the auction mechanism has no effect. Summary statistics on aggregate bidding behavior presented in Table 8 confirm this finding and show that the mean bid for all rounds is €11.51 as compared to €11.46 in the oath treatment without training (see Table 6).

Result 7 *Bidding behavior of subjects in the HG-Oath+training treatment is not significantly different from bidding behavior in the oath-only treatment.*

Support. Again, discrepancies in aggregate bidding behavior in HG-Oath and HG-Oath+training

[23]The statistical analysis is conducted on the whole sample. Results are similar when excluding the subject who did not take the oath (detailed results available on request from the authors).

are tested by implementing our unconditional mean difference bootstrap test. Mean difference is not significant with $p = .98$. ∎

In summary, results 4 and 5 still hold when training bidders with non-binding IV auctions before bidding in the HG auctions. The decrease of average bidding behavior in HG-Oath+training is significant as compared to HG-Baseline (one-sided mean difference bootstrap test: $p = .036$) and HG-Baseline+training (one-sided mean difference bootstrap test: $p = .089$).

5.2 Oath with consequential wording

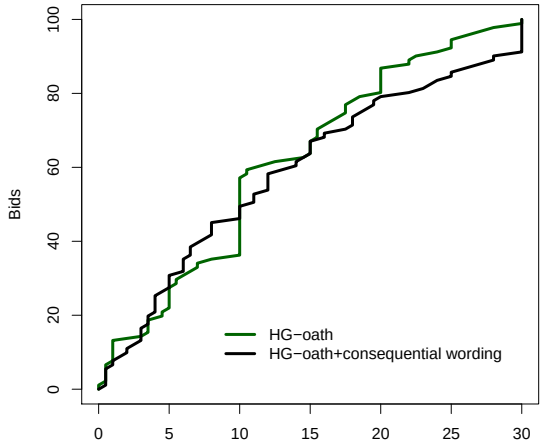
Our last treatment adds insights into the motives underlying the behavioral effects of the oath. Two types of explanation as to why the oath works have been put forward in Section 3.3. On the one hand, the guilt aversion explanation suggests that subjects tell the truth because they do not want to fail to deliver what they believe others expect from them. On the other hand, the commitment-based explanation, either using self-attribution theory or a preference for consistency, posits that subjects tell the truth for internal reasons – concern for others playing no role. To investigate this distinction further, we clearly point out in the procedure what the consequences of not telling the truth would be for the research: results would be biased if dishonest answers were given (because this is "what we observed in previous experimental studies").

In practice, this experiment is identical to HG-Oath except that *consequential wording* is added to the oath procedure. Consequential wording consists of one sentence added to the way the oath is described to the subjects. When they arrive at the desk, the monitor mentions that *(i)* generally speaking, in this kind of experiment, it has been observed in the past that people tend to provide insincere answers and *(ii)* in this case our results are biased (note, again, that nothing is said about what is going to happen later in the lab, i.e., experimental auctions). The oath is then introduced to the subject as a way of avoiding such undesirable results, but still underlining that it is neither mandatory for participating in the experiment, nor a condition of experimental earnings.[24] If and when the oath is signed, the experimenter adds "good, now I'm relieved", and the experiment runs according to the procedure used in the baseline treatment.[25] Hereafter, this treatment is called HG-Oath+consequential-wording.

[24] In a sense, this last treatment can be related to cheap talk scripts. Cheap talk either provides directional or neutral information: "most people typically state a value greater than what they would actually pay"; or "the amounts most people say they will pay for a good are sometimes different from what they would actually be willing to pay for the good." The evidence on the success of cheap talk is mixed – lengthy worded directional cheap talk can remove hypothetical bias (Cummings and Taylor, 1999), whereas neutral cheap talk can exacerbate the bias (Aadland and Caplan, 2006).

[25] In the stated preferences literature, consequential usually means that the respondent understands that he or she believes that he or she will probably pay what he or she says (Mitchell and Carson, 1989). Here consequential indicates that if the subject does not give honest answers, "our" results will be biased.

Figure 5: Empirical Distribution Functions of bids after a consequentially worded oath



Note. Empirical distribution function of bids, pooling all five rounds of the HG experiment: each point along the curve gives the proportion of observed bids that are lower than the one in abscissa.

5.2.1 Results

First of all, we observe that all subjects agreed to take an oath prior to the auction. This result, combined with the acceptance rates obtained in HG-Oath and HG-Oath+training, leads to an acceptance rate of 95.8% overall – subjects in all three treatments expressing no concern for being put under pressure in the oath procedure. EDF of bids in HG-Oath+consequential-wording is provided in Figure 5, along with the EDF of bids in HG-Oath. EDFs do not exhibit clear differences, suggesting that consequential wording does not add any further effect to the standard oath. A further comparison of mean bidding behavior confirms this finding. Table 9 presents summary statistics on bidding behavior in HG-Oath+consequential-wording. Average behavior is unchanged by the addition of consequential wording to the oath procedure: mean bid is €12.33 in HG-Oath+consequential-wording and €11.46 in HG-Oath.

Result 8 *Again our initial homegrown value results are robust. Bidding behavior of subjects in the oath-with-consequential-wording treatment is not significantly different from bidding behavior in the oath-only treatment.*

Support. Mean bidding behavior is not significantly different from mean bidding behavior in the oath-only treatment. The p-value from the two-sample mean difference bootstrap test is $p = 0.731$.

We finally pool data from all seven HG treatments in a random effects panel Tobit model. Dummy variables are introduced to control for the HG-Monetary-incentives and HG-Oath treatments (HG-Baseline being the referent) as well as total earnings and individual's characteristics as in the previous Tobit model. A dummy variable is added to control for trained bidders as well as

Table 9: Homegrown bidding behavior with consequentially worded oath

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
	Mean bid (€)	9.94	12.56	13.5	12.63	13	12.33
HG-Oath	Median bid (€)	6.5	10.5	12.25	12.5	12	10.5
+consequential	♯ zero bids	0	0	0	0	0	0 (0.0%)
-wording	♯ bids > gains	3	3	5	4	5	20 (22.2%)
Average oath-real gap		301.2%	422.9%	425.9%	398.4%	565.2%	413.7%
Average oath-hypothetical gap		64.7%	65.5%	77.9%	72.7%	68.8%	70.7%

Note. For each round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings (*fourth row*). The last two rows of the table gives the ratios between the average bids after an oath and: first the average baseline bids, second the average monetary-only bids.

two dummy variables that control for interaction terms between training and oath and training and monetary incentives. Last, we add a dummy variable that indicates that consequential wording has been used as a complement to the oath (the dummy variable controlling for oath is set to one for this treatment). Results are presented in Table 10. Econometric results confirm unambiguously our unconditional statistics results on aggregate bidding behavior. First, the parameter associated with monetary incentives is negative and significant, indicating that subjects bid on average €16.5 less when monetary incentives are binding than in baseline ($p < .001$)). Second, the dummy variable accounting for oath is also highly significant and negative: subject bid on average €6.61 less when they are committed by taking an oath as compared to baseline ($p = .01$). Third, the parameter associated with trained bidders is not significant ($p = .43$)) and this is also true for oath and monetary incentives interaction terms ($p = .69$ and $p = .17$ respectively). Fourth, adding consequential wording to the oath procedure has no additional effects on bidding behavior ($p = .62$). ∎

From the theory of commitment perspective, an oath is an extreme type of commitment that binds the subject to tell the truth. Result 8 suggests that adding explicit warnings about the consequences of lying for the research at stake (and consequently for the monitor, who is identified in our setting as one member of the research team), "our results would be biased", does not make any difference. This makes guilt aversion an unlikely candidate to explain why the oath has a powerful effect on bidding behavior. However, applying the theory of commitment leads to the plausible explanation that people behave differently under oath because the oath commits them to tell the truth, not because they dislike the consequences for others of possible lies. This last result is consistent with Vanberg (2008)'s conclusion in the context of promises through pre-play communication (see Section 3.3).

Table 10: Overall HG bidding behavior – Panel Tobit estimations

Variable	Parameter estimate	P-value
Treatment dummies		
Constant term	4.91	0.505
Monetary incentives	-16.54	0.000
Oath	-6.61	0.010
Earnings	0.37	0.330
Training	-2.00	0.430
Training × oath	1.47	0.688
Training × monetary incentives	5.11	0.166
Consequential wording	1.29	0.622
Round dummies	YES	
Individual's characteristics		
Age	-0.03	0.835
Male	-1.53	0.292
Participated to other experiments	-1.13	0.459
Experience with auctions websites	1.61	0.008
Knows WWF	0.34	.878
Agrees with WWF actions	1.70	0.002
Knows WWF's dolphin adoption programme	-.87	.705
σ_u (sd.)	7.37 (.51)	
σ_e (sd.)	3.29 (.11)	
Log likelihood	-1688.93	

Note. Individual random effects Tobit models (random effects are assumed Gaussian), $n = 126$ and $T = 5$. The *endogenous* variable is the bid posted. Monetary incentives and Oath are dummy variables. Training is introduced as a dummy variable and training effects specific to monetary incentives and oath are controlled by treatment-specific dummy variables. Consequential wording is introduced with a dummy variable (and oath is set to one for these data). Round (fixed) effects are controlled in the estimation but omitted; results are available upon request. Wald joint nullity test is 114.28 with $p < .0001$.

6 Conclusion

Incentive compatible mechanisms use external incentives to elicit the truth from people – true types, true bids, true preferences. But eliciting truthful behavior still requires people to be committed to telling the truth. Placing a person in a "market-like" valuation context – whether hypothetical or real, in the lab or in the field – seems insufficient to generate the internal commitment needed for sincere bidding. What is needed is a commitment device such as the oath, the centuries-old mechanism designed to align internal incentives with social goals.

Herein we study preference elicitation under oath. In induced valuation treatments, the oath-only treatment induced sincere bidding behavior in the second-price auction; the other treatments did not. In the homegrown value treatments, the oath did its job by inciting bidders to lower bids on the high end of the distribution and increase bids on the low end. Such behavior is consistent with the notions that bidders took their budget constraints and participation constraints seriously. Having subjects signing an oath on one's honor to "tell the truth and provide honest answers" before

bidding induced more rational economic behavior. These results were robust to additional training with the auction mechanism and additional consequential wording stressing the consequences of not telling the truth. Our findings hold promise, opening the way for better preference elicitation of non-market goods, like environmental protection.

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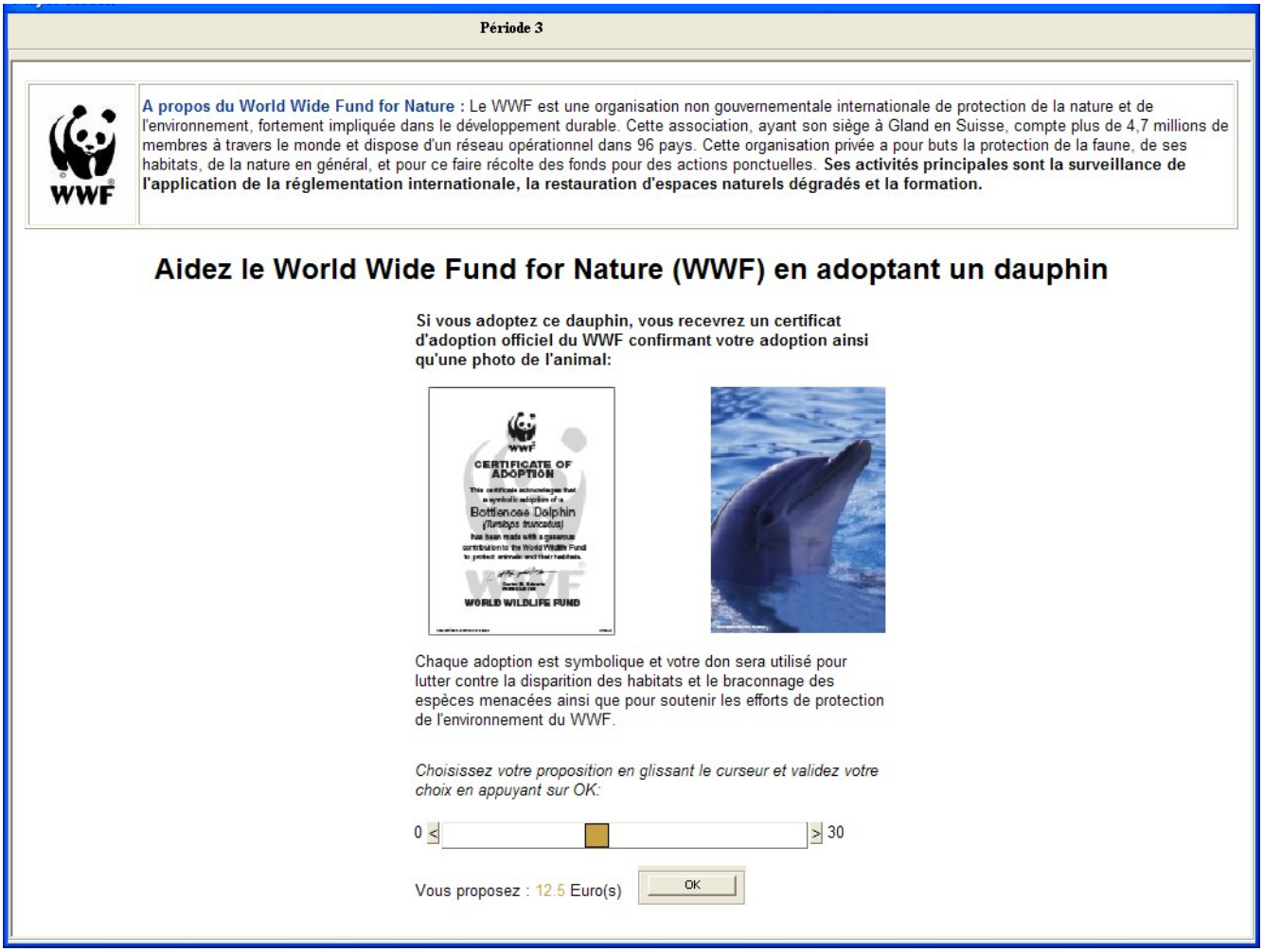
A Panel Tobits on IV-bidding behavior – Full results Table

Variable	IV-Monetary-incentives $n = 162$	IV-Baseline $n = 162$	IV-Oath-Monetary-incentives $n = 162$	IV-Oath $n = 162$
	Parameter estimates (p-value)			
ν_{it}	.855	.788	.756	.969
	(.000)	(.000)	(.000)	(.000)
Constant	.480	4.44	8.99	-6.1
	(.945)	(.394)	(.044)	(.182)
ϕ_2	7.33	3.00	4.49	8.21
	(.261)	(.528)	(.273)	(.053)
ϕ_3	17.2	11.4	7.94	8.35
	(.008)	(.016)	(.053)	(.049)
ϕ_4	12.4	9.56	5.88	6.94
	(.059)	(.045)	(.152)	(.102)
ϕ_5	5.98	15.9	8.11	13.8
	(.360)	(.001)	(.048)	(.001)
ϕ_6	17.8	11.9	10.9	12.0
	(.006)	(.012)	(.007)	(.005)
ϕ_7	20.8	14.1	14.2	10.6
	(.002)	(.003)	(.001)	(.012)
ϕ_8	13.2	11.8	15.3	6.65
	(.042)	(.013)	(.000)	(.115)
ϕ_9	10.2	18.7	13.9	11.8
	(.117)	(.000)	(.001)	(.005)
σ_u	2.40	9.19	4.88	8.72
	(.000)	(.000)	(.000)	(.000)
σ_ϵ	19.55	14.25	12.31	12.64
	(.000)	(.000)	(.000)	(.000)
Log-likelihood	-675.11597	-659.40375	-640.24359	-641.81659

Note. Individual random effects Tobit models. The random effects are assumed normal; round dummies included in all regressions. The *endogenous* variable is the bid posted. v_{it} denotes the induced private value.The columns report results from separate regressions on the IV treatments. The number of observations used in the regression is provided in the second row.

B Screen shot of the adoption page



C Supplementary material

C.1 Quiz questions

The four possible answers apppear just after the question. The right answer is the first one in the list. In the experiment, the order of answers is identical within sessions but randomized between sessions.

1. In what place did the delegates of the "Third Estate" meet, after the king rejected their list of grievances?

   The "salle du jeu de Paume", The National Assembly, Versailles, The Senate

2. What is the name of the current Director of the International Monetary Fund?

   Dominique Strauss-Kahn, Alan Greenspan, Alain Juppé, Pascal Lamy

3. Who wrote "Germinal" ?

   Emile Zola, Gustave Flaubert, Guy de Maupassant, Honoré de Balzac

4. What is the capital of Australia?

   Canberra, Sydney, Perth, Auckland

5. Who is the writer who said "l'enfer c'est les autres." ("Hell is other people")

   Jean-Paul Sartre, Gérard de Nerval, Boris Vian, Sacha Guitry

6. In France, in the event of vacancy or impeachment, who decides in the place of the President?

   The President of the Senate, The Prime Minister, The President of the national Assembly, The Minister of justice (Le Garde des Sceaux = keeper of the seals)

7. What is the American state with the largest population?

   California, Florida, New York, Texas

8. In the novel "Gulliver's travels", Gulliver discovers fantasy countries. What is special about people from Lilliput ?

   Their height is no more than six inches, All are giants, Their head is that of an animal, They are hugely rich

9. Who wrote the "Iliade" znd "Odyssey"' ?

   Homer, Socrates, Plato, Virgil

10. The date at which State and Church were separated is:

    A bill of 1905, The Concordat of 1801, A bill of 1889, The Lateran agreement of 1929

11. In chemistry, what are the letters used to symbolize the acidity of a liquid?

    Ph, Na, Ca, Ba

12. Which of the following writers, who would have been able to meet Jean-Jacques Rousseau ?

    Voltaire, Baudelaire, Hugo, Zola

13. Which of the following designates a word or sentence that says the same thing as what was said before?

    A pleonasm, A euphemism, A metaphor, An understatement

14. Before setting in Paris in 1944, the temporary government of the French Republic was:

    In Algiers, In London, In the free zone, In the occupied zone

15. Who was the first woman to become Prime Minister (1991-1992) under President François Mitterrand?

    Edith Cresson, Simon Veil, Martine Aubry, A woman has never occupied this position

16. Signed on 25 March 1957, this treaty is the starting point of the European Economic Community (CEE):

    The Rome treaty, the Versailles treaty, The Paris treaty, The Maastricht treaty

17. The human body's is composed of ...

    60% water, 20% water, 40% water, 80% water

18. What is the new regulation introduced by the Schengen agreement?

    the free circulation of people, The European flag, The implementation of the Euro, The European constitution project

19. How do you say "A vos souhaits!" ("Bless you !") in English?

    [A vos souhaits] !, Sorry !, Good bye !, Thank you !

20. Who wrote: "Les sanglots longs ; Des violons de l'automne ; Blessent mon coeur ; D'une langueur monotone"?

    Verlaine, Baudelaire, Musset, Rimbaud

C.2 Instructions common to all experiments

[*changes implemented according to treatment appear in brackets.*]

You're involved in an experiment in which you can earn money. The amount you will earn will depend on your own decisions as well as the decisions of other participants.

Before starting the experiment, we will ask you to answer a few questions aimed at getting to know you better (your age, your gender, your occupation, ...). **All this information as well as your monetary earnings will be kept anonymous and confidential**.

[*computerized administrative questionnaire answered at this point*]

Thank you.

Experiment procedure

[*In HG Experiments only:*] The experiment involves two [*Training: three*] parts. The instructions describing the procedure for each part will be distributed and read aloud before each part.

How will you take your decisions?

Your screen is divided into three areas:

The upper part shows all the information you need to take your decisions.

The middle part allows you to take your decisions, by pressing buttons displayed.

The bottom part provides a reminder of your past decisions and profits.

Payment of your earnings

Your earnings during the experiment will be expressed in ECU (for *Experimental Currency Unit*). These earnings are converted into Euros according to the rate: 3 ECU = 1 €. A fixed fee equal to 10€ is added to this payoff. You will be paid privately the corresponding monetary payoff in cash at the end of the experiment.

For obvious scientific reasons, **it is mandatory not to speak during the experiment**. Unfortunately, we will have to ask any participant not complying with this rule to leave the room without any opportunity to take potential earnings.

It is very important you understand the procedure of the experiment. If you have any questions, please raise your hand, someone will come and answer you. Thank you for following these rules.

Thank you for your participation.

C.3 Instructions for the IV-Experiment

[The instructions below are used for the IV-Baseline treatment.]

At the beginning of this part, **two groups composed of 9 participants** each are formed. **Each participant belongs to the same group throughout the part**.

Overview. You will be participating in an auction in which you are a buyer. Your have to offer, at each round, a price in ECU to buy a good. The experiment monitor will re-acquire this good from you. There will be several rounds of bidding. **The outcome of each auction in each round has no influence on [*Monetary incentives*: *directly influences*] how much you will get paid at the end of the experiment.**

Procedure for each round

Each round has 8 steps.

Step 1. Each bidder looks at his or her **resale value** on his or her screen. We term **resale value** the price in ECU the monitor will pay to buy back a unit of the good that is purchased in the auction. **The resale values of different participants in a group can be different**. Once you have looked at your resale value, press the OK button;

Step 2. Each bidder then submits a bid in ECU to buy one unit of the good. To do this, scroll down the until you find the price you want to submit. Then press the OK button below the scroll bar to confirm your choice;

Step 3. The monitor ranks the bids from highest to lowest. In event of ties, the ranking is drawn randomly. For instance:

n° 1 fs.l ECU **Highest bid**

n° 2 df.g ECU

n° 3 za.f ECU

n° 4 qs.a ECU

n° 5 qs.a ECU

n° 6 nj.h ECU

n° 7 hh.m ECU

n° 8 ht.t ECU

n° 9 ky.l ECU **Lowest bid**

Step 4. The second highest bid (bid n°2) determines the **market price**. In the above example, the second highest bid is df.g ECU so the market price would be df.g ECU:

n° 1 fs.l ECU

---

n° 2 df.g ECU **Second highest bid: market price**

---

n° 3 za.f ECU

n° 4 qs.a ECU

n° 5 qs.a ECU

n° 6 nj.h ECU

n° 7 hh.m ECU

n° 8 ht.t ECU

n° 9 ky.l ECU

Step 5. The buyer who bid the highest price (the buyer ranked n°1) purchases one unit of the good at the market price. In the above example the buyer who bid fs.l ECU purchases one unit of the good that costs df.g ECU.

Step 6. Buyer n°1 then sells the unit back to the monitor. The price of this transaction is the resale value listed for that round on his/her screen. The profit in ECU bidder n°1 earns for that round is the difference between the resale value and the market price:

$$\text{profit} = \text{Resale value - market price}$$

Important note. You can have negative profits: if you buy a unit of the good and the resale value is less than the market price, your profits will be negative.

Step 7. All bidders at or below the market price (buyers n°2 to n°9) buy nothing, **they make zero profit for that round**.

Step 8. End of the round. Your profit in ECU in that round appears on your screen. Press the OK button once you have read it. Your screen announces whether a new round is about to start, or whether the experiment is over.

Earnings for this part

Your payoff in ECU for this part is 0 whatever your earnings at each period. [Monetary incentives*: your payoff in* ECU *for this part is set equal to the sum of your earnings at each period.*]

C.4 IV: Pre-experiment questionnaire

1. Groups are reformed in each round.

   ☐ YES ☐ NO

2. Each group is composed of _____ participants.

3. At the beginning of each round, all participants belonging to my group are attributed the same resale value.

   ☐ YES ☐ NO

4. When I make a bid, I can bid any amount I wish.

   ☐ YES ☐ NO

5. The market price is set by the bid of the highest bidder in my group.

   ☐ YES ☐ NO

6. If my bid is the highest bid and is equal to RR.U ECU and the second highest bid in my group is GG.K ECU, then I buy the unit of the good.

   ☐ YES ☐ NO

   If yes, I pay: _____ for the good.

7. If I purchase a unit of the good and my resale value is greater than the market price, I will make positive profits.

   ☐ YES ☐ NO

8. The monetary payoff I will get at the end of the experiment depends on the amount of ECU I earned in the auction.

   ☐ YES ☐ NO

If you are surprised by some answers, please ask questions.

C.5 Instructions for HG experiments

Procedure for the first part

The first part involves **20 rounds**. At each round, you have to answer a question by choosing one out of **four possible answers**. To choose an answer, click on the corresponding button. Only one out of the four answers is correct.

- If the answer you choose is the correct one, your earnings for this round are 2 ECU.
- If the answer you choose is not the correct one, your earnings for this round are 0 ECU.

At the end of this part, a message displays your earnings in each round. Your payoff in ECU for the first part is the sum in ECU you earned in each period. Press OK once you've read this.

****

[*In HG+training experiments, the instructions used for IV-Baseline, along with the pre-experiment questionnaire, are inserted here.*]

****

Procedure for the second [*training: third*] part

The following information is required in order to fully understand this part.

The *World Wide Fund for Nature*, better known as the WWF, is an international non-governmental organisation for the protection of nature and of the environment, fully committed to sustainable development. The head office is in Gland, Switzerland, and the association has more than 4.7 million members worldwide, with an operational network in 96 countries. It is a private organisation aimed at protecting wild animals and their habitats as well as nature in general, which it does by collecting funds for specific programs. Principally, it keeps a watchful eye on whether international regulations are being respected, restores damaged natural areas and provides training.

As a way of financing its environmental protection activities, the WWF offers private individuals the opportunity to adopt an animal from an endangered species. The funds thereby collected enable the WWF to continue protecting the environment and preserving species diversity.

During this part, we ask you to imagine that you were taking part [*Monetary incentives: you are going to take part*] in this operation by making a donation, which would be [*Monetary incentives: will be*] deducted from your experimental earnings, to adopt a dolphin. The sums collected during this part would be [*Monetary incentives: will be*] passed on by us to the WWF, to support their environmental protection activities. Your donation to the WWF would be [*Monetary incentives: will be*] recorded on an official certificate, which would be [*Monetary incentives: will be*] sent to your home address. We ask you to make your decisions as if, in this part, we were genuinely offering you the opportunity to adopt a dolphin, according to the procedure described below. The decisions made during this part are not, however, taken into account when calculating your Euro experimental earnings. In actual fact, regardless of your decisions, you will not be adopting a dolphin and your experimental earnings will not be affected. [*Monetary incentives: We will genuinely make it possible for you to adopt a dolphin if you so decide, according to the procedure described below. The decisions made during this part are taken into account when calculating your Euro experimental earnings. This means that if you adopt a dolphin, your donation will be deducted from your earnings.*]

Description of the part

At the beginning of this part, two **groups of 9 participants** are formed. Each participant will belong to the same group throughout the part. One single participant from each group would be given [*Monetary incentives: will be given*] the opportunity to adopt a dolphin through the WWF.

This part is composed of **5 rounds**. During each round, you will take part in an auction, playing the part of a buyer. In each group, the participant offering the highest bid wins the auction, and the market price for the round is the second highest bid.

At the end of the part, one round alone will be drawn at random from the 5. In each group, the **participant who won the auction during the round drawn would donate [***Monetary incentives: will donate***] money to the WWF** by adopting a dolphin. The **amount of this**

donation would be [*Monetary incentives: will be***] the market price for the round drawn**. This donation would be [*Monetary incentives: will be* deducted from the experimental earnings of the participant who undertook [*Monetary incentives: undertakes*] the adoption. The earnings for this part for the participant winning the auction in the round drawn would be [*Monetary incentives: are*] therefore negative and would represent [*Monetary incentives: represent*] the donation that was [*Monetary incentives: is*] given to the WWF.

Procedure for the part

Before the part begins, you will receive a message showing your earnings in Ecu from the preceding part, the corresponding amount in Euros and your total earnings in Euros. This total includes the lump sum of €10. Press OK when you have read this information.

You will receive a second message reminding you of the description of the part. Please confirm that you have read this information by pressing the button on the screen.

Procedure for a round

Summary. You are participating in an auction, playing the part of a buyer. Monetary values in this auction are expressed in Euros. Your role consists of bidding a purchase price in Euros. This price is the donation you are prepared to make to help the WWF in its environmental protection activities by adopting a dolphin. In each group, the participant winning the auction for a given round is the one whose bid is highest, and the market price for that round is the second highest bid.

Each round consists of 7 steps.

Etape 1. A window will open on your screen, with a detailed description of how the WWF protects dolphins, as well as the documents sent to people adopting a dolphin. Please read this information carefully.

Etape 2. Each buyer bids a price in Euros. To do this, scroll down on the right-hand side of the information page until you find the amount you want to bid. Then press the OK button below to confirm your choice.

Etape 3. All the bids from your group are ranked from highest to lowest by the computer running the experiment. If there are identical bids, the ranking of identical bidders is determined by a random draw. For example:

n° 1 fs.l Euro **Highest bid**

n° 2 df.g Euro

n° 3 za.f Euro

n° 4 qs.a Euro

n° 5 qs.a Euro

n° 6 nj.h Euro

n° 7 hh.m Euro

n° 8 ht.t Euro

n° 9 ky.l Euro **Lowest bid**

Etape 4. The highest bidder (ranked no.l) wins the auction. In the preceding example, the person who bid fs.1 Euro won the auction.

Etape 5. The second highest bid (purchasing price no.2) determines the market price. In the above example, the second highest bid is df.g Euro and the market price for the round is therefore df.g Euro.

n° 1 fs.l Euro

---

n° 2 df.g Euro **Second highest bid**

---

n° 3 za.f Euro

n° 4 qs.a Euro

n° 5 qs.a Euro

n° 6 nj.h Euro

n° 7 hh.m Euro

n° 8 ht.t Euro

n° 9 ky.l Euro

End of the part

At the end of the part, you will receive a message giving the result of the auction for your group for each of the 5 rounds: an initial message will tell you whether or not you have won this round of the auction. If you have won this round of the auction, a second message will give you the market price for the round.

Next, you will receive a message giving the round drawn at random from the 5 rounds of the part. In each group, the participant winning the auction for the round drawn would undertake [*Monetary incentives: undertakes*] a WWF adoption. The amount of this donation would be [*Monetary incentives: is*] the market price for the round drawn. This donation would be [*Monetary incentives: is*] deducted from the experimental earnings of those participants adopting a dolphin.

Example: Suppose for example that you won round 3 of the auction and that the market price for round 3 was mm.i Euros. If round 3 is drawn, you would make [Monetary incentives: will make]

a donation to the WWF by adopting a dolphin. The amount of your donation would be [Monetary incentives: will be] mm.i Euros. This amount would be [Monetary incentives: will be] deducted from your experimental earnings.

Important Note. Your donation to the WWF could [*Monetary incentives: can*] be greater than your total experimental earnings. If you win the auction round drawn and if the market price for the round drawn is greater than your total experimental earnings, your donation to the WWF would be [*Monetary incentives: will be*] greater than your total experimental earnings. **In this event, you would have to [***Monetary incentives: will have to***] pay the difference in cash, or by any other means of payment, at the end of the experiment.** We would [*Monetary incentives: will*] pay this sum to the WWF, as part of your full donation.

Example: Returning to the previous example, and supposing that you won round 3 of the auction, that the market price for round 3 is mm.i Euros and that round 3 is drawn. You would therefore make [Monetary incentives: will make] a donation to the WWF by adopting a dolphin and the amount of your donation would be [Monetary incentives: is] mm.i Euros. If your total experimental earnings are less than mm.i Euros, you would have to [Monetary incentives: will have to] pay the difference at the end of the experiment.

The participants who do not win the auction round drawn would not undertake [*Monetary incentives: do not undertake*] an adoption and would not make [*Monetary incentives: do not make*] a donation to the WWF.

End of the experiment

At the end of the experiment, the participants who had won the auction would be asked [*Monetary incentives: will be asked*] to give us their personal address. The amount of their donation would be [*Monetary incentives: will be*] paid by us on their behalf to the WWF, to support their environmental protection activities. Those people adopting a dolphin would receive [*Monetary incentives: will receive*], at the address given, an adoption certificate and a photograph acknowledging their donation to the WWF. [*Monetary incentives:* **We undertake to respect confidentiality***, and will only use this information for the purposes of the donation.*]

Calculating your experimental earnings

Your experimental earnings in Euros are the amount given at the beginning of the second part. Your decisions in that part are not taken into account when calculating your experimental earnings in Euros. [*Monetary incentives: If you have adopted a dolphin, these earnings are reduced by the amount of your donation. If the amount of your donation is greater than your experimental earnings, we will ask you to pay the difference in cash at the end of the experiment in order to make up the full amount of your donation.*]

C.6 HG experiment: Pre-experiment questionnaire

1. Each group is composed of _____ participants.

2. When I bid, I can bid any amount I like.

   ☐ YES ☐ NO

3. The market price is determined by the highest bid in my group.

   ☐ YES ☐ NO

4. If my bid is the highest bid and is RR.U Euros, and if the second highest bid in my group is GG.K Euros, I win this round of the auction.

   ☐ YES ☐ NO

   The market price for this round is _____.

5. 1.If this round is drawn at random, I would make [*(Monetary incentives: will make*] a donation to the WWF by adopting a dolphin.

   ☐ YES ☐ NO

   If yes...

   ... the amount of my donation will be [*Monetary-incentives: would be*] _____.

   ... the amount of my donationwill be [*Monetary-incentives: would be*] deducted from my experimental earnings.

   ☐ YES ☐ NO

   ... if the market price is greater than my total experimental earnings, I would have to pay the difference out of my own pocket at the end of the experiment?

   ☐ YES ☐ NO

6. The amount earned in Euros that will be paid to me at the end of the experiment depends on my decisions during this part.

   ☐ YES ☐ NO

C.7 HG experiment: Post experiment questions

Debriefing questions appear one at a time, together with the answers in parentheses.

- Do you belong to an environmental association? (YES/NO)
- Did you know of the WWF before taking part in this experiment? (YES/NO)
- Did you know of the WWF's dolphin adoption programme before taking part in this experiment? (YES/NO)
- Have you previously adopted an animal to help an association for the protection of nature? (YES/NO)
- What is your opinion of the WWF's activities?

  (totally opposed, opposed, moderately opposed, no opinion, moderately in favour, in favour, totally in favour)
- Have you ever previously bought or sold an object on an online auction site of the ebay type? (YES/NO)