Preference Elicitation under Oath[1]

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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 and hypothetical). 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 in place of the zero bids used to opt out of auction). Based on companion treatments, we show the oath works through an increase in the willingness to tell the truth, due to a strengthening of the intrinsic motivation to do so.

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

JEL Classification: C9; H4; Q5

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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

Cost-benefit analysis are at the core of most public policies devoted to the production of non-market goods. The required elicitation of sincere preferences remains a challenge due to hypothetical bias – the so-called gap between hypothetical monetary values and real economic commitments. Accumulating experimental evidence from lab and field reveals the average person tends to overstate real economic commitments by a substantial amount.[2] 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 (Smith, 1994). 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

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[2]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); 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.

want to be in could serve to pressure the person, thereby violating his or her participation constraint. Bidders can opt out of the experiment by bidding zero, as a result of this violation (see, e.g., Cherry, Frykblom, Shogren, List, and Sullivan, 2004).

Hypothetical bids without binding budget constraints, or real economic commitments without binding participation constraints, both scenarios 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 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 tended to bid sincerely after taking an oath for a hypothetical auction; they did not bid sincerely with monetary incentives or under a no-oath hypothetical scenario. For homegrown values, the oath elicited preferences that seemed to reduce violations both in the budget constraint (only 18.9% of bids are above the disposable income earned in the experiment 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). Put together, those results unambiguously support having subjects sign an oath to tell the truth has a significant impact on bidding behavior. We moreover confirm the robustness of the results trough training subjects with the elicitation mechanism before the actual procedure takes place. We rely on further companion treatments to assess the reasons why it happens. First, we explore whether the oath works by helping subject to tell the truth, or through a change in their willing to do so, by adding an explicit exhortation to truthfully reveal preferences – a procedure known as *cheap talk scripts* since Cummings and Taylor (1999). Our results clearly support the second interpretation – oath makes subjects more willing to tell the truth. We then turn to the behavioral motives

underlying the observed change in behavior. We provide insights that help disentangle between the two main candidates: intrinsic preference to behave in accordance with past actions or aversion to guilt feelings. In an IV setting, adding external incentives through monetary payments to the oath reduces sincere bidding; in an HG setting, adding explicit warnings about the negative consequences associated with lying is neutral on the effect of the oath. Both results point to internal reasons underlying the effect of the oath, which favors the commitment-based interpretation. 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 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

[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.

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, 2008). When commitment is strong enough, the change in behavior is long-lasting, and survives a long time after the promise have been made (Geller, Kalsher, Rudd, and Lehman, 1989; Cobern, Porter, Leeming, and Dwyer, 1995; Boyce and Geller, 2000). 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. In this case, commitment induces strong and lasting changes in behavior.

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

2.2 Open questions in "real auction" bidding behavior

Preference elicitation methods are mechanisms to reveal individual preferences in contexts where the market cannot. 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

oath is above all a personal commitment.

[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.

(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" if there is a strong likelihood that the design is ripe for reactance. 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 as the culprit. Reflecting back, the experimental design most likely generated the large number of zero real bids observed given it created an environment that would promote reactance. 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.

2.3 Overview of the experimental design

In 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 purposefully chose to test the oath by working with the Vickrey auction rather than a discrete choice elicitation mechanism because the auction

[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 (see Cummings and Taylor, 1999), 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.

Table 1: Experimental design

Experimental design					
Experiment	Oath	Quiz	Vickrey with IV	Cheap Talk	Vickrey with HG
IV			×		
HG		×			×
HG + Training		×	×		×
HG + CheapTalk		×		×	×
Main features of each core part					
Number of repetitions	1	20 questions	9		5
Group sizes	1	1	9		9
Payments	-	Number of true answers	Sum of auctions' earnings		Winner's earnings decreased by 2nd price
Show-up fee	In all experiments, set equal to 10 €				
Experimental Treatments					
Baseline	All auctions are non-binding				
Incentives	The last set of auctions (IV or HG) of the experiment is binding				
Oath	Each subject is submitted to the oath procedure before entering the lab				

has three attractive features for our testbed purpose. 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, market price is endogenous rather than arbitrarily chosen by the experimenter. Third, bidders announce their value for the good in a continuous manner. It provides continuous observations on individual preferences – as compared to only mass points with provision point mechanisms – and allows for much more precision in the statistical analysis. Future work can explore how the oath operates in alternative discrete choice preference elicitation mechanisms, e.g., referenda voting, provision point.

In any case, note our main results should not be affected by this choice of mechanism, as all results are derived from differences in elicited demand – holding the mechanism constant. We start by testing the oath both with and without monetary incentives in an IV auction (Section 3). These different treatments allow us to calibrate observed bidding behavior against theory and against each treatment. We then move to settings closer to the field. First, we examine how the oath affects preference elicitation of HG values to protect dolphins compared to hypothetical and real bidding (Section 4). Second, the robustness of those results are assessed through additional treatments in which we rely on the IV auction design to train subjects with the mechanism before entering the dolphin adoption auctions (Section 5). We conclude the paper with further treatments aimed at exploring how and why

the oath works. Our first concern is to contrast the performance of the oath with explicit exhortations to tell the truth. This led us to design treatments that implement so-called cheap talk scripts, that inform subjects about hypothetical bias and its unwarranted consequences (Section 6). Last, we derive some insights about the behavioral motivations underlying this effect from alternative combinations of our experiment treatments (Section 7).

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 three-treatment design of an IV auction: (i) baseline hypothetical bidding, with neither an oath nor monetary incentives; (ii) baseline coupled with an oath and (iii) bidding with binding monetary incentives.

IV-Baseline. We use a Vickrey second price auction, in which the highest bidder pays the second-highest bid. We divide each 18-subjects session into two independent groups of 9 bidders to avoid too much distortion of bidding at the bottom of the distribution of bids (see Kagel, 1995). In each auction, each bidder is 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 we hold constant the points at which we elicit preferences, to avoid unwarranted noise between rounds. The demand curve in each auction 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 effects 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 on their screen about whether they won and, in this case only, the market clearing price and the hypothetical earnings for that period. Lastly, everybody is 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-Incentives. This treatment makes bidding behavior binding – auction winners earn their take-home pay based on the accumulated difference between their induced value and the market-clearing price for each round in which they win the auction. 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.

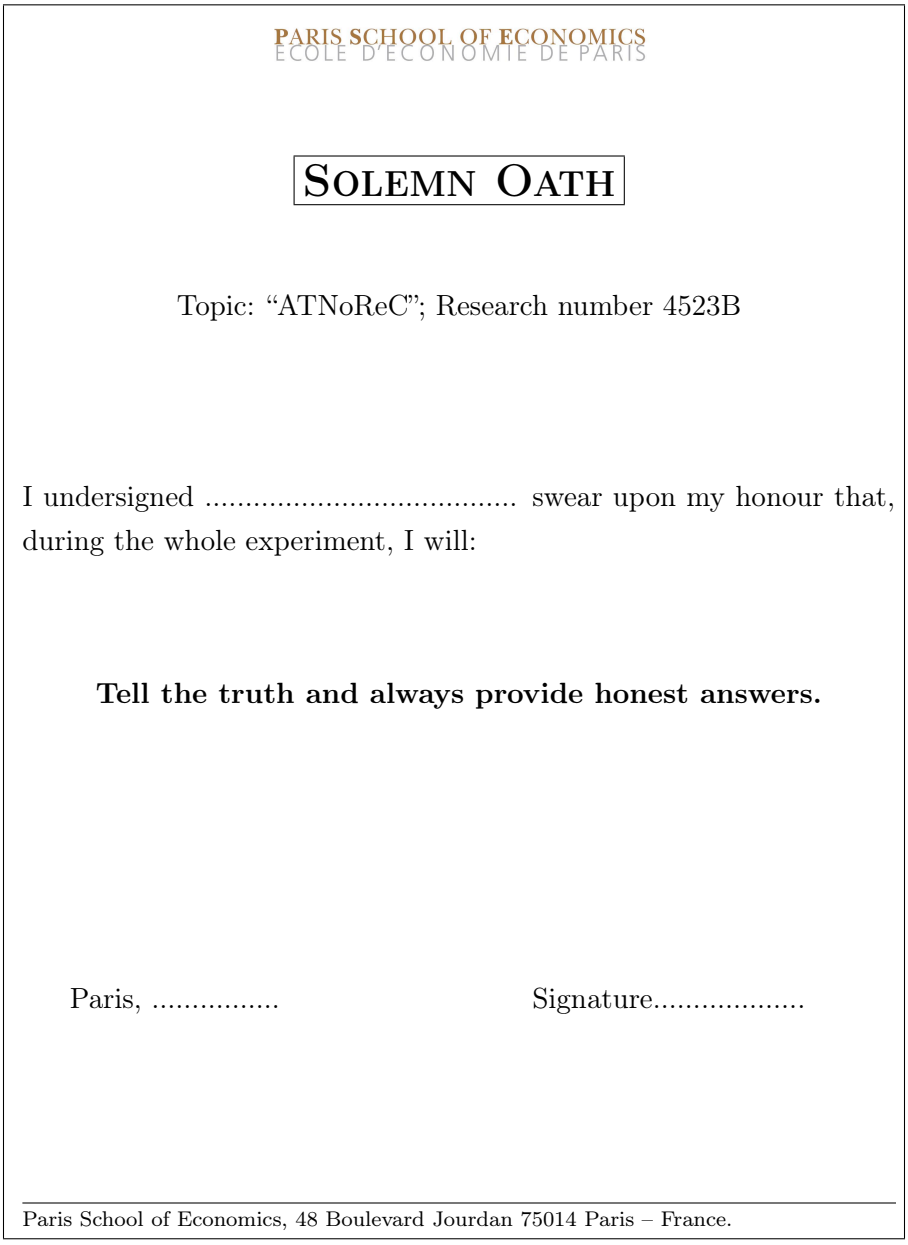
IV-Oath. This treatment is identical to the baseline except each subject is asked to sign an explicit oath before entering the lab. The oath is implemented as follows: each subject enters alone and is directed to a monitor at the front of the laboratory. The monitor then offers each subject a form to 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 the 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 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

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

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

[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.

Figure 1: Oath form used in the experiments



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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.

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.

3.2 Experimental procedures

Three experimental sessions – one per treatment – were conducted at University Paris I. Each session involved 18 subjects, providing two independent 9-times repeated Vickrey auctions.[10] 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)'.[11] 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 (gender, age, ...). 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 bid observations for each treatment. 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. Given the constant demand curve induced in each round, the induced aggregate demand is always the same, equal to 1084. For each treatment, the first row of the Table gives the aggregate demand we actually observe and the second row provides the ratio between the two – a score of 100 indicating perfect revelation of the demand induced in the auction. 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-Incentives treatments.[12] The average demand revelation is even slightly better when monetary incentives are dropped, falling from

[10] All experiments were computerized using a software program developed under REGATE (Zeiliger, 2000) and participants were recruited based on ORSEE (Greiner, 2004).

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

[12] 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: Induced value bidding behavior by group and round

Aggregate Demand	Round									
1084	1	2	3	4	5	6	7	8	9	Total
IV-Baseline	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-Incentives	949	1081	1245	1149	1062	1254	1290	1168	1130	10328
	87.5	99.7	114.8	105.9	98.0	115.7	119.0	107.7	104.2	105.9
IV-Oath	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

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 %.

106% to 104%. This feature has been replicated in valuation experiments in France (see e.g., Ehmke, Lusk, and List, 2008; Jacquemet, Joule, Luchini, and Shogren, 2009b).

But this does not imply that our bidders perfectly revealed their true value for the good. Past experimental evidence has shown that the second-price auction with monetary incentives only 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 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 reorganizes the same data by induced values. For each induced value, the aggregate demand is 18 times the individual value (9 bidding rounds times two auctions in each treatment) and the bottom part of the top row of the Table provides the resulting aggregate demand. For each treatment, we report the revealed aggregate demand and the ratio between induced and revealed demand. Note, each cell in this Table comes from bidding in all 9 rounds, since each value is induced once and only once in each round. Demand revelation almost monotonically 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%.

Table 3: Induced value bidding behavior by group and Induced Value

Induced value	24	38	53	63	65	68	71	76	84	Total
Aggregate demand	432	684	954	1134	1170	1224	1278	1368	1512	9756
IV-Baseline	626	808	1050	1193	1201	1192	1242	1290	1532	10134
	144.9	118.1	110.1	105.2	102.6	97.4	97.2	94.3	101.3	103.9
IV-Incentives	687	735	1078	1045	1318	1259	1281	1334	1591	10328
	159.0	107.5	113.0	92.2	112.6	102.9	100.2	97.5	105.2	105.9
IV-Oath	475	757	1046	1129	1261	1249	1331	1345	1535	10128
	109.9	110.7	109.6	99.6	107.8	102.0	104.1	98.3	101.5	103.8

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 %.

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

Support. We first test the assumption of perfect revealing bid at the aggregate level by examining bidding behavior of off-the-margin bidders. We do so by testing the bid to induced value ratio. If the bidder bids his or her induced value, i.e. tells the truth, the ratio equals one. Bootstrap tests reject the null of perfect demand-revealing bids for the lowest induced value (24 ECU) with p-values 0.016 in IV-Baseline and 0.018 in IV-Incentives.

We test the assumption of perfect revealing bids all along the demand curve 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 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 $H_0 : \{\beta = 1, \alpha = 0, \phi_t = 0\ \forall t\}$

Table 4: IV bidding behavior – Panel Tobit estimations

Variable	IV-Incentives $n = 162$	IV-Baseline $n = 162$	IV-Oath $n = 162$
	Parameter estimates (p-value)		
ν_{it}	0.855	0.788	0.969
	(0.000)	(0.000)	(0.000)
Constant	0.48	4.44	-6.10
	(0.945)	(0.394)	(0.182)
Round dummies	YES	YES	YES
σ_u	2.40	9.19	8.72
	(0.000)	(0.000)	(0.000)
σ_ϵ	19.55	14.25	12.64
	(0.000)	(0.000)	(0.000)
Log-likelihood	-675.11597	-659.40375	-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.

(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-Incentives	:	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 we can make sincere bidding more likely by asking bidders to freely take a truth-telling oath prior to participating in the baseline auction. Nearly all subjects had no problems with taking the oath; they chose to do so quickly. Some even pointed out that responding truthfully during the experiment was natural and entirely normal – as shown above, however, telling the truth is not obvious at all in neither IV-Baseline nor IV-Incentives treatments. The acceptance rate is 94.5% since only one subject out of eighteen refused to take the oath. Self-selection is not an issue.[13]

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,

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

we do not observe over-bidding for the lowest induced value: the ratio of revealed aggregate demand over induced aggregate demand is 110%. In addition, 16.0% of bids equal the induced value, and 70.4% are within a 10 percent interval centered on 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. We first consider off-the-margin bidders behavior by testing the equality to one of bid to induced value ratio. A bootstrap test cannot reject the null of perfect-demand revealing behavior for the lowest induced value: $p = 0.340$. Based on the Tobit regression provided in Table 4, we examine individual bidding bevahior all along the demand curve by applying 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.[14] ∎

To sum up, the oath procedure significantly improves the power of the Vickrey mechanism to reveal true values for the good. The device notably manages to discipline the bidding behavior of off-the margin bidders. 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 treatment: 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

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

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 (HG-Baseline), bids with binding monetary incentives (HG-Incentives), and an oath prior to hypothetical bidding (HG-Oath).

4.1 Design of the HG-experiment

The good. We focus on preferences for an environmental good, namely the protection of dolphins. 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, 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., an adoption certificate and photograph are sent for each 25 USD (18.50 Euros when the experiments took place) donation to the WWF. 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 upper bound of 30 euros is the same for all subjects and does not depend on experimental earnings. As a result, subjects can bid above their experimental earnings and instructions clearly state that any bid above experimental earnings would have to be completed by out-of-pocket money. Neither do we impose a lower bound or reservation

[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/`.

[16] The original page in English is available at `https://secure.worldwildlife.org/ogc/ogcAC_speciesDetail.cfm?gid=8`.

price in the provision rule – minimum bid is zero. The good sold in the experiment is potentially cheaper in the lab than in the market, so we subsidize the winning donation to reach the market price when monetary incentives are binding. Subjects are not told anything about this subsidy. This is enough to protect our data from the censoring issue raised by, e.g, Harrison, Harstad, and Rutström (2004), if all actual offers are below the market price (which is far from being the case in the experiment, see Section 4.3 below). In theory, this remains valid for every observed offer as long as subjects are not aware of the donation procedure and the market price of the donation outside the laboratory. To confirm that the observed values are independent of field opportunities, some items assessing subjects' knowledge about the procedure 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 auction: each bidder privately posts a bid, the highest bid determines the winner of the auction, and the market price is the second highest bid. We reduced the noise in elicited preferences by repeating five times the auction. At the end of the sequence, one auction round out of the five is randomly drawn. The winner of the randomly drawn auction is the subject entitled to adopt a dolphin, and the amount of donation is equal to the market price for this round. For comparison purpose, we also divide each 18-subject session into smaller groups of 9 bidders: 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, RutstrÃ¶m, 1998; Cherry, Frykblom, and Shogren, 2002). In the specific context of demand revelation using Vickrey auctions, Jacquemet, Joule, Luchini, and Shogren (2009a) 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 20 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. Accompanying each question is a list of four possible answers. 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. As in the IV experiment, we rely on three elicitation contexts: HG-Baseline, HG-Incentives and HG-Oath. The first two treatments only differ regarding 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-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 the HG-Baseline treatment: before entering the lab, each subject is asked to sign a "solemn oath" to tell the truth. 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 procedures

Three 18-subjects sessions, one per treatment, were conducted in the LEEP laboratory in Paris. Since each subject posts 5 bids for adopting a dolphin, this provides 90 bid observations for each treatment. 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, following the same procedures as those implemented in the IV-Oath treatment. 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. 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 and participants are encouraged to ask clarifying questions, privately answered by the monitor. All treatments start with the quiz (questions along with the four possible answers are displayed one after the other). 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 he same as in the IV experiment, 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 once 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. This description of the good is the main difference between the instructions used in the HG auction and those we used for the IV auction. The wording of the auction, in particular, is identical, calling the bids "prices" and the amount of the donation "market price". Subjects are grouped into two 9-bidder auctions. We also use the same non-numerical example and same questionnaire to check subjects' understanding at the end of the instructions. Treatments are implemented by slightly modifying the wording of the instructions between HG-Incentives and HG-Baseline. 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

Aggregate bidding behavior in HG-Baseline and HG-Incentives is presented in Table 5. For each treatment, we computed average and median bids as well as the number of bids above experimental earnings and the number of zero bids. Aggregate data exhibit a clear discrepancy between bidding behavior in HG-Baseline and HG-Incentives, i.e. a large hypothetical

Table 5: Homegrown bidding behavior in real and hypothetical treatments

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
HG-Baseline	Mean bid (€)	15.36	18.19	17.33	17.38	18.90	17.43
	Median bid(€)	16.25	20.50	19.75	19.75	20.75	19.50
	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	7	9	8	9	10	43 (47.7%)
HG-Incentives	Mean bid (€)	3.30	2.97	3.17	3.17	2.30	2.98
	Median bid (€)	1.25	1.00	1.25	1.00	0.75	1.00
	♯ zero bids	3	5	5	5	6	24 (26.7%)
	♯ bids > gains	0	0	0	0	0	0 (0.0%)
Average hyp.-real gap		465.5%	612.5%	546.7%	548.3%	821.7%	584.9%

Note. For each treatment and by round (in *column*), the table summarizes bidding behavior in the HG-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 HG-Baseline bids and average HG-Incentives bids.

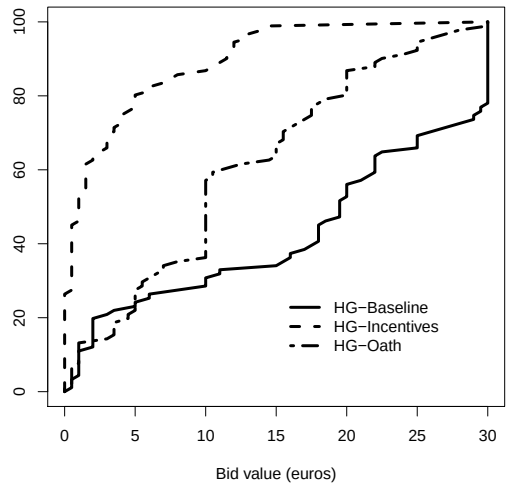
bias.[19] Mean and median bids in baseline are €17.43 and €19.5 as compared to €2.98 and €1 in monetary-only. This leads to an average baseline-incentives ratio of 584.9%. A closer look at individual behavior allows to identify two driving forces of this difference: First, at the low end of bids no bidder offers zero in the hypothetical treatment while we observe nearly 27% of bidders bidding zero in the monetary-only treatment. At 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 bids are outside the budget constraint in the HG-Baseline treatment. As a result, two distinct variations in behavior drive the result: the opting-out of the auction through zero bids when incentives are binding, and the unreliable willingness to pay out of ones' own pocket when incentives are dropped. Together, these two differences produce a significant gap between preferences elicited with or without monetary incentives.

We now turn to the way the oath mitigates this gap. In HG-Oath, all subjects but one took an oath prior to participating in the auction.[20] Figure 2 provides the empirical distribution of bids from those subjects that participated in the oath treatment, along with 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

[19] See Jacquemet, Joule, Luchini, and Shogren (2009b) for a detailed discussion on hypothetical bias in IV and HG auctions.

[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).

Figure 2: Distribution 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.

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

Result 3 *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) higher bids than those elicited using incentives and (ii) less violations of the budget constraint and lower bids than those observed in the baseline.*

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 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

Table 6: Homegrown bidding behavior in the oath treatment

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
HG-Oath	Mean bid (€)	9.42	10.72	11.28	11.83	14.02	11.46
	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 gap		285.5%	360.9%	356.2%	373.2%	617.4%	384.9%
Average oath-hyp. gap		61.3%	55.9%	65.1%	68.1%	74.1%	65.7%

Note. For each round (in *column*), the table summarizes bidding behavior in the HG-Oath treatment: mean and median bid (*first two rows*); 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 HG-Baseline bids; second, the average HG-Incentives bids.

bootstrap p-value (see Davidson and MacKinnon, 2006). The decrease in mean behavior as compared to the behavior we observe in the monetary treatment is significant at $p < 0.001$ according to this bootstrap test.

We statistically test the decrease in the number of bids above the experimental budget constraint induced by the oath as compared to the behavior in HG-Baseline by a bootstrap proportion test that allows for potential correlation over market periods. The test considers a dichotomous random variable which equals one if the bid is above experimental earnings and zero otherwise. It is based on the same procedure used for the two-sample bootstrap mean test presented above, i.e. bootstrapping subjects in their behavior in the five market periods in the sample (999 times) and take account of asymmetry in the empirical distribution. The decrease in bids outside the budget constraint is significant with $p < 0.016$ according to this procedure. In addition, the two-sample mean difference bootstrap test presented above shows that bids elicited in HG-Oath are in average significantly lower than those elicited in HG-Baseline ($p = 0.026$). 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 Result 3 using a random effects panel Tobit model pooling together data from HG-Baseline, HG-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

Table 7: HG bidding behavior – Panel Tobit estimations

Variable	Parameter estimate	P-value
Treatment dummies		
Constant term	16.27	0.114
Incentives	-17.67	0.000
Oath	-8.34	0.001
Earnings (€)	-0.10	0.853
Round dummies	YES	
Individual's characteristics		
Age	-0.15	0.496
Male	-1.79	0.413
Participated to other experiments	-2.83	0.186
Experience with auctions websites	1.35	0.115
Knows WWF	-0.20	0.951
Agrees with WWF actions	3.48	0.001
Knows WWF's dolphin adoption program	1.33	0.737
σ_u (sd.)	6.96 (0.755)	
σ_e (sd.)	3.95 (0.212)	
Log likelihood	-720.41	

Note. Individual random effects Tobit models (random effects are assumed Gaussian), $n = 54$ and $T = 5$. The *endogenous* variable is the bid posted. *Incentives* and *Oath* are dummy variables. *Round* (fixed) effects are controlled in the estimation but omitted – Wald test of joint nullity is 76.99 with $p < 0.001$.

on the adoption screen when subjects bid their valuation (see Section 4.1). Dummy variables are introduced to control for the HG-Incentives and HG-Oath treatments (HG-Baseline being the referent) as well as total earnings and individual's characteristics.[21] Results are presented in Table 7. As compared to behavior in HG-Baseline, monetary incentives drastically decrease revelation: associated parameter is -16.7 with $p < 0.001$. The oath also has a significant impact on bidding behavior as compared to bidding behavior in HG-Baseline: associated parameter is -7.41 with $p = 0.002$. This means that, on average, the decrease in bids is twice less in HG-Oath than in HG-Incentives as compared to HG-Baseline when accounting for observed heterogeneity of subjects. This difference is significant with $p < 0.001$.

■

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

[21] 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.

treatments, in which hypothetical bias exists due to violations in both budget and participation constraints, the oath seems to bind bidders to avoid both: people are less likely to overstate high bids and less likely to understate low bids. We take this as evidence that the oath strengthen the willingness of our subjects to reveal their true preferences for the goods sold in the auctions. In the next three sections, we report the results from complementary treatments that help understand what the oath actually changes on bidders' behavior, and the reasons why it happens.

5 Robustness treatments: Commitment of trained bidders

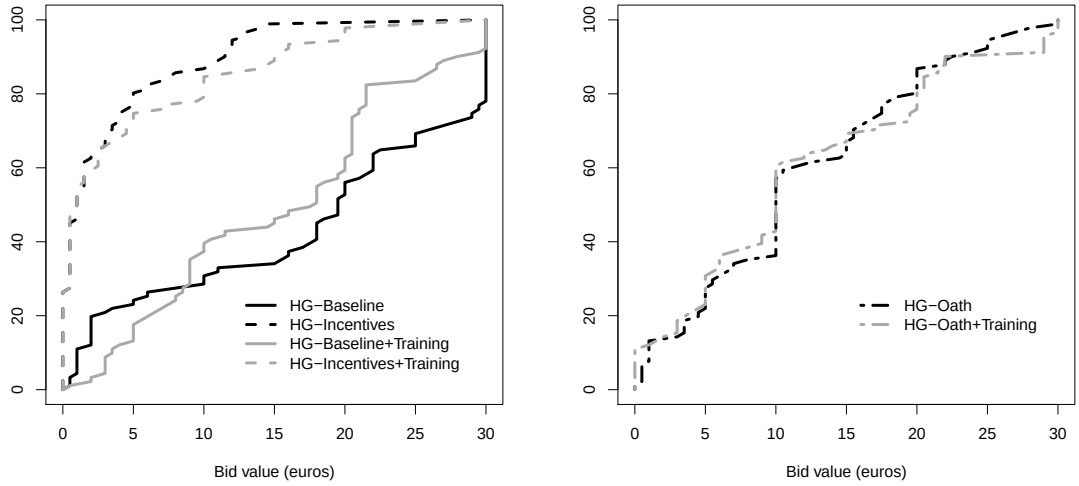
Ideally, preference elicitation should be more accurate if bidders have no misconceptions about the operations and procedures of the auction mechanism (see for example Lusk and Shogren, 2007; Shogren, Shin, Hayes, and Kliebenstein, 1994; Plott and Zeiler, 2005). While straightforward in theory, the second price auction used in our treatments is likely to be unfamiliar to many bidders. In particular, they might not immediately realize that bidding their true preferences is the weakly dominant strategy. The oath, by providing subjects with some information about what they should do in the experiment, i.e telling the truth,[22] might only have an effect on subjects unfamiliar with the mechanism and no effect when subjects are more familiar with the second price auction. We assess the truthfulness of this assumption by introducing a preliminary stage in which subjects learn how to behave in a second price auction. By training via practice rounds, bidders can learn the potential consequences of under- and over-bidding one's preferences for the good.

5.1 Design of the HG-Training treatments

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). To avoid confounding wealth effects, we chose to run the training auctions under a hypothetical setting – bids are expressed in experimental currency, without conversion into Euros whatever the homegrown treatment that follows. The introduction of an IV-Baseline treatment between the quiz and the homegrown auctions is the only difference between training experiments and those

[22]Recall however that subjects do not know in what the experiment will consist when they take the oath.

Figure 3: Distribution of bids from trained bidders



(a) Baseline and Monetary incentives (b) Oath

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

conducted in the previous section. The written instructions are the very same as those used for the IV-Experiment (Section 3.2) and the HG-Experiment (Section 4.2). The only differences between the two sets of auctions is 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 training phase is added to the three treatments presented in the previous section and three new treatments are implemented: HG-Baseline+Training, HG-Incentives+Training and HG-Oath+Training. Two sessions, one per treatment, were run at the LEEP.

5.2 Results

We look first at the effect of training in HG-Baseline+Training and HG-Incentives+Training. EDFs of bids are provided in Figure 3a, along with those associated with bidding behavior of untrained bidders observed in HG-Baseline and HG-Incentives. 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. Comparing Table 8 to Table 5 confirm that training marginally affects bidding behavior. In the baseline, the number of bids above experimental earnings is lower with training than without training (21.1% of bids compared to 47.7%, p=0.056). This in particular leads

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-Incentives	Median bid (€)	0.75	0.75	1.25	0.75	0.75	1.00
+Training	♯ zero bids	5	4	5	5	5	24 (26.7%)
	♯ bids > gains	0	0	0	0	0	0 (0.0%)
Average hyp.-real gap		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 (€)	10.00	10.00	10.00	10.00	10.00	10.00
+Training	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	3	3	3	3	4	16 (17.8%)
Average oath-real gap		326.1%	215.9%	254.5%	287.8%	291.8%	272.1%
Average oath-hyp. gap		74.3%	71.9%	78.1%	71.8%	78.1%	75.2%

Note. For each Treatment and round (in *column*), the table summarizes bidding behavior in the HG-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 HG-Baseline+Training and the average bids in HGH-Incentives+Training. The last two rows of the lower part give the ratios between the average bids after in HG-Oath+Training and: first, the average HG-Baseline+Training bids; second the average HG-Incentives+Training bids.

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 untrained ones (mean is €2.98).

We now turn to the effect of training combined with an oath. 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 3b. 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: the mean bid for all rounds is €11.51 as compared to €11.46 in the oath treatment without training (see Table 6). The difference in the average values we elicit is due to much less violations of the participation constraint as compared to the one observed in HG-

[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).

Incentives+Training (from 27% to zero) and a few less violations of the budget constraint as compared to the one we observe in HG-Baseline+Training (from 21% to 18%, $p = 0.370$).

Result 4 *The effect of the oath on trained bidders is observationally the same as its effect on untrained bidders.*

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 is $p = 0.494$ for HG-Baseline against HG-Baseline+Training; $p = 0.476$ for HG-Incentives against HG-Incentives+Training and $p = 0.980$ for HG-Oath against HG-Oath+Training. ∎

In summary, Result 3 still holds when training bidders with non-binding IV auctions before bidding in the HG auctions. Even when subjects are familiar with the mechanism, oath induces a decrease in average bidding behavior as compared to baseline and an increase in bids as compared to when monetary incentives are at stake.[24] We now turn to experimental investigations about what exactly the oath changes in the way subjects decide on their bid, and the reasons why it happens.

6 Does the oath reduce to a demand effect?

An alternative view of our results is that the oath works due to a demand effect – in the sense that subjects just comply with the implicit request to provide their true value contained in the oath – rather than a commitment of subjects to tell the truth. In this section, we disentangle between the two based on new experiments aimed at contrasting the preference elicited between having people taking an oath versus an exhortation on the importance of accurate reporting. Such exhortation procedure is commonly known in the preference elicitation literature under the name of "Cheap Talk Scripts" (see the initial paper by Cummings and Taylor, 1999). Cheap talk scripts tell subjects that there is a difference between what people say and do. The existing evidence is rather mixed. Telling bidders that they promise more than they deliver induces a decline in their bids, unless this is not said with enough force (see List and Gallet, 2001). In contrast, more neutral cheap talk scripts that tell bidders their promises differ than actions induces an increase in hypothetical bids, which exacerbates the bias (see Aadland and Caplan, 2003, 2006), exacerbating the bias. As a conservative test of our results, we focus on the first kind of script, i.e we give subjects extensive information on the size and the sign of the bias and exhort them to avoid it.

[24] The p-value of a one-sided mean difference bootstrap test of a decrease in HG-Oath+Training is $p = 0.036$ against HG-Baseline and $p = 0.089$ against HG-Baseline+Training; the p-value of a one-sided mean difference bootstrap test of an increase in HG-Oath+Training is $p < 0.001$ against HG-Incentives and $p = 0.004$ against HG-Incentives+Training.

6.1 Design of the Cheap-Talk experiment

We introduce explicit exhortations about the importance of telling the truth as a new treatment variable. Before the HG auctions take place, the instructions conclude with a text highlighting the hypothetical bias generally observed in preference elicitation literature. Our script is an adaptation to the case of a second price auction of the cheap talk script proposed by Cummings and Taylor (1999):

> "In a recent study, several different groups of people were involved in an auction just like the one you are about to be in. The earnings in Euros were independent from the decisions made during the auction, just as it will be for you. No one had to pay money in case of adoption. With another set of groups with similar people, the earnings in Euros from the experiment did depend on the decision made during the auction. The auction was the very same as the one you're involved in, the only difference being that earnings were deduced from the result of the auction, so the winner of the auction actually had to pay the second highest bid to the WWF for actually adopting a dolphin. What we observed based on those two groups is the donation offered in the auction is in average more than 5 times higher when earnings are independent from decisions made, as compared to when earnings do depend on decisions.
>
> We call this a "hypothetical bias". Hypothetical bias is the difference that we continually see in the way people propose prices in hypothetical auctions, in which earnings are independent from decisions, as compared to real auctions, in which earnings do depend on decisions.
>
> Now can we get people to think about their decision in a hypothetical auction like they think in a real auction, where if they win the auction they'll really have to pay money? How do we get them to think about what it means to really dig into their pocket and pay money, if in fact they really aren't going to have to do it. Let me tell you why I think that we continually see this hypothetical bias, why people behave differently in a hypothetical auction than they do when the auction is real.

I think that when we take decisions in an auction that involves doing something that is basically good – helping people in need, improving environmental quality, or anything else – we care too much about the resulting situation rather the actual payoff this induces. In a hypothetical auction like the one you're involved in, the basic reaction is to think: "sure, I would do this". I really want to offer an high donation and spend money on adopting a dolphin. But when the auction is real, and we would actually have to spend our money if we win the auction, we think a different way. We basically still would like to see good things happen, but when we are faced with the possibility of having to spend money we think about our options: if I spend money on this, that's money I don't have to spend on other things. So we offer a donation that takes into account the limited amount of money we have, accounting for the earnings in Euros that are realized by our decisions. This is just my opinion, of course, but it's what I think may be going on in hypothetical auctions. So if I were in your shoes I would ask myself: if this were a real auction, and I had to pay the second highest bid to the WWF: what is the actual donation I want to offer? Let me insist on what maybe going on in this hypothetical setting: you may mistakenly state a higher value than the one you would really be prepared to pay in a real setting. This may even happen if you try to overcome the hypothetical bias issue, simply because your mind setting is framed by this hypothetical scenario. This means you may still be influenced by your desire to help the WWF independently of any gains or losses. Please try to overcome this tendency, and take your decision just exactly as you would if you were really going to face the consequences of your decision: which is to spend money on the donation if you win the auction. Please keep this in mind in our auction."

The cheap talk exhortation is the only change in the experimental instructions and procedures. Once all instructions, including the cheap talk script, have been read aloud, questions are privately answered and the experiment starts. Two sessions (involving 18 subjects each) were run in the LEEP: the baseline homegrown auction described in Section 4.1 coupled with Cheap talk (HG-CheapTalk) and the same treatment but where subjects are first asked to take an oath (HG-Oath+CheapTalk).

6.2 Results

Table 9 provides aggregate data on bidding behavior in the cheap talk treatments. It appears cheap talk helps subjects to take more seriously experimental budget constraint. But we also see that cheap talk works best only after an oath is signed. The budget constraint with oath and cheap talk is almost the same as in HG-Incentives – the number of bids inducing out-of-pocket payments falls from 43 in the baseline to 1 when oath and cheap talk script are

Table 9: Homegrown bidding behavior after oath and/or cheap talk

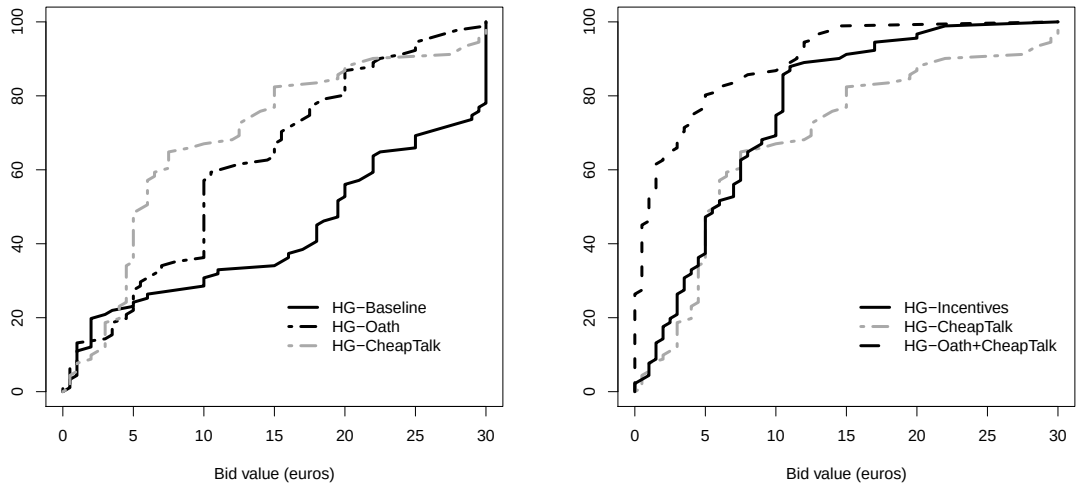
		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
HG-CheapTalk	Mean bid (€)	9.13	8.08	9.50	8.97	11.02	9.34
	Median bid (€)	5.50	5.00	6.00	5.50	6.75	5.75
	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	3	2	3	2	3	13 (14.4 %)
Average CheapTalk-real gap		276.7%	272.1 479.1%	313.4%			
Average CheapTalk-hyp. gap		59.4%	44.4%	54.8%	51.6%	58.3%	53.6%
HG-Oath +CheapTalk	Mean bid (€)	6.31	5.75	7.50	7.33	8.08	6.99
	Median bid (€)	5.00	5.00	6.25	6.25	7.75	5.75
	♯ zero bids	0	1	0	1	0	2 (0.02%)
	♯ bids > gains	0	0	1	0	0	1 (0.01%)
Average CheapTalk-real gap		191.2%	193.6%	236.6%	231.2%	351.3%	234.6%
Average CheapTalk-hyp. gap		41.1%	31.6%	43.3%	42.2%	42.8%	40.1%

Note. For each Treatment and round (in *column*), the table summarizes bidding behavior in the HG-Experiment involving a cheap talk script: 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 upper part provide the ratio between the average bids in HG-Baseline+CheapTalk and: first, the average HG-Incentives+CheapTalk bids; second the average HG-Baseline bids. The last two rows of the lower part give the ratios
between the average bids HG-Oath+CheapTalk and: first, the average HG-Incentives+CheapTalk bids; second, the average HG-Baseline+CheapTalk bids.

combined. To this regard, both cheap talk and oath alone perform equally well, resulting in an intermediary number of such bids equal to 13 and 17.

Figure 4 provides a more detailed description of behavior through the empirical distribution function of bids posted in each treatment. We derive three observations from the 2x2 comparison of the demand functions elicited according to whether a cheap talk script informs subjects before the auction, and whether subjects sign an oath. First, when used alone, the oath disciplines the upper end of the bid distribution observed in HG-Baseline – having subjects sign an oath leaves almost unchanged the lower end (bids less than 10 Euros) of the bid distribution as compared to HG-Baseline, but sharply moves upwards the upper end of the distribution. So, without any indication of what the expected behavior is, an important fraction of the subjects change their bidding strategy as the result of signing an oath to tell the truth. Second, when used alone, the cheap talk script disciplined some of the lowest bids observed in HG-Baseline – the EDF of bids in HG-CheapTalk dominates the one observed in HG-Baseline, but is similar to the EDF of bids in HG-Oath above 10 Euros. When explicitly told the behavior we expect from them, some subjects do change their bidding strategy to

Figure 4: Distribution of bids in Cheap Talk treatments



(a) Marginal performance (b) Performance of the mix

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

comply with our request to report accurate values. Last, combining the oath with the cheap talk script improves bidding behavior as compared to all other settings – the EDF of bids in HG-Oath+CheapTalk first order dominates all other distributions. Oath combined with cheap talk improves elicitation along (i) the lower end of the demand function as compared to oath alone, (ii) the upper end of the demand function as compared to cheap talk alone (iii) the whole demand function as compared to the Baseline. Overall, the inflexion in the bid distribution and the implementation of the experimental budget constraint result in a drop in the average bid from €17.43 in HG-Baseline to €6.99 in HG-Oath+CheapTalk. We also observe that all bids but one are below experimental earnings in HG-Oath+CheapTalk.

Result 5 *Cheap talk overcomes a bidder's lack of experience that makes some people think they are telling the truth when in actuality they are not. But it is the Oath, not cheap talk warnings, that increases the odds of truth-telling.*

Support. Based on the mean difference and proportion bootstrap tests described above, we compare behavior in HG-Oath+CheapTalk with bidding behavior in other treatments. First, mean bid is significantly lower in HG-Oath+CheapTalk as compared to HG-Oath ($p = 0.022$) but not significantly different from HG-CheapTalk ($p = 0.244$) – although mean bidding behavior in HG-CheapTalk in not different from mean bidding behavior in HG-Oath ($p = 0.369$). Second, when subjects take an oath without any exhortation on the importance of accurate reporting (HG-Oath), we observe a significant decrease of the

number of bids above experimental earnings as compared to HG-Baseline ($p = 0.016$) but still significantly different from zero as in HG-Incentives ($p = 0.023$). This is also true in HG-CheapTalk: number of bids is significantly lower than in HG-Baseline ($p = 0.008$). The number of violations is however still significantly different from zero ($p = 0.061$). When combined with a cheap talk script, the oath reduces the number of bids above experimental earnings as compared to HG-Baseline ($p < 0.0001$), oath alone ($p = 0.026$) and cheap talk only ($p = 0.081$). What is new is that we cannot now reject the null of no violation of the experimental budget constraint, i.e. number of bids above experimental earnings not different from zero ($p = 0.119$). ∎

Both the oath and the cheap talk procedure focus a person's attention on the task at hand. Unlike the oath, cheap talk is informative, providing information on how other people behave, and does not rely on any kind of commitment from subjects. In contrast, the oath induces each person to comply with their signed statement – which is to tell the truth. Those differences in the revelation devices are in line with observed behavior: while cheap talk helps subjects to better identify what their preferences are, the oath seems to induce more of them to truthfully reveal it.

7 Why the oath works: an experimental appraisal of behavioral motives

Both the testbed and application treatments support a rather strong effect of the oath on bidding behavior towards more truth telling behavior. In light of the treatments described in the previous section, this effect appears to stem from an increase in subjects' willing to reveal their true preferences for the good. This section provides a discussion of behavioral models that can rationalize such an effect: *(i)* cost of lying, which translates in economics as an intrinsic preference for keeping one's word while it is rationalized in psychology by a self-attribution process, and *(ii)* guilt aversion. We complement the discussion herein by implementing companion treatments that help disentangle between these two candidates.

7.1 Insights from psychology and behavioral economics

In social psychology, commitment is usually seen as the result of a self-attribution process: people infer their attitudes from their own behavior (Bem, 1972; Kiesler, Nisbett, and Zanna,

1969).[25] This means that people deduce from past 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 an action which shares some similarities with the initial behavior. Following this line of reasoning, the oath acts as a commitment device in our experiments 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.

This interpretation from social psychology is closely related to the debate recently opened in economic literature on pre-play communication in experimental games. Several works have 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. This means in our setting that the oath can impose a psychological cost on lying if bidders do not bid their induced value after having signed the oath. This in turn fosters truth-telling behavior.

An alternative explanation is based on guilt aversion, which is experienced by a player who fails 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.

Why people behave coherently with the words they give to others is however still an open question in the recent literature. Vanberg (2008) reports results from an experiment that compares 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" – explanation. Although this results are confirmed by recent evidence

[25]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.

(Ellingsen, Johannesson, Tjøtta, and Torsvik, 2009), they are still challenged on experimental grounds (Charness and Dufwenberg, 2008). It is well beyond the purpose of this paper to make any general claim about which one is true. In the next two sections, we rather rely on behaviorally testable restrictions induced by each assumption to try disentangle between the two candidates in the case of truth telling under oath: while the commitment-based explanations (either derived from self-attribution theory or a preference for consistency) posit that subjects tell the truth for internal reasons – concern for others playing no role – 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.

7.2 Oath and intrinsic motivation to tell the truth in the IV-Experiment

One common feature of the commitment-based explanations is 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 is also common in foot-in-the-door experiments (Burger, 1999). 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. By contrast, guilt aversion would imply that the oath succeeds because subjects want to comply with what the experimenter expects from them. In line with this interpretation, adding an oath should improve revelation further when added to incentives.

Our first companion treatment mixes the oath and monetary incentives in our testbed treatment. 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. This IV-Oath+Incentives treatment combines the IV-Oath procedures with the binding auction bidding in the IV-Incentives treatment (both described in Section 3.1). One experimental session was conducted at LEEP. The session involves 18 subjects, providing two independent 9-times repeated Vickrey auctions. Table 10 presents bidding behavior by round as well as by induced value. Aggregate data suggest that the

Table 10: Induced value bidding behavior with both oath and incentives

By round

Round	1	2	3	4	5	6	7	8	9	Total
Aggregate Demand	1084	1084	1084	1084	1084	1084	1084	1084	1084	9756
Revealed demand	991	1072	1134	1097	1137	1189	1236	1258	1242	10356
% revelation	91.4	98.9	104.6	101.2	104.9	109.7	114.0	116.0	114.6	106.2

By induced value

Induced Value	24	38	53	63	65	68	71	76	84	Total
Aggregate Demand	432	684	954	1134	1170	1224	1278	1368	1512	
Revealed demand	710	807	1045	1185	1218	1256	1234	1381	1520	10356
% revelation	164.4	118.0	109.5	104.5	104.1	102.6	96.6	101.0	100.5	106.2

Note. Observed bidding behavior in the IV-Oath+incentives treatment. The upper part organizes data by round – aggregate demand remains constant, as displayed in the second row of the sub-table. The lower part reorganizes the same data by induced value – induced aggregate demand displayed in the second row of the sub-table. In both part, the lower figure gives the ratio of the demand elicited in the auction to the aggregate induced demand, in %.

oath combined with monetary incentives performs poorly, in particular for off-the-margin bidders.

Result 6 *Combining the oath with monetary incentives leads to less sincere bidding compared to bidding with oath-only, monetary-only, and the baseline treatments.*

Support. Bootstrap tests of equality to one of the bid to induced value ratio reject the null of perfect revealing bids for the lowest induced value (24 ECU, $p = 0.003$) but also for the second lowest induced value (38 ECU, $p = 0.047$) and the third (53 ECU, $p = 0.021$). The results are confirmed by the econometric analysis. We apply the Tobit regression model described in Section 3.3 to this treatment. Perfect revelation is strongly rejected for the IV-Oath+Incentives treatment:

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

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

$$\begin{array}{lcrcr}
\text{IV-Baseline} & : & \text{bid} = 0.788 \text{ x Ind. Value} & + & 16.5 \\
\text{IV-Incentives} & : & \text{bid} = 0.855 \text{ x Ind. Value} & + & 13.9 \\
\text{IV-Oath} & : & \text{bid} = 0.969 \text{ x Ind. Value} & + & 3.7 \\
\text{IV-Oath+Incentives} & : & \text{bid} = 0.756 \text{ x Ind. Value} & + & 19.1
\end{array}$$

The IV-Oath+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+Incentives treatment is significantly different from IV-Baseline ($p = 0.05$), IV-Incentives ($p = 0.02$) and IV-Oath ($p < 0.01$). ∎

Crowding-out seems to apply as well to our procedure: IV-Oath+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+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.[26]

To sum up, guilt aversion would imply that behavior under IV-Oath+Incentives would be closer to perfect revelation than IV-Incentives. By contrast, commitment-based explanations can account for Result 6, based on the crowding-out of the intrinsic motivation to tell the truth.

7.3 Guilt enhancing oath in the HG experiment

A second important difference between guilt aversion and commitment-based explanations is that the guilt induces a positive correlation between the cost imposed on others by the lie, and the incentives to tell the truth. To investigate further why the oath works, we enhance the feeling of guilt associated with violating the oath signed in the HG experiment. To that end, we design a new oath procedure that clearly points out what the consequences of not telling the truth would be for the research at stake: results would be biased if dishonest answers were given (because this is "what we observed in previous experimental studies").

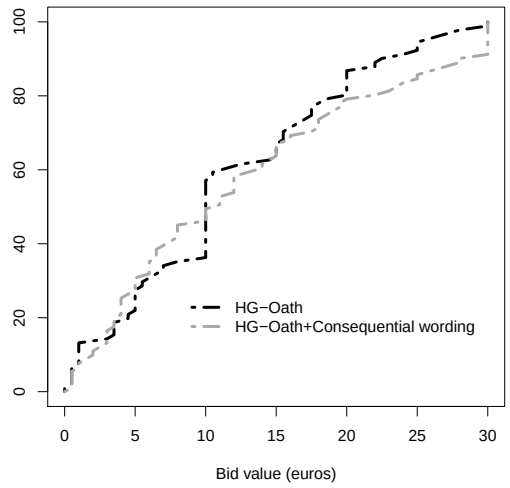
7.3.1 Design of the treatment

In practice, this experiment is identical to HG-Oath except that *consequential wording* is added to the oath procedure. Consequential wording[27] consists in additional sentences to

[26]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).

[27]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

Figure 5: Distribution of bids after a consequentially worded oath



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

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 to avoid such undesirable results; we still stress that signing the oath is neither mandatory for participating in the experiment nor for receiving experimental earnings. 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. This treatment is called HG-Oath+Consequential-wording.

7.3.2 Results

First, 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, HG-Oath+Training and HG-Oath+Cheaptalk[28], leads to an acceptance rate of 97.2% overall – subjects in all four 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

consequential indicates that if the subject does not give honest answers, "our" results will be biased.

[28] All subjects agreed to take an oath in HG-Oath+Cheaptalk.

Table 11: 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	13.50	12.63	13.00	12.33
HG-Oath	Median bid (€)	6.50	10.50	12.25	12.50	12.00	10.50
+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-hyp. 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 HG-oath+Consequential-wording treatment: mean and median bid (*first two rows*); 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 in this treatment and: first, the average HG-Baseline bids; second, the average HG-Incentives bids.

wording does not add any further effect to the standard oath. A comparison of mean bidding behavior confirms this finding. Table 11 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 7 *Enhancing the feeling of guilt induced by untruthful bidding behavior after an oath is neutral on the oath performance.*

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 $p = 0.731$. ∎

From the theory of commitment perspective, an oath is an extreme type of commitment that binds the subject to tell the truth. Result 7 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. In line with the theory of commitment, this 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.

Table 12: HG bidding behavior – Panel Tobit estimations

Variable	Parameter estimate	p-value
Treatment dummies		
Constant term	9.87	0.126
Incentives	-17.19	0.000
Oath	-7.11	0.007
Training	-2.47	0.339
Training × Oath	2.16	0.561
Training × Incentives	5.16	0.171
Consequential wording	1.43	0.590
Cheap Talk	-7.99	0.002
Cheap Talk × Oath	4.80	0.205
Round dummies	YES	
Individual's characteristics		
Earnings	0.06	0.854
Age	-0.03	0.741
Male	-1.49	0.250
Post-experiment questions		
Participated to other experiments	-.279	0.843
Experience with auctions websites	1.22	0.028
Knows WWF	-1.26	.529
Agrees with WWF actions	1.61	0.002
Knows WWF's dolphin adoption programme	-.87	.705
σ_u (sd.)	7.37 (.51)	
σ_e (sd.)	3.29 (.11)	
Log likelihood	-2185.96	

Note. Individual random effects Tobit models (random effects are assumed Gaussian), $n = 162$ 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 116.58 with $p < 0.001$.

8 Summary of the results

We finally pool data from all nine HG treatments in a random effects panel Tobit model. Dummy variables are introduced to control for the HG-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 (see Table 7). A dummy variable is added to control for trained bidders as well as two dummy variables that control for interaction terms between training and oath and training and monetary incentives. 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). Last, we add two dummy variables that account for the effect of cheap talk (variable equals one when cheap talk has been used, zero otherwise) and for the interaction between cheap talk and oath (variable equals one when oath and cheap talk have been used, zero otherwise).

The results presented in Table 12 confirm the conclusion derived from unconditional statistics run on aggregate bidding behavior. First, the parameter associated with monetary incentives is negative and significant, indicating that subjects bid on average €17.2 less when monetary incentives are binding than in baseline ($p < 0.001$)). Second, the dummy variable accounting for oath is also highly significant and negative: subject bid on average €7.1 less when they are committed by taking an oath as compared to baseline ($p = 0.007$). Third, the parameter associated with trained bidders is not significant ($p = 0.339$) and this is also true for oath and monetary incentives interaction terms ($p = 0.561$ and $p = 0.171$). Fourth, adding consequential wording to the oath procedure has no additional effects on bidding behavior ($p = 0.590$). Last, the parameter associated with the use of cheap-talk is negative and significant ($p = 0.002$) while the parameter associated with the interaction effect of oath and cheap talk is not significant ($p = 0.205$). This indicates that the oath combined with cheap talk leads to greater reductions in hypothetical bidding behavior.

From the cheap talk and training treatments, we can deduce that oath not only improves the ability of subjects to tell the the truth, but affects their willingness to do so. From both *(i)* the simultaneous use of incentives and the oath in IV and *(ii)* the addition of consequential wording in HG, we get some convergent pieces of evidence that this switch in bidding strategy comes from a commitment of subjects to tell the truth, rather than a concern to comply with other's expectation.

9 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 – can be 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 notion that having subjects signing an oath on one's honor to "tell the truth and provide honest answers" before bidding induce bidders to take their budget constraint and participation constraints seriously. These results are robust to additional training with the auction mechanism.

Several companion treatments further explore the reasons why the oath works. First, we assess whether subjects provide more truthful answers as the result of a demand effect by contrasting our results with the demand elicited after an explicit exhortation to provide truthful answers, also known in the valuation literature as cheap talk scripts. Although we confirm that cheap talk helps subjects to better identify what their true preferences are, we observe that only the oath increases the odds they bid in accordance with it. We take this as support for interpreting the oath as enhancing commitment to tell the truth from our subjects. Second, we provide two treatments that help position the oath in the promise-keeping literature. Both the crowding-out of the oath performance when combined with monetary incentives (in the IV setting) and the neutrality of reinforcing the consequences of lye (in the HG setting) favor the commitment-based explanation against an aversion to guilt feelings.

Our findings hold promise, opening the way for better preference elicitation of non-market goods, like environmental protection. The obvious avenue for further work is to assess whether our results are generalizable to alternative elicitation mechanisms, such as provision points. More generally, it is also of interest to explore how commitment could be able to complement incentives in games, such as coordination or cooperation, in which monetary stakes alone fail to reach efficiency.

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