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Social psychology and environmental economics: a new look at *ex ante* corrections of biased preference evaluation[1]

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Abstract

Environmental economics is now a long standing field of research; much has been learned on how environmental policy can use incentives to drive individual behaviors. Among the many examples, preference elicitation is the most discussed case in which incentives fail to accurately implement efficient behavior. Using this as our motivating example, herein we explore the cross-fertilization between environmental economics and social psychology . We first review how the lessons drawn from social psychology helped address the hypothetical bias issue. We then turn to the future of this process by focusing on how cheap talk scripts influence preference elicitation. Our experimental results shows CT scripts work through persuasion – *i.e.* changes mind, but poorly changes actions. In that sense, preference elicitation still lacks a way of making communication binding – *i.e.* a way to alter intrinsic motivation of subjects to behave truthfully.

Keywords: Social psychology, Commitment, Persuasive communication, Preference elicitation

JEL Classification: C9; H4; Q5

Résumé

L'économie de l'environnement a depuis longtemps permis de mettre en évidence les moyens par lesquels les politiques publiques menées dans ce domaine peuvent s'appuyer sur les incitations pour orienter les comportements. Les méthodes de révélation des préférences restent cependant l'un des exemples les plus étudiés de défaillance des incitations. En s'appuyant sur cette illustration, cet article propose une synthèse des progrès réalisés en économie de l'environnement grâce à l'intégration des résultats issus de la psychologie sociale. La première partie de l'article propose une revue des méthodes issues de cette discipline et destinées à corriger le biais hypothétique. La seconde partie s'appuie sur une expérience en laboratoire pour évaluer la capacité des scripts de *cheap talk* à résoudre ce problème. Les comportements observés montrent que les scripts de *cheap talk* influencent les comportements par un effet de persuasion – *i.e.* agissent sur les intentions, mais n'affectent que faiblement les actions. En ce sens, les méthodes de révélation des préférences souffrent toujours du manque d'une méthode de communication engageante – *i.e.* permettant d'affecter la motivation intrinsèque des individus à révéler leurs préférences vraies.

Mots-clés: Psychologie sociale, Engagement, Communication persuasive, Révélation des préférences

1 Introduction

Rationality in economics is a social construct (see Arrow, 1987). While the axioms used to define preferences and values are derived at the individual level, economic theory operates under the explicit recognition that real economic commitments are made within an active exchange institution that rewards specialization and trade. Markets create, but do not guarantee, the social environment that increases the likelihood of consistent and coherent economic behavior (Smith, 2003). Working with the confines of rational choice theory makes sense for economics given markets exists to reward consistent behavior and arbitrage inconstant behavior.

Missing markets, however, is the definition of most of the challenges within environmental economics. Estimating demand for environmental protection outside the domain of markets and exchange requires either indirect inferring value from complementary markets or directly eliciting value by creating new pseudo-markets (Freeman, 2003). From the earliest days, stated preference work recognized the need to create new Arrow-Debreu contingent claims markets through survey work a la contingent valuation. The test in creating these new markets has always been designing a survey to overcome the lack of binding budget constraint to create the sense of a real economic commitment (although see Sudgen, 2005).

The task necessarily involves understanding what experimental designs can create the milieu of a market-honed rational decision maker we assume exists in demand and welfare theory. A person making environmental choices interacts within a broader social context than just market exchange institutions. This social context may be more or less forgiving of typical self-interested behavior than occurs in the market. Now the predictions on behavior are less clear since the context is not strictly economic-social norms and principles can matter since the decision is not in strict isolation. Social psychology is one field that offers insight into how people act and react in the context of other people (see e.g., Spash and Biel, 2002). One popular definition is G.W. Allport's: an attempt to understand how the thought, feeling and behavior of the individuals are influenced by the actual, imagined or implied presence of others (Allport, 1968). This is a broad definition-encompassing both how the real and hypothetical presence of other people affects how a person behaves. Given the concern over gaps in real and hypothetical behavior within stated preference research, social psychology can play an understandable role in environmental economics.

Herein we focus on what the field of social psychology can offer work on preference elicitation. We concentrate on preference elicitation and cheap talk scripts to provide a concrete

[1]We wish to thank Romain Zeiliger and Maxim Frolov for their assistance in developing the software. Logistical help from the Paris School of Economics and funding from PACA Regional Council under project PsySoc was greatly appreciated.

example on how social psychology can add insight into valuation work. Additional work exists on the social theory of conflict, coordination, and cooperation (e.g., Kollock, 1998; Thøgersen, 2008; Vugt, 2009; Vatn, 2009); mechanism design and the crowding out of internal and external motivations (e.g., Frey and Oberholzer-Gee, 1997; Hatcher et al., 2000); the adoption of new energy savings technologies and prosocial behavior (e.g., Batson, 1998; Yoeli, 2008), and decision making over risk and time, and how social situations can create context-dependent choices (e.g., Bowles, 1998, 2002; Cherry and Shogren, 2008).

We begin by presenting how social psychology has been used within preference elicitation research to understand how people stated economic measures of value. We then present our new work on testing the impact of cheap talk scripts to reduce hypothetical bias.

2 Social Psychology in preference elicitation: A motivating example

Stated preference (SP) methods have been criticized on multiple fronts, especially the susceptibility to hypothetical bias (see e.g., Murphy et al., 2005). We first discuss economic approaches to addressing bias in preference elicitation to set the stage. We then briefly highlight key aspects of three applications of social psychology to preference elicitation– social isolation, social representations, and cheap talk scripts.

2.1 The economics of preference revelation

The problem of hypothetical bias in stated preference work matters for its credibility as a tool to measure economic values. One technique includes calibrating down hypothetical responses. There are multiple surveys of the SP literature which attempt to calculate the size of the hypothetical bias for calibration purposes. No golden rule exists for calibration. Diamond and Hausman (1994) predict that proper calibration stipulates dividing hypothetical estimates by anywhere from 1.5 to 10. Calibration appears to be good- and context-specific (e.g., Fox et al., 1998). A similar attempt of *ex-post* adjustment is the use of follow-up certainty questions (Champ et al., 1997).

While calibration adjusts hypothetical responses *ex post*, other literature has focused on the use of *ex ante* framing methods to reduce or eliminate the bias. In one of the first appearance of such a procedure, Bohm (1972) warms subjects involved in a public good game against strategic behavior. In a seminal contribution to the more specific field of preference valuation, the National Oceanic and Atmospheric Administration (NOAA) recommended to remind subjects about their actual budget constraint (Arrow et al., 1993). Loomis et al. (1994)

test the effectiveness of reminding subjects of their budget constraints and substitute goods, prior to elicitation. In a mail survey asking people to value old-growth forests in Oregon, they find that such a reminder had an insignificant effect. Neill et al. (1994) finds an analogous result: reminding subjects of the value of alternative environmental goods did not change response rates ; and a similar result is found by Loomis et al. (1994). The replication by Kotchen and Reiling (1999); Whitehead and Blomquist (1995, 1999) however shows this leads to narrower intervals of estimated preferences when applied to goods with which subjects are less familiar.

The *ex-ante* methods try to build on the reasons why HB appears to discipline revelation before it takes place. One possible reason for poor revelation is subjects do not take the valuation exercise seriously because it is an hypothetical scenario. Consequential procedures consists in improving the realism of the elicitation procedure (Carson et al., 2000; Cummings and Taylor, 1998) by providing subjects the probability that their own choice in the experiment will become real, which might actually impact the policy. Earlier experiments provide contrasted results: Cummings and Taylor (1998) show that probabilities have to be high (greater than 0.75) to produce an effect, while Carson et al. (2002) find a coincidence with preferences elicited in the real context from a probability level as low as p=0.2. The ability of consequentialist design to eliminate the hypothetical-real discrepancy has recently been substantiated in a referendum procedure (Vossler and Evans, 2009).

Even if subjects take the exercise seriously they can still lack experience with the elicitation mechanism, or the good to be valued. This led some authors to teach the valuation exercise to subjects *ex-ante*, either training them with being involved in the mechanism or by increasing familiarity with the good. In an attempt to address this last issue, Carlsson and Martinsson (2006) compare the WTP expressed in an open-ended survey before and after subjects experienced the negative consequences from which the good to be valued would protect them. The rather paradoxical result is that informed subjects tend to offer a zero WTP more often. The offers conditional on being positive, however, remain unaffected by the treatment. This could be explained by the increasing feeling that protection is a right that should be privately financed. Regarding subjects attitudes towards the mechanism, Bjornstad et al. (1997) show that experience with the CV procedure eliminates the bias. List and Gallet (2001) addresses both issues through comparing the preference elicited in a Vickrey auction depending on whether dealers are professional. Those subjects that are familiar with both the good and the mechanism reveal significantly different preferences. They do not, however, manage to overcome the discrepancy induced by the change in the incentives context.

Another reason is that subjects may face a dissonance between two competing wills: they want to provide their true preferences, but they also would like to indicate their support

for the provision of the good to be valued – and this is costless in an hypothetical context. The Dissonance Minimization procedure, introduced by Blamey et al. (1999), consists in an additional question in the survey, in which subjects are explicitly asked to express their attitude towards the good. The initial study of Blamey et al. (1999) showed DM questions elicit steeper demand functions, but they do not contrast their result with a real setting. Morrison and Brown (2009) compares the performance of DM with both: calibration and cheap talk. Among the three elicitation devices, only cheap talk fails to discipline revelation since it provides an over-correction of hypothetical bias (average yes votes are lower than in the real treatment).

These economic-based approaches focus on reminders and saliency of exercise. The implicit assumption is that the person is not the problem – people are rational. Rather it is the way the information is presented or the lack of information. We now approach the bias from the view point of social psychology in which it is the person that is the challenge. We will consider Social Representativeness, which gets at what is going on inside their heads, social isolation which gets at what others think of them, and cheap talk scripts, which addresses self-deception and persuasive information.

2.2 Social Representations

The social psychological idea of social representation is that valuation work can be more precise if the survey is designed to account for how people think about notion of "being social" and how "being social" is structured in a population. One way to do this is to capture the "distance" or conformity of people with the social construct (*i.e.* the aggregation of individual representations forms social representations). The method identifies people that distinguish themselves from the "central population" or the dominant social view. These people tend to be more in line with the predictions of the economic theory. This question of framing the "social being" matters for designing valuation surveys. The major issue is how to design a tool that measures individual heterogeneity with respect to framing effects. If we assume people differ in "being social", the question is how can we identify these groups of people in practice?

Borrowed from social psychology, Hollard and Luchini (1999); Luchini (2000) developed a method based on the concept of social representations. Social representation allows the researcher to define a new variable which can identify two types of people–those sensitive to framing effect and those who are not. Social representations are defined in a broad sense by social psychologists as a form of knowledge that can be used as a basis for perceiving and interpreting reality, and to organize behavior (Moscovici, 1961; Farr and Moscovici, 1984). This representation may either be composed of stereotypes or more personal views. The general

principle that underlies the method consists in detecting people who hold a representation of the object to be evaluated that differs from that of the majority, i.e. non conformists. The method first gathers information on individual representation by using free association open-ended question such as "What are the words that come to your mind when thinking of ...?". Second, each person lists words, which are aggregated based on principles derived from social choice to uncover the social representation of the sample. Individuals are then split into two categories, those who are close to the majority point of view and those who hold a different representation.

Flachaire et al. (2007) apply this method to study the anchoring phenomenon that arises in contingent valuation survey based on dichotomous choice elicitation questions. Using a model developed by Herriges and Shogren (1996), they show that the method is successful in discriminating between those who anchor and those who do not. Individuals holding a different representation from the majority are shown to not anchor their answer on the proposed bid while other respondents are characterized by a strong anchoring effect. Flachaire and Hollard (2008) add to this evidence the point that the method can discriminate between individuals with different WTA-WTP discrepancies, those holding a different representation from the majority view being less subject to WTA-WTP discrepancy. In both cases, the interpretation of these results is the following. Non conformists have already a much more elaborated view of the subject, which does not conform to the stereotypical representation. They are not citing the most obvious of the representation, but have a constructed discourse, which reflects their own personal opinion. In that sense, the methodology helps identifying people with more experience on subject, which may give rise to stronger opinions and preferences. People with enhanced preferences are more likely to behave according to standard economic rationality. This means that non-conformists attach much more importance to their own prior value of the object and are less subject to framing effects. The general line of thought parallels experimental, which show that experienced subjects are more likely to conform standard economic rationality. While one can rely on repetition in an experimental setting (Grether, 1980), or identified experienced subjects (List, 2004), to come up with this conclusion, this literature associates repetition and experience with non-conformist representations of the subject under consideration.

2.3 Social Isolation

Social psychology comes into play in survey designs that ex ante frames a person's actions within the context of other people. Critics have questioned the impact of social isolation during preference elicitation. The mode by which SP surveys are administered vary. For example,

a survey could be completed over the phone, by mail or in person. In-person interviews are preferred method of preference elicitation because it, among things, allows for the surveyor to convey necessary information more clearly. The literature suggests that in-person surveys may elevate the level of social pressure, which would bias values upwards.

In a natural field experiment, Alpizar et al. (2008) investigate the importance of anonymity when eliciting stated preferences for a public park in Costa Rica. The experiment tested the importance of social isolation by providing international tourists with the opportunity to contribute funds to a national park while varying the level of anonymity in giving. Social pressure was assumed to be alleviated by having subjects place their contribution in a sealed envelope before giving the contribution to the solicitor. They found that contributions were 25% higher when social pressure (from the solicitor) had been alleviated via placing contributions in a sealed envelope.

In a field experiment of 30 churches Soetevent (2005) tests the importance of social pressure with a similar experimental design. The crux of the experiment consists of varying the type of offering containers used by a sample of Baptist churches in the Netherlands. Two types of offering containers were used: a basket which allows for individual contributions to be locally known by nosey neighbors and a "bag" which keeps contributions private. He finds that removing social pressure causes contributions to decrease for charities external to the church but finds it has no effect on contributions for internal charities.

List (2004) vary the level of social pressure in a lab experiment and find similar results to those experiments done in the field. In a referendum format, subjects were given funds and were provided with the opportunity to donate their money to a public good - the Center for Environmental Policy Analysis (CEPA). A "PEER" treatment consisted of informing subjects of a 25% probability that their vote would be made public. A "Randomized Response" (RR) treatment alleviated any social pressure via the Unrelated Question Technique proposed by Greenberg et al. (1969). List determine that the subjects in the PEER treatment were 30% more likely to donate toward CEPA than those in the RR treatment. These results are not robust to replication, however.

James et al. (2009) finds that subjects were at least as likely to donate to a public good in social isolation as in public for both a referendum and dichotomous choice framework. Social exposure seems to be no panacea for increasing contributions to public goods. Removing social isolation does not guarantee greater contributions to a public good for a referendum and holds across preference elicitation mechanisms. The relationship between social isolation and stated preferences appears more complicated than the present literature suggests. Similar to the case of calibration of hypothetical and real values, perhaps the affect of social isolation is good- and context-specific (see e.g., List and Shogren, 2002). Incorporating social isolation

into stated preference methods might be more productive if it focuses the social psychology associated with a person's contributions relative to one's peer group.

2.4 Cheap Talk

Another example of social psychology in the ex ante framing design are "cheap talk scripts". A cheap talk script provides "persuasive" information within a social context to realign a person's behavioral expectations through communication. These scripts set the social context by revealing that people teornd to overbid in hypothetical surveys (Cummings et al., 1995). While effective under some conditions, a cheap talk design is not a panacea to hypothetical bias. Aadland and Caplan (2003) show that a short and neutral (*i.e.* no information on the sign of the bias) cheap talk script mitigates the bias. Aadland and Caplan (2006) however find that if the cheap talk script is short, it can actually worsen the hypothetical bias. Accumulated evidence favor the conclusion that short cheap talk script can not work (e.g., Cummings et al., 1995; Poe et al., 2002). Long and informative cheap talk scripts has proven more fruitful (Cummings and Taylor, 1999). In the only attempt to assess cheap talk in a such an environment, Mozumder and Berrens (2007) confirm the ability of a directional cheap talk to move hypothetical votes closer to real ones in an IV experiment (note however they do not contrast observed behavior with truth-revelation). This success does not come without restriction, however. In an experiment in which people stated their willingness to pay for sports cards, List and Gallet (2001) finds that cheap talk did not effectively decrease the hypothetical bias when agents are well informed about the good being valued. Similarly, Lusk (2003) find that a cheap talk script is effective in attenuating hypothetical bias only for certain classes of subjects – those with less market experience or less familiarity with the good being valued. This suggest cheap talk works as a learning booster, providing subjects before the valuation exercise take place the information they would acquire through a costly trial and errors process. Brown et al. (2003); Murphy et al. (2005) moreover find that cheap talk scripts that are long and directional work only for higher levels of the provision threshold subjects vote on. Carlsson and Martinsson (2006), by contrast, observe that the only effect of cheap talk is to move down the number of zero offers, letting unchanged the mean value among positive offers.[2]

Based on accumulated evidence, cheap talk scripts have to be long and detailed enough to shave preferences elicited in hypothetical context towards truth-revelation. Cheap talk is a matter of information, not only of request, and people have to be convincing to work. Why cheap talk scripts work point in this direction. Ajzen et al. (2004) hypothesize cheap

[2]Ami et al. (2009) show that cheap talk can even increase the number of protest responses

talk scripts modify the disposition of subjects by realigning beliefs, attitudes, and intentions with those in the real context. Aadland et al. (2007) suggest CT is nothing else than an informative signal, that interacts with the anchoring effect produced by the threshold provided in dichotomous choice formats. Interestingly, this interaction results in cheap talk driving a decrease of preferences in favor of low values but an increase of preferences against high values.

The main idea driving cheap talk scripts is that the researcher can modify the ideas of the respondents to make them behave in the desired way – bid their "true" values. If the information is persuasive enough, the appropriate social behavior will emerge without fuss. But as noted by Joule et al. (2008), "good ideas" do not automatically lead to "proper behavior". They illustrate the gap between good ideas and proper behavior with a study on smoking prevention; Peterson et al. (2000) found no difference in behavior between students 8-17 years old who were part of preventive sessions in class and the control group. Persuasive communication is necessary but not sufficient, because it changes minds but poorly acts on actions. We now explore the idea of cheap talk scripts as "persuasive information" in detail.

3 Experiments

We use two related experiments: induced value (IV) and homegrown value (HG) experiments. The IV experiment elicits preferences based on an induced demand function; the HG experiment elicits each bidders own homegrown preferences for a real-world good. In both experiments, we induce people to reveal their preferences using a Vickrey auction. The focus on the Vickrey (1961) auction stems from its revelation property: without an outside option, a rational bidder's weakly dominant strategy is to bid his induced value. In addition, experimental evidence confirms that the second-price auction performs reasonably well in revealing preferences on average for both induced (Kagel, 1995) and non-induced (e.g., Rutström, 1998) values auctions. The Vickrey mechanism is well-suited for a testbed analysis such as our, since it allows to observe the whole demand curve instead of only the mass points revealed through dichotomous choice settings.

In the IV-experiment, we consider three treatments: IV-Hypothetical with hypothetical bidding, IV-Real with monetary incentives and IV-CheapTalk identical to IV-Hypothetical but with the introduction of a dedicated IV cheap talk script. In the HG-experiment, we first consider two baseline treatments: HG-Hypothetical and HG-incentives, depending on whether decisions in the auction have consequences on the monetary earnings from the experiment. In HG-Hypothetical+Training and HG-Real+Training, we train subjects with an additional induced values hypothetical second-price auction before the homegrown auctions start. We then consider two different cheap talk interventions. In HG-CheapTalk, we consider a standard

heavy and positive cheap talk script added to a standard HG-Hypothetical treatment. A second cheap talk treatment studies potential cheap talk spill-overs on bidding in HG auctions by combining training and a cheap talk intervention implemented in the training phase. This amounts to implement a IV-CheapTalk treatment before an HG-Hypothetical treatment.

3.1 Design of the IV experiment

In the IV experiment, an unspecified "good" is sold in a Vickrey second-price auction: the highest bidder wins and pays the second-highest bidder's bid. An auction has 9 bidders each endowed with a unique induced value – *i.e.* the price at which the bidder can sell the good to the monitor after the auction (see, e.g., Kagel, 1995). All monetary values are expressed in ECU (*Experimental Currency Unit*). The induced demand curve is identical in all auctions and is defined by: $\{84; 76; 71; 68; 65; 63; 53; 38; 24\}$. The auction is repeated over 9 periods, implementing all possible permutations between individual private values: each participant experiences only once each private value; and the whole demand curve is induced in every period. Although the repetition is deterministic, we avoid end-game effect by providing the subjects with no information on that point – except for the repetition itself. The bidders do not know the other bidders' induced value or the induced demand curve. A bidding period ends when every bidder has chosen a bid between 0 and 100. At the end of the period, subjects are privately informed about whether they win the auction (along with the price paid in this case), their gain for the period and, lastly, whether a new auction period is about to start.

Each subject receives a 10€ show-up/participation fee.[3] We complement the show-up fee with additional earned money. Following Cherry et al. (2002, 2005), subjects earned their wealth by answering 20 questions of general interest. [4] The payment rate is 2 ECU *per* correct answer (the exchange rate is again 3 ECU for 1 €). Once all subjects answer all questions, the Vickrey auction begins.

In the IV-Real treatment, the ECU accumulated across all auction periods are added to this fee – would it happen, negative total earnings would decrease the show up fee and earned money up to 5€.[5] In contrast, only this fee and earned money are paid under the IV-Hypothetical treatment. This is made common knowledge by stating explicitly in the written

[3]Minimum hourly wage was 6.50 Euros at the time of the experiment (source: http://www.urssaf.fr).

[4]Questions were selected from the sheets used by the French government to select some of its civil servants. The procedure is labeled *Concours de Catï¿½gorie B de la fonction publique*. Our source is `http://pagesperso-orange.fr/bac-es/qcm/annales_c02_r01.html`. See Jacquemet et al. (2009b) for more details on the earned money phase and a discussion on how earned money can affect bidding behavior in second price Vickrey auctions with induced values.

[5]This lower bound stems from how we recruited participants: we contractually commit ourselves to a minimum earnings equaling 5€.

instructions that payments are either constant (hypothetical) or depend on decisions made in each period (real). Details about the nature of the monetary earnings is the only difference between the instructions used in both conditions.[6]

The IV-CheapTalk treatment is identical to the IV-Hypothetical treatment except that an additional cheap talk script is introduced at the end of the instructions. Our cheap talk intervention is based on the script proposed by Cummings and Taylor (1999) but adapted to an induced value second price price auction.[7] The cheap talk section of the instructions is untitled "For your information" and insist on the difference in bidding behavior observed when monetary incentives are used or not. The wording "hypothetical bias" is stated explicitly and two explanations for this phenomenon, that stems from the literature on experimental auctions, are given : spite and joy of winning (see, for instance, Cooper and Fang (2008)):

> "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. 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 same as the one you are involved in, the only difference being that earnings were realized from the result of the auction. What we observed based on those two groups is the price proposed in the auction is on average up to 1,5 times higher when earnings are independent from decisions as compared to when earnings do depend on decisions. This phenomenon is particularly significant for those people who have a seemingly low induced value.
>
> 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 the number of Euros they earn or lose is determined by the difference between their own private value and the market price, if they win the auction? How do we get them to think about what it means to really deal with Euros, if in fact their decisions aren't going to have any monetary consequence? 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 that have

[6]For replication purpose, the instructions we use are as close as possible to those of Cherry et al. (2004). They are available from authors upon request .

[7]In Cummings and Taylor (1999), the elicitation mechanism is a referendum.

no monetary consequences, 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 want to win, I really want to offer an high price even if I incur some monetary losses from that. But when the auction is real, and we would actually incur the monetary consequences of our decisions, we think a different way. We basically still would like to see good things happen, but we also account 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 incurred the monetary consequences of my decisions: what is the actual price 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 "win" the auction independently of any gains or losses. Please try to overcome this potential desire to "win" for winnings sake, and take your decision just exactly as you would if you were really going to face the consequences of your decision: which is to earn the difference between your private value and the market price if you win the auction. Please keep this in mind in our auction."

3.2 Design of the HG experiment

The HG experiment examines preference elicitation of homegrown values for a real-world non-market good: adopting a dolphin. Subjects' homegrown values are elicited using the same elicitation mechanism as before, a second-price auction. The price for improved parallelism with decisions in the real world is the lack of control over true preferences: subjects enter the lab with their own private value, unknown to the experimenter, for the good.

The good sold in the HG auction is provided by the *World Wide Fund* (hereafter WWF), a well-known non-governmental organization devoted to "protecting the future of nature".[8] 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 pre-determined values), donators are sent gifts such as an adoption certificate, a photograph of the animal, a cuddly stuffed toy dolphin, a

[8]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/`.

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.[9] 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 bidders see the good description until they confirm their choice. Note the upper bound imposed on the bid is the same for all bidders and does not depend on experimental earnings. We clearly stated in the instructions 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 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.[10]

We kept the elicitation mechanism used in the IV-experiment: a Vickrey (second-price) auction. Subjects are grouped into markets of 9 bidders. Auctions are repeated five times and one of the five periods is randomly drawn at the end of the auctions. The winner of the randomly drawn auction is the bidder entitled to adopt a dolphin, and the market price of this auction is the amount of the donation.

The two baseline treatments correspond to those implemented in the IV-experiment: the adoption is hypothetical in HG-Hypothetical; whereas the donations are subtracted from subjects' earnings in HG-Real. This implies donations are declarative in the hypothetical auction; no funds are actually transferred to the WWF and no adoption certificate is sent to

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

[10]As shown in Section 4, this feature implies that most offers elicited in the real context are below the market price. The observed values are independent of field opportunities, which protects our data from the censoring issue raised by, e.g, Harrison et al. (2004). The discrepancy between in-the-lab and market prices may nowadays be influential *ex ante* on bidding behavior if subjects are actually aware of the donation procedure and the market price of the donation. Questions to assess subjects' knowledge are included in a debriefing questionnaire – see Section 3.3 below.

the adopter. Those features are stressed within the instructions read to the participants.[11] All other experimental features are identical in these two treatments – earnings from the quiz are always paid for real to avoid unwarranted wealth differences between our treatments.

Two additional treatments introduce a prior training phase before the HG auction starts. While straightforward in theory, the second price auction used in our treatments is likely to be unfamiliar to many bidders. They might not immediately realize that bidding their true preferences is the weakly dominant strategy. By training via practice rounds, bidders can learn the potential consequences of under- and over-bidding one's preferences for the good. The prior training phase consists of an additional induced values hypothetical second-price auction, inserted between the quiz and the homegrown auction. Training auctions are identical to those conducted in the IV-Hypothetical 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). IV auctions are hypothetical to avoid any wealth effects in the HG auctions. In HG-Hypothetical+Training, the session starts with the earned money phase. Second, subjects bid in hypothetical auctions, which are followed by hypothetical HG auctions; whereas in HG-Real+Training, monetary incentives are binding in the HG auctions.

We close our design by two cheap talk treatments. A first cheap talk treatment introduces a cheap talk script at the end of the instructions in a HG-Hypothetical treatment, hereafter called HG-CheapTalk. The cheap talk script is similar to that used in the IV-experiment but the two explanations, typical to induced values experimental auctions, spite and joy of winning are not mentioned anymore. In the HG cheap talk script, we rather keep with Cummings and Taylor script, only adapting the script to second price Vickrey auctions:

> "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.

[11] The instructions are available from the authors upon request .

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 to 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."

A second cheap talk treatment, hereafter HG-CheapTalk-Spillover, involves a prior IV training auction with a cheap talk script before hypothetical second price HG auctions. This is done by combining a IV-CheapTalk treatment with a HG-Hypothetical treatment.

3.3 Experimental procedure

All sessions of the IV and HG experiments (one for each treatment) were run at the LEEP, University Paris 1. For both experiments, each session used 18 subjects separated into two independent 9-bidder auctions. Overall, 126 subjects participated to the IV and HG experiments. IV-Hypothetical and IV-CheapTalk were the prior training phase of the HG-Hypothetical+Training and HG-CheapTalk-Spillover treatments respectively. Participants were mostly first to third-year undergraduate students in law, economics or chemistry. Both experiments were computerized using a software developed under REGATE (Zeiliger, 2000). Recruitment was internet-based (and made use of ORSEE, Greiner, 2004) and all email-messages were harmonized.

Whatever the experiment, a typical session proceeds as follows. First, each subject signs an individual consent form before entering the lab and is assigned randomly to a computer. Next, the written instructions are distributed and read aloud. The monitor uses both a non-numerical example and quiz to highlight the most salient features of the design. Finally, participants are encouraged to ask clarifying questions before starting the experiment. Both experiments begin by asking the subjects to fill out a computerized questionnaire about socio-economic characteristics (gender, age,...). The first part of the instructions, describing the quiz, is then distributed and read aloud. 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 common knowledge exchange rate is 3 ECU for 1 €. Once all 20 questions are answered by all subjects, the auction is introduced. To improve understanding of the game, a non-numerical example is developed covering all the instructions. The instructions do not, however, indicate that bidding one's induced value is the weakly dominant strategy. Participants are also asked to answer a short questionnaire highlighting the most salient features of the game. Before the game begins, bidders are encouraged to ask clarifying questions, which were privately answered by the monitor. In the IV-experiment, the winning bidder's profit in ECU equals in each round the difference between his or her induced value and the price he or she pays for the good (the second highest bid). For the 8 non-winning bidders, their profits are zero for that round. Only the winner sees the two highest bids at the end of the round. The only common knowledge difference between the two treatments is that ECU accumulated across rounds are not converted into Euros in IV-Hypothetical, while they do are in IV-Real.

The instructions for the HG auction describe in detail the WWF, the adoption procedure, and how the collected funds will be used. The auction is then described using the same instructions as in the IV experiment (same non-numerical example and same questionnaire to check subjects' understanding at the end of the instructions). The only difference is the

good and its description. The wording of the instructions is slightly modified between the HG-Real and HG-Hypothetical. 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). The experimental earnings are adjusted accordingly: the two subjects entitled to adopt a dolphin in each session (one per 9-bidders group) actually lose the amount of the donation in (and only in) HG-Real, and we buy a donation from the WWF for each of them. Before the end of the HG experiment, subjects answer a computerized debriefing questionnaire. The questions 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.

At the end of both experiments, subjects are privately paid their monetary payoff in cash: 10€ in the hypothetical conditions, plus the result from the quizz in the HG-experiment only ; or computed as the sum of this total and the profits/losses ECU accumulated during the auction, in the real conditions. The experiment lasted between an hour and half and an hour.

4 Results

We first consider bidding behavior in the IV-experiment. Table 1 illustrates bidding behavior at the aggregate level by induced value and treatment. In Table 1, we add up the bids and sort by induced value for each of the treatments. The last column presents the total elicited demand in each treatment. For each treatment, we tested under and over bidding by applying a mean difference test of the bid to induced value ratio. Strictly rational bidding implying a ratio of one, we tested underbidding (overbidding) by considering a one-sided mean difference test that test for a ratio less than one (resp. greater than one). Underbidding and overbidding tests are reported for each induced values in the last two rows of each treatment in Table 1. We observe overbidding in IV-hypothetical for the lowest induced value (24 ECU): bid to induced value ratio in percent is 155.1% with ($p = .021$). We also observe overbidding for the lowest induced value in IV-real but to a lesser extent: ratio in percent equals 113.9% ($p = .065$). In addition, underbidding is significant in IV-real when the induced value is 53 ECU ($p = .049$), although bidding behavior is closer to perfect demand revealing bids.

Table 1: Aggregate bidding behavior by group and induced value

Induced value	24	38	53	63	65	68	71	76	84	All
Ag. Demand (AD)	432	684	954	1134	1170	1224	1278	1368	1512	9756
IV-Hypothetical										
Revealed AD	670	746	1045	1215	1205	1348	1334	1453	1479	10495
Ratio RAD/AD	155.1%	109.1%	109.5%	107.1%	103.0%	110.1%	104.4%	106.2%	97.8%	107.6%
Over bidding p-value	.021	.222	.205	.155	.345	.119	.251	.104	.668	
Under bidding p-value	.979	.778	.795	.845	.655	.881	.749	.896	.332	
IV-Real										
Revealed AD	492	678	816	1145	1121	1229	1260	1406	1490	9637
Ratio RAD/AD	113.9%	99.1%	85.5%	101.0%	95.8%	100.4%	98.6%	102.8%	98.5%	98.8%
Over bidding p-value	.065	.543	.951	.416	.792	.475	.645	.210	.682	
Under bidding p-value	.935	.456	.049	.584	.208	.525	.355	.790	.320	
IV-CheapTalk										
Revealed AD	608	768	938	1087	1147	1202	1375	1417	1382	9756
Ratio RAD/AD	140,7%	112.3%	98.3%	95.9%	98.0%	98.2%	107.6%	103.6%	91.4%	101.7%
Over bidding p-value	.024	.097	.411	.253	.636	.616	.045	.079	.934	
Under bidding p-value	.976	.903	.589	.747	.364	.384	.955	.921	.066	

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

Table 2: IV bidding behavior – Panel Tobit estimations

Variable	IV-Hypothetical $n = 162$	IV-Real $n = 162$	IV-CheapTalk $n = 162$
	Parameter estimates (p-value)		
ν_{it}	0.88	0.98	0.82
	(.000)	(.000)	(.000)
Constant	4.78	-9.62	-5.71
	(.521)	(.032)	(.262)
Round dummies	YES	YES	YES
σ_u	10.4	4.15	8.86
	(.000)	(.000)	(.003)
σ_ϵ	19.69	12.31	13.10
	(.000)	(.000)	(.000)
Log-likelihood	-648.42	-627.14	-650.76

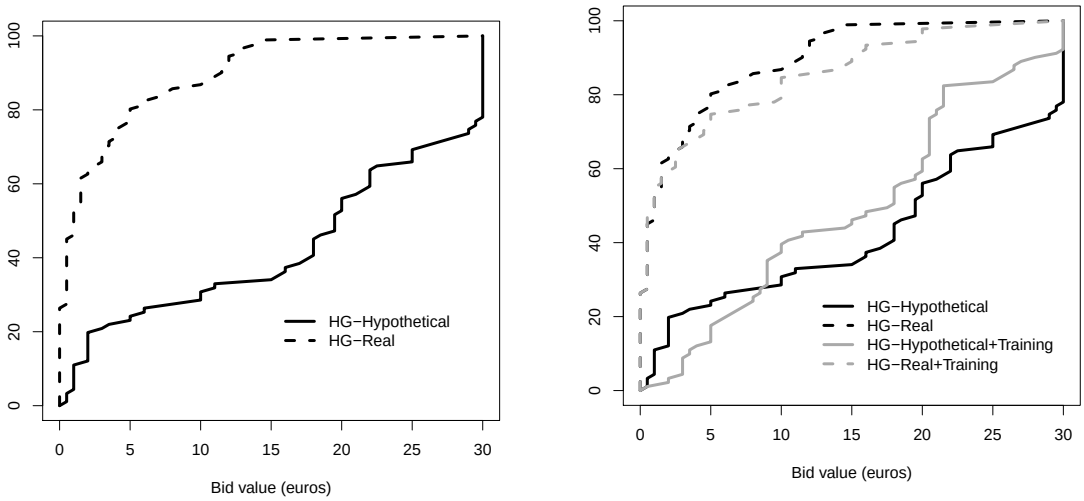
Note. Individual random effects Tobit models. The random effects are assumed normal; round (fixed) effects are controlled for in the estimation, but omitted in the Table. The *endogenous* variable is the bid posted. ν_i denotes the induced private value. The columns report results from separate regressions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

We test the assumption of perfect demand 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 2. Based on the estimated parameters of the bidding function, we can test for perfect demand revealing bids for each treatment by considering $H_0 : \{\beta = 1, \alpha = 0, \phi_t = 0\ \forall t\}$ (see, for instance, Shogren et al., 2001). Results from Wald tests for the first two treatments are:

IV-Hypothetical	:	W=23.19	p =0.010	H_0	Rejected
IV-Real	:	W=17.34	p =0.067	H_0	Rejected

Figure 1: Empirical Distribution Functions of bids in baseline and training treatments



(a) Bidding behavior in baseline treatments

(b) Bidding behavior with training

We reject perfect demand revealing bids for both treatments. This indicates that for both hypothetical and real treatments, there is some room for cheap talk to impact on hypothetical bidding behavior in an IV second price auction. Aggregate bidding behavior is described in the last four rows of Table 1. Results are not convincing regarding cheap talk effects. We still observe overbidding for the lowest induced value (140.7%, $p = .024$) and to a lesser extent for the second lowest induced value (38 ECU) at a ten percent threshold (112.3%, $p = .097$). Overbidding also holds at the upper end of the demand curve for the third and second highest induced values, 71 ECU and 76 ECU: percentage bid to induced value ratio is 107.6% and 103.6% with $p = .045$ and $p = .079$ respectively. For the highest induced value (84 ECU) we observe a significant underbidding at a ten percent threshold: percentage bid to induce value ratio is 91.4% with underbidding test with $p = .066$. Econometric results presented in the last column of Table 2 confirms descriptive results and tests. The estimated parameter associated with the induced value ν_{it} equals 0.82 as compared to 0.88 in IV-Hypothetical and 0.99 in IV-Real. Result from the Wald test of perfect demand revealing bids is:

$$\text{IV-CheapTalk} \quad : \quad W{=}18.05 \quad p{=}0.054 \quad H_0 \quad \text{Rejected}$$

Cheap talk has not the expected effect on bidding behavior in IV auctions, in which subjects perfectly know their true value for the good. In this context, Cheap talk even proves to increase overbidding at the low end but also at the high end of the demand curve, even inducing underbidding for the highest induced value.

Table 3: Homegrown bidding behavior in real and hypothetical treatments

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
HG-Hypothetical	Mean bid (€)	15.36	18.19	17.33	17.38	18.9	17.43
	Median bid(€)	16.25	20.5	19.75	19.75	20.75	19.5
	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	7	9	8	9	10	43 (47.7%)
HG-Real	Mean bid (€)	3.3	2.97	3.17	3.17	2.3	2.98
	Median bid (€)	1.25	1	1.25	1	0.75	1
	♯ 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 (in *row*) and by round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings (*fourth row*). The last row of the table gives the ratio between average baseline bids and average monetary-only bids.

We now turn to the HG experiment, in which subjects enter the lab with their own, unobserved, preferences. First, we find evidence of a substantial hypothetical bias in HG auctions by contrasting bidding behavior in HG-Hypothetical and HG-Real. Figure 1.a presents the empirical distribution functions (EDF) of bids in HG-Hypothetical and HG-Real. The EDF of bids in HG-Hypothetical first order dominates the EDF of bids elicited in HG-Real. This means that data exhibit a hypothetical bias for low bids as well as for high bids. Summary statistics of HG-Hypothetical and HG-Real treatments are presented in Table 3. For each treatment, we compute average and median bids as well as the number of bids above experimental earnings and the number of zero bids. Mean and median bids in HG-Hypothetical are €17.43 and €19.5 as compared to €2.98 and €1 in HG-Real. This leads to an average hypothetical-real ratio of 584.9%. This means that bids in HG-Real are on average six times lower than in HG-Hypothetical – indicating a substantial hypothetical bias.[12]

Bidding behavior in HG-Hypothetical+Training and HG-Real+Training confirm further the existence of hypothetical bias in HG auctions. EDFs of bids are provided in Figure 1.b, along with those associated with bidding behavior of untrained bidders. 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

[12]See Jacquemet et al. (2009a) for a detailed discussion on hypothetical bias in IV and HG experimental auctions.

Table 4: 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-Hypothetical	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-Real	Median bid (€)	0.75	0.75	1.25	0.75	0.75	1
+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%

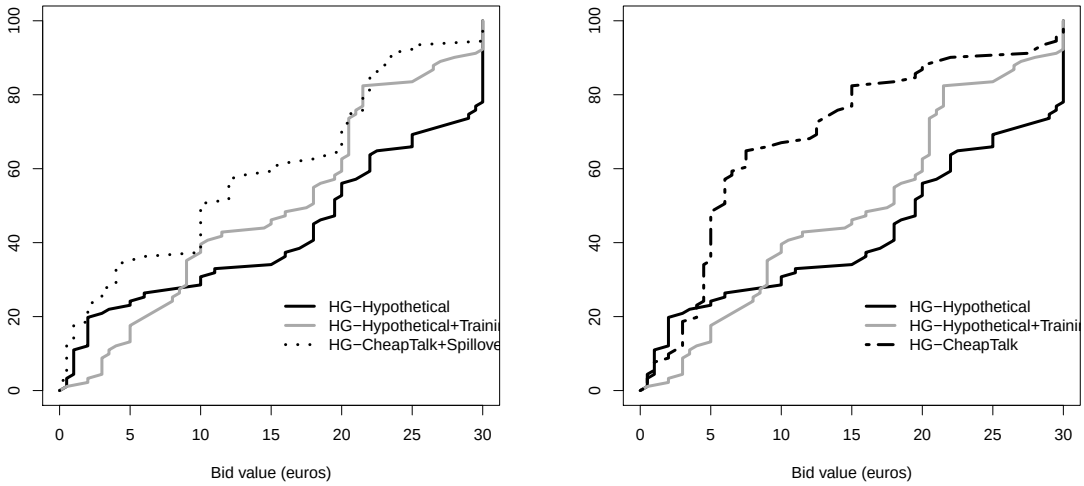
Note. For each Treatment (in *column*) and round (in *column*), the table provides bidding behavior in the homegrown (adopt a dolphin) experiment for trained subjects: mean and median bid (*first two rows* for each treatment) ; number of zero bids (*third row*) and bids above subject's experimental earnings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

decrease higher bids. Examining the summary statistics provided in Table 4 shows again that training marginally affects bidding behavior. In HG-Real+Training, the number of bids above experimental earnings is lower with training than without training (21.1% of bids compared to 47.7%, p=.056). This in particular leads bidders to bid on average less (€15.31) than in HG-Hypothetical (€17.43). With incentives, trained bidders bid more (mean is €4.23) than untrained ones (mean is €2.98). The differences as regards training are not significant: the p-value of a two-sample bootstrap mean difference test leads to a p-value of 0.494 for HG-Hypothetical against HG-Hypothetical+Training ; $p = 0.476$ for HG-Real against HG-Real+Training.[13] Because it increases familiarity with the mechanism, training helps subjects to better figure out how to implement their intended behavior through actual decisions inside the elicitation mechanism. Overall, this only slightly disciplines bidding behavior. In an hypothetical context, the main effect of training is to reduce budget constraint violations, which are almost twice as low for trained bidders.

We now examine bidding behavior in the two HG cheaptalk treatments. Figure 2.a presents EDF of bidding behavior in HG-Hypothetical, HG-Hypothetical+Training and HG-

[13]We test the difference in mean bids by 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 (see Jacquemet et al. (2009c) for more details on the testing procedure).

Figure 2: Empirical Distribution Functions of bids in Cheap Talk treatments



(a) Cheap talk in the training phase

(b) Cheap talk in HG auctions

CheapTalk-Spillover. The EDF of bids in HG-CheapTalk-Spillover dominates the distribution of bids in HG-Hypothetical and HG-Hypothetical+Training. Table 5 presents summary statistics on bidding behavior. We observe a decrease in mean bid (€12.22) as regards to bidding behavior in HG-Hypothetical (€17.43) and HG-Hypothetical+training (€15.31). The decrease is significant between HG-Hypothetical and HG-CheapTalk-Spillover ($p = .068$). Recall training alone did not induce a significant decrease, but similarly strongly disciplined budget constraint violations. The difference between bidding behavior in HG-Hypothetical+training and HG-CheapTalk-Spillover is in fact not significant ($p = .147$). There exists a small spillover effect of providing information about hypothetical bias in experimental auction in a prior training phase, that fosters the effect of training.

Cheap talk has a larger effect on bidding behavior when implemented just before the HG auctions without prior training, *i.e.* in HG-CheapTalk. Figure 2.b presents EDF of bids in HG-Hypothetical, HG-Real and HG-CheapTalk. It shows that bids in HG-CheapTalk are scaled downward in comparison to other hypothetical treatments. Table 5 provides further statistics on bidding behavior in HG-CheapTalk: mean bid decreases from €17.43 in HG-Hypothetical to €9.34 in HG-CheapTalk – decrease is significant with $p = .007$. Moreover, a cheap talk script presented in the HG instructions has the greatest effect on bidding behavior in comparison to prior training alone ($p = .014$). The decrease in bids is however not significant in comparision to prior training with cheap talk ($p = .147$). When designed so as to highlight the main reasons why hypothetical bias appears, cheap talk achieves a strong decrease in

Table 5: Homegrown bidding behavior after oath and/or cheap talk

		Round 1	Round 2	Round 3	Round 4	Round 5	All rounds
	Mean bid (€)	11.5	11.31	12.5	12.8	13.0	12.2
HG-CheapTalk	Median bid (€)	10	10	11.5	11	12	10
-Spillover	♯ zero bids	0	0	2	0	0	2 (0.02%)
	♯ bids > gains	3	3	5	5	5	21 (23.33%)
Average Cheap-Talk-real gap		191.2%	193.6%	236.6%	231.2%	351.3%	234.6%
Average Cheap-Talk-hyp. gap		41.1%	31.6%	43.3%	42.2%	42.8%	40.1%
	Mean bid (€)	9.1	8.1	9.5	9.0	11.0	9.3
HG-CheapTalk	Median bid (€)	5.5	5	6	5.5	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 Cheap-Talk-real gap		276.7%	272.1%	299.7%	283.0%	479.1%	313.4%
Average Cheap-Talk-hyp. gap		59.4%	44.4%	54.8%	51.6%	58.3%	53.6%

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

positive bids (as measured by the median), while still performing as a learning booster that disciplines budget constraint violations.

We finally pool data from all six HG treatments in a random effects panel Tobit model. A dummy variable is introduced to control for the HG-Real treatment (HG-Hypothetical being the referent) as well as total earnings and individual's characteristics as in the previous Tobit model. Two dummy variables are added to control for trained bidders: one when training is implemented prior to an hypothetical HG auction and one when it is implemented prior to a real HG auction. Last, we add two dummy variables that account for cheap talk effects, one for each cheap talk treatment. Note that for HG-CheapTalk-Spillover, two dummy variables are set to one: training in hypothetical and cheap talk in IV. Results are presented in Table 6. The tobit model confirms previous summary statistics and tests. First, bids in HG-Real are significantly and greatly lower than in HG-Hypothetical. Second, training has no significant effect on bidding, either prior to an HG-Hypothetical auction ($p = .411$) or prior to an HG real auction ($p = .808$). Cheap talk implemented in a prior IV training phase has a negative effect on bidding behavior but not strong enough to be significantly different from bidding behavior in HG-Hypothetical ($p = .305$). The effect of cheap talk in IV on HG hypothetical bids is

Table 6: HG bidding behavior – Panel Tobit estimations

	Parameter estimate	P-value
Treatment dummies		
Constant term	13.08	0.090
Monetary incentives	-16.51	0.000
Earnings	0.18	0.646
Training × Hypothetical	-2.32	0.411
Training × Real	0.74	0.808
Cheap talk in IV	-2.87	0.305
Cheap talk in HG	-6.74	0.020
Round dummies	YES	
Individual's characteristics		
Age	-0.06	0.627
Male	-1.11	0.530
Participated to other experiments	0.81	0.666
Experience with auctions websites	-1.11	0.173
Knows WWF	0.73	0.814
Agrees with WWF actions	2.12	0.002
Knows WWF's dolphin adoption programme	-1.79	0.476
σ_u (sd.)	8.16	(0.62)
σ_e (sd.)	3.42	(0.13)

Note. Individual random effects Tobit models (random effects are assumed Gaussian), $n = 107$ (one individual has missing values in socio-demographic variables in HG-CheapTalk-Spillover) 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 83.9 with $p < .001$.

however significant in comparison to HG-Hypothetical (Wald test=3.31 with $p = .069$).[14] This is in line with the mean difference test. Cheap talk in IV has however a smaller effect on bids than a cheap talk script implemented in HG instructions (around twice less). The effect of cheap talk in HG instructions is highly significant ($p = .020$)

[14]Recall that two dummies light up for bids observed in HG-CheapTalk-Spillover: Training × Hypothetical and Cheap talk in IV. Wald test amounts to test the linear hypothesis: Training × Hypothetical+Cheap talk in IV =0.

5 Discussion and Directions

Social psychology provides insight into how other people's expectations and perceptions, real or imagined, can affect one person's behavior. Our results on cheap talk scripts support the social psychologist's view that persuasive information is necessary but not sufficient to trigger sincere bidding in incentive compatible auctions. Cheap talk scripts seem most useful at reducing the problem of "self-deception" about one's true value, but not at triggering sincere bidding. Overall, our results show that in Induced Value treatments, bidders know the truth, which leads to the neutrality on cheap talk. Insincere bidders know they are shaving or exaggerating their bids, so they do not care about the warnings. In the Homegrown treatments, the results suggest: training with the valuation exercise slightly push upwards the real bids and gently disciplines the hypothetical bidding. Embedding a cheap talk script in the training phase marginally intensifies the effect of training alone – spillover effects through further discipline of hypothetical bidding. Cheap talk scripts in the Homegrown auction leaves unchanged the number of budget constraint violations as compared to training, but strongly pushes down the average bids. Training is a way to make subjects better aware of the link between their decision in the mechanism and their true intentions. Cheap talk has the same effect, but seems more powerful.

Given the discrepancy that still remains between real and hypothetical, one still looks for a device that can induce people to implement their intended decisions, and influences their intrinsic willing to comply with the truth-telling strategy–we want communication that is "binding". Social psychology provides a key insight into one mechanism that is binding–the oath. The literature focuses on how to design an ex ante frame based on the ideas promoted by commitment theory and the use of the oath as a commitment device Jacquemet et al. (2009c). Commitment theory posits a person is less likely to tell untruths after a strong pledge such as oath (see Joule and Beauvois, 1998). The classic example of commitment theory is the panhandler story. First, the panhandler asks a passer-by the time of day; then he asks for spare change. Voluntary contributions increase since the passer-by has already committed himself as a person that gives away something for free-the time, then money. We are more likely to a second request if we have already agreed to the initial one (see Burger, 1999). And people are more likely to agree if the action is freely selected and voluntary. Economic experiments support this theory. After pre-play communication, people who make promises about future actions are more likely to keep them when playing in both hold-up and trust games (Ellingsen and Johannesson, 2004; Charness and Dufwenberg, 2006). Commitments are more likely to bind when made freely, expressed publicly, and with consequences.

The oath acts as a commitment device by binding a person to a future behavioral act. The

oath is a real-world commitment device that is publicly expressed, taken freely and signed, appears an extreme and more accentuated commitment device than a verbal promise or a written undertaking. The open question addressed by Jacquemet et al. is whether the oath can induce people to reveal their preferences sincerely. They use a solemn oath as a truth-telling commitment device, asking our bidders to swear on their honor to give honest answers prior to participating in an incentive compatible second-price auction. Bidders are free to sign the oath, and participation and earnings are independent of the oath.

Jacquemet et al. design several experimental treatments based on both an induced value (IV) second-price auction and a homegrown value (HG) second-price auction. Comparing bidding behavior in hypothetical, real, and oath-only treatments, they find for induced values, the oath improved demand revelation relative to both hypothetical and real bidding behavior. For homegrown values, the oath induced overbidders in hypothetical auctions to lower bids and underbidders in real auctions to increase their bids. These results suggest the oath has promise to create the commitment needed to better link intentions and actions.

An analogous result is found in Jacquemet et al. (2010). They find that signing an oath can decrease or even eliminate the presence of a hypothetical bias. In a referendum valuing a wind energy research center, hypothetical bias exists in non-oath treatments and that the proportion of "yea" votes to donate to WERC are significantly decreased as a result of signing an oath to tell the truth. They find evidence that subjects were unaware that signing the oath affected their hypothetical stated preference.

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