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

Nicolas Jacquemet† Alexander G. James‡ Stéphane Luchini§ Jason F. Shogren¶

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Abstract

The field of social psychology explores how a person behaves within the context of other people. The social context can play a substantive role in non-market allocation decisions given peoples choices and values extend beyond the classic market-based exchange institution. Herein we explore how social psychology has affected one aspect of environmental economics: preference elicitation through survey work. We discuss social representation, social isolation, framing through cheap talk, and commitment theory through an oath.

Keywords: Social psychology, Commitment, Persuasive communication, Preference elicitation.
JEL Classification: C9; H4; Q5

1 Introduction

Social psychology offers insight into how people act and react in the context of other people (see e.g., Spash and Biel, 2002). One popular definition comes from G.W. Allport: 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 broad definition encompasses how the real and hypothetical presence of other people and alternative contexts affects a person's

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attitudes and how he or she behaves. The overall relationship between attitudes and behavior can be multi-faceted, such that one behavior can arise from many attitudes (see Dawes and Smith, 1985).

In contrast, rational choice theory relies on preferences to motivate behavior, not attitudes. The origin of preferences is less important than the observed or stated behavior. Rational behavior arises when people make consistent choices and behave in predictable ways to changes in prices and income. But rationality in economics is a social construct, in which the main social context is the exchange institution, e.g., a market (see Arrow, 1987). Rational choice theory operates under assumption that behavior arises from real economic commitments made within an active exchange institution that rewards specialization and trade. Markets create, but do not guarantee, a social context that makes consistent and coherent behavior more likely (Smith, 2003). Working within 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 main motivation for the field of environmental economics. Estimating demand for environmental protection outside the domain of markets and exchange requires indirectly inferring value from complementary markets or directly eliciting value by creating new pseudo-markets, or both (Freeman, 2003). From the earliest days, researchers interested in eliciting values for non-market goods recognized the need to create new Arrow-Debreu contingent claims markets. 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 involves understanding how to capture the relevant social context, market and non-market. 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. Here social psychology can play an understandable role in many aspects of environmental economics – risk, conflict, coordination, cooperation, incentive design, and valuation.

Herein we focus on what social psychology offers to one aspect: valuation and preference elicitation. We concentrate on preference elicitation and cheap talk scripts (a framing effect) to provide a concrete example on how social psychology can add insight into valuation work. Given space constraints, the reader can find work 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), 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 first present how social psychology has been used within preference elicitation research to understand how stated preferences of 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) – the discrepancy between revealed and true preferences induced by the hypothetical nature of the revelation exercise. We first discuss economic approaches to address bias in preference elicitation to set the stage. We then briefly highlight three applications of social psychology to preference elicitation – social isolation, social representations, and cheap talk scripts.

2.1 The economics of preference revelation

Hypothetical bias remains one of the major challenges in stated preference work. Budget constraints are not binding in hypothetical choices. 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) warns 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. (Also see Blumenschein et al., 2008, who compare hypothetical bias given cheap talk and asking how certain a person is about their stated value).

The *ex-ante* methods try to build on the reasons why hypothetical bias 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 a 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 of p=0.2, which is still substantially above the probability level any reasonable person would conjecture. 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 (2001) address both issues through comparing the preference elicited in a Vickrey auction depending on whether card-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 a hypothetical context. The Dissonance Minimization (DM) procedure, introduced by Blamey et al. (1999), consists in additional response categories in the survey, in which subjects are explicitly asked to express their attitude towards the good. More specifically, theses additional response categories dissociate explicitely respondents' support for the program and their willingness to pay for the program. For example, Blamey et al. (1999) provide respondents with the following extra response categories: "I support the [program]

... but it's not worth $50 to me", "I support the [program]...but I cannot afford $50", and "I support the [program] but not if it requires a [fee] of any amount". 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 the exercise. The implicit assumption is 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 consider Social Representativeness, which gets at what is going on inside a person's mind, 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 the 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 a 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 questions 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 surveys 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. People holding a different representation from the majority are shown to not anchor their answer on the proposed bid; whereas other people reveal a strong anchoring effect. Flachaire and Hollard (2008) add to this evidence – they point out the method can discriminate between people with different WTA-WTP discrepancies. People who differ from the majority view are less subject to WTA-WTP discrepancy. In both cases, the interpretation of these results is the following. Non-conformists have a more elaborated view of the good/service, which does not conform to the stereotypical representation. They do focus on the most obvious representation; rather they have a constructed discourse that reflects their own personal opinion. One can use this method to identify people with more experience with a good/service, 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 non-conformists attach more importance to their own prior values and are less subject to framing effects. The general line of thought parallels the experimental literature, which show that experienced subjects within a market 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 arrive at this conclusion, this literature associates repetition and experience with non-conformist representations of the good/service under consideration.

The open question is what we should do with this information with respect to environmental valuation. If we can identify people as either conformists or non-conformists we can gain insight into whether they will be more or less susceptible to framing effects. If a person can be easily manipulated through framing, his or her preferences are transient and context-dependent as captured by, say prospect theory. In this case, standard demand modeling is suspect because the preferences are fungible. In contrast, if a person is not easily influenced, his or her preferences are more like the permanent-context-independent type we assume exists in rational choice theory. Standard demand modeling now provides a solid foundation for eliciting preferences. The next question regarding the applicability of demand theory is to estimate the fraction of the population that reveal context-dependent versus context-independent preferences. If context-dependent people play a minor role, we do not have to change our typical economic structural model. Otherwise, if they play a big role, researchers will need to address how demand models must be re-structured to capture context-dependence, which is not a straightforward task.

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 a 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.

James et al. (2009) develop an analytical framework which suggests a voting referendum may be relatively more susceptible to social pressure bias than a dichotomous choice mechanism. According to their model, the important distinction between the two mechanisms is the group nature of a referendum. In a referendum, voting "no" to contribute money to a costly public good indicates to an audience that limited personal gain is preferred to collective group action. A referendum raises the cost of selfish behavior for a person concerned with social stigma. Experimental evidence supports the prediction of their model. Within a voting referendum, subjects were significantly

less likely to contribute to a public good once they were provided with social isolation. This result did not carry over to a dichotomous choice format. Social exposure seems to be no panacea for increasing contributions to public goods. Rather the effect of social isolation appears to be institution specific.

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 tend 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 cannot work (e.g., Cummings et al., 1995; Poe et al., 2002). Long and informative cheap talk scripts have proven more fruitful (Cummings and Taylor, 1999). In the only attempt to assess cheap talk in this 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 (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 can work as a learning booster, if the researcher provides subjects information they would only 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.[1]

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 talk scripts modify the disposition of subjects by realigning beliefs, attitudes, and intentions with those in the real context.

[1] Ami et al. (2009) show that a neutral and short cheap talk script can even increase the number of protest responses.

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 believes he or she identify "strength of manipulation" of the chosen words, and use these words to modify how respondent's social context 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 participated in preventive sessions in class and the control group. Persuasive communication is necessary but not sufficient because it changes minds but not necessarily their actions. We now explore the idea of cheap talk scripts as "persuasive information" in detail.

3 Cheap Talk Experiments

We now design an experiment to illustrate the role of cheap talk within a "homegrown values" (HG) experiment. The HG experiment elicits each bidders own "homegrown" preferences for a real-world good. We use the classic Vickrey second price auction to create a weakly demand inducing environment to elicit values. The highest bidder wins and pays the second-highest bidder's bid. 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. While recognizing the potential for framing effects through the auction, experimental evidence confirms that the second-price auction performs reasonably well in revealing preferences on average for both "induced" (Kagel, 1995) and homegrown (e.g., Rutström, 1998) values auctions. The Vickrey mechanism is well-suited for our testbed analysis since it allows one to observe the whole demand curve instead of only the mass points revealed through dichotomous choice settings (see Lusk and Shogren, 2007).

We first consider two baseline treatments: HG-Hypothetical and HG-Real, 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 consider two different cheap talk interventions. In HG-CheapTalk, we consider a standard heavy-positive cheap talk script added to a standard HG-Hypothetical treatment. A second cheap talk treatment studies potential cheap talk spillovers on bidding in HG auctions by combining training and a cheap talk intervention implemented in the training phase.

3.1 Design of the 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 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 homegrown value, unknown to the experimenter.

The good sold in the auction is provided by the *World Wide Fund* (hereafter WWF), a well-known non-governmental organization devoted to "protecting the future of nature".[2] 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 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 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.[3] 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

[2]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/`.

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

price when monetary incentives are binding. Subjects are not told anything about this subsidy.[4]

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 are: the adoption is hypothetical in HG-Hypothetical; whereas the donations are deducted from subjects' earnings in HG-Real. This implies donations are declarative in the hypothetical auction; no funds are transferred to the WWF and no adoption certificate is sent to the adopter. Those features are stressed within the instructions read to the participants.[5] 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 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 (IV) hypothetical second-price auction, inserted between the quiz and the homegrown auction. The IV auction is ran on a 9-bidders market, is repeated over 9 periods, and implements all permutations between private values from the induced demand curve: $\{84; 76; 71; 68; 65; 63; 53; 38; 24\}$; so that the whole demand curve is induced in every period and each bidder experiences each private value once.[6] 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 complete our design with 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. In the HG cheap talk script, we follow the Cummings and Taylor script, only adapting the script to second price Vickrey auctions:

[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.

[5]Written instructions are available from authors upon request.

[6]See Jacquemet et al. (2009a) for a detailed description of the design used in the IV auction phase.

"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 on 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. How 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 haveto 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 make 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 a 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 make 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 training auction with a cheap talk script before hypothetical second price HG auctions. This is done by combining an induced value-CheapTalk treatment with a HG-Hypothetical treatment.[7]

[7]The script closely follows the one we implement in the HG auctions, but is adapted to an induced value context.

3.2 Experimental procedure

All sessions were run at the LEEP, University Paris 1. Each session used 18 subjects separated into two independent 9-bidder auctions. Participants were first- to third-year undergraduate students in law, economics or chemistry. The experiments were computerized using a software developed under REGATE (Zeiliger, 2000); Recruitment was internet-based (using ORSEE, Greiner, 2004).

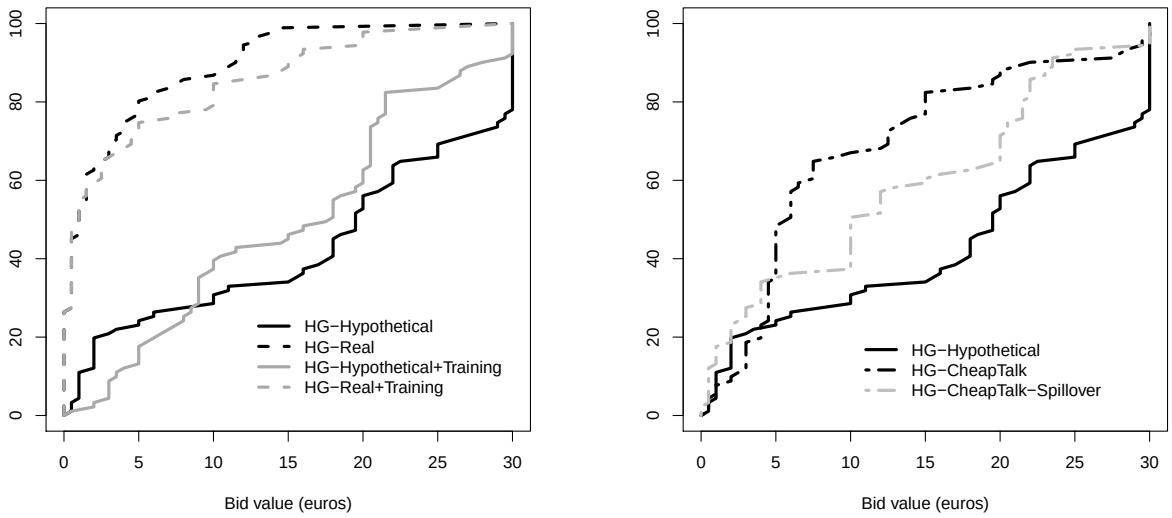
A typical session proceeded as follows. First, each subject signs an individual consent form before entering the lab and is assigned randomly to a computer. Next, the written instructions are distributed and read aloud. The monitor uses both a non-numerical example and quiz to highlight the most salient features of the design. Finally, participants are encouraged to ask clarifying questions before starting the experiment. The experiments begins 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, say 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.

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 in detail. The wording of the instructions is slightly modified between 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 with regard to 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 the sessions, subjects are privately paid their monetary payoff in cash: 10€ plus

The text is available from the authors upon request.

Figure 1: Empirical Distribution Functions of bids in Cheap Talk treatments



(a) With and whithout training

(b) After cheap talk scripts

the result from the quizz in the hypothetical conditions ; 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 1 to 1.5 hours.

4 Cheap Talk: Results

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 the upper part of Table 1. 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.[8]

Bidding behavior in HG-Hypothetical+Training and HG-Real+Training confirm further the

[8]See Jacquemet et al. (2010b) for a detailed discussion on hypothetical bias in induced value and HG experimental auctions.

Table 1: Homegrown bidding behavior with and without (IV) training

		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 hypothetical-real gap		465.5%	612.5%	546.7%	548.3%	821.7%	584.9%
HG-Hypothetical +Training	Mean bid (€)	14.61	15.25	14.41	16.72	15.58	15.31
	Median bid (€)	13.25	18.25	16.75	19.75	14.75	17.75
	♯ zero bids	0	0	0	0	0	0 (0.0%)
	♯ bids > gains	3	4	3	6	3	19 (21.1%)
HG-Real +Training	Mean bid (€)	3.33	5.08	4.42	4.17	4.17	4.23
	Median bid (€)	0.75	0.75	1.25	0.75	0.75	1
	♯ zero bids	5	4	5	5	5	24 (26.7%)
	♯ bids > gains	0	0	0	0	0	0 (0.0%)
Average hypothetical-real gap		438.7%	300.2%	326.0%	401.0%	373.6%	361.9%

Note. For each Treatment (in *row*) and round (in *column*), the table provides bidding behavior in the experiment for trained and untrained 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*).

existence of hypothetical bias in HG auctions. EDFs of bids are provided in Figure 1.a, 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 hypothetical treatment. Here, training seems to increase low bids but decrease higher bids. Examining the summary statistics provided in Table 1 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).[9] This in particular leads bidders to bid on average less (€15.31) than in HG-Hypothetical (€17.43). With incentives,

[9]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., 2009b, for more details on the testing procedure).

Table 2: Homegrown bidding behavior after cheap talk scripts

		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-Hypothetical 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-Hypothetical 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 experiment after 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*).

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. 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 a hypothetical context, the main effect of training is to reduce budget constraint violations, which are almost twice as low for trained bidders.

Now consider bidding behavior in the two HG cheap-talk treatments. Figure 1.b presents the EDFs of bidding behavior in HG-Hypothetical, HG-Hypothetical+Training and HG-CheapTalk-Spillover. The EDF of bids in HG-CheapTalk-Spillover dominates the distribution of bids in HG-Hypothetical and HG-Hypothetical+Training. Table 2 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 insignificant ($p = .147$). A small spillover effect exists – providing information about hypothetical

bias in a prior training phase can affect bidding behavior in a subsequent auction.

Cheap talk has a larger effect on bidding behavior when implemented just before the HG auctions without prior training, *i.e.* in HG-CheapTalk. Based on Figure 1.b, we see bids in HG-CheapTalk are scaled downward in comparison to other hypothetical treatments. Table 2 provides further statistics on bidding behavior in HG-CheapTalk: the mean bid decreases from €17.43 in HG-Hypothetical to €9.34 in HG-CheapTalk – the 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 positive bids (as measured by the median), while still acting as a method to discipline budget constraint violations.

Finally, we pool data from all HG treatments in a random effects panel Tobit model. We use a dummy variable to control for the HG-Real treatment vs HG-Hypothetical treatments. Two dummy variables control for trained bidders: one for training implemented prior to an hypothetical HG auction; the other for training prior to a real HG auction. Last, we add two dummy variables that account for cheap talk effects, one for each cheap talk treatment. For HG-CheapTalk-Spillover, two dummy variables are set to one: training in hypothetical and cheap talk in IV. Results are presented in Table 3.

The Tobit model confirms previous summary statistics and tests. First, bids in HG-Real are significantly and by far 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 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 training on HG hypothetical bids is significant in comparison to no-training HG-Hypothetical (Wald test=3.31 with $p = .069$).[10] 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 ($p = .020$)

Cheap talk scripts seem most useful at reducing the problem of "self-deception" about one's true value, but not at triggering sincere bidding. In a classic setting of induced values in which bidders know the truth, cheap talk has no influence on bidding. Insincere bidders know they are shaving or exaggerating their bids, so they do not worry about the warnings. In the Homegrown treatments, our 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

[10] 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.

Table 3: 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. All treatments are introduced as dummy variables. Round (fixed) effects are controlled in the estimation but omitted; results are available upon request. Wald joint nullity test is 83.9 with $p < .001$.

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.

5 Commitment and the Oath

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. Social psychologists have questioned the idea that all one has to do is to change the ideas of people, as cheap talk scripts intend to do, to get them to

act in the appropriate way. They argue that many persuasion campaigns fail because professionals have presumed that if the arguments are strong enough and the information is clear, the desired behavior will emerge, e.g., smoking, drug abuse, food safety. Instead, social psychologists like Joule et al. (2007) stress that trying to inform and convince people is useful but not enough. People might be aware but they also have to be committed to the task (see Albarracìn et al., 2005; Durantini et al., 2006). So how then can behavior be changed? Social psychologists have been trying to answer this question for the past 60 years, and there is now a wealth of reliable scientific knowledge on this topic. Joule et al. (2007) have suggested that the research carried out in this particular domain could be brought back to a single basic paradigm: commitment (*e.g.* Joule and Beauvois, 1998, 2002) – in the 1940s, Kurt Lewin blazed the trail for research into free will compliance . The commitment idea is to find the tools likely to lead others to modify their behavior of their own free will.

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 willingness to comply with the truth-telling strategy – we want communication that is "binding" (Joule et al., 2007, 2008). Commitment theory posits a person is less likely to tell untruths after a strong pledge (see Joule and Beauvois, 1998; Kiesler and Sakumura, 1966; Schelling, 1960). The classic example of how commitment works 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 comply with a second request if we have already agreed to the initial one (see Burger, 1999). This means that, in a given course of action, one's past actions influence actions to come. 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). In a recycling experiment, people who signed their names when agreeing to recycling recycled twice the amount of those not asked to sign (Wang and Katsev, 1990). Research indicates that the strength of commitment increases if made freely, expressed publicly, and has consequences (Kulik and Carlino, 1987; Pallack et al., 1980; Katzev and Wang, 1994).

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. 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 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. The oath acts as a commitment device by binding a person to a future behavioral act (Kiesler and Sakumura, 1966).

The oath that is publicly expressed, taken freely and signed, is a more accentuated commitment device than a verbal promise or a written undertaking.

We conclude our paper with a brief review of our work on 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. Induced values experiments "induce" preferences on subjects by providing them a resale value for any object they purchase. If they can buy the good for less than their resale value, they make a profit; otherwise, they make a loss. We focus our discussion on the HG treatments to be consistent with our earlier discussion on our cheap talk experiments.

The experiment design is nearly identical to the Cheap Talk experiments: a second-price auction is used to elicit values to adopt a dolphin through a monetary donation to the *World Wide Fund*. 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 elicited preferences in five auctions, in which one was randomly selected as the binding auction. We also divide each 18-subject session into smaller groups of 9 bidders. Three 18-subject sessions, one per treatment, were conducted in the LEEP laboratory in Paris. Each subject was assigned a computer; the monitor distributed the instructions and reads them aloud.

We use three contexts: HG-Baseline, HG-Incentives and HG-Oath. The first two treatments were identical except for the monetary consequences of the adoption auction. The HG-Oath treatment adds the oath procedure to the HG-Baseline treatment: before entering the lab, each subject is asked to sign (in private) a "solemn oath" to tell the truth. 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. 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).

Our results based on aggregate data reveal a substantial hypothetical bias when comparing the HG-Baseline and HG-Incentives. Mean and median bids in baseline are €17.43 and €19.5; €2.98 and €1 in monetary-only. The hypothetical bias seems to arise for two distinct behavioral reasons. First, the budget constraint did not seem to bind in the hypothetical treatment – nearly half the bids exceed the budget constraint (no bidder exceeded the budget constraint in the real treatment). Second, at the low end of the distribution, the participation constraint did not seem to bind in the real treatment – nearly 27% of bidders bid zero in the monetary-only treatment (no bidders offered zero in the hypothetical treatment).

Our results suggest that the oath seemed to mitigate both types of behavior. In HG-Oath,

Figure 2: Distribution of bids in the baseline, incentives and oath treatments

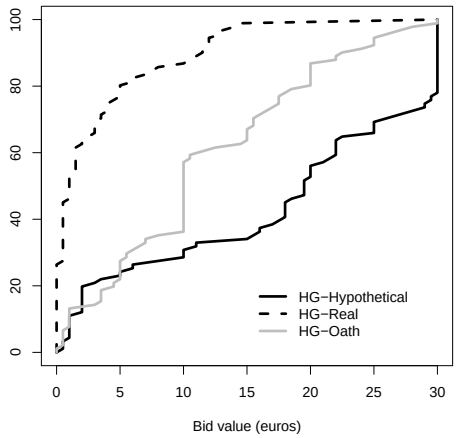


Figure 2 shows the empirical distribution of bids in the oath treatment relative to the hypothetical baseline and real treatments. We see that the oath induced a bidding distribution that first order dominates both the hypothetical and real treatments. Under oath, bidders seem to take both the budget constraint (19 percent of bids exceed the budget) and participation constraint (13 percent bid zero under oath) more seriously. These results suggest the oath increase the commitment for some of our subjects to reveal their true preferences for the goods sold in the auctions. We confirmed this tendency with several robustness tests which can be read in detail in the full paper.

An analogous result is found in Jacquemet et al. (2010a). 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.

6 Concluding remarks

We conclude by acknowledging the limits of our review of social psychology and environmental economics. We have purposefully followed a narrow path by focusing on preference elicitation as the motivating example, and examining how different aspects of social context affect valuation using experimental methods as our voice. Many of the lessons discussed on context could be applied to broader work on attitudes (e.g., political views, opinions on greenbelts and brownfields) and surveys/questionnaires (e.g., contingent valuation, choice experiments).

One emergent area is exploring what psychology, social and otherwise, has to offer to envi-

ronmental policy design and tax policy. Economists are now considering how insight from social psychology might affect the optimal tax structure. Economists measure the success and usefulness of a tax by the degree to which the desired outcome is achieved and the size of the associated deadweight loss. But how people respond to taxation is a behavioral question. If people behave irrationally or have non-standard preferences, this behavior should play a role in shaping optimal public policy. If one assumes people are imperfectly rational, the literal tax rate is no longer the important variable. Rather it is the perceived tax rate that matters. This provides policy makers with another choice variable: tax saliency (Congdon et al., 2009). For example, people discount sales taxes. By imposing a sales tax rather than, say, an excise tax a government can minimize the associated substitution effect. Market elasticities are endogenous upon the tax structure and personal and social saliency.

Psychological insight can also be used to think about optimal fiscal policy decisions. For example, policy makers need information over whether tax cuts can stimulate a weak aggregate demand. Psychology suggests how much stimulus arises from a tax cut depends on how the tax cut is "framed". For example, if a tax cut is represented as a "bonus" rather than a "rebate", people seem more likely to spend the extra wealth generated by the tax cut (Epley et al., 2006; Congdon et al., 2009). Decreasing tax withholdings has potential to create greater stimulus than does a tax rebate. While economists are beginning to recognize the importance of using behavioral economics to generate effective and efficient public policy, the melding of the two is currently in an infantile stage. The limited literature suggests understanding more about social psychology and tax policy is worthy area of future research. Finally, a large literature exists on the social psychology of cooperation, negotiation, and coordination of actions and institutions. We save this review for another day.

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