University of Heidelberg

Department of Economics

Discussion Paper Series | No. 514

Giving in a Large Economy: Price vs. Non-Price Effects in a Field Experiment

Johannes Diederich
and Timo Goeschl

June 2011

Giving in a Large Economy: Price vs. Non-Price Effects in a Field Experiment*

Johannes Diederich† and Timo Goeschl‡
Department of Economics
Heidelberg University

June 14, 2011

Abstract

We conduct a large-scale field experiment with 2,440 subjects in which we exogenously vary the price of contributing to the closest empirical counterpart of an infinitely large public good, climate change mitigation. We find that the price effect is robust and negative, but quantitatively weak, with a price elasticity of -0.25. Socioeconomic variables such as education, situational variables such as meteorological conditions around the time of the experiment, and attitudinal variables that can be linked to guilt and moral responsibility dominate the price effect. The latter also explain better than price arbitrage the decision of subjects to declare to be field price censored. The results provide an experimental window on the absolute and relative role of price effects on public goods contributions in a large economy and inform current attempts to build a coherent theory of charitable giving.

Keywords: private provision of public goods, large economy, price elasticity, field experiment, charitable giving, moral motivation, field price censoring

JEL Classifications: C93, D12, H41, Q54

*The authors are grateful to seminar participants at Cambridge, Heidelberg, the London School of Economics, and Manchester for helpful comments and the German Science Foundation DFG for financial support.

†Email: diederich@eco.uni-heidelberg.de. Postal address: Bergheimer Str. 20, 69115 Heidelberg, Germany.

‡Corresponding author. Email: goeschl@eco.uni-heidelberg.de. Postal address: Bergheimer Str. 20, 69115 Heidelberg, Germany.

1 Introduction

Better understanding voluntary contributions to large-scale public goods has attracted considerable research efforts over recent years. One key area of interest has been the question whether and how prices matter relative to other factors such as donor characteristics or donor attitudes in the private provision of public goods (e.g. Eckel and Grossman 2008, Karlan and List 2007, Peloza and Steel 2005, Kingma 2007). An awareness of the absolute and relative role of prices in the private provision of public goods is important for a number of reasons. Fundraising practitioners and policy-makers require a clear understanding of the role of prices as they construct fundraising and taxation schemes that may involve matching grants, rebate subsidies, or more exotic instruments in order to mobilize contributions (Eckel and Grossman 2008, Morgan 2000). To the economist, it is important for helping to decide what the building blocks of a more comprehensive theory of voluntary contributions would have to deliver (Konow 2010, Andreoni 2006).

In order to improve our understanding of the voluntary provision of public goods, we carry out a large-scale field experiment[1] on private contributions in a very large public goods game. Its design enables us to clarify, in a real-world setting, the absolute and relative role of prices in explaining whether subjects choose to contribute. Previous papers employing a field setting (Karlan and List 2007, Karlan, List and Shafir 2011, Eckel and Grossman 2008, Eckel and Grossman 2003) have explored the role of prices by manipulating match ratios and rebate rates. Both instruments can be used to reduce the effective price of public goods provisions below the field price outside the experiment in a controlled fashion. This strategy allows not only to understand the effects of common fundraising methods. Researchers also use it to triangulate experimental results with econometric evidence from tax rebate schemes (e.g. Peloza and Steel 2005, Auten, Sieg and Clotfelter 2002). One drawback of the strategy is that the design forces the researcher to adopt an indirect route to price variation: Subjects do not observe different prices, but contribution multipliers. Interpreting results derived by this route as those of an equivalent price change may or may not be valid. A second drawback is directional asymmetry with respect to price: A design based on matching or rebate instruments can only generate observations for effective prices of contributions below, and not above, the field price. This directional asymmetry may impose undesirable limits both for discriminating between theoretical approaches and for addressing policy-relevant issues.

In this paper, we follow the direct line of attack on the price effect charted by Andreoni and Vesterlund (2001) and Andreoni and Miller (2002): These authors exogenously vary the price of charitable behavior, specifically the price of benefit sharing in a dictator game. There are important benefits to this direct assault on the price issue: In contrast to matching grants and rebates, different subjects do indeed observe different prices in a clear and unambiguous way, thus enhancing experimenter control over the price treatment. It also generates observable behavior over a wider price range, including above a field

[1] Following the nomenclature in List (2004), the present research strategy represents an in-between case of framed and natural field experiments. Our design falls short of a natural field experiment by virtue of the setting, which is familiar, but not natural, and by virtue of the awareness by subjects that their choices are being observed by an experimenter.

price. Moving the strategy of direct price variation from the laboratory setting of Andreoni and Vesterlund (2001) into the field, however, gives rise to a number of challenges, among them the possibility of field price censoring (FPC). The solution adopted here is to combine direct experimenter control over price with FPC detection methods originally developed in the context of non-market goods valuation (Harrison, Lau and Williams 2002) in order to shed more light on the price effect in voluntary giving.

In addition to understanding the absolute role of price for whether individuals contribute privately to large-scale public goods, the design also provides an opportunity to study the role of price relative to other determinants. There is a lively debate about what drives individual contribution decisions. Drivers discussed in the literature include socioeconomic determinants such as age (e.g. List 2004), gender (e.g. Andreoni and Vesterlund 2001), and education (e.g. Karlan 2005). But also attitudinal and psychological factors such as image motivation (e.g. Benabou and Tirole 2006), guilt (e.g. Battigalli and Dufwenberg 2007), or offset motives (Kotchen 2009) have received attention. The experiment we conduct speaks to this debate by exploiting observable characteristics of individual subjects in several of these dimensions and linking them to a subject's contribution choice.

We use a large-scale field experiment in which subjects have a single choice between a given cash prize and a contribution to a very large-scale public good. The cash prize on offer is the outcome of a random draw from a range of €2 to €100. The public goods contribution that subjects can make is a fixed amount of climate change mitigation efforts in the form of an emissions reduction of one ton of CO_2, carried out through the documented retirement of emissions allowances (EUA) under the European Union Emissions Trading Scheme (EU-ETS). Climate change mitigation is not only highly policy relevant, it also arguably provides (see e.g. Nordhaus 1993) the closest empirical counterpart to the theoretical analysis of infinitely large public goods games initiated by Andreoni (1988). The single choice format of the experiment reflects our focus on the extensive margin of contribution (Bergstrom, Blume and Varian 1986): Subjects only choose whether to contribute or not, given the alternative of a cash prize. 'Field' refers to several features of the experimental design, namely a subject sample that polling companies take as representative for the population of German voters, a familiar, non-laboratory environment in which subjects take their decision, and the use of subjects' information set as they bring it to the task (Harrison and List 2004). 'Large-scale' refers to the sample of 2,440 subjects that participated in the experiment.

Our key results can be summarized as follows. In absolute terms, the price effect is present, but small. Positive variations in the price of providing the public good do have the predicted negative impact on the propensity to contribute (Andreoni and Miller 2002): At the individual level, the marginal effect of a €1 increase in the price of the contribution decreases the probability that the individual will contribute by around 0.1 percent. At the aggregate level, the price elasticity of the extensive margin is -0.25. The price effect is therefore small, whether measured at the individual or the aggregate level. The point estimates of low price elasticity of contributing to public goods by Karlan and List (2007) and Eckel and Grossman (2008) are therefore part of a more general phenomenon that holds at higher degrees of resolution and over a wider price range.

In relative terms, our evidence supports the notion that, compared to price, other variables have more power in explaining the observed contribution decisions in a very large economy. Among socioeconomic variables, we fail to confirm previous findings that gender or age correlate with contribution decisions, but we find that education stands out. Our findings also confirm recent results on situational or 'mood' factors driving contribution decisions (Konow 2010, Kirchsteiger, Rigotti and Rustichini 2006): Meteorological conditions such as ambient temperature at the time of the experiment appear to influence the probability of contributing. Extending the analysis towards attitudinal variables that have been raised in the context of non-price determinants of public goods contribution such as 'moral satisfaction' (Kahneman and Knetsch 1992), warm glow (Andreoni 1990), offsets (Kotchen 2009), or guilt avoidance (Battigalli and Dufwenberg 2007), we find that contributions are positively associated both with a perception of next-generation benefits and personal benefits and with an acknowledgement of previous negative contributions to the public good. Attitudinal variables, e.g. shame avoidance (Andreoni and Petrie 2004), are also better able than price variables to explain the evidence generated by a test for FPC.

The paper proceeds as follows: We explain the experimental design considerations, the experimental protocol, and the nature of the sample in detail in the following section. We then present the experimental results and their econometric analysis in section 3 before interpreting and discussing the results. Section 4 concludes.

2 Experimental design

2.1 Basic design and protocol

Economists have long noted that voluntary emissions reductions to mitigate climate change constitute the closest empirical counterpart to a contribution in an infinitely large public goods game (e.g. Nordhaus 1993). We embrace this feature in the design of our field experiment by offering 2,440 subjects a choice between, on the one hand, a cash award and, on the other, a guaranteed greenhouse gas (GHG) emissions reduction of 1 ton of CO_2. The choices are then implemented under a random incentive system (Grether and Plott 1979, Starmer and Sugden 1991, Lee 2008). The cash prize presented to the subject is the outcome of an equiprobable draw from prizes between €2 and €100 in steps of €2. The GHG emissions reduction is in the form of the documented and verifiable retirement of an emissions allowance under the EU-ETS.[2] The random incentive system is between-subjects (Baltussen, Post, Van den Assem and Wakker 2010, Abdellaoui, Baillon, Placido and Wakker 2011, Tversky and Kahneman 1981) with odds of one in fifty that the subject's choice of either

[2]The European Union's Emissions Trading System regulates industrial CO_2 emissions across all EU member states. Emitters can trade in units of EUAs, EU emissions allowances, each corresponding to one metric ton of CO_2. Total emissions for the trading period 2008-12, the relevant one for this experiment, are capped at 1.856 billion tons. The total ceiling is binding and enforced. ETS account holders will typically trade EUAs, but can also purchase and delete ('*retire*') an EUA. This lowers the total ceiling, and hence emissions, by one ton. Each EUA is uniquely identified by its issue number and hence traceable for both experimenter and subject. EUAs, however, are not paper currency and have therefore no curiosity value as a tangible private commodity.

cash or emissions reductions is realized.[3] Subjects do not learn about others' choices before, during, or after the experiment.

In order to retain a narrow focus on the public good dimensions of the contribution, the design excludes to the greatest extent possible confounding public or private goods aspects associated with the experiment. For example, if subjects received EUA retirement certificates in hardcopy, it would plausibly increase the willingness to contribute in order to purchase a good with a strong curiosity dimension. Additional goods dimensions as well as the visibility of a subject's contribution to others are therefore minimized.

Our subjects are drawn from the approximately 65,000 Internet panel members of one of the largest polling companies in Germany[4] and are representative for the country's Internet using population of voting age.[5] The Internet experiment ran in two sessions in May and July 2010.[6] Session 1 lasted from May 25th to June 2nd and generated 1,640 complete observations from 1,817 invitations. Session 2 lasted from July 19th to 27th and generated 800 complete observations out of 888 invitations. The recruitment of subjects followed the standard routine in which panel members are invited via an email message to proceed to the poll via a hypertext link. In presentation, the poll was indistinguishable from a standard poll run by the polling company: The introductory screen explained the thematic focus of the poll, the expected duration (ten minutes), and the specifics of the random incentive system. These design criteria would have been familiar to panel members in format and content from previous polls as they decided on whether to proceed. Following the invitation screen, there was a filter screen to focus on German subjects.[7] Participants then faced a sequence of ten to thirteen computer screens during the experiment, depending on their decisions.[8]

The centerpiece of the experimental design were two screens, the *information screen* that set up and the *decision screen* that collected the subject's choice. The *information screen* explained three features of the experiment, (1) the choice between a cash prize in Euros and the CO_2 emissions reduction, (2)

[3] Between-subjects (BS) and within-subject (WS) random incentives systems have been subjected to examination for possible biases. While BS introduces noise, there is no evidence of a systematic bias for simple tasks such as the dichotomous decision explored here (Baltussen et al. 2010, Cubitt, Starmer and Sugden 1998, Bolle 1990).

[4] The polling company, YouGov, incentivizes panel members in each poll through either a piece-rate reward of approximately €1 for 20 minutes expected survey time or random (lottery) prizes, e.g. in the form of shopping vouchers in denominations of €25 or €50. All cash awards accrue to the subject's personal account with the polling company. The random incentive scheme was therefore procedurally familiar and—with an average award of €50 and an expected award of €1—comparable in monetary terms.

[5] We test whether our sample differs from one drawn from the general population of German voters. Using two-sided t-tests, we reject the hypothesis that the means of the socio-demographic characteristics coincide at the one percent level. Our subjects are more likely to be male, younger, and educated than the average German of voting age. Income is self-reported, and therefore the lower average income in the sample is unsurprising.

[6] Prior to the experiment we ran a set of pre-tests and a pilot experiment with 200 economics students at Heidelberg University to test the online implementation, refine the set of texts and questions, and test the procedure that addresses field-price censoring.

[7] Subjects of other nationalities were redirected to other surveys running at the same time. Again, this is familiar to panel members as political polls often restrict the sample to those eligible to vote.

[8] The screens required an answer for each question by entering text or choosing at least one of the options given (including "I don't know" options) before being able to proceed to the subsequent screen.

a succinct explanation of how the deletion of an EUA reliably and verifiably reduces EU CO_2 emissions, and (3) an explanation of the random incentive system with odds of 100 in every 5,000.[9] Except for reminding subjects that emissions reductions have the same effect on climate change irrespective of the location of the abatement activity, the experiment did not contain further material to educate or inform experimental subjects. The observation that subjects of field experiments on public goods contributions are typically not very well informed about and may differ significantly in their assessment of the real impact of their contribution on public goods provision also applies to this setting. The contribution decisions observed are therefore based on knowledge that subjects bring to the experiment.[10]

The *decision* screen explained how the subject would receive their chosen prize if the subject was drawn as a winner.[11] The screen then collected the subject's choice, i.e. the specific cash award or the EUA, which were presented on the screen in a randomized ordering.

Following the information and the decision screen, the experiment concluded with a set of screens containing follow-up questions. Subjects that had chosen the cash prize were automatically directed to a screen that provided subjects with an non-incentivized opportunity to explain their choice. Subjects were asked to (1) check whether the experimental cash prize was simply preferable to the value they attached to the CO_2 reduction, (2) check whether the given cash prize was higher than their estimate of alternative CO_2 reduction options available, and (3) enter free text to explain the preference for the cash prize. All subjects were then asked to provide estimates of the EUA price and their availability to subjects outside the experiment. Another set of questions was targeted at subjects' beliefs about benefits from today's emission reductions as well as their perceived personal contribution to climate change. The survey concluded with collecting specific socio-demographic information in addition to subject's socio-demographic profile on record with the polling company.

2.2 Price Treatment

The central design feature of the experiment is the direct variation in the opportunity cost of contributing to the large-scale public good. Experience with direct price variation in the lab comes from Andreoni and Vesterlund (2001) and Andreoni and Miller (2002). These authors use a within-subject random incentive system to examine how subjects behave in eight different economic environments characterized by exogenous variations in the price of giving and endowment. Employing a between-subjects random incentive system instead,

[9] Note that this representation of the odds could lead subjects to believe that at least 5,000 people participate in the poll. This is not misleading: This paper reports only on the baseline share of 2,440 of the roughly 6,800 individual experiments carried out in total.

[10] Offering subjects potentially choice-relevant information prior or during the experiment has been shown to lead to inevitable biases and potential misinterpretations (Arrow, Solow, Portney, Leamer, Radner and Schuman 1993, Munro and Hanley 1999). Instead, the field nature of the experiment allows subjects to collect relevant information while the experiment is in progress, something that the choice of the universal metric deliberately facilitates and that we can indirectly observe.

[11] As in other polls by the polling company, all winners would be informed via a personal email message. Cash prizes were directly credited to the member's account. The retirement of EUAs was individually and privately traceable and verifiable by EUA issue number through a public-sector Internet site.

our field design randomly assigns subjects to one of fifty different experimental prices. The prices range, in steps of €2 from €2 to €100, is roughly one order of magnitude above and below the actual €15 field price of an EUA at the time of the experiment. The group size is on average 49 subjects per experimental price[12]. Neither the price randomization, the price range, nor the sample size are communicated to the subjects. Subjects only learn their individual realization of the random draw of a price and, after the experiment, whether or not their decision is randomly selected for implementation from the pool of participants.

Important design considerations are the direction and strength of the price effect as well as the issue of potential field price censoring that arises as a result of taking the direct price variation from the lab into the field.

2.2.1 The price effect and the price elasticity of contributions

With a focus on the extensive margin of contributing in a large economy (Bergstrom et al. 1986), the experimental design leads to clear theoretical predictions on the presence and direction of the price effect. Under standard assumptions, models of pure altruism (Andreoni 1988), impure altruism (Andreoni 1990), and offset (Kotchen 2009) predict that at a high price of giving, the probability that an individual will contribute is lower than at a low price of giving. Similarly, a conditional cooperation model of behavior would predict higher contributions at lower prices if sophisticated subjects interpret cash prizes below their perceived field price of the public good as evidence of matching by the experimenter as Karlan and List (2007) show.[13] Confirming the thrust of these predictions, Karlan and List (2007) find a negative price effect at the extensive margin of contributing to a non-profit organization. Eckel and Grossman (2008), on the other hand, do not identify a price effect when examining the impact of offering subsidy rates on response rates of potential contributors in a field experiment on fund-raising for public radio.

While the presence and direction of the price effect are mostly clear, theory provides less guidance on its strength. There are a number of arguments, such as lack of substitutes and salience of price in deciding on public goods contributions, that support the notion that the price elasticity for contributing to public goods should be low (Green 1992). Also, both in a pure and impure altruism model in the spirit of Andreoni (1988) and Andreoni (1990), respectively, the subjects' strategic interdependence in providing the public good reduces the price elasticity of the Nash contributions as long as subjects believe that all subjects face the same change in price. Support for predicting low price elasticity comes from experimental studies that examine a limited number of discrete price variations and report low estimates at the extensive margin of contributing: Smith, Kehoe and Cremer (1995) find that the decision whether to make a charitable contribution for a rural health care facility is insensitive to price. Likewise, examining contribution choices for an unmatched baseline and three match ratios, Karlan and List (2007) find that while the probability of donating

[12] Standard deviation is 6.4. The smallest group consisted of 31, the largest of 66 subjects.

[13] The same authors also discuss that a broader class of models makes for more equivocal predictions: For example, alternative predictions on price effects arise in an indirect way out of studies on quantity effects in hypothetical valuation exercises on public goods. These demonstrate that individuals' stated values are insensitive to quantity (Baron and Greene 1996, Kahneman and Knetsch 1992), which gives rise to at least the theoretical possibility that their revealed valuations may also be insensitive to price.

responds to the presence of a match, the response is inelastic with respect to the match ratios. Eckel and Grossman (2003) and Eckel and Grossman (2008) on the other hand find unit elasticity of a price match both in the field and the laboratory, but a much lower elasticity when using a rebate match.

By random assignment of subjects to one of fifty experimental prices, both the presence and direction of the price effect as well as the price elasticity can be studied at a higher degree of resolution and over a wider price range than field experiments that typically offer no more than four price variations below the actual field price. This allows us not only to test the robustness of our current understanding of price effects, but also to check whether the existing results can truly be understood as point estimates of a continuous price effect and whether these insights also apply to variations above the field price.

2.2.2 Field price censoring

The challenge created by the directly varying prices in the field in order to determine the price effect is that of field price censoring (FPC) (Harrison and List 2004). FPC arises because prices for goods within the experiment are difficult to isolate from prices of those same goods or close substitutes in the real world (Harrison et al. 2002, Harrison, Harstad and Rutström 2004, Cherry, Frykblom, Shogren, List and Sullivan 2004). In other words, the experiment exogenously introduces an arbitrage opportunity for subjects. As a result, the experimentally observable contribution decision may be truncated at the level of the subject's perceived field price plus transaction costs.[14] In the present experiment, subjects may believe that they are able to provide an equivalent CO_2 emissions reduction at a lower total cost (including transaction costs) than the prize offered as an alternative, for example by avoiding unnecessary car trips.

To detect FPC, we follow the strategy of a debriefing questionnaire (Coller and Williams 1999, Harrison et al. 2002), which consists of the three FPC questions that follow the *decision screen* in the experimental protocol. The questionnaire functions as an *ex post* choice filter to detect subjects constrained by FPC from revealing there "true" contribution decision. As a result, the FPC filter contains all subjects that did not check the first of the three questions ('*Was the cash prize simply more valuable to you than the CO_2 emissions reduction?*'), but checked the second question ('*Do you think the given cash prize was higher than the cost of alternative ways for you to reduce CO_2 emissions?*') or made a qualitatively equivalent statement in response to question 3 ('*Did you have other reasons for choosing the cash prize?*').

Recoding those observations contained in the FPC filter as having chosen the contribution at the given price is one strategy for correcting for field price censoring and delivers a candidate for the set of uncensored contributors. The

[14] For our purposes, FPC is present if a subject with a reservation price for the public goods contribution r_i accepts the experiment cash prize e_i even though $r_i > e_i$ simply because the field price of an equivalent contribution in the field $\hat{f}_i$ estimated by the subject (inclusive of transaction costs) obeys $e_i > \hat{f}_i$. In cases then where $r_i > e_i > \hat{f}_i$, the experimenter may mistakenly conclude that the unobservable reservation price r_i is smaller than e_i on the basis of the subject choosing cash instead of the good and therefore systematically understate the probability to contribute. Since there is no secondary market for retired EUAs, we need not be concerned about the situation $\hat{f}_i > e_i > r_i$ in which subjects opt for the EUA despite $r_i < e_i$ in order to pocket the arbitrage margin $\hat{f}_i - e_i$.

plausibility of this candidate depends, however, on the credibility of the FPC filter. A common problem in debriefing methods of this type is that, while easily implemented, the FPC filter is not immune to contamination through strategic behavior or ex post rationalization (Corrigan and Rousu 2008). Subjects choosing cash may declare to be field price censored for self-serving reasons, e.g. in order not to appear to be motivated by personal gain. We return to this point in section 3.3.

3 Results and analysis

3.1 Experimental results

2,440 subjects completed the experiment with a median completion time of 5 minutes.[15] A total of 382 subjects (15.7 percent) in the experiment chose the public goods contribution, 2,058 opted for the cash prize.[16] Figure 1 presents the observations and fitted shares of original contributors for each of the fifty price treatments. Looking at the extreme ends of the price range we find on the lower end that the average subject does not voluntarily contribute to the global public good even at a minimal cost of donating. On the other hand, the share of subjects contributing is clearly positive throughout and up to the upper bound of €100, even though the contributions take place in a very large economy.[17]

The extensive margin of contribution decisions in figure 1 traces out a negatively sloped, almost vertical contribution schedule. At the same time, the telltale signs of field price censoring—such as a price that truncates contributions—are absent. Also, the price elasticity of the contribution schedule appears low at first sight. To confirm, we estimate the overall price elasticity of the contribution schedule under a constant elasticity assumption. The estimated price elasticity of -0.25 is lower than the empirical price elasticities estimated by Auten, Sieg and Clotfelter (2002) for tax rebates on charitable donations and closer to the field experimental price elasticities of contribution decisions reported by Karlan and List (2007) with -0.225 for matching grants and by Eckel and Grossman (2008) with -0.11 for rebate subsidies.[18]

3.2 Econometric analysis

The econometric analysis of the subjects' discrete choices requires a number of model and parametric assumptions. We employ a probit model to study the

[15] Average completion time was 1 hour 17 minutes. This is driven by a small fraction of surveys (approx. 3%) in which subjects availed themselves of the opportunity to leave the survey and continue hours or days later.

[16] The following results exclude the observations from those 83 subjects that expressed disbelief about the payment and EUA vehicle in freetext options provided. On theoretical grounds, we decided to exclude these observations, but the results are not sensitive to their inclusion.

[17] A possible caveat regarding the share of contributors arises from our use of what has variously been called 'found', 'windfall' (Keeler et al. 1985) or 'house money' (Clark 2002, Harrison 2007). Since subjects are trading off a contribution to a public good against income that they did not earn themselves, there is a concern in the literature that this might bias results compared to a situation where subjects have to sacrifice their own money. The literature is divided, however, on the likely direction of the bias, if any.

[18] For matching grants, Eckel and Grossman (2008) find a price elasticity of -1.045.

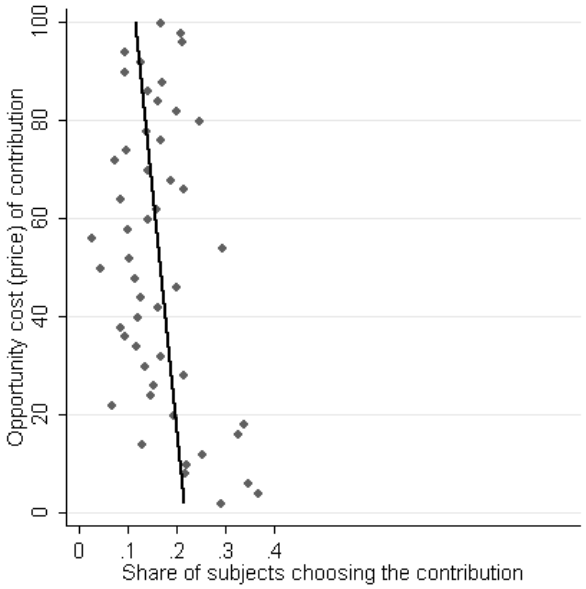

Figure 1: Share of contributors for fifty price treatments. The Figure plots the cash prize offered to subjects against the share of subjects choosing to contribute at this price. Cash prizes are between €2 and €100, in steps of €2. Plotted is the linear fitted regression line (n=2,357).

impact of price and non-price variables on the likelihood of contributing. We estimate two basic specifications:

$$Y_i = \alpha_0 + \alpha_1 P_i + \varepsilon_i \quad (1)$$

$$Y_i = \gamma_0 + \gamma_1 P_i + \gamma_2 N_i + \gamma_3 P_i n_i + \varepsilon_i \quad (2)$$

with Y_i denoting a binary variable with $Y_i = 1$ if subject i chose the contribution and P_i denoting the cash prize offered to subject i while N_i and n_i are vectors of the subject's non-price attributes with n_i representing a subset of N_i. The first model is an unconditional model of the price effect on the extensive margin of contributing based on the fifty price treatments. This model gives the price effect and has the additional benefit of allowing a direct comparison to similar unconditional models such as that of Karlan and List (2007). The second model combines both price and non-price effects and allows for variants that examine specific interaction terms between price and non-price variables.

The non-price variables considered in the estimation comprise a range of socio-demographic attributes of the subject available through the polling company (age, gender, income, education, residence) and attitudinal attributes specific to the public good (beliefs in benefits, familiarity). The nature of the Internet experiment also allows us to observe when exactly subjects completed the experiment and how much time subjects spent at each screen. Finally, we combine the experimental dataset with environmental controls connected to the experiment such as media presence of the public good around the time of the experiment based on media data (LexisNexis) and regional temperature data from the National Weather Service (DWD). Table 1 reports the summary statistics of the sample variables.

Table 2 reports the results of the maximum likelihood (ML) probit regressions of both models. Column 1 corresponds to model 1. The following columns provide variants of model 2 with alternative specifications, focusing on the important results discussed below. The complete results that include fixed effects estimates can be found in the appendix and confirm the robustness of coefficient estimates and significance levels across specifications. The remainder of the section covers the results on the individual price and non-price determinants to arise out of the different specifications.

3.2.1 Price effects

The first result is that price matters: Converting probit coefficient estimates of the experimental price (*cash prize*) into marginal effects (table 3), we find that a price increase of €1 at the sample mean decreases the propensity to choose the EUA retirement by approximately 0.1 percent. The effect therefore has the desired negative sign, is significant at the one percent level and robust across specifications. At the same time, price does not matter substantially. A one Euro increase shifts an average of about 2.4 subjects from contributing to not contributing. This means that while there *are* subjects populating the margin, they are few. The robustness across specifications means that the magnitude of the price effect changes only slightly when allowing for both price and non-price effects in model 2.

The result on the presence and statistical relevance of the price effect is in line with the clear predictions of standard theories of private contributions to

Table 1: Summary statistics

Variable	Description	Mean	S.d.	Obs.
Socio-demographic characteristics				
Female	Indicator variable for gender, 1 if female	0.470	0.499	2,357
Age	Age of subject (years)	45.42	14.68	2,355
Years of education	Based on subject's stated highest educational degree	12.28	3.217	2,302
Income	Midpoint[a] of subject's reported monthly net income category (Euros)	2,557	1,709	1,953
Environmental controls				
Ambient temperature	Mean temperature in subject's region of residence before subject started experiment[b] (°C)	15.1	4.189	2,303
Media attention	Number of hits for a climate change related keyword search[c] in German print and online media before subject started experiment[b]	136.9	28.12	2,303
Climate change attitudes and beliefs				
Personal benefits	Believes in personal benefits from effects of carbon emissions reductions[d]	2.365	0.990	2,098
Future benefits	Believes in benefits for following generations from today's emissions reductions[d]	2.899	0.969	2,115
Negative contributions	Believes that personal lifestyle has contributed to climate change[d]	2.758	0.952	2,094
Footprint estimate	Estimate of yearly CO_2 emissions from lifestyle (tons)	3,017[e]	15,330	2,357
Footprint confidence	Confidence in the footprint estimate given, 1 if at least "rather sure"	0.074	0.262	2,357
EUA price estimate	Estimate of the spot price of EU emissions allowances	1,652	10,300	2,357
EUA price confidence	Confidence in the price estimate given, 1 if at least "rather sure"	0.106	0.308	2,357
EUA availability	Believes EUAs would be available for purchase by private individuals (1 if at least "rather yes")	0.197	0.398	2,357

Notes: [a] In our income approximation, for the 'less than €500' category, we assume €450. For the two categories above €5,000, we assume €8,000 for compatibility with German census data. The remaining categories have widths of €500. [b] Average of the daily values of the day of the experiment and the day before [c] Keywords used: '*climate change*', '*climate protection*', '*global warming*', '*carbon dioxide*', 'CO_2' [d] 1='no', 2='rather no', 3='rather yes', 4='yes' [e] Median is 10

Table 2: Probit coefficient estimates

	Model 1	*Variants of model 2*		
	(1)	(2)	(3)	(4)
Cash prize	-0.0038***	-0.0040***	-0.0048***	-0.0056***
	(0.001)	(0.001)	(0.001)	(0.002)
Demographics				
Female	–	0.1018	0.0435	0.0390
		(0.074)	(0.084)	(0.085)
Age	–	0.0035	0.0032	0.0034
		(0.002)	(0.003)	(0.003)
Years of education	–	0.0594***	0.0551***	0.0609***
		(0.011)	(0.012)	(0.012)
Net income (T€)	–	-0.0133	-0.0188	-0.0135
		(0.021)	(0.024)	(0.024)
Environmental controls				
Ambient temperature	–	–	0.0202**	0.0219**
			(0.009)	(0.009)
Climate change attitudes and beliefs				
Personal benefits	–	–	0.1625***	0.1900***
			(0.056)	(0.057)
Future benefits	–	–	0.2248***	0.2179***
			(0.063)	(0.065)
Negative contributions	–	–	0.1714***	0.1934***
			(0.055)	(0.058)
Footprint estimate (Tt)	–	–	-0.0028	-0.0030
			(0.003)	(0.003)
Footprint confidence	–	–	-0.6510***	0.7308
			(0.181)	(0.627)
EUA price estimate (T€)	–	–	0.0058*	0.0059*
			(0.003)	(0.003)
EUA price confidence	–	–	0.2727**	0.2761**
			(0.134)	(0.136)
EUA availability	–	–	-0.0818	-0.0832
			(0.096)	(0.096)
Survey completion time	–	–	0.0005	0.0007
			(0.001)	(0.001)
Interaction terms				
Cashprize *	–	–	–	0.0014***
years of education				(0.000)
Cashprize *	–	–	–	0.0028
personal benefits				(0.002)
Cashprize *	–	–	–	-0.0013
future benefits				(0.002)
Cashprize *	–	–	–	-0.0001
negative contributions				(0.002)
Cashprize *	–	–	–	-0.0066
footprint confidence				(0.007)
Cashprize *	–	–	–	-0.0007
EUA price confidence				(0.005)
Footprint confidence *	–	–	–	-0.4302**
negative contributions				(0.194)
Footprint confidence *	–	–	–	-0.0001
footprint estimate				(0.000)
EUA price confidence *	–	–	–	-0.0950
EUA price estimate				(0.303)
Constant	-0.7965***	-1.7152***	-3.4114***	-1.5418***
	(0.061)	(0.195)	(0.314)	(0.227)
N	2357.000	1920.000	1600.000	1600.000
Log-likelihood	-1038.039	-821.496	-650.457	-639.513
χ^2	12.635	44.678	179.905	201.792
Pseudo R^2	0.006	0.026	0.121	0.136

Notes: Dependent variable: 1 if subject chose the contribution over the cash award. Standard errors in parentheses. *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent

Table 3: Marginal effects

	Model 1	*Variants of model 2*		
	(1)	(2)	(3)	(4)
Cash prize	-0.0009***	-0.0009***	-0.0011***	-0.0012***
	(0.000)	(0.000)	(0.000)	(0.000)
Demographics				
Female (d)	–	0.0243	0.0098	0.0084
		(0.018)	(0.019)	(0.019)
Age	–	0.0008	0.0007	0.0007
		(0.001)	(0.001)	(0.001)
Years of education	–	0.0141***	0.0124***	0.0132***
		(0.003)	(0.003)	(0.003)
Net income (T€)	–	-0.0032	-0.0042	-0.0029
		(0.005)	(0.005)	(0.005)
Environmental controls				
Ambient temperature	–	–	0.0045**	0.0047**
			(0.002)	(0.002)
Climate change attitudes and beliefs				
Personal benefits	–	–	0.0364***	0.0411***
			(0.012)	(0.012)
Future benefits	–	–	0.0504***	0.0471***
			(0.014)	(0.014)
Negative contributions	–	–	0.0384***	0.0418***
			(0.012)	(0.012)
Footprint estimate (Tt)	–	–	-0.0006	-0.0006
			(0.001)	(0.001)
Footprint confidence (d)	–	–	-0.1064***	0.2100
			(0.020)	(0.220)
EUA price estimate (T€)	–	–	0.0013*	0.0013*
			(0.001)	(0.001)
EUA price confidence (d)	–	–	0.0681*	0.0668*
			(0.037)	(0.036)
EUA availability (d)	–	–	-0.0179	-0.0175
			(0.020)	(0.020)
Survey completion time	–	–	0.0001	0.0001
			(0.000)	(0.000)
Interaction terms				
Cashprize *	–	–	–	0.0003***
years of education				(0.000)
Cashprize *	–	–	–	0.0006
personal benefits				(0.000)
Cashprize *	–	–	–	-0.0003
future benefits				(0.000)
Cashprize *	–	–	–	-0.0000
negative contributions				(0.000)
Cashprize *	–	–	–	-0.0014
footprint confidence				(0.001)
Cashprize *	–	–	–	-0.0001
EUA price confidence				(0.001)
Footprint confidence *	–	–	–	-0.0930**
negative contributions				(0.042)
Footprint confidence *	–	–	–	-0.0000
footprint estimate				(0.000)
EUA price confidence *	–	–	–	-0.0206
EUA price estimate				(0.065)
N	2357.000	1920.000	1600.000	1600.000
Log-likelihood	-1038.039	-821.496	-650.457	-639.513
χ^2	12.635	44.678	179.905	201.792
Pseudo R^2	0.006	0.026	0.121	0.136

Notes: Marginal effects evaluated at the sample means. (d) denotes the marginal effect of an indicator variable. Dependent variable: 1 if subject chose the contribution over the cash award. Standard errors in parentheses. *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent

public goods (Bergstrom et al. 1986, Andreoni 1990): At the extensive margin of whether to contribute or not, a higher price of donating decreases the probability of contributing. In this, our findings provide further confirmation of the field results by Karlan and List (2007). They find a significant price effect at the extensive margin by introducing a match (i.e. reducing the cost of contribution below the field price), but no effect through step-wise variations in the match. In this point, our findings differ: When allowing for non-linearities, we find the most significant and largest effect of price on the contribution decision at low prices, especially below the field price.[19].

Overall the price effect is small and recalls previous results on the price elasticity of demand for public goods (e.g. Green 1993). One argument is that in decisions about public goods, in particular political or charitable goods, non-price factors such as moral and ethical considerations may dominate price consideration. We examine a number of plausible proxies for these considerations in the following section on non-price effects. Another argument is that the insensitivity with respect to price is the result of possible confounding effects of experimental prices on valuation when subjects are poorly informed or unfamiliar with the good (Green 1992, List and Jason 1999): Higher prices offered might conceivably lead uninformed subjects to infer that the good is more valuable. To test for the possibility of a confounding price effect, we re-estimate the model with interaction terms between price and variables that are likely to be associated with greater familiarity with the good such as subjects' confidence in their knowledge about the donation context (confidence in own carbon footprint estimate, confidence in EUA price estimate) and their education. A confounding effect would mean that better informed subjects should be more price sensitive compared to less informed subjects, who would be more likely to base their valuation of the contribution on the cash prize offered in the lottery. We find either no or a positive relationship between the propensity to provide the mitigation effort and the "information-weighted" price (see column 4 in tables 2 and 3): Contrary to the hypothesis of the confounding price effect, more familiarity does not change the price elasticity of contributing or decreases it. This resonates with experimental findings that price elasticity does not systematically vary with uncertainty about good characteristics (Heffetz and Shaya 2009).

3.2.2 Non-price effects

The non-price variables that can be considered as possible drivers of the contribution decision fall into two categories. One set of variables consists of characteristics that are truly exogenous to the experiment. These are subjects' socio-demographic attributes such as education, gender, and age as well as the environmental controls that inform the experimenter about the time and likely place of the subject's participation. The published literature allows us to compare and contrast our findings with those in other laboratory and field experiments in which data on these characteristics have been collected.

The other set of variables, e.g. benefits and negative contributions, consists of characteristics that are based on subjects' statements elicited *after* the contribution decision and, for those choosing the cash prize, *after* explaining their choice. These variables provide an opportunity to give experimental traction

[19] Estimation results are available upon request.

to some of the more recent attempts to understand psychological drivers of contributions. Subjects' perception of benefits of their contribution to future generations or their assessment of own previous negative contributions to the public good can be plausibly linked to motives of altruism or a sense of moral responsibility. However, observations based on statements made *ex post* are inherently problematic as subjects may answer not only truthfully, but also strategically. As a result, the relevant results need to be interpreted cautiously.

Demographics Subjects' profiles on record with the polling company provide the bulk of socioeconomic data on the subject sample. As an aggregate, the sample is representative for the population and yet allows linking contribution decisions to a subject's age, gender, income, and education on an individual basis. All four socioeconomic dimensions have received some attention in the literature so far. List (2004) succinctly sums up experimental evidence on the socioeconomic drivers of a failure to contribute in public goods games through his dictum of "young, selfish, and male".

Gender differences with respect to social preferences have attracted a great deal of attention in the past. In a recent review of the literature, Croson and Gneezy (2009) report that the behavioral salience of gender in areas such as risk taking or competition is well understood by now. The evidence that gender differerences are present in public goods settings, however, is less clear-cut (see Croson and Gneezy 2009 and references therein). Also, Andreoni and Vesterlund (2001) point to the possible subtleties in examining the impact of gender on behavior in social dilemmas: In a laboratory setting, they find male subjects to be more altruistic than female subjects when the price of giving is low, and vice versa.

The regression results of table 2 report on the presence of a simple shift effect of gender on the probability of contributing. The estimated coefficient for female gender is positive, but never significant. Following the findings by Andreoni and Vesterlund (2001), we also test for a possible price-gender interaction term to allow for elasticities to differ between men and women and again find no evidence for a gender effect in the present setting. Taken together, these results strengthen the currently equivocal evidence on gender effects in public goods settings.

Along gender effects, *age* has also started to attract some attention as a determinant of behavior in public goods settings (List 2004, Harbaugh and Krause 2000). List (2004) and Carpenter, Connolly and Myers (2008), for example, find that social preferences increase with age in laboratory public good games and charitable donations experiments. The specifics of our field experiment, however, provide reasons for expecting a positive age effect to be tempered, neutralized, or even reversed. Well-informed subjects may reason that due to the inertia of the climate system, contributions today create public goods far in the future, three to five decades from now. Subjects and their cohort are therefore less likely to benefit the older they are today. The net effect of a possible age-induced strengthening of social preferences on the one hand and an age-related decrease in personal benefits from GHG emissions reductions on the other is unclear ex ante.

To test how subjects view the intertemporal nature of the public good we include interaction terms of age with perceptions of personal or next generation

benefits from mitigation efforts but fail to establish a significant non-linear effect of age.[20] Overall, there is no evidence that age influences the probability of contributing in our experiment, as tables 2 and 3 show. While positive, the coefficient estimate fails significance tests even at the 10 percent level.

The effect of *income* is insignificant in every model specification.[21] While surprising in the context of the tax rebate literature (Auten et al. 2002), income elasticities of contribution close to zero have also been reported in a field experiment on charitable contributions by Eckel and Grossman (2008). However, the authors warn against overinterpreting the result due to data limitations. In the present experiment, income data is indeed available on an individual level. While this strengthens the plausibility of the results, caution is advised as income is self-reported and therefore subject to potential biases and reporting is somewhat incomplete.

Among otherwise inconclusive socioeconomic determinants, *education* stands out as highly significant across all specifications. As the results on marginal effects in table 3 show, subjects' propensity to contribute increases by as much as one percent for a every year spent in education. Both the presence and strength of the education effect are interesting. Many papers studying charitable behavior do not report on the educational status of participants. Notable exceptions are List (2004) and Karlan (2005): In three field experiments measuring social preferences reported by List (2004), education is either insignificant or weakly associated with higher contributions. On the other hand, in an experimental study in the context of a Peruvian microcredit program, Karlan (2005) finds that educational attainment is a determinant of observed behavior in a number of archetypical strategic situations such as the trust game, but is not associated with a greater willingness to contribute in public good games.

If pro-social behavior is not acquired through education, the strong relationship observed in the data must arise from a different source. One possible explanation for a strong education effect relates to the specific public good used in the experiment: The effect of emission reductions is complicated by the inherent long-term nature and complexity of climate change. Patience and cognitive ability are therefore likely to matter. A number of empirical studies link cognitive ability and its proxy, education, with lower discount rates when assessing future costs and benefits and with overall stronger forward-looking behavior by individuals (Parker and Fischhoff 2005, Bettinger and Slonim 2007, Kirby, Winston and Santiesteban 2005). Other studies emphasize the lower cognitive cost to abler individuals of making decisions in complex settings (Peters, Västfjäll, Slovic, Mertz, Mazzocco and Dickert 2006). Against the background of self-reported income, another explanation is that education is a possible alternative measure of income and wealth. Since both tend to be positively correlated with cognitive ability (Banks and Oldfield 2007), this provides an additional causal channel through which education could enter as a significant explanatory variable.

Environmental controls The sample of subjects taking part in the experiment is drawn from all over Germany, introducing possibly important spatial

[20] Results are available upon request.

[21] Data speaks against multicollinearity of income and education as explanation for the persistent insignificance of the one and strong significance of the other: excluding education or any other variable does not induce income to pick up an effect (Correlation coefficient 0.29).

or structural determinants of behavior that are easily overlooked. At the same time, situational factors may play a role in explaining observed variations in contribution choices between subjects. We pursue two distinct strategies to account for these possibilities. One is to estimate the models with fixed effects for location and time, exploiting the polling company's records on each panel member and the time stamps recorded for each screen completed in the experiment. The results of this exercise are reported in columns 3 to 5 of tables 5 and 6 in the appendix. The main message of the results is to underscore the robustness of the coefficient estimates derived under simpler specifications.

The other strategy we pursue is to examine the applicability to the experiment of claims that the behavior in public goods settings may not only be determined by cognitive processes. Konow (2010) and Kirchsteiger et al. (2006), for example, are among recent papers that demonstrate that emotional states or 'moods' have explanatory power in such settings. Kirchsteiger et al. (2006), for example, 'engineer' moods by exposing subjects to 'sad' and 'happy' movies. Konow (2010) varies the emotional context by varying the recipient group in a dictator game. Both find impacts on contribution choices.

Our approach employs a different strategy, with results reported in columns 3 and 4 of tables 2 and 3: We exploit exogenously generated variations in the decision environment of our subjects by drawing on regionally disaggregated meteorological data from the National Weather Service (DWD). We are specifically interested in outside temperatures present at or close to the time and location of the individual experiment: Subjects' perception of the salience of climate change or 'moods' related to climate change mitigation efforts may plausibly be linked to such environmental conditions. Linking contribution choices and temperature data in the subject's region of residence, we find that the probability of contributing increases with ambient temperatures where temperature is computed as the average of the mean temperatures on the day the subject took the experiment and on the day before.[22] More specifically, we find that an increase of one degree centigrade in ambient mean temperature increases the probability to contribute by around 0.4 percent. In relative terms, the marginal effect of one degree centigrade on the probability of contributing is therefore four times that of a one Euro price decrease. Including media presence of the topic of climate change alongside the temperature data does not appreciably change this estimate.

While the results on situational factors may provide evidence on the presence of a very different type of '*warm glow*' in our field setting, there is the possibility that ambient temperatures either proxy for a different driver or that other conventional drivers linked to temperature are present. For example, ambient temperatures may simply proxy for latitude or similar spatial effects. Including latitude alongside the temperature variable marginally affects the p-values of coefficient estimates,[23] but does not alter the results otherwise. The effect of temperature may also be explained by other unobserved selection biases or socioeconomic factors that make ambient temperatures particularly salient to subjects, such as employment in the construction sector, even though the socioeconomic data provides no further evidence for this hypothesis.

[22] The results also hold for daily temperature maxima and for a larger time window of 48 hours. These results are excluded for brevity and available from the authors.

[23] E.g. the p-value of the 24h temperature variable increases from p=0.031 to p=0.034.

Benefits Asking subjects to provide an *ex post* assessment of perceived benefits from a public goods contribution chosen or not chosen may seem superfluous on the grounds of both theory and preference revelation. The *ex post* assessment of stated benefits, however, is useful for informing a broader behavioral model determining choice between the alternatives.

We solicit statements on perceived personal and next generation benefits from all subjects in the experiment. The results reported in tables 2 and 3 are in line with expectations: A greater perception of total benefits (personal and next generation) increases the probability of contributing. What is of interest, however, is the relative role of personal and next generation benefits in explaining contributions. Table 3 reports on the marginal impact of a one-unit (response category) increase in both variables. A one-unit increase in next generation benefits raises the probability of contributing by almost 5.0 percent. The same increase in the expectation of personal benefits, on the other hand, increases the probability by 3.6 percent. The marginal effect of the altruistic motive at the sample mean is therefore one-and-a-half times as strong as that of personal benefits.

Negative contributions to the public good Subjects can make and usually will have made negative contributions to the public good under consideration in this field experiment: Carbon emissions are coupled to most economic activities that subjects will be engaged in. If subjects rationally take into consideration their negative contributions to the large-scale public good when making the contribution decision in the experiment, the predictions regarding the probability of contributing differ markedly from a model where they do not (Vicary 2000). Following a closely related line of reasoning, Kotchen (2009) derives the resultant demand for contributions to the public good that are driven by a desire to 'offset' simultaneous negative contributions. These extensions of the traditional model of private provision of public goods provide explanations for contribution rates that exceed those predicted under conventional assumptions such as Andreoni (1988) and lead to a prediction that subjects with higher negative contributions to the public good should have a higher probability of contributing positively when provided with the opportunity.

Theories of offset are not the only possible explanation for a positive relationship between acknowledged negative contributions and observed positive contributions. Similar qualitative predictions can arise out of theories that consider guilt (Battigalli and Dufwenberg 2007) or shame (Andreoni and Petrie 2004) as drivers. In both theories, perceptions of norms or 'expected behavior' play a role. If the amount of negative contributions exceeds a normative target level, guilt or shame provide arguable reasons for deciding to contribute in the experiment, given the opportunity. Theories of environmental offset, guilt and shame jointly provide three possible reasons why lifestyle-related negative contributions to the public good may matter for the contribution decision.

The experiment offers subjects the opportunity to acknowledge a personal negative contribution to climate change through two channels. One invites subjects to rank on a scale of 1 to 4 the extent to which they believe their lifestyle has contributed to climate change. The other asks subjects about a quantitative estimate of their annual carbon footprint in tons of carbon. Tables 2 and 3 report on the results of testing for evidence of negative contributions being a driver

of contribution decisions: Carbon footprint estimates exhibit high noise and do not return significant results. Contribution ranks, on the other hand, are linked to subjects' contribution choice in a positive and significant way: An increase in one rank is associated with an approximately four percent higher probability of contributing. This 'acknowledgement effect' is in the same order of magnitude as the perception of personal and future generation benefits, with a quantitative impact roughly at the same level as that of perceived private benefits from a contribution decision, but consistently smaller than that of perceived future generation benefits.

One exception to the acknowledgement effect is a small group of subjects (7.4 percent) who claim confidence in their estimate of their carbon footprint. As column 4 of tables 2 and 3 shows, the behavior of this group accounts for the negative effect of footprint confidence on the probability of contributing reported in column 3. Interacting footprint confidence with the subject's contribution rank we find that this group is significantly more likely to choose the cash prize. Closer examination of this group reveals two important distinguishing characteristics: On the one hand, members of this group tend to provide footprint estimates that are more plausible than the average. On the other, these subjects are readier to acknowledge their own negative contributions to the public good. The negative effect of the interaction of both characteristics on the contribution choice could be due to a number of different reasons. One possibility is that this small group consists of highly climate-conscious subjects who believe to be already contributing to emissions reductions in a major way. This could explain why we find no acknowledgement effect among this group.

3.3 Field price censoring or moral economy? - A test

Of the 2,440 subjects in the experiment, 2,058 choose the cash prize. The experimental protocol for FPC directs these subjects to two *ex post* questions[24] targeted at the reasons for choosing the cash prize. The questions sort subjects into two groups: The one group consists of subjects that respond that given the alternatives, the cash prize was strictly preferable to contributing to the public good. This group makes up 72.7 percent of those choosing the cash prize. The other group consists of subjects that declare that at the given experimental price, they would make a contribution, but chose not to because they believe they can make the same contribution to the public good at a lower price elsewhere. This 'FPC' group makes up 25.8 percent of subjects.

The key question is how to interpret the 'FPC' group. Subjects selecting into this group may indeed be field price censored, in line with the design of the ex post questionnaire (Coller and Williams 1999, Harrison et al. 2002, Corrigan and Rousu 2008). A closer examination of the data cautions against this interpretation, however: Column 1 of table 4 reports the results of a probit estimation that tries to explain the decision to declare FPC as a function of plausible and observable drivers of arbitrage opportunities available to the subject. Such drivers are the offered cash prize, the subject's estimate of and confidence in the EUA price, knowledge about transactions costs, knowledge about and confidence in the individual carbon footprint, and the time a subject

[24] In reality, there are three questions: two given and one free text. However, with the exception of 116 subjects (cp. footnote 16), all free text answers provided paraphrases of the given questions and could therefore be reassigned.

takes to complete different screens, indicating either careful consideration or active information acquisition.[25] In addition, the model includes the subject's socio-demographic characteristics. The model performs weakly in explaining subjects' statement for a number of reasons: Given the random assignment of subjects to price treatments, the probability of the experimental price exceeding the subject's perceived field price strictly increases in the experimental price. The model, however, estimates a decreasing effect of the experimental price. Secondly, subjects' price estimate of the field substitute should enter negatively into the estimation. This price estimate is not directly observable, but is likely to be correlated with subjects' perceptions of the EUA price and their carbon footprints. While some of these proxies show the expected sign, none of them provides a statistically significant contribution to explaining why subjects sort into the 'FPC' group. Thirdly, variables on subjects' time stamps also show no significant effect. With the exception of education, which has an ambiguous interpretation (see below), the arbitrage mechanism underpinning FPC lacks support by the experimental data.

An alternative interpretation of the 'FPC' group is the possibility that there is moral economy at work that provides subjects with an incentive to declare to be driven by FPC even when they are not. This possibility has been raised by other scholars in the field (Karlan and List 2007, Kahneman and Knetsch 1992). Non-contributors to public goods may feel discomfort on account of their behavior deviating from some notion of 'expected behavior' or behavioral norm. Economic theories of self-image or guilt (Battigalli and Dufwenberg 2007), for example, consider norms imposed on oneself, economic theories of image (Benabou and Tirole 2006) and shame (Andreoni and Petrie 2004) consider norms held by peers or other individuals, often of a higher social standing, whose respect matters to the subject. There may be subjects for whom the perceived behavioral norm is that individuals ought to contribute to public goods unless doing so is excessively costly. If such subjects choose the cash instead, the FPC filter presents an opportunity to explain their decision to an outside observer as motivated by objective cost-benefit considerations rather than personal gain.

Column 2 of table 4 reports the results of a possible 'moral economy' model that includes motivational variables. Once a notion of shame avoidance *à la* Andreoni and Petrie (2004) is considered as a driver,[26] the negative and sta-

[25] Evidence for information acquisition during the experiment, e.g. by searching the Internet for EUA spot prices, comes from a careful examination of the time stamps of each screen in each individual experiment. The time stamp measures the exact time at which the subject moved on to the next screen. As information collection requires time for targeted search, search activity should be associated with time delay at screens that ask for relevant information relative to other screens. We impose ambitious assumptions on how quickly a subject can collect the information: For example, subjects would need to find EUA prices and information on annual per capita emissions on the Internet in under 2 minutes while pilot data indicates the average subject requires between 3 and 5 minutes to locate relevant information. We find no more than 1.4 percent of subjects with time delays that would be consistent with information collection. In addition, these candidates do not exhibit above average accuracy on the factual questions in the experiment. On this basis, we conclude that information acquisition does not play a role in explaining the results. Importantly, this result also means that the field-price censoring observed is not a product of endogenous information acquisition by subjects during the experiment, but can at most be generated by exogenous differences in information.

[26] Among guilt, image and shame, shame has most theoretical traction in the present setting: If subjects felt guilty for having chosen the cash, claiming in the follow-up questions to be field price censored when they were not, only compounds the previous deviation from expected behavior with a subsequent untruthfulness. Image concern vis-a-vis a group of peers also

Table 4: Arbitrage vs. morals: alternative explanations for declaring FPC

	'Arbitrage'	*'Moral'*	*Combined*
	(1)	(2)	(3)
Cash prize	-0.0023*	-0.0033**	-0.0034**
	(0.001)	(0.001)	(0.001)
EUA price estimate (T€)	-0.0046	–	-0.0112**
	(0.004)		(0.005)
EUA price confidence	0.1430	–	0.1224
	(0.129)		(0.136)
EUAs available	-0.0584	–	-0.0885
	(0.090)		(0.098)
Footprint estimate (Tt)	-0.0026	–	-0.0008
	(0.002)		(0.002)
Footprint confidence	-0.0214	–	-0.0502
	(0.144)		(0.153)
Survey completion time	-0.0001	–	0.0018
	(0.000)		(0.002)
Lottery screens completion time	0.0001	–	0.0001
	(0.000)		(0.000)
Personal benefits	–	-0.2023***	-0.2117***
		(0.054)	(0.055)
Future benefits	–	0.1398***	0.1544***
		(0.054)	(0.054)
Negative contributions	–	0.2198***	0.2205***
		(0.048)	(0.049)
Female	-0.0641	-0.0249	-0.0224
	(0.072)	(0.080)	(0.080)
Age	0.0062**	0.0064**	0.0060**
	(0.003)	(0.003)	(0.003)
Years of education	0.0357***	0.0327***	0.0321***
	(0.011)	(0.012)	(0.012)
Net income (T€)	0.0199	0.0165	0.0168
	(0.021)	(0.022)	(0.022)
Ambient temperature	–	-0.0067	-0.0060
		(0.009)	(0.009)
Constant	-1.2601***	-1.6102***	-1.6171***
	(0.201)	(0.293)	(0.299)
N	1602.000	1321.000	1311.000
Log-likelihood	-893.933	-737.793	-725.181
χ^2	36.562	62.051	70.985
Pseudo R^2	0.020	0.040	0.047

Notes: Probit coefficient estimates. Standard errors in parentheses. Dependent variable: 1 if subject declared that cheaper perceived outside opportunities prevented the choice of the contribution (field-price censoring). *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent

tistically significant coefficient on the experimental price would be consistent with the idea that those subjects have most to gain in moral terms from declaring FPC who were offered the lowest experimental price. This would be the case if shame is higher the smaller the personal sacrifice required for the public good. The results on the motivational variables underline the importance of declaring not to be motivated by personal gain: A more emphatic belief in next generation benefits of the public goods contribution and a greater acknowledgement of negative lifestyle-related impacts on the public good increase the likelihood of claiming to be FPC. This is consistent if subjects believe that the '*expected behavior*' is to be more willing to contribute for public goods that provide greater benefits and to be willing to compensate the public for damages imposed through own behavior. The converse also holds: Subjects that attach personal benefits to contributing to the public good would see less of a moral dimension, feel less shame, and are therefore less likely to claim FPC, consistent with the estimation results.

The last column of table 4 merges both models as a robustness check. The experimental price effect remains negative and highly significant. For the variables related to the perceived field price, the combined model identifies a significantly negative effect of the EUA price estimate, providing stronger evidence of a genuine FPC component. While not consistent with a simplistic interpretation of shame avoidance[27], the effect is quantitatively weak.[28] In addition, the caveat that the subject provides its field price estimate after passing the FPC filter applies. The education and the age effect are reconcilable with both models: Education and age may be associated with better information about alternative ways of providing the public good and hence give rise to genuine FPC. Alternatively, more highly educated and older subjects may be more acutely aware of notions of expected behavior or be wealthier, both of which would increase the moral gain for claiming not to be driven by personal gain. We test additional specifications to check for interaction effects between gender and price of the type predicted by Andreoni and Vesterlund (2001) and for shift effects that predict that male subjects are more likely to claim the hidden altruist status (Brown and Taylor 2000). These tests fail to return statistically salient results.[29]

4 Conclusion

The role of price and non-price effects in explaining voluntary contributions to public goods in a large economy remains an area of important debate among economists. More empirical and experimental evidence is required in order to assemble the building blocks for a more comprehensive theoretical model of voluntary giving. In this spirit, we conducted a large-scale field experiment

has limited traction here as there is no visibility of contribution decisions to other subjects. Shame has the greatest traction since it combines the feeling of an objective standard of right behavior with an authority gradient that makes the experimentor as only observer also the implicit judge of the subject's behavior.

[27] A higher field price should *ceteris paribus* be associated with more shame for not having provided the public good when it was relatively cheap to do so.

[28] Note that the EUA field price is measured in thousand Euros.

[29] Likewise, testing for interaction effects of the experimental price with the remaining determinants in the FPC model does not yield significant results.

with 2,440 subjects in which we exogenously vary the price of contributing to the closest empirical counterpart to an infinitely large public goods game, climate change mitigation, while simultaneously observing a large number of possible non-price determinants.

We find that the price effect is robust and negative, but quantitatively weak, with a price elasticity of -0.25. The weakness of the absolute price effect puts the onus of explaining the evidence on non-price variables. These variables deliver in both expected and unexpected ways. Among socioeconomic variables, education stands out as a key determinant. Keeping in mind the possible limitations of self-reported income data and the lack of an established education-social preference channel in the literature, the role of education could be due to both cognitive and income and wealth effects. For gender and age, on the other hand, the literature provides reasons for expecting a significant role, but gender and age effects fail to materialize in the experiment. Instead, situational variables such as ambient temperatures around the time of the experiment are statistically related to contribution decisions. Most importantly perhaps, variables that can be plausibly linked to guilt and moral responsibility dominate the price effect.

The importance of non-price factors such as moral gain through contributing is underlined by evidence that arises from the battery of questions originally designed to detect field price censoring: A different interpretation of the FPC filter is that it offers non-contributors a costless opportunity to claim that the experimental design prevented them from revealing their 'true' altruistic nature by setting the price of contributing too high. Contrary to the prediction of a negative price effect by the FPC hypothesis, we find that the probability of claiming to be a 'hidden altruist' is higher the lower the experimental price. Our favored interpretation of this finding builds on the notion of a 'moral economy' in which subjects compare expected and actual behavior. When giving is cheap, the expected behavior is to give. Not giving when giving is cheap reveals a person to be overly motivated by personal gain at the expense of the common good. Subjects that choose the cash over the contribution when the contribution is cheap therefore have most to gain in moral terms by claiming to be field price censored. This behavior lends further support to the direction and scale of moral factors and is a useful point of departure for further work.

References

Abdellaoui, M., Baillon, A., Placido, L. and Wakker, P. P.: 2011, The rich domain of uncertainty: Source functions and their experimental implementation, *The American Economic Review* **101**, 695–723.

Andreoni, J.: 1988, Privately provided public goods in a large economy: The limits of altruism, *Journal of Public Economics* **35**(1), 57–73.

Andreoni, J.: 1990, Impure altruism and donations to public goods: A theory of warm-glow giving, *Economic Journal* **100**(401), 464–477.

Andreoni, J.: 2006, Philanthropy, *in* K. Serge-Christophe and Y. Jean Mercier (eds), *Handbook on the Economics of Giving, Reciprocity and Altruism*, Vol. 2, Elsevier, pp. 1201–1269.

Andreoni, J. and Miller, J.: 2002, Giving according to GARP: An experimental test of the consistency of preferences for altruism, *Econometrica* **70**(2), 737–753.

Andreoni, J. and Petrie, R.: 2004, Public goods experiments without confidentiality: a glimpse into fund-raising, *Journal of Public Economics* **88**(7-8).

Andreoni, J. and Vesterlund, L.: 2001, Which is the fair sex? Gender differences in altruism, *The Quarterly Journal of Economics* **116**(1), 293–312.

Arrow, K. J., Solow, R., Portney, P. R., Leamer, E. E., Radner, R. and Schuman, H.: 1993, Report of the NOAA Panel on Contingent Valuation, *Technical report*. Washington D.C.

Auten, G. E., Sieg, H. and Clotfelter, C. T.: 2002, Charitable giving, income, and taxes: An analysis of panel data, *The American Economic Review* **92**(1), 371–382.

Baltussen, G., Post, T., Van den Assem, M. J. and Wakker, P. P.: 2010, Random incentive systems in a dynamic choice experiment. Working Paper.

Banks, J. and Oldfield, Z.: 2007, Understanding pensions: Cognitive function, numerical ability and retirement saving, *Fiscal Studies* **28**(2), 143–170.

Baron, J. and Greene, J.: 1996, Determinants of insensitivity to quantity in valuation of public goods: Contribution, warm glow, budget constraints, availability, and prominence, *Journal of Experimental Psychology: Applied* **2**(2), 107–125.

Battigalli, P. and Dufwenberg, M.: 2007, Guilt in games, *American Economic Review* **97**(2), 170–176.

Benabou, R. and Tirole, J.: 2006, Incentives and prosocial behavior, *American Economic Review* **96**(5), 1652–1678.

Bergstrom, T., Blume, L. and Varian, H.: 1986, On the private provision of public goods, *Journal of Public Economics* **29**(1), 25–49.

Bettinger, E. and Slonim, R.: 2007, Patience among children, *Journal of Public Economics* **91**(1-2), 343–363.

Bolle, F.: 1990, High reward experiments without high expenditure for the experimenter?, *Journal of Economic Psychology* **11**(2), 157–167.

Brown, K. M. and Taylor, L. O.: 2000, Do as you say, say as you do: evidence on gender differences in actual and stated contributions to public goods, *Journal of Economic Behavior & Organization* **43**(1), 127–139.

Carpenter, J., Connolly, C. and Myers, C.: 2008, Altruistic behavior in a representative dictator experiment, *Experimental Economics* **11**(3), 282–298.

Cherry, T., Frykblom, P., Shogren, J., List, J. and Sullivan, M.: 2004, Laboratory testbeds and non-market valuation: The case of bidding behavior in a second-price auction with an outside option, *Environmental and Resource Economics* **29**(3), 285–294.

Coller, M. and Williams, M. B.: 1999, Eliciting individual discount rates, *Experimental Economics* **2**(2), 107–127.

Corrigan, J. R. and Rousu, M. C.: 2008, Testing whether field auction experiments are demand revealing in practice, *Journal of Agricultural and Resource Economics* **33**(2), 290–301.

Croson, R. and Gneezy, U.: 2009, Gender differences in preferences, *Journal of Economic Literature* **47**(2), 448–474.

Cubitt, R., Starmer, C. and Sugden, R.: 1998, Dynamic choice and the common ratio effect: an experimental investigation, *Economic Journal* **108**(450), 1362–1380.

Eckel, C. C. and Grossman, P. J.: 2003, Rebate versus matching: does how we subsidize charitable contributions matter?, *Journal of Public Economics* **87**(3-4), 681–701.

Eckel, C. and Grossman, P.: 2008, Subsidizing charitable contributions: a natural field experiment comparing matching and rebate subsidies, *Experimental Economics* **11**(3), 234–252.

Green, D. P.: 1992, The price elasticity of mass preferences, *The American Political Science Review* **86**(1), 128–148.

Grether, D. M. and Plott, C. R.: 1979, Economic theory of choice and the preference reversal phenomenon, *The American Economic Review* **69**(4), 623–638.

Harbaugh, W. T. and Krause, K.: 2000, Children's altruism in public good and dictator experiments, *Economic Inquiry* **38**(1), 95–109.

Harrison, G. W., Harstad, R. M. and Rutström, E. E.: 2004, Experimental methods and elicitation of values, *Experimental Economics* **7**(2), 123–140.

Harrison, G. W., Lau, M. I. and Williams, M. B.: 2002, Estimating individual discount rates in Denmark: A field experiment, *American Economic Review* **92**(5), 1606–1617.

Harrison, G. W. and List, J. A.: 2004, Field experiments, *Journal of Economic Literature* **42**(4), 1009–1055.

Heffetz, O. and Shaya, M.: 2009, How large are non-budget-constraint effects of prices on demand?, *American Economic Journal: Applied Economics* **1**, 170–199.

Kahneman, D. and Knetsch, J. L.: 1992, Valuing public goods: The purchase of moral satisfaction, *Journal of Environmental Economics and Management* **22**(1), 57–70.

Karlan, D. and List, J. A.: 2007, Does price matter in charitable giving? Evidence from a large-scale natural field experiment, *American Economic Review* **97**(5), 1774–1793.

Karlan, D., List, J. A. and Shafir, E.: 2011, Small matches and charitable giving: Evidence from a natural field experiment, *Journal of Public Economics* **95**(5-6), 344–350.

Karlan, D. S.: 2005, Using experimental economics to measure social capital and predict financial decisions, *The American Economic Review* **95**(5), 1688–1699.

Kingma, B. R.: 1989, An accurate measurement of the crowd-out effect, income effect, and price effect for charitable contributions, *The Journal of Political Economy* **97**(5), 1197–1207.

Kirby, K. N., Winston, G. C. and Santiesteban, M.: 2005, Impatience and grades: Delay-discount rates correlate negatively with college GPA, *Learning and Individual Differences* **15**(3), 213–222.

Kirchsteiger, G., Rigotti, L. and Rustichini, A.: 2006, Your morals might be your moods, *Journal of Economic Behavior & Organization* **59**(2), 155–172.

Konow, J.: 2010, Mixed feelings: Theories of and evidence on giving, *Journal of Public Economics* **94**(3-4), 279–297.

Kotchen, M. J.: 2009, Voluntary provision of public goods for bads: A theory of environmental offsets, *Economic Journal* **119**(537), 883–899.

Lee, J.: 2008, The effect of the background risk in a simple chance improving decision model, *Journal of Risk and Uncertainty* **36**(1), 19–41.

List, J. A.: 2004, Young, selfish and male: Field evidence of social preferences, *Economic Journal* **114**(492), 121–149.

List, J. A. and Jason, F. S.: 1999, Price information and bidding behavior in repeated second-price auctions, *American Journal of Agricultural Economics* **81**(4), 942–949.

Morgan, J.: 2000, Financing public goods by means of lotteries, *Review of Economic Studies* **67**(4), 761–784.

Munro, A. and Hanley, N.: 1999, Information, uncertainty, and contingent valuation, *in* I. Bateman, K. G. Willis and K. J. Arrow (eds), *Valuing Environmental Preferences: Theory and Practice of the Contingent Valuation Method in the US, EU, and Developing Countries*, Oxford University Press, Oxford, pp. 258–279.

Nordhaus, W. D.: 1993, Reflections on the economics of climate change, *The Journal of Economic Perspectives* **7**(4), 11–25.

Parker, A. M. and Fischhoff, B.: 2005, Decision-making competence: External validation through an individual-differences approach, *Journal of Behavioral Decision Making* **18**(1), 1–27.

Peloza, J. and Steel, P.: 2005, The price elasticities of charitable contributions: A meta-analysis, *Journal of Public Policy & Marketing* **24**(2), 260–272.

Peters, E., Västfjäll, D., Slovic, P., Mertz, C., Mazzocco, K. and Dickert, S.: 2006, Numeracy and decision making, *Psychological Science* **17**(5), 407–413.

Smith, V. H., Kehoe, M. R. and Cremer, M. E.: 1995, The private provision of public goods: Altruism and voluntary giving, *Journal of Public Economics* **58**(1), 107–126.

Starmer, C. and Sugden, R.: 1991, Does the random-lottery incentive system elicit true preferences? An experimental investigation, *American Economic Review* **81**(4), 971–978.

Tversky, A. and Kahneman, D.: 1981, The framing of decisions and the psychology of choice, *Science* **211**(4481), 453–458.

Vicary, S.: 2000, Donations to a public good in a large economy, *European Economic Review* **44**(3), 609–618.

A Appendix

A.1 Experimental screens

A.1.1 Welcome screen

Dear participants,

we would like to invite you to participate in two lotteries and to answer some questions about CO_2-emissions and climate change.

Your participation will take approximately ten minutes. In the lotteries, you have the chance to win points worth up to a three-digit amount in euros.

As usual, all your information will be treated confidentially.

A.1.2 Citizenship screen

Of which country do you hold citizenship?
In case you hold more than one, please tick all applicable boxes!
(...)

A.1.3 Information screen

In the lotteries, you may choose between the following two prizes:

A cash prize in points
or
the reduction of carbon (CO_2) emissions by 1 ton

How will the reduction of the CO_2 emissions take place? We will make use of a reliable opportunity provided by the EU emissions trading system: We will purchase and delete an *EU emissions allowance* for you. Emissions allowances are needed by power plants and other large installations within the EU in order to be allowed to

emit CO_2. Since there is only a fixed overall amount of allowances in place, deleted ones are no longer available to facilitate emissions. Emissions in Germany and other EU countries decrease by exactly one ton through one deleted allowance.

Because of the way in which CO_2 mixes in the air, it does not matter for the effect on the climate where CO_2 emissions are reduced. What counts is only total emissions worldwide.

In the lotteries, 100 winners will be randomly selected out of about 5,000 participants. The following two lotteries may differ in the prizes offered as well as in the payoff procedures.

A.1.4 Decision screen

In this lottery, you have the choice between the two prizes listed below.

- If you choose the cash amount and win, then the corresponding amount of points will be transferred to your points account within the next few days. All winners will receive a short notification email.
- The deletion of emissions allowances will, in this lottery, take place as a collective order for all winners. For every winner who chooses the emissions reduction one additional allowance will be deleted. Winners will receive a short notification email containing a hyperlink to Heidelberg University webpages where they can reliably verify the deletion.

Please choose now, which prize you prefer if drawn as winner:

( ) The reduction of CO_2 emissions by one ton through the deletion of one EU emissions allowance

( ) 46 Euro[30] in bonus points

A.1.5 FPC filter question

Please give now any particulars as to why you chose the amount in euros. In order to do this, please tick all applicable boxes. Please answer spontaneously.

( ) Given the two prizes, I did not want to forgo the chance of winning 46 euros.

( ) I assume that there is another possibility for me to reduce CO2-emissions by one ton for less than 46 euros.

( ) There were other reasons as to why I chose the amount of euros, namely:

———

A.1.6 Introduction follow-up questions

Thank you. On the following pages we would like to ask you some concluding questions.

[30] Example amount. The order in which the two prizes appeared was randomized.

A.1.7 Follow-up questions

What is your estimate of the current market price for one ton of CO_2 in the EU emissions trading system?

_ _ _ _ euros

How sure are you about your estimate?

( ) I know the price

( ) Very sure

( ) Rather sure

( ) Rather unsure

( ) Very unsure

( ) I don't know

A.1.8 Follow-up questions

In this lottery, EU emission allowances are bought and deleted by the organizer. Do you think that there exists a possibility for you to personally buy and delete EU emissions allowances?

Options: 'Yes', 'rather yes', 'rather no', 'no', 'I don't know'

Do you think you will personally benefit from the positive effects of reduced CO_2 emissions (for example from the mitigation of climate change)?

Options: 'Yes', 'rather yes', 'rather no', 'no', 'I don't know'

Do you think that future generations in Germany (for instance your children and grand-children) will benefit from climate change mitigating CO_2 emissions reductions that are carried out today?

Options: 'Yes', 'rather yes', 'rather no', 'no', 'I don't know'

Do you think that your personal behavior or lifestyle has contributed to climate change?

Options: 'Yes', 'rather yes', 'rather no', 'no', 'I don't know'

A.1.9 Follow-up questions

What is your estimate of the yearly CO_2 emissions caused by your lifestyle?

_ _ _ _ tons

How sure are you about your estimate?

( ) I had the emissions calculated

( ) Very sure

( ) Rather sure

( ) Rather unsure

( ) Very unsure

( ) I don't know

A.1.10 Follow-up questions

Do you consciously act in a climate-protecting way? If yes, please list some forms of behavior, decisions and measures through which you have consciously contributed or are contributing to the reduction of CO_2 or other greenhouse gases (in keywords).

A.1.11 Enquiry of socio-demographic information if not on record

Please state your gender.

( ) male

( ) female

In what year were you born? _ _ _
How many children under 18 live in your household? _ _ _

A.1.12 Enquiry of socio-demographic information if not on record

What is your highest educational degree?

( ) Still in school

( ) Special-needs school

( ) Elementary secondary school ('Hauptschule', 9th grade)

( ) Polytechnic school of the GDR (10th grade)

( ) Highschool ('Realschule', 10th grade)

( ) Advanced technical college entrance qualification

( ) A-levels (12th or 13th grade)

( ) Advanced technical college (Diploma (advanced technical college), Bachelor, Master)

( ) University degree (diploma, magister, bachelor, master)

( ) Ph.D.

( ) Dropout

( ) No specification

What is the overall net income of the household that you live in?

( ) under EUR 500

( ) from EUR 500 up to EUR 1000

( ) from EUR 1000 up to EUR 1500

( ) from EUR 1500 up to EUR 2000

( ) from EUR 2000 up to EUR 2500

( ) from EUR 2500 up to EUR 3000

( ) from EUR 3000 up to EUR 3500

( ) from EUR 3500 up to EUR 4000

( ) from EUR 4000 up to EUR 4500

( ) from EUR 4500 up to EUR 5000

( ) from EUR 5000 up to EUR 10000

( ) EUR 10000 and more

( ) no specification

A.1.13 Closing screen

Dear participant,

Thank you very much for your participation in this survey. If you are one of the winners, we will contact you by e-mail shortly.

B Additional estimation results

Table 5: Additional variants of model 2 (probit coefficient estimates)

	(1)	(2)	(3)	(4)	(5)	(6)
Cash prize	-0.0039***	-0.0048***	-0.0047***	-0.0046***	-0.0052***	-0.0042***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Demographics						
Female	–	0.0397	0.0421	0.0323	0.0309	–
		(0.084)	(0.084)	(0.085)	(0.087)	
Age	–	0.0028	0.0029	0.0026	0.0017	–
		(0.003)	(0.003)	(0.003)	(0.003)	
Years of education	–	0.0555***	0.0546***	0.0585***	0.0618***	0.0483***
		(0.012)	(0.012)	(0.012)	(0.012)	(0.010)
Net income (T€)	–	-0.0198	-0.0189	-0.0292	-0.0330	–
		(0.024)	(0.024)	(0.025)	(0.025)	
Environmental controls						
Ambient temperature	–	0.0169*	-0.0029	0.0030	-0.0174	0.0162*
		(0.010)	(0.014)	(0.017)	(0.027)	(0.009)
Media attention	–	-0.0014	–	–	–	–
		(0.002)				
Climate change attitudes and beliefs						
Personal benefits	0.1464***	0.1619***	0.1608***	0.1522***	0.1660***	0.1406***
	(0.048)	(0.056)	(0.056)	(0.056)	(0.058)	(0.049)
Future benefits	0.2109***	0.2268***	0.2242***	0.2280***	0.2283***	0.2470***
	(0.055)	(0.063)	(0.063)	(0.064)	(0.065)	(0.058)
Negative contributions	0.1633***	0.1718***	0.1743***	0.1852***	0.1744***	0.1627***
	(0.048)	(0.055)	(0.055)	(0.056)	(0.057)	(0.050)
Footprint estimate (Tt)	-0.0027	-0.0028	-0.0027	-0.0030	-0.0030	–
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	
Footprint confidence	-0.5088***	-0.6563***	-0.6552***	-0.6265***	-0.6854***	-0.6026***
	(0.152)	(0.181)	(0.182)	(0.184)	(0.189)	(0.161)
EUA price estimate (T€)	0.0062**	0.0057*	0.0061*	0.0057*	0.0065**	0.0064**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
EUA price confidence	0.2505**	0.2678**	0.2776**	0.2475*	0.2674*	0.2360**
	(0.116)	(0.134)	(0.134)	(0.137)	(0.140)	(0.120)
EUA availability	-0.0070	-0.0836	-0.0776	-0.0905	-0.0747	–
	(0.083)	(0.096)	(0.096)	(0.097)	(0.098)	
Survey completion time	–	0.0006	0.0005	0.0006	0.0005	–
		(0.001)	(0.001)	(0.001)	(0.001)	
Interaction terms						
Cashprize * years of education	–	–	–	–	–	–
Cashprize * personal benefits	–	–	–	–	–	–
Cashprize * future benefits	–	–	–	–	–	–
Cashprize * negative contributions	–	–	–	–	–	–
Cashprize * footprint confidence	–	–	–	–	–	–
Cashprize * EUA price confidence	–	–	–	–	–	–
Footprint confidence * negative contributions	–	–	–	–	–	–
Footprint confidence * footprint estimate	–	–	–	–	–	–
EUA price confidence * EUA price estimate	–	–	–	–	–	–
Constant	-2.2297***	-3.1595***	-3.1437***	-3.2718***	-2.8330***	-3.1773***
	(0.158)	(0.413)	(0.337)	(0.424)	(0.541)	(0.248)
Session effects	No	No	Yes	No	No	No
Day effects	No	No	No	Yes	Yes	No
Daytime effects	No	No	No	No	Yes	No
Region effects	No	No	No	No	Yes	No
N	1983.000	1600.000	1600.000	1600.000	1600.000	1902.000
Log-likelihood	-844.467	-650.018	-648.033	-639.577	-625.393	-789.883
χ^2	162.478	180.783	184.754	201.666	230.033	200.124
Pseudo R^2	0.088	0.122	0.125	0.136	0.155	0.112
AIC	1708.933	1334.036	1330.066	1339.153	1346.786	1599.767
BIC	1764.857	1425.458	1421.487	1500.486	1604.919	1655.273

Notes: Dependent variable: 1 if subject chose the contribution over the cash award. 'Session effects' is an indicator variable and 1 if the subject took the experiment in July. 'Daytime effects' denote indicator variables for the four time intervals 6:00-12:00, 12:00-18:00, 18:00-24:00, and 0:00-6:00 at which the subject started the experiment. 'Region effects' are dummies for the subject's residential state (Bundesland). Standard errors in parentheses. *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent

Table 6: Additional variants of model 2 (marginal effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Cash prize	-0.0009***	-0.0011***	-0.0011***	-0.0010***	-0.0011***	-0.0010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Demographics						
Female (d)	–	0.0089	0.0094	0.0071	0.0066	–
		(0.638)	(0.618)	(0.705)	(0.723)	
Age	–	0.0006	0.0006	0.0006	0.0004	–
		(0.333)	(0.318)	(0.386)	(0.573)	
Years of education	–	0.0124***	0.0122***	0.0129***	0.0133***	0.0111***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Net income (T€)	–	-0.0044	-0.0042	-0.0064	-0.0071	–
		(0.406)	(0.427)	(0.234)	(0.194)	
Environmental controls						
Ambient temperature	–	0.0038*	-0.0006	0.0007	-0.0037	0.0037*
		(0.091)	(0.837)	(0.862)	(0.519)	(0.056)
Media attention	–	-0.0003	–	–	–	–
		(0.347)				
Climate change attitudes and beliefs						
Personal benefits	0.0346***	0.0363***	0.0359***	0.0336***	0.0356***	0.0323***
	(0.003)	(0.004)	(0.004)	(0.007)	(0.004)	(0.004)
Future benefits	0.0498***	0.0508***	0.0500***	0.0503***	0.0490***	0.0568***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Negative contributions	0.0386***	0.0385***	0.0389***	0.0408***	0.0374***	0.0374***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Footprint estimate (Tt)	-0.0006	-0.0006	-0.0006	-0.0007	-0.0007	–
	(0.306)	(0.336)	(0.350)	(0.307)	(0.308)	
Footprint confidence (d)	-0.0951***	-0.1068***	-0.1062***	-0.1015***	-0.1043***	-0.1041***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
EUA price estimate (T€)	0.0015**	0.0013*	0.0014*	0.0012*	0.0014**	0.0015**
	(0.036)	(0.075)	(0.059)	(0.080)	(0.046)	(0.032)
EUA price confidence (d)	0.0650**	0.0666*	0.0691*	0.0602*	0.0640*	0.0594*
	(0.047)	(0.069)	(0.060)	(0.098)	(0.083)	(0.069)
EUA availability (d)	-0.0016	-0.0183	-0.0169	-0.0194	-0.0157	–
	(0.933)	(0.370)	(0.406)	(0.337)	(0.437)	
Survey completion time	–	0.0001	0.0001	0.0001	0.0001	–
		(0.629)	(0.647)	(0.647)	(0.700)	
Interaction terms						
Cashprize * years of education	–	–	–	–	–	–
Cashprize * personal benefits	–	–	–	–	–	–
Cashprize * future benefits	–	–	–	–	–	–
Cashprize * negative contributions	–	–	–	–	–	–
Cashprize * footprint confidence	–	–	–	–	–	–
Cashprize * EUA price confidence	–	–	–	–	–	–
Footprint confidence * negative contributions	–	–	–	–	–	–
Footprint confidence * footprint estimate	–	–	–	–	–	–
EUA price confidence * EUA price estimate	–	–	–	–	–	–
Session effects	No	No	Yes	No	No	No
Day effects	No	No	No	Yes	Yes	No
Daytime effects	No	No	No	No	Yes	No
Region effects	No	No	No	No	Yes	No
N	1983.000	1600.000	1600.000	1600.000	1600.000	1902.000
Log-likelihood	-844.467	-650.018	-648.033	-639.577	-625.393	-789.883
χ^2	162.478	180.783	184.754	201.666	230.033	200.124
Pseudo R^2	0.088	0.122	0.125	0.136	0.155	0.112
AIC	1708.933	1334.036	1330.066	1339.153	1346.786	1599.767
BIC	1764.857	1425.458	1421.487	1500.486	1604.919	1655.273

Notes: Marginal effects evaluated at the sample means. (d) denotes the marginal effect of an indicator variable. Dependent variable: 1 if subject chose the contribution over the cash award. 'Session effects' is an indicator variable and 1 if the subject took the experiment in July. 'Daytime effects' denote indicator variables for the four time intervals 6:00-12:00, 12:00-18:00, 18:00-24:00, and 0:00-6:00 at which the subject started the experiment. 'Region effects' are dummies for the subject's residential state (Bundesland). Standard errors in parentheses. *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent