Price and Non-Price Effects in Giving to a Global Public Good: Evidence from a Framed Field Experiment

Johannes H. Diederich* Timo Goeschl†

Heidelberg University Heidelberg University

June 25, 2012

Abstract

In a framed field experiment with 2,440 subjects from the general population we exogenously vary the price of contributing to a global public good. We find a direct price effect at the extensive margin that is highly significant and negative, but also highly inelastic. Socioeconomic variables such as education, attitudinal variables related to moral dimensions, and situational variables strongly correlate with the decision to contribute. Our results help to understand the absolute and relative role of price and a large set of non-price covariates on voluntary contributions to a public good in a very large economy. They relate to other field experimental findings that have not identified a price effect at the extensive margin using indirect pricing mechanisms in the context of charitable donations.

Keywords: voluntary contributions; public goods; large economy; price elasticity; field experiment; online experiment

JEL Classifications: C93, D12, H41

*Email: diederich@eco.uni-heidelberg.de. Postal address: Department of Economics, Bergheimer Str. 20, 69115 Heidelberg, Germany.

†Corresponding author. Email: goeschl@eco.uni-heidelberg.de. Postal address: Department of Economics, Bergheimer Str. 20, 69115 Heidelberg, Germany. Phone: +49 6221 548010, Fax: +49 6221 548020.

1 Introduction

Better understanding voluntary contributions to large-scale public goods has attracted considerable research efforts over recent years. One reason is that the absolute and relative role of prices and other determinants in the private provision of public goods continues to be of obvious interest to economists as they build a more comprehensive theory of voluntary contributions (Andreoni 2006a, Konow 2010). Another reason is that the methodology of field experiments has opened up novel approaches to a research question that had previously to rely exclusively on empirical data for large-scale public goods (e.g. Auten et al. 2002).

The present paper contributes to this research by studying, in a field setting, a simple, yet fundamental economic question, namely how the price of contributing to large-scale public goods influences the decision to contribute. In an ideal world, the researcher would proceed by designing a natural field experiment in which different subjects face different prices of contributing to a large-scale public good. This research strategy would have two desirable features: One is that the researcher would vary observed prices directly rather than indirectly, thus enhancing experimenter control. The other is that the observed contribution choices would be taken under the subject's own budget constraint, thus avoiding possible contamination with a 'windfall' (Keeler et al. 1985) or 'house money' (Thaler and Johnson 1990) effect familiar from lab experiments. Given the well-known difficulties of implementing natural field experiments, a second-best strategy would be to follow the approach taken by researchers studying charitable contributions (Karlan and List 2007, Eckel and Grossman 2008, Huck and Rasul 2011, Karlan et al. 2011). These researchers study in the context of common fundraising methods how contribution decisions respond to variations in the ratios at which a lead donor matches subjects' contributions or the rates at which rebates are given. Following this approach, which has contributed much to our understanding of charitable giving, would have the benefit of subjects making choices under their own budget constraint. One drawback would be that the design forces the researcher to adopt an indirect route to price variation: Experimental subjects do not observe different prices, but contribution multipliers. Given evidence in the context of private goods (Davis and Millner 2005), the extent to which results on public goods derived by an indirect route can be interpreted as those of an equivalent direct price change is an open question.

In this paper, we report on an alternative approach, namely a framed field experiment[1] that

[1] Following the nomenclature of Harrison and List (2004), 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 of subjects that their choices are being observed.

features direct variations in the price. To vary the price, we follow the line of attack on the price effect charted by Andreoni and Vesterlund (2001) and Andreoni and Miller (2002) in the lab: These authors have subjects face different prices of giving, specifically the price of benefit sharing in a dictator game. This direct approach, however, has the disadvantage that observed choices are not taken under the subject's own budget constraint. Like in lab experiments, both financial incentives and public good benefits for subjects in our design are "on the house". The evidence on whether or not this introduces a bias in a public goods context, and if so in which direction, is mixed. While the classic finding is that with 'house money' individuals behave less risk-averse (Thaler and Johnson 1990), Clark (2002) finds no significant difference in contribution behavior in a standard voluntary contribution mechanism (VCM) in the lab. Harrison (2007) reviews Clark's analysis of the data and identifies a decrease of contributors at the extensive margin by 8 percent. To our knowledge, there is no field experimental evidence on the effect of 'house money' on public good contributions.

In the present framed field experiment, 6,525 subjects had a choice between a given cash payout of varying amounts and a fixed contribution to a global public good. The simple dichotomous choice format of the experiment reflects the focus of the design on the extensive margin of contribution (Bergstrom et al. 1986): Subjects only decided whether to contribute or not, given their experimental price. The between-subjects design of the experiment featured a significant variation in prices, a subject sample of "normals" that is representative for the Internet using population of voting-aged Germans, a familiar, non-laboratory environment in which subjects took their decision, and the unaltered use of subjects' information set. In this paper, the focus is on the price effect among 2,440 subjects who completed the 'baseline' treatment of the experiment as described below.

In addition to understanding the absolute role of the price for whether individuals contribute privately to large-scale public goods, our 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.

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: a €1 increase in the price of the contribution decreases the probability that the individual will contribute by around 0.1 percent. This corresponds to a price elasticity of the extensive margin of -0.31. This result is new as previous field experiments using indirect price variations have not been able to identify a significant price effect at the extensive margin of giving.

In relative terms, our evidence supports the notion that, compared to price, other variables are at least as relevant 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 significantly with contribution decisions, but we find that education stands out. Our findings also provide some further evidence to recent results on exogenous situational or 'mood' factors driving contribution decisions (Kirchsteiger et al. 2006, Konow 2010). 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.

The paper proceeds as follows: We explain the experimental design considerations and the experimental protocol in the following section. We then present the empirical analysis in section 3 before interpreting and discussing the results in section 4. Section 5 concludes.

2 Experimental design

2.1 Basic design and protocol

To facilitate contributions to a global public good in the experiment we used reductions in European greenhouse gas (GHG) emissions. Economists have long noted that voluntary emissions reductions to mitigate climate change constitute a close empirical counterpart to a contribution in an infinitely large public goods game (e.g. Nordhaus 1993). This insight is shared by the German population, which largely accepts both the significance of climate change and GHG emissions as its cause, mostly irrespective of age, sex, education, or political orientation (European

Commission 2008).[2] We embrace this feature in a framed field experiment by offering 6,525 subjects a choice between, on the one hand, a cash award and, on the other, a real GHG emissions reduction of 1 ton of CO_2. The GHG emissions reduction is in the form of the documented and verifiable retirement (*'deletion'*) of an emissions allowance (EUA) under the European Union Emissions Trading Scheme (EU-ETS). Retiring one EUA lowers the total ceiling of the Scheme, and hence emissions, by one ton.[3]

In this paper, we focus on the 2,440 subjects who were assigned to the 'baseline' treatment in the experiment. In this treatment, the choice that subjects face closely corresponds to the consumption-contribution trade-off at the extensive margin within a classic model of private provision of public goods (e.g. Bergstrom et al. 1986): First, subjects do not learn about others' choices before, during, or after the experiment. Second, the design minimizes additional private dimensions of the public good contribution. For example, if subjects received EUA retirement certificates in hardcopy, it would plausibly bias their willingness to contribute in order to purchase a (private) good with a strong curiosity dimension. Through minimizing the visibility of a subject's contribution to others, we also try to exclude confounding private dimensions related to the recognition of the contribution by others, such as image motivation.

Subjects are drawn from the approximately 65,000 Internet panel members of the German section of a leading international online polling company and are representative for Germany's Internet using population of voting age.[4] The Internet experiment ran in two sessions in May and July 2010.[5] Session 1 lasted from May 25th to June 2nd and generated 1,640 complete observations from 1,817 invitations to the 'baseline' treatment. 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. The introductory screen then explained, as common

[2] 90 percent of both Germans and Europeans in general rate climate change a serious or very serious problem. For Europeans, the lowest awareness of the problem is among citizens above 55 years of age (87 percent) as well as among unemployed and retired citizens (88 percent).

[3] Among several possibilities, the regulatory framework of the EU-ETS, regulating the bulk of industrial CO_2 emissions across EU member states, provides the most reliable and transparent technology for real contributions to global GHG reductions in an experiment. First, retiring EUAs avoids the problem of 'additionality' frequently encountered for project-based carbon offsets as the total cap of the EU-ETS is binding and enforced. Second, each EUA is uniquely identified by its issue number and hence traceable for both experimenter and subject. Third, EUAs are not paper currency and have therefore no curiosity value as a tangible private commodity. Total emissions for the trading period 2008-2012, the relevant one for this experiment, are capped at 1.856 billion tons.

[4] 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 slightly 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.

[5] 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 and refine the set of texts and questions.

with the pollster's regular surveys, the thematic focus of the poll (CO_2 emissions and climate change), the expected duration (ten minutes), and the payment (in form of a lottery).[6]

Subjects' choices were implemented under a random incentive system (RIS) (Grether and Plott 1979, Starmer and Sugden 1991, Lee 2008) in order to limit total costs of the experiment. The RIS was between-subjects (Tversky and Kahneman 1981, Baltussen et al. 2010, Abdellaoui et al. 2011) with odds of one in fifty that the subject's choice of either cash or emissions reductions was realized. Between-subjects (BS) and within-subject (WS) random incentives systems have been subjected to examination for possible biases. While BS introduces noise and decreases risk aversion, there is less evidence of a systematic bias for simple tasks (Cubitt et al. 1998, Baltussen et al. 2010). However, BS-RIS has been shown to affect behavior in dictator games (Sefton 1992) while for ultimatum games, behavior was unaffected (Bolle 1990).

Following the introductory screen, there was a filter screen to focus on German subjects.[7] Participants then faced a sequence of 10 to 13 computer screens, 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) 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. Furthermore, the text reminded subjects of the public nature of the contribution by emphasizing that emissions reductions have the same effect on climate change irrespective of the location of the abatement activity. The instructions did not contain further material to educate or inform experimental subjects about climate change. This had three advantages: First, it kept the instructions short and simple while utilizing the existing public awareness about climate change (European Commission 2008). Second, it avoids biases and potential misinterpretations from providing subjects with potentially choice-relevant information for an environmental public good (Arrow et al. 1993, Munro and Hanley 1999). Third, we feel that this most closely compares to the setting of other field experimental designs in which the link between a specific amount given and the quantity of public good purchased through this

[6] The polling company 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. A member's account balance can be converted into a variety of shopping vouchers or, having reached a threshold of EUR 50, wired to the member's bank account.

[7] Subjects of other nationalities were redirected to other surveys running at the same time.

[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. This helps to prevent subjects from "rushing" through a survey.

amount has also not been made explicit.[9] The subsequent *decision* screen then explained how the subject would receive their chosen prize if the subject was drawn as a winner.[10] 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, which we describe in more detail below. 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 subjects' socio-demographic profile on record with the polling company.

2.2 Direct price variation

A central design feature of the experiment was the direct variation in the opportunity cost of contributing to the 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 modified dictator game to examine how subjects behave in eight different economic environments characterized by exogenous variations in price of giving and endowment. Our field design randomly assigned subjects to one of fifty different experimental prices. The prices ranged, in increments of €2, from €2 to €100, the upper bound roughly reflecting possible maximum marginal costs of carbon abatement (Tol 1999, Tol 2009, Tol 2010). To each experimental price, on average 49 subjects were assigned.[11]

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, fewer

[9] Other public goods used in the field experimental literature on the price effect of giving include providing funds to a liberal political organization (Karlan and List 2007, Karlan et al. 2011), to a public radio station (Eckel and Grossman 2008), or to a children project of the Bavarian State Opera (Huck and Rasul 2011).

[10] 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 EUA issue numbers was privately traceable and verifiable through a public-sector Internet site.

[11] The smallest group contained 31, the largest 66 subjects (standard deviation 6.4 subjects).

individuals will contribute than at a low price of giving. Similarly, a conditional cooperation model of behavior would predict more 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.[12]

Field experimental evidence on the presence of the price effect at the extensive margin is less clear. Existing papers on public good giving employing a field setting use indirect variations, such as linear matching grants or tax rebates, to manipulate the price of giving (Karlan and List 2007, Eckel and Grossman 2008, Huck and Rasul 2011, Karlan et al. 2011). Karlan and List (2007) find that while the probability of donating responds significantly to the presence of a match offer, the response rate is inelastic with respect to match ratios. However, as Huck and Rasul (2011) point out, introducing a match may affect giving for additional reasons besides changing the price. The mere presence of lead donor may serve as a coordination device (Andreoni 1998), it may signal credibility and quality of the public good in question (Rose-Ackermann 1986, Glazer and Konrad 1996, Vesterlund 2003, Andreoni 2006b, Potters et al. 2007), or it may induce elements of sequentiality in the game (Romano and Yildirim 2001).[13] Huck and Rasul (2011) therefore report an additional treatment with a lead donor but without matching to disentangle the effects of lead donorship and indirect price changes through matching. In their results, lead donor presence has a large positive effect on donations at the intensive margin. On the extensive margin of giving, however, no significant effect can be established, neither from introducing a lead donor nor from varying prices through different matching rates. Likewise, Karlan et al. (2011), who analyze small matches, do not find a significant price effect.

One reason why existing studies could not reliably identify the price effect at the extensive margin could be low statistical power as response rates in these types of mail surveys are generally low in absolute terms (Huck and Rasul 2011). This would hold especially if the effect were small. 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'

[12] 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.

[13] Another possibility would be that, depending on the wording of the experimental instructions, subjects perceive the strategic environment similar to a threshold public good game instead of a linear game if they feel that with the lead donor it is more likely to reach an "appropriate" level of funding.

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. In the present experiment, through randomly assigning a sufficient number of subjects to one of fifty prices, the presence, direction, and strength of the price effect could be studied at a higher degree of resolution, over a wider price range, and with higher statistical power compared to experiments that offer only few experimental prices and suffer from low response rates.

Another reason for the invariance of donations observed at the extensive margin so far would be if indirect variations of experimental prices elicited different responses than direct variations. The fundraising literature benefits from several contributions examining the relative performance of indirect instruments (Eckel and Grossman 2003, Davis et al. 2005, Eckel and Grossman 2006). To our knowledge, there has been no systematic comparison, however, between direct and indirect mechanisms to vary the price of giving in a public goods setting.[14] While the direct variation design we report on here also does not provide a systematic comparison, one advantage is that it circumvents any potential lead donor effects.

2.3 Field price censoring

A potential challenge created by directly varying prices in order to determine the price effect is the possibility of field price censoring (Harrison and List 2004). Field price censoring, henceforth '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, Cherry et al. 2004, Harrison et al. 2004). In other words, there is a possibility that subjects perceive an arbitrage opportunity introduced by the experiment, biasing the observable contribution decision. 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.[15]

The going spot price for EUAs at the time of the experiment was about €15 per ton. While this price is the objective field price for the experimental good, it is also a political artefact,

[14] For private goods, Davis and Millner (2005) compare direct price variations to rebates and matches and find significant differences with respect to matching.

[15] 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 $\hat{f}_i - e_i$.

determined through the 'cap' set by the European Union in order to fulfill their commitment in the Kyoto negotiations and their own emissions targets. Given the available marginal abatement costs estimates, the spot price does certainly not reflect the upper bound on possible efficient market prices for 1 ton of carbon. The experiment therefore covered a more realistic range of prices. While controlling for FPC, we also expected subjects not to be very well-informed about the spot price. Our questionnaire results support this prior.

Two aspects are relevant for detecting the possible presence of FPC in this experiment. First, it is relatively costly for private individuals to purchase and delete EUAs at the going spot price—a fact that largely excludes the possibility of FPC from perfect substitutes.[16] However, subjects may be aware that a variety of imperfect substitutes exist at different prices and degrees of substitutability. The alternatives range from close substitutes such as having a EUA retired through a broker[17] or purchasing an offset based on a carbon reduction project[18] to more remote substitutes such as making costly changes in everyday life to reduce one's own carbon footprint. The second issue is that the researcher should expect a high degree of heterogeneity in subjects' knowledge about these substitutes and thus, in the levels of *perceived* field prices. In fact, subjects' information status and FPC may be interrelated phenomena: uninformed subjects may have an incentive to opt for the cash prize in order to make an informed decision later.[19] In the context of the experiment, therefore, there is no single explicit field price that will censor all responses. Instead, FPC would be driven by subjects' possible perception that field opportunities are available at certain prices (Harrison et al. 2004).

To detect subjects potentially constrained by FPC without interfering with subjects' information status, we follow the strategy of a debriefing questionnaire as in Coller and Williams (1999) and Harrison et al. (2002). Our identification strategy is threefold and consists of several follow-up questions after subjects chose their desired prize. First, we gave subjects who chose the cash price the opportunity to agree to three statements following the *decision screen*. As a result, this FPC "filter" contained all subjects that did not check the first option ('*Given the*

[16] The EU-ETS gives private individuals the opportunity to open an account for a fixed fee of €200. The account does not include trading, though. Obtaining EUAs in small numbers is not straightforward without an additional intermediary.

[17] At the time of the experiment, there existed only very few opportunities via the internet to commission EUA retirements, none of them in German language. One example is the UK based Carbon Retirement Ltd. (www.carbonretirement.com) with a price of around €23 per ton of CO_2 at the time of the experiment.

[18] For example, *Certified Emissions Reductions* (CER) under the United Nations Clean Development Mechanism (CDM). Being available at various grades (e.g. Gold Standard, www.cdmgoldstandard.org), prices are quite heterogenous. Typically, CERs were available below and above the EUA spot price at the time of the experiment.

[19] Our design prevents this effect to a certain extent since the online survey implementation allows subjects to search the internet while doing the survey, or leave the survey and take it up again later. We do not find much evidence on this behavior, though (cp. footnote 28).

two prizes, I did not want to forgo the chance of winning x euros'), but checked the second option ('*I assume that there is another possibility for me to reduce CO2 emissions by one ton for less than x euros.*') or made a qualitatively equivalent statement in the open-ended option 3 ('*I had other reasons for choosing the cash prize, namely...*'). Second, we asked all subjects to estimate current EUA spot prices and the availability of EUAs to private individuals. Third, an open-ended question at the end of the follow-up questionaire on climate change asked all subjects to list existing efforts to mitigate climate change. Thus, while the first and the third part of the strategy aimed at FPC from both perfect and imperfect field substitutes, part two targeted perfect substitutes only. Section 4.5 reports on several robustness checks for our results with respect to a potential bias from FPC.

3 Analysis

For the parametric analysis of subjects' discrete choices, we employ a simple probit model to study the impact of price and non-price variables on the likelihood of contributing. We estimate two basic specifications:

$$Y_i = \alpha_0 + \alpha_1 P_i + \varepsilon_i \tag{1}$$

$$Y_i = \gamma_0 + \gamma_1 P_i + \gamma_2 N_i + \gamma_3 P_i n_i + \varepsilon_i \tag{2}$$

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. 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 its 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 two exogenous environmental controls connected to the experiment: first, the extent at which public media covered the issue of climate change around the

time of the experiment based on German print and online media data (LexisNexis database) and second, regional temperature data from the German National Weather Service (DWD).

4 Results and Discussion

4.1 Descriptive results

2,440 subjects completed the experiment with a median completion time of 5 minutes.[20] A total of 382 subjects (15.7 percent) in the experiment chose the public goods contribution, 2,058 opted for the cash prize. Of the latter group, 85 subjects expressed some disbelief about the payment or the EUA vehicle in answers to open-ended survey questions and were excluded from the subsequent analysis.[21] Figure 1 presents the mean contribution rate for each of the fifty price treatments. Looking at the extreme ends of the price range we find on the lower end that the median 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. Mean contribution rates significantly exceed zero for all prices except €50 and €56 at the 5 percent level using a one sided t-test.

The observed contribution decisions trace out a negatively sloped, almost vertical demand schedule, illustrated by a linear fitted regression line for the mean contribution rates in Figure 1. The price elasticity of contributions at the extensive margin thus appears low at first sight.

4.2 Regression results

Table 1 describes the coviariates employed and presents summary statistics. Table 2 reports the marginal effects of the probit regressions of both models. Column 1 corresponds to model 1. Columns 2 to 4 provide variants of model 2 with alternative specifications, focusing on the important results discussed below. Additional specifications supporting the results of Table 2 include, e.g., controls for media coverage, region of residence, or experimental day and daytime and can be found in tables 7 and 8 in the appendix. The remainder of the section presents and discusses the results on both the price and non-price determinants that arise out of the different specifications.

[20] Average completion time was 1 hour 17 minutes. The difference between mean and median is largely driven by a small fraction of outliers (approx. 3%) in which subjects availed themselves of the opportunity to leave the survey and continue hours or days later.

[21] Results are not sensitive to their inclusion.

Table 1: Summary statistics

Variable	Description	Mean	S.d.	Obs.
Socio-demographic characteristics				
Female	Indicator variable for gender, 1 if female	0.469	0.499	2,355
Age	Subject's age (years)	45.43	14.68	2,353
Years of education	Based on subject's stated highest educational degree	12.27	3.214	2,300
Income	Midpoint[a] of subject's reported monthly household net income category (Euros)	2,556	1,705	1,951
Environmental controls				
Ambient temperature	Mean temperature in subject's region of residence before subject started experiment[b] (°C)	15.1	4.186	2,301
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	28.13	2,301
Climate change attitudes and beliefs				
Personal benefits	Believes in personal benefits from the effects of carbon emissions reductions	2.366[d]	0.990	2,096
Future benefits	Believes in benefits for following generations from today's emissions reductions	2.901[d]	0.968	2,113
Negative contributions	Believes that his or her personal lifestyle has contributed to climate change	2.761[d]	0.952	2,092
Footprint estimate	Estimate of yearly CO_2 emissions from lifestyle (tons)	3,020[e]	15,336	2,355
Footprint confidence	Indicator variable for confidence in the footprint estimate given, 1 if at least "somewhat sure"	0.075	0.263	2,355
EUA price estimate	Estimate of the EUA spot price (Euros)	1,655[f]	10,304	2,355
EUA price confidence	Indicator variable for confidence in the EUA price estimate, 1 if at least "somewhat sure"	0.106	0.308	2,355
EUA availability	Indicator variable, 1 if subject (tends to) believe that there exists a possibility for him or her to purchase EUAs somewhere else	0.197	0.398	2,355

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 the subject took the experiment and the day before [c] Keywords used: '*climate change*', '*climate protection*', '*global warming*', '*carbon dioxide*', 'CO_2' [d] Answer categories 1=disagree, 2=tend to disagree, 3=tend to agree, 4=agree [e] Median is 10 [f] Median is 50

Table 2: Price and non-price 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)	–	0.0249	0.0102	0.0090
		(0.018)	(0.019)	(0.019)
Age	–	0.0008	0.0007	0.0007
		(0.001)	(0.001)	(0.001)
Years of education	–	0.0142***	0.0124***	0.0132***
		(0.003)	(0.003)	(0.003)
Net income (T€)	–	-0.0031	-0.0042	-0.0028
		(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	–	–	0.0366***	0.0412***
			(0.012)	(0.012)
Future benefits	–	–	0.0503***	0.0468***
			(0.014)	(0.014)
Negative contributions	–	–	0.0381***	0.04187***
			(0.012)	(0.012)
Footprint estimate (Tt)	–	–	-0.0006	-0.0006
			(0.001)	(0.001)
Footprint confidence (d)	–	–	-0.1072***	0.2140
			(0.020)	(0.220)
EUA price estimate (T€)	–	–	0.0013*	0.0013*
			(0.001)	(0.001)
EUA price confidence (d)	–	–	0.0684*	0.0669*
			(0.037)	(0.036)
EUA availability (d)	–	–	-0.0177	-0.0172
			(0.020)	(0.020)
Survey completion time	–	–	0.0001	0.0001
			(0.000)	(0.000)
Interaction terms				
Cashprize * years of education	–	–	–	0.0003***
				(0.000)
Cashprize * personal benefits	–	–	–	0.0006
				(0.000)
Cashprize * future benefits	–	–	–	-0.0003
				(0.000)
Cashprize * negative contributions	–	–	–	-0.0000
				(0.000)
Cashprize * footprint confidence	–	–	–	-0.0013
				(0.001)
Cashprize * EUA price confidence	–	–	–	-0.0002
				(0.001)
Footprint confidence * negative contributions	–	–	–	-0.0942**
				(0.042)
Footprint confidence * footprint estimate	–	–	–	-0.0000
				(0.000)
EUA price confidence * EUA price estimate	–	–	–	-0.0202
				(0.065)
N	2355.000	1918.000	1598.000	1598.000
Log-likelihood	-1037.688	-820.911	-650.237	-639.311
χ^2	12.630	45.150	179.579	201.430
Pseudo R^2	0.006	0.027	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

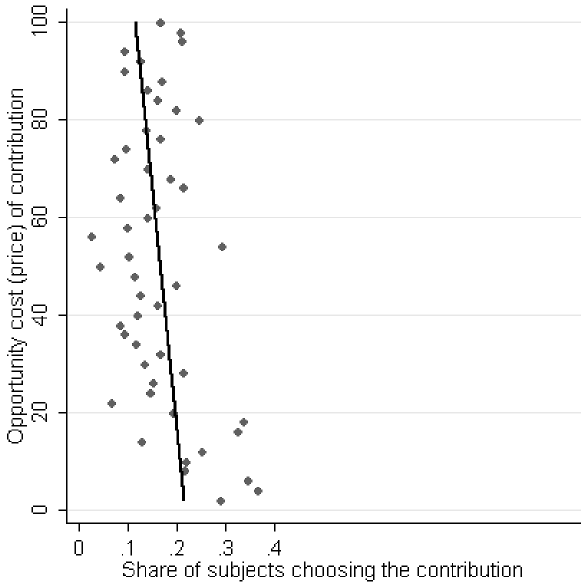

Figure 1: Share of contributors for fifty price treatments. The Figure plots the cash prize offered to subjects against the share of subjects choosing to contribute at this price. Cash prizes are between €2 and €100, in increments of €2. Also plotted is the linear fitted regression line.

4.3 Price effects

The first result is that price matters: Converting probit coefficient estimates of the experimental price (*cash prize*) into marginal effects (Table 2), 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 is robust across specifications. At the same time, price does not matter substantially. A one Euro increase shifts only 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. Another way to put the result is in terms of the elasticity of the probability of contributing.[22] We calculate the elasticity of probability based on model 1 as -0.31 (standard error 0.09).

The result on the presence and direction of the price effect is in line with the unambigious

[22] The elasticity of probability is defined as $\eta_{\mathrm{Pr}} = \frac{\partial \Pr(Y=1)}{\partial p} \frac{p}{\Pr(Y=1)}$ where p denotes the cash prize (e.g. Miklius et al. 1976, LeClere 1992).

prediction of standard theory (Bergstrom et al. 1986, Andreoni 1990): At the extensive margin, a higher price decreases the probability of observing a contribution. In comparison to field experimental results using indirect price variation, our finding is to a certain extent comparable to Karlan and List (2007). They find a significantly negative price effect at the extensive margin from introducing a matching lead donor.[23] Both Karlan and List (2007) and Karlan et al. (2011), however, do not establish a significant effect when comparing step-wise variations of the match. Likewise, Huck and Rasul (2011) do not find an effect when introducing a match without concurrently introducing a lead donor. In contrast to these findings, our data shows an highly significant effect at the extensive margin in a direct price change design in the field. Our result also contrasts empirical elasticity estimates at the extensive margin: For example, Smith et al. (1995) find that the decision whether to make a charitable contribution for a rural health care facility is insensitive to price.

Regarding size, the price effect is small and inelastic and thus recalls previous results on the price elasticity of demand for public goods (e.g. Green 1992). 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 Shogren 1999): Higher prices offered might conceivably lead uninformed subjects to infer that the good is more valuable. To test for the possibility of such an anchoring 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. An anchoring 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 a non-negative relationship between the propensity to provide the mitigation effort and the "information-weighted" price (see column 4 in Table 2): Contrary to the hypothesis of the confounding price effect, more familiarity does not change the price elasticity of contributing or even decreases it. This resonates with experimental findings that price elasticity does not

[23] The overall (extensive and intensive margin) elasticity estimate they provide ist -0.30 before and -0.225 after taxes. We do not account for taxes in our estimate as emissions reductions are not tax-deductible in Germany.

systematically vary with uncertainty about good characteristics (Heffetz and Shaya 2009).

4.4 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 as well as the environmental controls that inform the experimenter about the time, likely place and environmental conditions of the subject's participation. The other set of variables consists of survey answers elicited *after* the contribution decision and, for those choosing the cash prize, *after* explaining their choice. These variables concern subjects' attitudes, experiences and general behavior with respect to the public good and provide an opportunity to give experimental traction to some of the more recent attempts to understand psychological drivers of contributions. 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.

The published literature on giving to public goods 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. Compared to these papers, our data provides a relatively large set of non-price variables. Due to the specific nature of the public good used, our experimental findings also speak to the ongoing debate about people's valuation of climate change mitigation and the determinants of their willingness to pay (WTP). Being beyond the scope of this paper, these results have been discussed elsewhere (Diederich and Goeschl 2011).

4.4.1 Demographics

All four socioeconomic dimensions have received some attention in the literature on giving 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". In our experiment, *females* seem to be more inclined to opt for the public good contribution throughout the tested specifications, but the effect never becomes significant (Table 2). The result compares to the currently equivocal evidence on gender effects in public goods settings where the evidence on gender differences are less clear-cut than its behavioral salience in areas such as risk taking or competition (see Croson and Gneezy 2009 and references therein). Findings by 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. We therefore 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.[24]

Likewise, the effect of *age* is consistently positive but insignificant in all model specifications. Age has also started to attract some attention as a determinant of behavior in public goods settings (Harbaugh and Krause 2000, List 2004). List (2004) and Carpenter et al. (2008), for example, find that social preferences increase with age in laboratory public goods games and charitable donations experiments. In our experiment, the public good of emissions reductions provide reasons for expecting a positive age effect to be tempered, neutralized, or even reversed if subjects are aware of the high 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. The data provides some evidence on subjects' awareness of the inertia from the difference in answers about personal and future benefits from the contribution (Table 1). To test how subjects view the intertemporal nature of the public good we included 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.[25]

Among otherwise inconclusive socioeconomic determinants, *education* stands out as highly significant across all specifications. As the results in Table 2 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. 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 goods games. If pro-social behavior is not acquired through education,

[24] Results are available from the authors upon request.
[25] Results are available upon request.

the strong relationship observed in the data may arise from a different source. One plausible explanation could be that education and awareness of climate change as a serious problem are positively correlated, as U.S. survey data indicates (Borick et al. 2011). However, there is less evidence of this type of correlation in EU countries: 89 percent among those with a high-school degree or less and 92 percent of those with tertiary education regard climate change as at least "a fairly serious problem" (European Commission 2008). The strong education effect may also be explained by 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 (Bettinger and Slonim 2007, Kirby et al. 2005, Parker and Fischhoff 2005). Other studies emphasize the lower cognitive cost to abler individuals of making decisions in complex settings (Peters et al. 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.

The effect of *income* is insignificant in every model specification. 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 in contrast to Eckel and Grossman (2008). However, caution is advised as income is self-reported and therefore subject to potential biases and reporting is somewhat incomplete. 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 with education is 0.29).

4.4.2 Environmental controls

The sample of subjects taking part in the experiment is drawn from all over Germany, introducing possibly important spatial or structural determinants of behavior that are easily overlooked. At the same time, situational factors may play a role in explaining observed variations as the field provides less control than the lab would. We pursue two distinct strategies to account for these

possibilities. One is to estimate the models with controls for location and time, exploiting the polling company's records on each panel member and the recorded times at which each subject individually started the experiment. The results of this exercise are reported in columns 3 to 5 of tables 7 and 8 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 table 2: 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. Indeed, we find that the probability of contributing increases with ambient temperatures. 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. The result is significant not only for daily mean temperatures, where the average of the day at which the subject took the experiment and the day before is taken to reflect an approximately 24 hours time window prior to the experiment, but also holds for daily maximum temperatures and larger time windows (e.g., 48 hours). Including media coverage of the topic of climate change alongside the temperature data does also not appreciably change the estimates (see tables 7 and 8 in the appendix).[26] Additional support for the plausibility of a temperature effect comes from the psychological literature where it has been shown that questionnaire answers about climate change are sensitive to both indoor and

[26]One should consider other possibilities that ambient temperatures proxy for a different driver or that other conventional drivers linked to temperature are present. For example, ambient temperatures may simply proxy for latitude. Including latitude alongside the temperature variable does not alter the results, however (results available upon request).

outdoor temperature and other environmental conditions (Joireman et al. 2010).

4.4.3 Benefits

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. We solicited *ex post* statements on perceived personal and next generation benefits of emissions reductions undertaken today from all subjects in the experiment (table 1). The regression results reported in table 2 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. A one-unit (response category) 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 only 3.7 percent. The marginal effect of the altruistic motive at the sample mean appears therefore stronger than that of personal benefits across specifications, although differences between the coefficient estimates never become statistically significant.

4.4.4 Negative contributions

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 6 and 2 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 4 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 6 and 2 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.

Table 3: FPC 'filter': joint distribution of subjects' statements about their choice of cash

Given the two prizes, I did not want to forgo the chance of winning x euros	I assume that there is another possibility for me to reduce CO_2 emissions by 1 ton for less than x euros: 0	1	Total
0	18	511	529
1	1,321	123	1,444
Total	1,339	634	1,973

Note: x denotes the cash prize the subject was assigned to

4.5 Field price censoring

As pointed out earlier, the experimental design raises the possibility of FPC among subjects, which has the potential of biasing results. To identify subjects affected by FPC, we draw on the FPC 'filter' statements described in section 2.3 as well as on answers to the follow-up questions on EUAs and on efforts for climate change mitigation. A common problem in debriefing questionnaires that are not payoff-relevant is that, while easily implemented, they are not immune to contamination through strategic behavior or ex post rationalization (Corrigan and Rousu 2008). In the context of the FPC identification strategy pursued here, both a subject's 'filter' statements and his or her estimate of the EUA spot price may be endogenous to the preceding choice whether to contribute or not at the given price. We therefore use these answers to identify the choices that are *potentially* subject to FPC and test whether their inclusion causes a bias in the overall price effect. Taken together, the available evidence points against a substantial, if any, bias of the results from FPC.

Table 3 summarizes subjects' FPC 'filter' statements and identifies 511 (25.9 percent) of 1,973 cash choosing subjects who 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.[27] Did the inclusion of these subjects bias the estimate of the price effect of Model 1 in tables 6 and 2? If FPC played a role, the estimated coefficient of price on the contribution decision in the full sample would be plausibly biased towards zero since a rational agent making those statements would always choose cash, irrespective of the price.

Column (1) in Table 4 reports that the price coefficient of the reduced sample that excludes the 511 potentially affected subjects does not differ significantly from the coefficient of the full sample.

[27] Among the 1,973 cash choosing subjects, 276 gave an open-ended answer in own words without checking one of the two statements. 258 answers provided paraphrases of the given statements and could therefore be reassigned. 249 of them implied an actual comparison of benefits and costs of the prizes (statement 1), 9 answers corresponded to a preferred opportunity outside the experiment given the choice (statement 2).

The regression replicates the significantly negative price effect on the decision to contribute in the full sample and compares it to that in the reduced sample. The coefficient of the interaction term is insignificant ($p = 0.69$). Naturally, the intercept is significantly higher for the reduced sample which excludes cash choosing subjects identified by the statements. For the reduced sample, we obtain a price elasticity of probability of -0.33 (standard error 0.089) compared to -0.31 (standard error 0.09) for the full. Another way of utilizing the 'filter' statements is to assume that all subjects identified by the statements were indeed subject to FPC and then recode their choice. Column (2) compares the original and the recoded sample. Again, a significant difference in the coefficients on cash prize cannot be established. The evidence based on the 'filter' statements thus points against the presence of a significant bias from FPC.

Table 4: Robustness of price effect to field price censoring

	(1)	(2)	(3)	(4)
Cash prize	-0.0038***	-0.0038***	-0.0038***	-0.0042***
	(0.001)	(0.001)	(0.001)	(0.001)
Reduced sample	0.2024**	–	0.1161	–
	(0.090)		(0.092)	
Reduced sample * cash prize	-0.0006	–	-0.0004	–
	(0.002)		(0.002)	
Recoded sample	–	0.6557***	–	–
		(0.081)		
Recoded sample * cash prize	–	0.0005	–	–
		(0.001)		
EUA estimate below	–	–	–	-0.5297***
				(0.148)
EUA estimate below * cash prize	–	–	–	0.0048**
				(0.002)
Constant	-0.7960***	-0.7960***	-0.7960***	-0.6799***
	(0.061)	(0.061)	(0.061)	(0.069)
N	4199.000	4710.000	3714.000	2355.000
Log-likelihood	-1970.881	-2594.222	-1698.694	-1027.371
χ^2	41.701	312.406	28.654	33.265
Pseudo R^2	0.010	0.057	0.008	0.016

Notes: Probit coefficient estimates. Standard errors in parentheses. Dependent variable: 1 if subject chose the contribution over the cash award. Independent variables: '*Reduced sample*' is 0 if the observation belongs to the full sample and 1 if the observation belongs to the sample excluding subjects that are potentially affected by FPC according to the 'filter' statements (column 1) or EUA price estimates (column 3). '*Recoded sample*' is 0 if the observation belongs to the original sample and 1 if the observation belongs to the sample with recoded choices according to the FPC 'filter' statements. '*EUA estimate below*' is an indicator variable and 1 if the observation is potentially affected by FPC according to subject's EUA price estimate. *** Significant at or below 1 percent ** Significant at or below 5 percent * Significant at or below 10 percent

Columns (3) and (4) of Table 4 present the results of the second part of the strategy to detect FPC. This part specifically targets the *perfect* substitute and is based on subjects' reported estimates of the EUA spot price.[28] Table 5 gives a detailed summary of this variable. About 74 percent of subjects gave an estimate within in the range of the randomly assigned experimental prices (€2 to €100) while the median subject gave an estimate of €50, close to the experimental mean and median. Thus, most subjects do not seem to be much informed about this example of a politically determined carbon price. From comparing experimental and estimated field price, we identify 996 subjects who estimated an EUA price below the cash prize amount they were assigned to. 1,359 subjects gave an EUA price estimate greater or equal to the cash prize. If subjects implicitly or explicitly took their perception of this field price into account when assessing the contribution decision, and not vice versa, then the choice of subjects who anticipate an EUA price below the experimental price may be affected by FPC.[29]

Table 5: Subjects' EUA price estimates

Survey question		Freq.	Rel. freq.	Cum.
What is your estimate	Below 2	100	4.25	4.25
of the current market price	2 to below 10	110	4.67	8.92
(in EUR) for 1 ton of CO_2	10 to below 20	328	13.93	22.85
in the EU emissions trading	20 to below 30	240	10.19	33.04
system?	30 to below 50	213	9.04	42.08
	50	286	12.14	54.22
	Above 50 to below 100	496	21.06	63.14
	100	355	15.07	78.21
	Above 100 to below 1,000	215	9.13	87.35
	1,000 to below 10,000	210	8.92	96.26
	10,000 and more	88	3.74	100.00

Notes: Continuous variable (open-ended question).

As before, we compare the unconditional price coefficient of the full sample (Model 1 in Table 6) with that of the reduced sample which excludes subjects potentially affected by FPC according to their EUA price estimate. Column (3) in 4 reports on the results. Again, the price

[28] 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. 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 endogenous information acquisition does not play a role in explaining the results and confirm results by Berrens et al. (2004). Importantly, this result also means that a potential field price censoring is not a product of endogenous information acquisition by subjects during the experiment, but can at most be generated by exogenous differences in information.

[29] To a rational agent, the choice would also depend on perceived transaction costs.

coefficient of the reduced sample is not significantly different from that of the full sample. The corresponding elasticity of probability for the reduced sample is -0.29 (standard error 0.095).

For the final column (4) of Table 4, we split the original full sample into two subsamples, one consisting of subjects whose EUA price estimate exceeds the cash prize and the other of those whose estimates are below the cash prize. Column (4) reports on the results of a direct comparison of contribution choices between the two subsamples with respect to price. The results show that, first, controlling for cash prize, subjects who estimate an EUA price below their cash prize are significantly less likely to contribute than those who estimate a higher spot price. Second, the choice to contribute of the former group is not significantly correlated with price: the interaction term is significantly positive and of about the same magnitude as the significantly negative main effect.[30] While these two results are clearly in line with the FPC hypothesis, there are at least two competing explanations besides FPC in this split-sample case. First, given the distributions of the cash prize and the price estimate variables, a relatively small number of observations where the estimate undercuts the cash prize is available for low prices, inflating the variance at low prices for this group. The second explanation is endogeneity of the price estimate to the preceding choice. Unfortunately, we cannot dissect between these hypotheses given the data.

Finally, we qualitatively analyzed the answers to the open-ended question on subjects' existing efforts to mitigate climate change. Most comments related to behavioral changes or investments into energy saving measures. None of the subjects mentioned any type of carbon offset or certificate. We take this as further evidence that close substitutes and their field prices did not play a role in determining subjects' contribution choices.

5 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. Clearly, the optimal experiment would vary the price of contributing in a direct manner and observe contribution behavior in the field without the involvement of experimentally provided 'house money'. Previous natural field experiments on the price effect observed behavior in the context of charitable giving and thus, facilitated

[30] Performing a seperate regression for the reduced sample gives an insignificant effect of the price.

variations in the experimental price of giving through common indirect mechanisms (matching and rebates). In contrast, the framed field experiment we report on in this paper employs a direct variation of experimental prices of giving known from the lab which, however, implies full use of 'house money'. If one wanted to compare our contribution vehicle to those of the fundraising literature, it could be best understood as a fundraising mechanism which gives different potential contributors an opportunity to donate different refund amounts they are eligible to, for example tax or utility refunds, to provide a fixed amount of the public good in a large economy.

We find that the price effect at the extensive margin is robust and negative, but quantitatively weak, with a price elasticity of the probability to contribute of -0.31. The significant finding is robust with respect to a potential bias from field price censoring and contrasts the results from the field experiments on charitable giving that have not established a significant price effect at the extensive margin. This difference may have several reasons for which exploration is left to future research. Apart from differences in statistical power, one reason may be different effects from the direct versus indirect framing of the experimental price, as it has been found for private goods. Another reason may be structural differences between charitable giving and contributions to other types of large-scale public goods. The difference appears even more relevant given that one might expect our price effect to be if anything then more, not less, blurry from the use of 'house money' and of a lottery-based incentive system.

The weakness in size of the absolute price effect puts the onus of explaining the choice whether to contribute or not 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 significantly correlated to contribution decisions. Most importantly perhaps, variables that can be plausibly linked to guilt and moral responsibility strongly deliver. This behavior lends further support to the direction and scale of moral factors and is a useful point of departure for further work.

Acknowledgements

The authors are grateful to seminar participants at Cambridge, Heidelberg, the London School of Economics, and Manchester for helpful comments. We also thank Svenja Espenhorst and Dennis Mignon at First Climate for support and Ruth Fieber, Christina Grimm, and Thomas Scheuerle for student assistance. Financial support by the German Science Foundation (DFG) is gratefully acknowledged.

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A Appendix: Instructions (translation of experimental screens into English)

A.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.2 Citizenship screen

Of which country do you hold citizenship?

In case you hold more than one, please tick all applicable boxes!

A.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.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[31] in bonus points

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

A.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.6 Introduction follow-up questions

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

A.7 Follow-up questions (screen 1)

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

( ) Somewhat sure

( ) Somewhat unsure

( ) Very unsure

( ) I don't know

A.8 Follow-up questions (screen 2)

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?

( ) Yes

( ) Somewhat yes

( ) Somewhat no

( ) No

( ) I don't know

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

( ) [Same answer options as above]

Do you think that future generations in Germany (for instance your children and grand-children) will benefit if climate change mitigating CO_2 emissions reductions are undertaken in the present time?

( ) [Same answer options as above]

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

( ) [Same answer options as above]

A.9 Follow-up questions (screen 3)

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

( ) Somewhat sure

( ) Somewhat unsure

( ) Very unsure

( ) I don't know

A.10 Follow-up questions (screen 4)

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.11 Enquiry of socio-demographic information (if not or only partially on record)

Please state your gender.

( ) Male

( ) Female

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

A.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.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 Appendix: Additional estimation results

Table 6: 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	–	0.1044	0.0435	0.0412
		(0.074)	(0.084)	(0.085)
Age	–	0.0034	0.0031	0.0034
		(0.002)	(0.003)	(0.003)
Years of education	–	0.0598***	0.0554***	0.0611***
		(0.011)	(0.012)	(0.012)
Net income (T€)	–	-0.0130	-0.0188	-0.0132
		(0.021)	(0.024)	(0.024)
Environmental controls				
Ambient temperature	–	–	0.0201**	0.0219**
			(0.009)	(0.010)
Climate change attitudes and beliefs				
Personal benefits	–	–	0.1631***	0.1904***
			(0.056)	(0.057)
Future benefits	–	–	0.2238***	0.2163***
			(0.063)	(0.065)
Negative contributions	–	–	0.1698***	0.1928***
			(0.055)	(0.058)
Footprint estimate (Tt)	–	–	-0.0028	-0.0030
			(0.003)	(0.003)
Footprint confidence	–	–	-0.6563***	0.7425
			(0.180)	(0.624)
EUA price estimate (T€)	–	–	0.0058*	0.0058*
			(0.003)	(0.003)
EUA price confidence	–	–	0.2736**	0.2764**
			(0.134)	(0.136)
EUA availability	–	–	-0.0808	-0.0813
			(0.096)	(0.097)
Survey completion time	–	–	0.0005	0.0007
			(0.001)	(0.001)
Interaction terms				
Cashprize * years of education	–	–	–	0.0014***
				(0.000)
Cashprize * personal benefits	–	–	–	0.0027
				(0.002)
Cashprize * future benefits	–	–	–	-0.0013
				(0.002)
Cashprize * neg. contributions	–	–	–	-0.0001
				(0.002)
Cashprize * footprint confidence	–	–	–	-0.0062
				(0.007)
Cashprize * EUA price confidence	–	–	–	-0.0008
				(0.005)
Footprint confidence * negative contributions	–	–	–	-0.4352**
				(0.193)
Footprint confidence * footprint estimate	–	–	–	-0.0001
				(0.000)
EUA price confidence * EUA price estimate	–	–	–	-0.0932
				(0.302)
Constant	-0.7960***	-1.7199***	-3.4071***	-1.5407***
	(0.061)	(0.195)	(0.314)	(0.227)
N	2355.000	1918.000	1598.000	1598.000
Log-likelihood	-1037.688	-820.911	-650.237	-639.311
χ^2	12.630	45.150	179.579	201.430
Pseudo R^2	0.006	0.027	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. Main effects of continuous variables in (4) are evaluated at the sample means

Table 7: 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	–	0.0415	0.0440	0.0349	0.0347	–
		(0.084)	(0.084)	(0.085)	(0.087)	
Age	–	0.0028	0.0028	0.0026	0.0017	–
		(0.003)	(0.003)	(0.003)	(0.003)	
Years of education	–	0.0558***	0.0550***	0.0589***	0.0622***	0.0485***
		(0.012)	(0.012)	(0.012)	(0.012)	(0.010)
Net income (T€)	–	-0.0198	-0.0190	-0.0295	-0.0338	–
		(0.024)	(0.024)	(0.025)	(0.025)	
Environmental controls						
Ambient temperature	–	0.0168*	-0.0027	0.0030	-0.0167	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	0.1469***	0.1625***	0.1615***	0.1527***	0.1656***	0.1414***
	(0.048)	(0.056)	(0.056)	(0.056)	(0.058)	(0.049)
Future benefits	0.2104***	0.2258***	0.2235***	0.2281***	0.2296***	0.2461***
	(0.055)	(0.063)	(0.064)	(0.064)	(0.065)	(0.058)
Negative contributions	0.1619***	0.1703***	0.1725***	0.1829***	0.1718***	0.1613***
	(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	-0.5141***	-0.6617***	-0.6614***	-0.6328***	-0.6888***	-0.6079***
	(0.151)	(0.181)	(0.181)	(0.183)	(0.189)	(0.161)
EUA price estimate (T€)	0.0062**	0.0057*	0.0060*	0.0056*	0.0065**	0.0064**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
EUA price confidence	0.2521**	0.2688**	0.2790**	0.2489*	0.2667*	0.2372**
	(0.116)	(0.134)	(0.134)	(0.137)	(0.140)	(0.120)
EUA availability	-0.0063	-0.0826	-0.0768	-0.0891	-0.0727	–
	(0.083)	(0.096)	(0.096)	(0.097)	(0.098)	
Additional controls						
Survey completion time	–	0.0006	0.0006	0.0006	0.0005	–
		(0.001)	(0.001)	(0.001)	(0.001)	
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
Constant	-2.2255***	-3.1566***	-3.1429***	-3.2700***	-2.8409***	-3.1747***
	(0.158)	(0.413)	(0.337)	(0.424)	(0.541)	(0.248)
N	1981.000	1598.000	1598.000	1598.000	1598.000	1900.000
Log-likelihood	-844.358	-649.802	-647.856	-639.386	-625.434	-789.674
χ^2	161.916	180.448	184.341	201.279	229.183	199.760
Pseudo R^2	0.087	0.122	0.125	0.136	0.155	0.112
AIC	1708.716	1333.604	1329.711	1338.773	1346.869	1599.347
BIC	1764.630	1425.005	1421.112	1500.068	1604.941	1654.843

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 8: 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)	–	0.0093	0.0099	0.0077	0.0075	–
		(0.623)	(0.603)	(0.683)	(0.690)	
Age	–	0.0006	0.0006	0.0006	0.0004	–
		(0.337)	(0.321)	(0.388)	(0.570)	
Years of education	–	0.0125***	0.0123***	0.0130***	0.0134***	0.0112***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Net income (T€)	–	-0.0044	-0.0043	-0.0065	-0.0073	–
		(0.406)	(0.424)	(0.230)	(0.184)	
Environmental controls						
Ambient temperature	–	0.0038*	-0.0006	0.0007	-0.0036	0.0037*
		(0.093)	(0.846)	(0.863)	(0.536)	(0.057)
Media attention	–	-0.0003	–	–	–	–
		(0.349)				
Climate change attitudes and beliefs						
Personal benefits	0.0348***	0.0364***	0.0361***	0.0337***	0.0356***	0.0326***
	(0.002)	(0.003)	(0.004)	(0.007)	(0.004)	(0.004)
Future benefits	0.0498***	0.0506***	0.0499***	0.0504***	0.0494***	0.0567***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Negative contributions	0.0383***	0.0382***	0.0385***	0.0404***	0.0369***	0.0371***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Footprint estimate (Tt)	-0.0006	-0.0006	-0.0006	-0.0007	-0.0006	–
	(0.305)	(0.335)	(0.350)	(0.308)	(0.311)	
Footprint confidence (d)	-0.0960***	-0.1076***	-0.1071***	-0.1025***	-0.1050***	-0.1050***
	(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)	(0.076)	(0.059)	(0.082)	(0.047)	(0.033)
EUA price confidence (d)	0.0655**	0.0670*	0.0696*	0.0607*	0.0642*	0.0598*
	(0.046)	(0.068)	(0.059)	(0.096)	(0.083)	(0.068)
EUA availability (d)	-0.0015	-0.0181	-0.0168	-0.0192	-0.0153	–
	(0.940)	(0.377)	(0.412)	(0.345)	(0.450)	
Additional controls						
Survey completion time	–	0.0001	0.0001	0.0001	0.0001	–
		(0.626)	(0.644)	(0.639)	(0.688)	
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	1981.000	1598.000	1598.000	1598.000	1598.000	1900.000
Log-likelihood	-844.358	-649.802	-647.856	-639.386	-625.434	-789.674
χ^2	162.478	180.448	184.341	201.279	229.183	199.760
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AIC	1708.716	1333.604	1329.711	1338.773	1346.869	1599.347
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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