To Mitigate or Not to Mitigate: The Price Elasticity of Pro-Environmental Behavior*

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Abstract

This paper investigates the relationship between the price of pro-environmental behavior (PEB) and individuals' voluntary choice to engage in PEB. Extending previous literature that has used indirect variations of the price, we exploit a unique empirical opportunity provided by markets for tradable emission permits in order to derive a price elasticity based on *direct* price variation. In an online field experiment we observe a representative sample of 2,440 subjects deciding whether to reduce the emissions of CO_2 into the atmosphere by one metric ton or receive a monetary reward between €2 and €100. In contrast to the previous evidence, the theoretical prediction of a clear negative relationship between price and public goods provision is borne out by our experimental data. We estimate an elasticity of probability across the treatment range of about −0.3. This inelastic response of the probability of PEB to direct price variation is robust with respect to a range of

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controls and with respect to the potential problem of field price censoring.

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1 Introduction

Individual pro-environmental behavior (PEB) has been invoked by many as an important complement, or even substitute, for mandatory policy measures to improve environmental quality. This is also the case in the climate change debate where policy makers, scholars, and other commentators have emphasized the potential of voluntary behavioral change in order to reach mitigation targets (e.g., Gore and Guggenheim, 2006; Pachauri, 2007; Vandenbergh and Steinemann, 2007; European Commission, 2011).[1]

In light of this potential, individual PEB and the factors driving it have been subject to extensive study in economics and other disciplines.[2] Factors found in empirical studies range from external economic and sociodemographic variables such as household income and size (Clark, Kotchen, and Moore, 2003; Kotchen and Moore, 2007), education (Diederich and Goeschl, 2014), knowledge about the relevant environmental problem (Bamberg and Möser, 2007; Diederich and Goeschl, 2014), and the presence of offsetting behavior (Kotchen, 2006; Kotchen and Moore, 2007; Jacobsen, Kotchen, and Vandenbergh, 2012) to internal psychological variables such as pro-environmental and altruistic attitudes (Clark, Kotchen, and Moore, 2003; Kotchen and Moore, 2007; Diederich and Goeschl, 2014), moral and social norms (Bamberg and Möser, 2007), feelings of guilt (Bamberg and Möser, 2007), expected personal benefits from improving environmental quality (Diederich and Goeschl, 2014), and religious beliefs (Owen and Videras, 2007).

A variable that has so far received little attention in the empirical literature is the *price* of pro-environmental behavior. Given the centrality of prices and

[1] For example, Vandenbergh and Steinemann (2007) point out the significant share of greenhouse gas emissions that are under the direct influence of individual households, such as transportation, heating and air conditioning, and lighting. They estimate the share of U.S. emissions that can be directly traced back to individual consumer decisions to amount to 1.85 billion tons in 2000 (roughly 35% of total U.S. emissions). This illustrates, in their view, the inherent potential of voluntary PEB.

[2] Several authors have pointed out that while each discipline has a tradition of focusing on variables close to their area of expertise, much progress has been made in reconciling and unifying the approaches (Clark, Kotchen, and Moore, 2003; Turaga, Howarth, and Borsuk, 2010). The area of behavioral environmental economics is such an example.

price elasticities for an economic understanding of the relationship between preferences and observed choices, our knowledge about the price-PEB relationship remains surprisingly scant. The reasons for the paucity of evidence are clear: Rather than being directly observable, variations in the price of PEB are often implicit, a property that PEB shares with other public or charitable goods. The implicit price of PEB arises frequently as a non-monetary opportunity cost (such as spending time and effort to recycle or volunteer as an activist) and even when opportunity costs are monetary (such as in the case of donations to environmental projects), the unit relationship between the size of the contribution (in dollars) and the desired outcome (e.g. in terms of habitat conservation) is often unclear. Observing the economic trade-off that individuals face when they decide to engage in PEB is therefore empirically challenging.

In face of the empirical challenge, the environmental economics literature has followed three broad approaches in order to get at the price elasticity of engaging in PEB. Building on the pioneering work by Feldstein and Taylor (1976) and Feldstein and Clotfelter (1976) in public economics,[3] there is a small empirical literature that has exploited observable variations in the marginal income tax rate between households to study the price effect in settings in which PEB such as donations to wildlife programs is tax deductible and, therefore, subsidized. Examining evidence from the Minnesota tax checkoff program for nongame wildlife conservation, Eubanks and Wyckoff (1989) find that the volume of the individual donation is highly price elastic. Yen, Boxall, and Adamowicz (1997), on the other hand, find no statistically significant evidence for a non-zero price elasticity, neither for the decision to contribute nor for the levels of contributions to wildlife habitat protection in three Canadian prairie provinces.

The second approach employs laboratory and field experiments to measure the effect of price by exogenously varying the so-called "match ratio", i.e. the amount of money that some third party will contribute for every unit of money

[3]See, e.g., Peloza and Steel (2005) for a comprehensive review of the public finance literature and its empirical estimates of the price elasticity of giving.

donated by the subject (Rondeau and List, 2008; Kotani, Messer, and Schulze, 2010). A major benefit of this approach is that the researcher is no longer restricted by given variations in marginal income tax rates. Instead, exogenous variations in the price of PEB can be introduced in a controlled manner and independent of subjects' household income. Matches are also a familiar feature of fundraising, easy to implement, and the conversion of match ratios into theoretically equivalent price changes is simple. A 1:1 (1:2) match ratio should have the same effect as a reduction in the price by 50% (67%). In a field experiment on donations for conservation measures, for example, Rondeau and List (2008) compare a treatment in which subjects have to provide every dollar going to conservation out of their own pocket with a treatment in which every dollar donated is matched 1:1 so that the amount going to conservation is doubled. Like other field experiments on matched fundraising (Karlan and List, 2007; Eckel and Grossman, 2008; Karlan, List, and Shafir, 2011; Huck and Rasul, 2011) and like Yen, Boxall, and Adamowicz (1997) above, they find that the implicit price elasticity regarding the decision of subjects whether to contribute or not (i.e., at the extensive margin of giving) is not significantly different from zero.[4]

A third approach studies prices for "green goods". Here, the price of PEB varies on account of differences in the price premium that consumers pay to purchase a "green" version of a good, i.e. a version that has fewer negative environmental side effects. To our knowledge, price effects have so far only been studied for "green electricity". For example, Kotchen and Moore (2007) finds a negative effect of price on the probability of participating in a green electricity program.[5] Mewton and Cacho (2011) estimate a price elasticity of -0.95 for a price premium for green electricity in Australia.

[4]The study by Kotani, Messer, and Schulze (2010) does not allow price elasticities to be determined

[5]A potential caveat is that the authors vary an "effective" price that multiplies an identical price premium per unit of electricity with the demanded quantity. Thus, the price per unit of environmental benefit provided (i.e. per unit of reduced emissions from not using conventional electricity) does in fact not vary. Rather, it is the total amount spent on participating in the program that varies. This may or may not capture a more conventional price effect.

Taken together, our current knowledge on the price-PEB link is not only reliant on a relative small set of studies. It also relies on evidence from approaches in which individuals do not observe prices directly. Instead, the effect of prices on PEB is *inferred* on the basis of effects of either tax rebates, match ratios, or price premiums on PEB. The implicit assumption is that subjects' response to variations in these instruments can safely be interpreted as those of the theoretically equivalent price variation. The validity of this assumption, however, is not beyond dispute: Experimental evidence on charitable giving shows that match ratios and their theoretically equivalent rebate rates give rise to systematically different behavior among potential contributors, both in the laboratory (Eckel and Grossman, 2003) and in the field (Eckel and Grossman, 2008). In a laboratory experiment, Davis and Millner (2005) show that rebates, matches, and direct price variations for private goods that should be equivalent on theoretical grounds produce systematic differences in the quantity responses to these vehicles. As a result, price elasticities derived on the basis of variations in rebate rates, match ratios, and price premiums are not guaranteed to be unbiased estimators of the *price* elasticity in a narrow sense.

The present paper introduces a fourth approach to studying the price-PEB link by estimating the price elasticity of PEB on the basis of *direct*, rather than indirect, variations in the price of PEB. Such direct price variations are possible thanks to the adoption, by policy-makers, of tradable emissions permits as a pollution control instrument. The fact that tradable permits come with prices creates the opportunity for a framed field experiment[6] in which subjects directly observe the price of a permit and decide whether to give up money in order to withdraw the permit from the trading scheme.[7]

[6]We follow the terminology by Harrison and List (2004) here.

[7]The basic idea of simply using direct price variation as a treatment in an experiment on giving is, of course, not new. For example, Andreoni and Miller (2002) and Andreoni and Vesterlund (2001) introduce, in a within-subject variant of the dictator game in the lab, a direct variation in the price of giving by changing how many units of their experimental endowment a dictator has to give up in order to transfer a unit to the recipient. However, the idea has to our knowledge not been used in the context of public goods provision and in a framed field experiment. The latter enables us to control for a number of subject attributes such as age (e.g. List, 2004), gender (e.g. Andreoni and Vesterlund, 2001), education (e.g. Karlan, 2005) and culture (Ockenfels and Weimann, 1999; Brandts, Saijo, and Schram, 2004;

Such a *direct* variation closely relates to the notion of the price effect from the theory of the private provision of public goods (Bergstrom, Blume, and Varian, 1986; Andreoni, 1990). In this paper, we analyze data from such an experiment and observe the effect of price on the probability to contribute. The binary contribution decision observed presents a useful test bed for the possible added value of a new approach since despite a clear theoretical prediction, the negative impact of a higher price on the propensity to engage in PEB has eluded approaches based on indirect price variation, specifically, match ratios (Rondeau and List, 2008) and tax rebate rates (Yen, Boxall, and Adamowicz, 1997). Recovering the predicted negative effect from the data would be a first indication that direct price variation can replicate basic theoretical results.

The experiment was administered to a non-student population of 2,440 subjects, employing a between-subjects design. Subjects were randomly assigned to one of 50 price treatments. The experimental price of retiring an European Union Emission Trading Scheme (EU-ETS) Phase II emissions allowance (EUA) represented one metric ton of CO_2 emissions reductions and varied between €2 and €100. Based on this design, we estimate a direct price effect on the probability to contribute to the public good. In contrast to earlier studies, the price elasticity of PEB is not zero, but negative and statistically significant. On average, increasing the price for supplying a unit of the public good by €10 decreases the probability that the individual will contribute by around 1%. Estimated across all price treatments, the probability to contribute has a price elasticity of about -0.3. The decision to engage in PEB is thus highly price-inelastic, with implications for whether subsidizing PEB constitutes a good use of social funds. There is some evidence of non-linearity in the price effect, but the net effect is vanishingly small within the treatment range. The direct price effect therefore confirms the theoretical prediction that, all else equal, the number of contributors is a decreasing function of the price of contributing.

The paper proceeds as follows: We explain the experimental design consid-

Brosig-Koch et al., 2011) that conceivably interact with the price effect and also to check for the presence of field price censoring among subjects.

erations and procedures in Section 2. Section 3 presents the empirical analysis and discusses the results. Section 4 concludes.

2 Experimental design

The estimation of the direct price effect on the individual probability of engaging in PEB relies on an experimental design that directly manipulates this price. Basic economic intuition dictates that in a sufficiently heterogeneous and large population, a higher price of giving will be associated with fewer individuals deciding in favor of contributing. The intuition can be confirmed by introducing a unit price for the public good into a variant of Andreoni's 1989; 1990 classical impurely altruistic model (Appendix A). The experimental implementation of the intuition corresponds to combining the idea of direct price variation by the experimenter (e.g. Andreoni and Miller, 2002; Andreoni and Vesterlund, 2001) with the idea of controlled contributions to a public good explored by Kingma (1989); Eckel and Grossman (1996); Karlan and List (2007); Eckel and Grossman (2008); Karlan, List, and Shafir (2011), to name just a few. The core feature of the treatment condition consists of different units of experimental pay-off that subjects have to give up in order to engage in a fixed amount of PEB. The PEB used in this context is a verified CO_2 emissions reduction which is realized in the form of the documented and verifiable retirement ("deletion") of one Phase II EUA. Retiring one EUA lowers the total ceiling of the Scheme, and hence emissions, by one ton.[8]

Subjects are randomly assigned to one of the 50 different treatment groups, differentiated by price. The price of contributing ranges, in increments of €2, from €2 to €100, the upper bound corresponding to current estimates of the

[8] Among several possibilities, the framework of the EU ETS, regulating the bulk of industrial CO_2 emissions across EU member states, provides a particular reliable and transparent technology for real contributions to global greenhouse gas emissions reductions in an experiment. One major reason is that it avoids the problem of additionality known for Certified Emission Reductions under the Kyoto Protocol and other offsets (Diederich and Goeschl, 2014). In addition, EUAs are not paper currency and have therefore no curiosity value as a tangible *private* commodity. Total EU emissions for the relevant trading period for this experiment were capped at 1.856 billion tons.

maximum marginal cost of emissions reductions per metric ton of CO_2 (Tol, 2010). Subjects only decide whether to contribute or not the one ton emissions reduction at the given price. They do not learn about others' choices before, during, or after the experiment. We adopt a strict between-subjects design which adds further robustness to our procedure as it does not provide subjects with a reference point such as when testing within-subject variations.

Subjects' choices are implemented under a random incentive system (RIS) in order to limit total cost of the experiment (Grether and Plott, 1979; Starmer and Sugden, 1991; Lee, 2008). The RIS is between-subjects (Tversky and Kahneman, 1981; Abdellaoui et al., 2011; Baltussen et al., 2012) with odds of 1:50 that the subject's choice (of either cash or contribution) was realized. In the instructions on the experimental screens, the between-subjects RIS is framed as a lottery in which the winners' prize choices will be implemented.[9]

Like in most lab experiments, both the monetary reward and the PEB opportunity in the present design are "on the house".[10] In the literature, there is an ongoing debate on potential effects of "house money" on contributions in public good experiments.[11] Based on these results, however, there is little

[9]Between-subjects and within-subject RIS have been subjected to examination for possible biases. While between-subjects introduces noise and decreases risk aversion, there is less evidence of a systematic bias for simple tasks (Cubitt, Starmer, and Sugden, 1998; Baltussen et al., 2012). In one example, between-subjects RIS has been shown to affect behavior in dictator games (Sefton, 1992) while for ultimatum games, behavior was unaffected (Bolle, 1990).

[10]An alternative procedure that was considered would have involved requiring subjects to give up own money when choosing to contribute to the public good. Our choice in favor of the standard lab procedure was mainly due to questions of practicality and the cost of time and effort to the subject of transferring funds in an Internet experiment from the subject to the experimenter. For example, the infrastructure of our cooperation partner is not designed to facilitate payments *from* subjects to the company. Cost of time and transaction costs for subjects are equivalent to an individual minimum price on the contribution that would be unobservable and therefore out of control of the experimenter.

[11]The evidence on a "windfall" (Keeler, James, and Abdel-Ghany, 1985) or "house money" (Thaler and Johnson, 1990) effect in public goods experiments, 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) find 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% when using house money. Engel and Moffat (2012) use a panel version of the double hurdle model on the same data and find that house money increases the probability

evidence to inform whether price elasticities would be affected by a difference in contribution probabilities, if any.

2.1 Subjects and procedures

The framed field experiment was administered via the Internet to a non-student population of 2,440 subjects drawn from the approximately 65,000 Internet panel members of the German section of YouGov. The sample was representative for Germany's Internet using population of voting age.[12] The choice of population has some significance for an experiment that relies on economists' view of emissions reductions as public goods contributions: Irrespective of age, sex, education, or political orientation, previous surveys have concluded that German citizens overwhelmingly accept the empirical veracity of climate change and its anthropogenic cause in the form of greenhouse gas emissions (European Commission, 2008). An exit questionnaire was administered to all subjects that confirmed the prior evidence.

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 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).[13]

Following the introductory screen, there was a filter screen to focus on German subjects.[14] Participants then faced a sequence of 10 to 13 computer

of being a "potential contributor". Carlsson, He, and Martinsson (2013) find in a dictator game that subjects behave more generously with house money than with own money both in the lab and in the field.

[12]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 1% 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.

[13]The polling company usually incentivizes panel members participating in a in polls 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.

[14]Subjects of other nationalities were redirected to other surveys running at the same

screens, depending on their decisions. To help to prevent subjects from "rushing" through the survey, each question required an answer 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.

The centerpiece of the experiment 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 choosing the emissions reduction results in a real, reliable, and verifiable reduction in EU CO_2 emissions through the deletion of an EUA, and (3) an explanation of the RIS with odds of 100 in every 5,000.[15] Furthermore, the text reminded subjects of the purely public nature of the contribution. Like in other field experiments on public and charitable goods, the instructions did not contain further information on what the precise public goods effects of this form of PEB are.[16] Instructions were kept short and simple in order to avoid well-known biases and misinterpretations that arise when potentially choice-relevant information about the public good is given around the time of the contribution decision (Arrow et al., 1993).

The *decision* screen of the experiment explained how the subject's choice would materialize if the subject was drawn in the lottery.[17] The screen then

time.

[15]The number of participants implied here is due to additional experiments running at the same time.

[16]When subjects in comparable experiments in public economics are invited to contribute to give to a liberal political organization (Karlan and List, 2007; Karlan, List, and Shafir, 2011), a public radio station (Eckel and Grossman, 2008), to a children project of an opera house (Huck and Rasul, 2011), or to CO_2 emissions reductions, information about productivity should matter. Despite this, giving decisions are typically poorly informed (Krasteva and Yildirim, 2013). Other authors also find that when given the opportunity, subjects make only modest effort to access additional relevant information (Berrens et al., 2004) and no more than one third of subjects have a positive willingness to pay for relevant information (Fong and Oberholzer-Gee, 2011).

[17]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 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 €50, wired to the member's bank account. Subjects were notified about the retirement of EUA issue numbers which they could verify

Table 1: Summary statistics of sample sociodemographics

Variable	Description	Mean	SD	N
Female	Indicator variable for gender	0.469	0.499	2,354
Age	Subject's age (years)	45.42	14.68	2,352
Education	Years of education based on subject's highest educational degree	12.27	3.213	2,299
Income	Midpoint of subject's monthly household net income category (in thousand €)[a]	2.556	1.706	1,950
East Germany	Indicator variable for residence on former GDR territory	0.190	0.392	2,354

Notes: [a] 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.

collected the subject's choice of either the specific cash award or the real emissions reduction, which were presented on the screen in a randomized ordering. Subjects that chose the cash prize were automatically directed to a screen that provided them with an non-incentivized opportunity to explain their choice, which we describe in more detail below.

The experiment concluded with a set of follow-up questions eliciting subjects' perceptions and beliefs about EUAs and emission reductions as well as sociodemographics (age, gender, income, education, residence). Correlation of the latter variables with subjects' profiles on record with YouGov was checked. Table 1 presents summary statistics. The nature of the Internet experiment also allowed us to observe when exactly subjects completed the experiment and how much time subjects spent at each screen.

The Internet experiment ran in two sessions in May and July 2010. Session 1 lasted from May 25th to June 2nd and generated 1,640 complete observations from 1,817 invitations. Session 2 lasted from July 19th to 27th and generated 800 complete observations out of 888 invitations. On average, 49 subjects were randomly assigned to each of the 50 experimental prices. Subjects completed the experiment with a median completion time of five minutes.[18] Prior to

through a public-sector Internet site we provided.

[18]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.

the experiment, a set of pre-tests and a pilot experiment with 200 economics students at Heidelberg University helped testing and refining the online implementation and the wording of the instructions.

2.2 Field price censoring

A well-understood challenge created by directly varying prices in order to determine the price effect is that it can give rise to 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, Lau, and Williams, 2002; Cherry et al., 2004; Harrison, Harstad, and Rutström, 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 who would otherwise have chosen the public good contribution might choose the cash prize instead because they believe that they are able to provide an equivalent CO_2 emissions reduction at a lower total cost (including time and transaction costs) than the prize offered as an alternative.[19]

Two aspects are relevant for detecting the possible presence of FPC in the experiment. First, it is relatively costly for private individuals to purchase and delete EUAs at the going spot price (€15 per metric ton at the time of the experiment)—a fact that largely excludes the possibility of FPC from perfect substitutes.[20] A subset of subjects may be aware that a variety of

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

[20]The EU ETS gives private individuals the opportunity to open an account for a fixed fee of €200. Holding an account, however, is only a prerequisite trading EUAs. Trading EUAs requires broker status on an trading platform. Trading of quantities of less than

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[21] or purchasing an emissions offset based on a carbon reduction project[22] 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.[23] 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, Harstad, and Rutström, 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, Lau, and Williams (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 prize 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 two prizes, I did not want to forgo*

several thousand units is therefore only possible and meaningful with the help of additional intermediary.

[21] 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.

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

[23] 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 29).

the chance of winning x Euros'), but checked the second option ('*I believe that there is another way for me to reduce CO_2 emissions by one ton for less than x Euros.*') or made a qualitatively equivalent statement in the open-ended third option ('*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 in the follow-up survey. Third, an open-ended question in the survey 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 3.2 reports on several robustness checks for our results with respect to a potential bias from FPC.

3 Results and Discussion

Of the 2,440 experimental subjects, 382 contributed to the public good. We observe contributions in each of the 50 price treatments between €2 and €100. In 48 treatments, the share of contributors exceeds zero at the 5% level of significance, using a one sided t-test. Of the 2,058 subjects that decided not to contribute, 86 subjects expressed some form of disbelief about the payment or the real provision of the public good in the survey answers and therefore were excluded from the analysis.[24]

Using a probit model to analyze subjects' choices the basic specification employed is

$$Y_i^* = \gamma_0 + \gamma_1 P_i + \gamma_2 P_i^2 + \mathbf{N}_i \gamma_3 + \varepsilon_i \tag{1}$$

with Y_i^* denoting the latent variable for subject i's decision ($Y_i = 1$ if she chose the contribution to the public good), P_i denoting the size of the cash prize offered to subject i, and $\mathbf{N}_i$ representing a vector of non-price controls including the subject's sociodemographic attributes and indicator variables for experimental session, day, and daytime.

Tables 2 and 3 report the probit coefficient estimates and the corresponding

[24]Results are not sensitive to their inclusion or exclusion.

Table 2: Probit model of choosing the contribution

	(1)	(2)	(3)	(4)
Price (1 unit=€10)	-0.0383***	-0.2233***	-0.0403***	-0.2294***
	(0.011)	(0.043)	(0.012)	(0.049)
Price squared	–	0.0185***	–	0.0188***
		(0.004)		(0.005)
Female	–	–	0.0952	0.0925
			(0.076)	(0.076)
Age	–	–	0.0037	0.0036
			(0.003)	(0.003)
Education	–	–	0.0641***	0.0639***
			(0.011)	(0.011)
Income (1 unit=€1,000)	–	–	-0.0258	-0.0253
			(0.022)	(0.023)
East Germany	–	–	-0.1092	-0.1043
			(0.095)	(0.096)
Constant	-0.7947***	-0.4904***	-1.7739***	-1.4482***
	(0.061)	(0.090)	(0.283)	(0.295)
Additional controls	No	No	Yes	Yes
N	2354	2354	1872	1872
Log-likelihood	-1037.451	-1027.442	-786.483	-778.666
χ^2	12.749	32.767	81.359	96.991
Pseudo R^2	0.006	0.016	0.049	0.059

Notes: Additional controls include dummies for experimental session, day, and daytime. Standard errors are in parentheses. *** Significant at or below 1%, ** Significant at or below 5%, * Significant at or below 10%.

sample mean marginal effects, respectively, of four different models based on eq.(1). The first two columns of both tables report on price-only specifications: Column (1) estimates a linear price effect while column (2) allows for a non-linear component. Columns (3) and (4) augment the price-only specifications by including the non-price controls.

3.1 Estimated Price Elasticities of PEB

Table 2 delivers a highly significant ($p \leq 0.001$) negative effect of price on the probability of PEB. Estimation of marginal effects in Table 3 shows that raising the price of the contribution by €10 at the sample mean decreases the propensity to contribute to the public good by approximately 1%. The effect is also robust: The magnitude of the price effect changes only slightly

Table 3: Marginal effects

	(1)	(2)	(3)	(4)
Price (1 unit=€10)	-0.0093***	-0.0541***	-0.0094***	-0.0531***
	(0.003)	(0.010)	(0.003)	(0.011)
Price squared	–	0.0045***	–	0.0044***
		(0.001)		(0.001)
Female	–	–	0.0223	0.0215
			(0.018)	(0.018)
Age	–	–	0.0009	0.0008
			(0.001)	(0.001)
Education	–	–	0.0150***	0.0148***
			(0.003)	(0.003)
Income (1 unit=€1,000)	–	–	-0.0060	-0.0059
			(0.005)	(0.005)
East Germany	–	–	-0.0246	-0.0233
			(0.021)	(0.021)

Notes: Marginal effects are evaluated at the sample means of the regressors. Additional controls include dummies for experimental session, day, and daytime. Standard errors are in parentheses. *** Significant at or below 1%, ** Significant at or below 5%, * Significant at or below 10%.

when adding non-price controls in column (3). Columns (2) and (4) illustrate that the "demand schedule" for the contribution is slightly, but significantly, convex. Estimating a single measure of elasticity from the results gives the elasticity of the probability of contributing[25] as -0.31 (S.E. 0.09) based on column (1) and as -0.33 (S.E. 0.11) based on column (3).

A price elasticity estimate for the decision whether to engage in PEB at around -0.3 is noteworthy for a number of reasons. First of all, it differs from the comparable evidence derived on the basis of indirect prices. Even though the match ratios in Rondeau and List (2008) lead to price reductions of up to 50% and despite considerable variation in marginal tax rates in Yen, Boxall, and Adamowicz (1997), these studies have found no evidence that the decision to engage in PEB varies with the price.[26] Secondly, the empirical result of a

[25]The elasticity of probability is defined as $\eta_{\Pr} = \frac{\partial \Pr(Y=1)}{\partial p} \frac{p}{\Pr(Y=1)}$ where p denotes the cash prize (LeClere, 1992).

[26]In line with their results for PEB, indirect price variation through matches and rebates has also not been found to shift the probability of engaging in charitable giving, whether the recipient is a political campaign organization (Karlan and List, 2007; Karlan, List, and Shafir, 2011), a public broadcasting service (Eckel and Grossman, 2008), an educational outreach program of the fine arts (Huck and Rasul, 2011), or rural health care facilities

Table 4: Price effects on sub-intervals of prices

	Specification (1) (price only)			Specification (3) (with controls)		
Price range	N	Marg. eff. (1 unit=€10)	Elasticity	N	Marg. eff. (1 unit=€10)	Elasticity
$2 \leq p < 15$	326	-0.1535***	-0.5462**	235	-0.1806***	-0.6904**
		(0.056)	(0.222)		(0.065)	(0.288)
$2 \leq p < 35$	791	-0.0571***	-0.5496***	624	-0.0545***	-0.5625**
		(0.014)	(0.151)		(0.016)	(0.180)
$2 \leq p \leq 100$	2,354	-0.0093***	-0.3076***	1,872	-0.0094***	-0.3333***
		(0.003)	(0.090)		(0.003)	(0.107)

Notes: Specifications are based on Table 3. Standard errors are in parentheses. *** Significant at or below 1%, ** significant at or below 5%, * significant at or below 10%.

negative significant price elasticity successfully recovers the theoretical prediction that in a heterogeneous population, the share of individuals engaging in PEB should vary negatively with the price of doing so. The direct price approach therefore returns a price elasticity estimate that is in line with basic economic intuition. Thirdly, the elasticity estimate is robustly smaller than one. Thus, the share of individuals that engages in PEB responds inelastically to a price change. This can be compared to the elasticity of probability of other exogenous variables in the data (LeClere, 1992). Another variable that exhibits a large degree of variation and that highly significantly correlates with the probability of PEB is education. With an elasticity estimate of about 1.2 based on Tables 2 and 3, the probability of PEB turns out not only to be elastic with respect to years of education but also to be more affected by increases in education relative to decreases in price.

As a robustness check, we estimate the price elasticity across increasingly wider intervals of the treatment range. The results are reported in Table 4. We first consider the price interval between the lower bound (€2) of the treatment range and the current field price at the time of taking the experiment (€15). This price interval compares most closely with previous research that has examined match ratios and rebates since their common effect is to lower the price relative to the price in the field. We find a price elasticity of the probability to engage in PEB of -0.55 without non-price controls and -0.69

(Smith, Kehoe, and Cremer, 1995).

with controls. This estimate provides reassurance that the effect across all treatments does not obscure a perfectly inelastic extensive margin for experimental prices below the field price. On the contrary, the estimate points to a more elastic demand compared to the entire range. Enlarging the price interval to €35 includes the historical maximum of the EUA price range in the field and therefore covers all prices that subjects could conceivably have observed or heard about. The estimated elasticity comes in at -0.55 without and at -0.56 with additional controls, pointing to a small reduction in the elasticity. The final row repeats the corresponding estimates for the entire treatment range, which also covers realistic field prices for voluntary CO_2 emissions reductions that have so far not materialized. Taken together, the range estimates of Table 4 reinforce the general pattern in Tables 2 and 3 that the extensive margin response is generally inelastic while more elastic for lower prices.

Before turning to the possibility of FPC as a potential source of bias, we discuss two possible concerns regarding the estimated direct price effect. One concern is about the possibility of an anchoring effect: When subjects are poorly informed or unfamiliar with a good (Green, 1992; List and Shogren, 1999), higher prices offered might lead uninformed subjects to infer that the good is more valuable than they originally thought. Such a response could conceivably induce affected subjects to choose the public goods contribution. Experimental prices would therefore confound the contribution decision, leading to an underestimation of the true direct price effect. 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 estimate of the carbon "footprint" caused by personal lifestyle, confidence in own estimate of the going EUA spot price) 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 experiment. The data, however, shows a non-negative relationship between the propensity to provide the mitigation effort

and the "information-weighted" price: More familiarity does not change the price elasticity of contributing (for the knowledge variables) or even decreases it (for education, see the following section). This resonates with experimental findings that price elasticity does not systematically vary with uncertainty about good characteristics (Heffetz and Shaya, 2009).

A second concern could relate to the possibility of an endowment effect, triggered by a perception that there is 'cash on the table' when subjects are offered a money reward in exchange for giving up the opportunity to engage in PEB. Whether such a perception is present is not observable with the chosen design. At the same time, it is worth recalling that *both* decision alternatives are "on the house"and that both are subject to the same lottery.[27] Concern about a bias in the price elasticity estimate would therefore have to invoke an asymmetry between the endowment effects induced by the monetary reward and the PEB opportunity that is simultaneously on offer. There is little evidence to support the notion that there are good-specific differences in endowment effects, but asymmetries have successfully been linked to differences in procedures (Plott and Zeiler, 2005). Choice order is one possible procedural source of asymmetric perception of the decision alternatives. Our design randomized the choice order across subjects.

3.2 Field price censoring

As pointed out earlier, one potential drawback of varying the price of contributing directly and in the field is the possibility of field price censoring (FPC) among subjects. If present, FPC has the potential of biasing results. In the limit, e.g. in the context of highly familiar goods, the presence and magnitude of the direct price effect could conceivably hinge entirely on the fact that subjects know or believe that they can provide the public good more cheaply outside the experiment.

To identify subjects possibly affected by FPC, we draw on the FPC "filter" statements described in Section 2.2 as well as on answers to the follow-up

[27] This also means that the design does not induce an asymmetry between the goods under prospect theory considerations (Plott and Zeiler, 2005; Isoni, Loomes, and Sugden, 2011).

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. The conservative strategy we adopt here is to use these answers to identify the observations that are *potentially* subject to FPC and test in three different ways whether their inclusion causes a bias in the overall price effect. Previewing the results, the available evidence points against a substantive bias in the price effect on account of omitted FPC. In all estimates, the coefficient for the price effect is not affected.

Table 5 summarizes subjects' FPC "filter" statements and identifies 511 (25.9%) of 1,973 cash choosing subjects who declare, by not checking statement 1 but checking statement 2, 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.[28] The question now is whether the inclusion of these subjects bias the estimate of the price effect in column 1 of Table 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 6 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. The regression uses the original sample, appended by the "filtered" reduced sample whose observations are identified by an indicator variable. The coefficient on the price variable repli-

[28] 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).

Table 5: 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"		Total
	0	1	
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

cates the significantly negative price effect of column 1 in Table 2. The price effect is not different in the reduced sample, as the insignificant coefficient of the interaction term demonstrates ($p = 0.69$). The coefficient on the indicator variable for observations belonging to the reduced sample shows a significantly higher probability of choosing the reduction since by construction, the "filter" statements leading to the reduced sample only exclude cash choosing subjects. We obtain a price elasticity of probability of -0.33 (standard error 0.089) if we compute it for the reduced sample only, compared to -0.31 (standard error 0.09) derived for the full sample.

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 from choosing cash to choosing the reduction. Column (2) compares the original and the recoded sample the same way column 1 does for the reduced sample. Again, a significant difference in the coefficients on cash prize cannot be established. The evidence based on the "filter" statements thus points against a significant bias from FPC.

Columns (3) and (4) of Table 6 present the results of the second part of the strategy to detect FPC. This part specifically targets FPC from the potential availability of a *perfect* substitute and is based on subjects' estimates of the going EUA spot price elicited in the ex-post questionnaire.[29] Table 7 gives a

[29]Evidence for endogenous 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

Table 6: Robustness of the price effect to field price censoring

	(1)	(2)	(3)
Price (€)	-0.0038***	-0.0038***	-0.0038***
	(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	–	–	–
EUA estimate below * cash prize	–	–	–
Constant	-0.7960***	-0.7960***	-0.7960***
	(0.061)	(0.061)	(0.061)
N	4199.000	4710.000	3714.000
Log-likelihood	-1970.881	-2594.222	-1698.694
χ^2	41.701	312.406	28.654
Pseudo R^2	0.010	0.057	0.008

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. *** Significant at or below 1%, ** significant at or below 5%, * significant at or below 10%.

Table 7: 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).

detailed summary of this variable. About 74% of subjects gave an estimate within 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 well informed about the field price (about €15 at the time of the experiment). Comparing assigned experimental cash prizes and estimated field prices, 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 a field price into account when pondering their contribution decision, then the choice of subjects who anticipate an EUA price below the experimental price may be affected by FPC.

As before, we compare the unconditional price coefficient of the full sam-

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% 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) and Fong and Oberholzer-Gee (2011). 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 differences in information prior to the experiment.

ple with that of a reduced sample. This time, the reduced sample excludes subjects potentially affected by FPC due to their EUA price estimate as explained above. Column (3) in Table 6 reports on the results. Again, the price 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).

In the third and final part of the detection strategy for FPC, 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.

4 Conclusion

The relationship between the price of pro-environmental behavior (PEB) and individuals' voluntary choice to engage in PEB is a natural subject of interest to economists, but poses a number of empirical challenges. Our current understanding of this relationship is informed by empirical and experimental approaches that have responded to this challenge by skillfully exploiting variations in rebate rates, match ratios, and 'green' prices. Variations in rebate rates and match ratios, in particular, can be easily converted into theoretically equivalent price variations. One surprising finding in this literature, that is echoed in the charitable giving literature, is that the decision whether to engage in PEB (the extensive margin) appears to be largely immune to variations in match ratios or rebate rates. Theory would predict, however, that in an heterogeneous population, the price elasticity of participating in PEB is negative. Further disturbance comes from experimental evidence that has refuted the behavioral equivalence between matches and rebates for public goods as well as between these indirect approaches and direct price variation for private goods. Using price elasticities derived on the basis of their theoretically

equivalent match rate or rebate rate elasticities may therefore be problematic.

This paper exploits field experimental evidence from a fourth approach to examining the relationship between the price of PEB and voluntary participation in PEB, namely through *direct* price variation. Markets for tradable emission permits provide a unique empirical opportunity to do so. We compare, across thousands of subjects, how the decision whether to mitigate the emission of one metric ton of CO_2 into the atmosphere systematically varies with the amount of money that subjects have to give up. In our results, the theoretical prediction of a negative relationship between price and public good provision at the extensive margin is clearly borne out by the experimental data. We estimate its mean elasticity across the treatment range as about -0.3. Our direct price effect is robust with respect to a range of controls and with respect to the potential problem of field price censoring.

To our knowledge, the price elasticities reported in this paper represent the first direct estimate on how the price of PEB impacts on individuals' decision to engage in PEB. Importantly, these elasticities are based on genuine price observations by subjects in a context of a price-per-unit environment and therefore represent a close empirical counterpart to theory. Moreover, they are based on a heterogeneous, Internet-representative sample of the population. Our result of a consistently inelastic response of the probability of PEB to price variation questions whether monetary subsidies of a price would be a preferred policy instrument to expand the set of contributors, in particular compared to the elastic response to education in our sample.

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Appendices

A Model of the direct price effect at the extensive margin of contributions

In Andreoni's (1989, 1990) classical impurely altruistic model of public good provision and its variants in the literature, the price of the public good is conventionally normalized to one along with the private good. We introduce a unit price for the public good to guide the intuition for the effects of a direct price change and of non-price factors at the extensive margin. Assume n individuals who derive utility from the amount of private numéraire x, the level of a public good G, and their own contributions to the public good of size g_i ("warm glow"). Let preferences also depend on a vector of individual-specific characteristics, $\boldsymbol{\theta}_i$. Thus, we write the utility function as

$$U_i = U\left(x_i, \delta_i G, g_i; \boldsymbol{\theta}_i\right)$$

where $\delta_i \in [0,1]$ denotes heterogeneous perceptions about the value of the public good (Karlan and List, 2006). Another interpretation of δ_i is incomplete information about the benefits produced by the public good. In our case, δ_i represents any heterogeneous beliefs about the size of climatic changes and thus the benefits generated by the total provision of emissions reductions.

Let the public good be measured in units which individuals can "purchase" and provide at price p. Total provision is the sum of individual provisions, $G = \sum_{i=1}^{n} g_i$. Also define $G_{-i} = \sum_{j \neq i} g_i$. Individuals are endowed with wealth w_i and thus maximize utility subject to their budget constraint,

$$\max_{x_i, g_i} U\left(x_i, \delta_i G, g_i; \boldsymbol{\theta}_i\right)$$

$$\text{s.t. } x_i + p g_i = w_i \tag{2}$$

$$G = G_{-i} + g_i \tag{3}$$

$$g_i \geq 0 \ . \tag{4}$$

Substituting for g_i, the problem reduces to

$$\max_{x_i, G} U\left(x_i, \delta_i G, G - G_{-i}; \boldsymbol{\theta}_i\right)$$

$$\text{s.t. } x_i + pG = w_i + pG_{-i}$$
$$G \geq G_{-i} \ .$$

U is assumed to be strictly quasi-concave and increasing in the first three arguments. Thus, if we ignore the inequality constraint for a moment, this resembles an ordinary consumer choice problem. The demand function for G solving the problem is

$$f\left(p, w_i + pG_{-i}, G_{-i}, \delta_i; \boldsymbol{\theta}_i\right) \ .$$

The third argument in f is the warm glow argument. Taking into account the

inequality constraint (4), demand for the public good is

$$G = \max\left\{f(p, w_i + pG_{-i}, G_{-i}, \delta_i; \boldsymbol{\theta}_i), G_{-i}\right\} \ .$$

In order to derive first-order effects at the extensive margin, we take the inverse of f with respect to the second argument, $w_i + pG_{-i}$ and add pg_i to both sides. Solving for g_i gives

$$g_i = (1/p)\left[w_i - f^{-1}\left(p, G, G_{-i}, \delta_i; \boldsymbol{\theta}_i\right)\right] + G \ .$$

Given (4), the condition to provide a strictly positive amount of public good is

$$w_i > f^{-1}(p, G, G_{-i}, \delta_i; \boldsymbol{\theta}_i) - pG \ .$$

Let w_i^*denote the threshold level of wealth at which individual i switches from non-contribution to contribution. Here, (4) holds with equality and thus, $G = G_{-i}$. It follows that

$$w_i^* = f^{-1}(p, G_{-i}, \delta_i; \boldsymbol{\theta}_i) - pG_{-i} \tag{5}$$

Note that the third argument of f^{-1} drops out since at $g_i = 0$ the individual does not derive any utility from warm glow. Also note that w_i^* is not identical for all individuals because of δ_i and $\boldsymbol{\theta}_i$.

We are now interested in how the set of contributors changes if certain parameters change. From (5) it follows that

$$\frac{\partial w_i^*}{\partial p} = f_p^{-1} - G_{-i} > 0$$

if we assume normality for both goods.[30] Thus, an increase in price *ceteris paribus* increases the threshold level of wealth for individual i, which makes it less likely that individual i will contribute. Similarly, normality of both goods

[30]Note that normality implies that any increase in wealth will always go in consumption of both goods.

implies that[31]

$$\frac{\partial w_i^*}{\partial \delta_i} = f_\delta^{-1} < 0 \ .$$

Intuitively, if individual i's perceived benefits from the public good provision increase then it is more likely that i will provide a strictly positive amount of the public good. With regard to individual characteristics, we have already demonstrated that w_i^* depends on $\boldsymbol{\theta}_i$.

B Instructions (translation of experimental screens into English) – *not for publication*

B.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.

B.2 Citizenship screen

Of which country do you hold citizenship?

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

B.3 Information Screen

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

[31] Note that an increase of δ_i in f^{-1} *ceteris paribus* implies lower demand for x, hence $f_\delta^{-1} < 0$.

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."

B.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[32] in bonus points

B.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:

B.6 Introduction follow-up questions

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

B.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?

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

( ) I know the price

( ) Very sure

( ) Somewhat sure

( ) Somewhat unsure

( ) Very unsure

( ) I don't know

B.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]

B.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

B.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).

B.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? ___

B.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

B.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.