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. Its approach combines two literatures: An experimental one that varies the price of consequential contribution decisions indirectly, and the stated preferences literature that elicits responses from subjects to direct, but hypothetical price variations. We apply this approach to the price elasticity of participating in climate change mitigation. In an online field experiment, we observe the decision of a representative sample of 2,440 subjects 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 previous findings from

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indirect price variation, but in line with stated preference approaches, voluntary PEB is found to have non-zero elasticity of probability (about -0.3 across the treatment range). The inelastic response of the probability of PEB to direct price variation is robust with respect to a range of controls and with respect to the potential problem of field price censoring.

Keywords: Pro-environmental behavior; environmental public goods; price elasticity; field experiment; online experiment; EU-ETS.

JEL Classifications: D10, H41, Q54

1 Introduction

Voluntary pro-environmental behavior (PEB) by individuals is often invoked as an important complement, or even substitute, for mandatory or coercive policy measures to improve environmental quality. Climate policies are a case in point: There, policy makers, scholars, and other commentators have emphasized the potential of voluntary behavioral change for reaching ambitious mitigation targets (e.g., Gore and Guggenheim, 2006; Pachauri, 2007; Vandenbergh and Steinemann, 2007; European Commission, 2011).[1]

Behavioral changes undertaken as part of PEB impose opportunity costs on individuals, and such opportunity costs constitute a "price" of PEB that individuals face. The relationship between this price and the share of individuals in an economy that will engage in PEB is of obvious interest to economists: How sensitive are individuals' decisions about participating in PEB to changes in that price? In other words, is engaging in PEB price-elastic or inelastic and if so, over which range of prices?[2] A better understanding of the price elasticity of PEB and the development of a toolbox for its measurement are valuable both to the academic economist and the policy-maker.[3] If voluntary PEB is

[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 roughly 35% of total U.S. emissions. This illustrates, in their view, the inherent potential of voluntary PEB. A variety of changes in individual behavior constitute PEB in the climate context, e.g. changes in consumption and travel patterns (IPCC 2014). In other environmental contexts, similar forms of PEB exist. Other examples are voluntary recycling of household waste, ecotourism, and reduced meat consumption.

[2]The price elasticity of the aggregate amount of PEB provided is an obviously related question. In the present paper, we focus purely on the extensive margin considerations.

[3]Other factors driving the decision to engage in individual PEB are of interest, too, and have been subject to extensive study in economics and other disciplines. Correlates

found to be broadly price inelastic, this means that the level of opportunity costs of PEB does not substantially affect the share of individuals in the economy that engage in PEB. It also implies that the price of PEB need not be known with great accuracy to predict the amount of PEB voluntarily provided with sufficient confidence. If, on the other hand, PEB is found to be price elastic, then meaningful ex-ante estimates of participation in PEB participation will hinge critically on the accuracy of cost predictions. Under price elasticity, policy also has to consider how the nature and scale of its own mandatory mitigation measures impact on the price at which voluntary PEB can be carried out, e.g. as mandatory programs exhaust least-cost approaches and raise the price of voluntary PEB. Finally, evidence on price elasticity also speaks to the economic merits of potential measures to subsidize voluntary PEB. Such measures, e.g. in the form of income tax rebates, are familiar from the charitable goods sector, and subsidizing voluntary PEB could be justified if it was found that price elasticity is high (Peloza and Steel, 2005).

Given the centrality of prices and price elasticities for understanding the participation of individuals in PEB, evidence about the price-PEB relationship under controlled conditions is scant. The reasons is that, rather than being directly observable, variations in the price of PEB are often implicit, a property

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).

that PEB shares with other public or charitable goods. The opportunity costs that determine the implicit price of PEB often take a non-pecuniary form (such as spending time and effort to recycle) 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 and recovering information on price elasticities of PEB is therefore empirically challenging.

Our paper builds on two distinct, but related literatures that try to address the empirical challenge. One is a literature that infers the price elasticity of PEB by observing behavioral changes associated with indirect variations in its price. There are three main approaches. The first exploits 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 propensity for an individual donation is 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. A second approach analyzes empirical data on prices for "green goods" where 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). This approach has been used to study, for instance, "green electricity": Kotchen and Moore (2007) finds a negative effect of price on the probability of participating in a green electricity program.[4]

The present paper has the greatest methodological parallels with the third approach, which employs laboratory and field experiments. These experiments exogenously vary the so-called "match ratio", i.e. the amount of money that some third party will contribute for every unit of money 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 or price premiums. 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

[4]The elasticity estimate at the extensive margin which the authors report is -0.83. Note, however, 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 public environmental benefit provided does in fact not vary. Rather, it is the total amount spent on participating in the program that varies. This limits comparability. A related paper, Mewton and Cacho (2011), provides an intensive-margin estimate of the price elasticity for green electricity in Australia of -0.95, but does not investigate the participation (extensive margin) decision.

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.[5]

While embracing the consequentiality paradigm of the experimental approach, we depart from a common element in this literature. This element is that individuals do not observe prices directly. Instead, the effect of prices on PEB is *inferred* on the basis of effects of indirect price variation through tax rebates, match ratios, or price premiums. Two considerations motivate our departure from the indirect price strategy. One is that it makes an implicit assumption of behavioral equivalence: Subjects' responses to variations in indirect price instruments can be interpreted as those of the theoretically equivalent direct price variation. The validity of the behavioral equivalence assumption 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, 2006) and in the field (Eckel and Grossman, 2008). Experiments by Davis and Millner (2005) show for standard private goods that elasticities with respect to match rates or rebates are biased estimates of price elasticity, narrowly defined. This raises as a possibility the presence of similar biases in public goods (such as PEB). The second, related,

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

consideration is the difficulty that experiments using *indirect* prices have in recovering the basic negative price effect predicted by theory at the extensive margin. This difficulty could either point to an unusually inelastic relationship between the decision to engage in PEB and its price or to the presence of the aforementioned biases.

Both considerations lead us to a second literature, the large body of research on stated preferences in environmental valuation. The core of many stated preference studies consists of eliciting responses by subjects to direct price variation. While price elasticities are rarely of central concern, many stated preference studies report extensive margin effects, e.g. the effect of bid or offer amount on stated participation as in the case of a dichotomous choice (DC) valuation question. The choice task in the present paper directly borrows from the format of single-bounded DC questions, even though important features of the choice setting differs. These differences limit the comparability of the bid offers and participation effects in most of these studies to the results from our design.[6] However, there are parallels with stated-preference papers that study willingness-to-pay (WTP) for individual carbon offsetting in the context of aviation (Brouwer, Brander, and Van Beukering, 2008; Akter

[6]The main difference between the "bid effect" of a typical DC stated preference study and the price effect on individual PEB studied here is that the former typically varies "prices" for a given policy change or for a scenario where some fixed amount of the environmental public good would be collectively provided (e.g., Akter and Bennett, 2011). Hence, the typical DC stated preferences study lacks the freeriding possibility implied by individual PEB. Several influential papers use study designs where freeriding is possible by estimating willingness-to-donate instead of willingness-to-pay (e.g., Champ et al., 1997; Champ and Bishop, 2001; Whitehead and Cherry, 2007). Although this does correspond to individual PEB, there is again a fundamental difference to studying the price effect: Variations in the offer amount for a donation simultaneously vary prices and quantities of the public good provided and thus are not equivalent to variations in the price per unit of public good provided.

et al., 2009; MacKerron et al., 2009; Lu and Shon, 2012) and, using the same mitigation vehicle that we employ, on stated willingness to purchase and retire EU carbon emissions allowances (Lindman, Ek, and Söderholm, 2013). Of these, only MacKerron et al. (2009) and Lindman, Ek, and Söderholm (2013) will be directly comparable to our approach because the offer bids in these two papers can be interpreted as a price per unit of PEB offered, i.e., hold constant the amount of emissions reduced. In contrast to the experimental papers using indirect price variation, these papers using hypothetical but *direct* (per-unit) price variation in a DC question format find a significant effect of price on participation and are consistent with an elastic relationship at the extensive margin. This difference in findings provides additional justification for an approach based on direct price variation.

The present paper studies the price elasticity of PEB by combining the experimental approach in which prices are exogenously manipulated in a consequential decision environment with the direct price variations common in the DC question of stated preference studies.[7] Consequential 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[8] in which subjects directly observe the price of a permit and decide

[7]The only other paper combining direct prices of PEB and consequentiality that we are aware of is Löschel, Sturm, and Vogt (2013) who sell EU emissions allowances in a market experiment. While the focus of these authors is again on WTP, not price elasticity, our design also differs from theirs in that it features a larger and considerably more heterogeneous subject sample, a wider range of prices offered, and by the design of the experimental task which in our case corresponds to a DC choice task.

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

whether to give up money in order to withdraw the permit from the trading scheme.[9] 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). Using this approach, we observe the effect of price on the *probability* to contribute to mitigation (the extensive margin of PEB). This binary contribution decision presents a useful test bed for combining consequentiality and direct, controlled price variation.

Our online experiment was administered to a sample of 2,440 subjects that was representative for the Internet-using population of German adults with respect to gender, age, and region of residence. Subjects were randomly assigned to one of 50 price treatments in a between-subjects design. The experimental price of retiring an 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 experimental studies, the extensive-margin 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

[9] 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; 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.

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.

The paper proceeds as follows: We derive the simple theoretical framework for the price elasticity of probabilty for PEB in Section 2 and explain the experimental design considerations and procedures in Section 3. Section 4 presents the empirical analysis and discusses the results. Section 5 concludes.

2 Theoretical framework

The basic price-participation prediction for PEB can be derived in a few simple steps from the classical public good models by Bergstrom, Blume, and Varian (1986) and Andreoni (1989, 1990). The required change is the introduction of an explicit unit price for the public good, thus replacing the standard normalization of that price. With the unit price of contributing explicit, it is then possible to derive the comparative statics for a variation in the price of providing the public good and to make the notion of the elasticity precise.

Assume a large economy with 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"). Individuals are endowed with wealth w_i, distributed across the economy based on a cumulative distribution function $F(w)$ with density $f(w)$. Let preferences also depend on a vector of individual-specific characteristics, $\boldsymbol{\theta}_i$. Individuals' utility function

can therefore be expressed as $U_i = U(x_i, \delta_i G, g_i; \boldsymbol{\theta}_i)$, where $\delta_i \in [0,1]$ denotes individual-level heterogeneity in the valuation of the public good (Karlan and List, 2006).[10]

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 thus maximize utility subject to their wealth constraint,

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

$$\text{s.t. } x_i + p g_i = w_i \tag{1}$$

$$G = G_{-i} + g_i \tag{2}$$

$$g_i \geq 0 \ . \tag{3}$$

Substituting for g_i, the problem reduces to

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

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

$$G \geq G_{-i} \ .$$

[10] Another interpretation of δ_i could be incomplete information about the benefits produced by the public good. In our case, δ_i could therefore represents any heterogeneous beliefs about the size of climatic changes and thus the benefits generated by the total provision of emissions reductions.

U is assumed to be strictly quasi-concave and increasing in the first three arguments. The demand function for G solving the problem is

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

Taking into account the inequality constraint (3), demand for the public good is

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

Assuming differentiability of γ and normal goods (Andreoni, 1989), the first-order effects at the extensive margin can be derived by taking the inverse of γ with respect to the second argument, $w_i + pG_{-i}$ and adding pg_i to both sides. Solving for g_i gives

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

Given (3), the participation condition to provide a strictly positive amount of public good is

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

Let w_i^* denote the threshold level of wealth at which individual i switches from non-contribution to contribution. Here, (3) holds with equality and thus, $G = G_{-i}$. It follows that

$$w_i^* = \gamma^{-1}(p, G_{-i}, \delta_i; \boldsymbol{\theta}_i) - pG_{-i} \tag{4}$$

The wealth threshold w_i^*, in combination with the cumulative probability density function of wealth in the population $F(w)$, defines the share of contributors κ as $\kappa = 1 - F(w_i^*)$.[11] The change in the share κ in response to variations in the price is then given by

$$\frac{\partial \kappa}{\partial p} = -f(w_i^*)\frac{\partial w_i^*}{\partial p}, \tag{5}$$

which is the negative of the density of the wealth distribution function at the threshold multiplied by the impact of the price change on the wealth threshold. From (4) follows that the overall sign of (5) is negative since, given normal goods, the second part of the right-hand side of (5) is positive,

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

In line with intuition, theory therefore predicts that the share of contributors κ decreases, *ceteris paribus*, in the price of contributing. The empirical question is the price elasticity of this share. This requires an exogenous assignment of prices of providing the public good to a large set of potential contributors. In the empirical implementation with individual level data, expression (5) is then equivalent to the change in the probability that an individual i in the economy is a contributor given a change dp in the price of contributing. The objective of the experimental design is therefore to determine, for a set of subjects, the elasticity of probability $\eta_{\Pr} = \frac{\partial \Pr(Y_i=1)}{\partial p}\frac{p}{\Pr(Y_i=1)}$, where Y_i is an

[11] Note that the threshold wealth w_i^* need not be identical for all individuals because of δ_i and $\boldsymbol{\theta}_i$.

indicator variable that takes the value of 1 for a contributor (LeClere, 1992). An elasticity below one then describes a less-than-proportionate change in the probability relative to the price variable, and vice versa. We turn to the empirical implementation below.

3 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. The experimental implementation combines 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.[12]

[12] 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

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 estimates of the maximum marginal abatement cost per ton of CO_2 equivalent (McKinsey & Company, 2010; Kesicki and Ekins, 2012). Subjects only decide whether to contribute or not the one ton emissions reduction at the given price, with the quantity of the emissions reduction fixed. 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.[13]

Like in most lab experiments, both the monetary reward and the PEB

a tangible *private* commodity. Total EU emissions for the relevant trading period for this experiment were capped at 1.856 billion tons.

[13]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).

opportunity in the present design are "on the house".[14] In the literature, there is an ongoing debate on potential effects of "house money" on contributions in public good experiments.[15] Based on these results, however, there is little evidence to inform whether price elasticities would be affected by a difference in contribution probabilities, if any.

3.1 Subjects and procedures

The framed field experiment was administered via the Internet to a sample of 2,440 subjects drawn from the approximately 65,000 Internet panel members of the German section of YouGov. The sample was representative with respect to age, gender, and region of residence for Germany's Internet using population of voting age.[16] The choice of population has some significance for an

[14] 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.

[15] 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 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.

[16] We test whether our sample differs from one drawn from the general population of German voters using census data from 2008 and 2009 by the Destatis Federal Statistical Office (Diederich and Goeschl, 2014). Using two-sided t-tests, we reject the hypothesis that the means of the socio-demographic characteristics coincide at the 1% level. Our subjects

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).[17]

Following the introductory screen, there was a filter screen to focus on German subjects.[18] Participants then faced a sequence of 10 to 13 computer 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.

are more likely to be male (53.1% vs. 47.9%), younger (average age 45.42 vs. 50.05 years), and educated (12.27 vs. 11.02 years) than the average German of voting age. Income is self-reported, and therefore the lower average income in the sample is unsurprising (€2,556 vs. €4,057). See also Table 1 for descriptive statistics.

[17]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.

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

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.[19] 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.[20] 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.[21] The screen then

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

[20]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).

[21]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

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. 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 (response rate: 90.3%). Session 2 lasted from July 19th to 27th and generated 800 complete observations out of 888 invitations (response rate: 90.1%). A subject was classified as an incomplete response by YouGov if she did not react to the invitation email, was redirected due to a different nationality, or abandoned the experiment before the final dismissal screen. 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.[22] Prior to the experiment, a set of pre-tests

through a public-sector Internet site we provided.

[22]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.

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

3.2 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.[23]

Two aspects are relevant for detecting the possible presence of FPC in the

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

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.[24] A subset of 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[25] or purchasing an emissions offset based on a carbon reduction project[26] 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.[27] In the context of the experiment, therefore, there is no single explicit field price that

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

[25]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.

[26]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.

[27]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 33).

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 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 4.2 reports on the robustness checks for our results with respect to a potential bias from FPC.

Table 1: Summary statistics of sample sociodemographics

Variable	Description	Mean	SD	Min	Max	N^a
Female	Indicator variable for gender	0.469	0.499	0	1	2,354
Age	Subject's age (years)	45.42	14.68	18	89	2,352
Education	Education based on subject's highest educational degree (years)	12.27	3.213	9	22	2,299
Income	Midpoint[b] of subject's monthly household net income category (in thousand €)	2.556	1.706	0.45	8	1,950
Eastern Germany	Indicator variable for residence on former GDR territory	0.190	0.392	0	1	2,354

[a] Missing observations result from unanswered survey questions. [b] 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.

4 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.[28] Table 1 presents summary statistics of the sociodemographic covariates.

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

$$Y_i^* = \gamma_0 + \gamma_1 P_i + \mathbf{N}_i \gamma_2 + \varepsilon_i \tag{6}$$

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

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

Table 2: Probit regressions

	(1)		(2)	
	Coeff.	Marg. Eff.	Coeff.	Marg. Eff.
Price (1 unit=€10)	-0.0383***	-0.0093***	-0.0403***	-0.0094***
	(0.011)	(0.003)	(0.012)	(0.0**03**)
Female	–	–	0.0952	0.02**22**
			(0.076)	(0.018)
Age	–	–	0.0037	0.000**9**
			(0.003)	(0.001)
Education	–	–	0.0641***	0.014**9***
			(0.011)	(0.003)
Income	–	–	-0.0258	-0.00**60**
			(0.022)	(0.005)
Eastern Germany	–	–	-0.1092	-0.02**54**
			(0.095)	(0.02**2**)
Constant	-0.7947***	–	-1.7739***	–
	(0.061)		(0.283)	
Additional controls	(No)		(Yes)	
N	2354		1872	
Log-likelihood	-1037.451		-786.483	
χ^2	12.749		81.359	
Pseudo R^2	0.006		0.049	

Notes: Dependent variable is one if the subject chose the emissions reduction. Additional controls include dummies for experimental session, day, and daytime. Marginal effects are average marginal effects. Standard errors are in parentheses. *** Significant at or below 1%, ** Significant at or below 5%, * Significant at or below 10%.

prize offered to subject i, and $\mathbf{N}_i$ representing a vector of non-price controls including subject's sociodemographic attributes and indicator variables for experimental session, day, and daytime.

Table 2 reports the probit coefficient estimates and the corresponding average marginal effects of two different models based on eq. (6). The first model is a price-only specification. The second model augments the price-only specification by including the non-price controls.

4.1 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 shows that raising the price of the contribution by €10 decreases the propensity to contribute to the public good by approximately one percentage point on average. The magnitude of the price effect changes only slightly when adding non-price controls in column (2). Following Section 2, these price effects can be converted into a single measure to express the elasticity of the probability of contributing (LeClere, 1992). Across the entire price range, this elasticity is estimated at -0.31 (S.E. 0.09) based on column (1) and -0.33 (S.E. 0.11) based on column (2).

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.[29] Secondly, the empirical result of a negative significant price elasticity successfully recovers the theoretical prediction from Section 2 that in a heterogeneous population, the share of individuals engaging in PEB should vary negatively with the price of doing so. The direct

[29] 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 (Smith, Kehoe, and Cremer, 1995).

Table 3: Price effects on sub-intervals of prices

	Specification (1) (price only)			Specification (2) (with controls)		
	N	Marg. eff.	Elasticity	N	Marg. eff.	Elasticity
Price range		(1 unit=€ 10)			(1 unit=€ 10)	
$2 \leq p < 15$	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 2. Standard errors are in parentheses. *** Significant at or below 1%, ** significant at or below 5%, * significant at or below 10%.

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 Table 2, the probability of PEB turns out not only to respond elastically with respect to education. To the extent that variations in education correlate with variations in wealth (Charles and Hurst, 2003), this is in line with the basic predictions of the classic model of impure altruism (Andreoni, 1989).

To illustrate the nonlinearity of the price effect in the probit estimates, we estimate the price elasticity across increasingly wider intervals of the treatment range. The results are reported in Table 3. 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 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 3 reinforce the general pattern in Table 2 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 identify potential anchoring effects, 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 no evidence for 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). That said, our test cannot exclude more complex anchoring mechanisms and, hence, an *underestimation* of the true price effect.[30]

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

[30]Conceivably, subjects could be both better informed and less price-sensitive because they value PEB more and hence, information status is likely to be not exogenous.

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.[31] 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.

4.2 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 3.2 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

[31] 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).

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 4 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.[32] 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.

[32] 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 4: 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 award the subject was assigned to

Column (1) in Table 5 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 replicates 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

Table 5: Robustness of the price effect to field price censoring

	Appended sample		
	reduced	recoded	reduced
	based on "filter" statements		based on EUA price estimates
	(1)	(2)	(3)
Price (€)	-0.0038***	-0.0038***	-0.0038***
	(0.001)	(0.001)	(0.001)
Reduced or recoded sample	0.2024**	0.6557***	0.1161
	(0.090)	(0.081)	(0.092)
Reduced or recoded sample * price	-0.0006	0.0005	-0.0004
	(0.002)	(0.001)	(0.002)
Constant	-0.7960***	-0.7960***	-0.7960***
	(0.061)	(0.061)	(0.061)
N	4199	4710	3714
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. '*Reduced or recoded sample*' is 0 if the observation belongs to the original sample and 1 if the observation belongs to the appended sample excluding subjects potentially affected by FPC according to the "filter" statements (column 1), or to the appended sample with recoded choices for subjects identified by the FPC "filter" statements (column 2), or to the appended sample excluding subjects potentially affected by FPC according to EUA price estimates (column 3). *** Significant at or below 1%, ** significant at or below 5%, * significant at or below 10%.

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.

The second part of the strategy to detect FPC targets 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.[33] Table 6 gives a detailed

[33]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

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 sample 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 5 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 this subsample is

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.

Table 6: Subjects' EUA price estimates

Survey question		Freq.	Rel. freq.	Cum.
"What is your estimate of the current market price (in EUR) for 1 ton of CO_2 in the EU emissions trading system?"	Below 2	100	4.25	4.25
	2 to below 10	110	4.67	8.92
	10 to below 20	328	13.93	22.85
	20 to below 30	240	10.19	33.04
	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).

-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.

5 Conclusion

This paper presented the results of a framed field experiment that employs direct price variation in order to estimate the price elasticity of engaging in PEB. Its estimation is not only important for academic economists and policymakers interested in voluntary PEB and its response to changes in prices. The empirical strategy explored in this paper also demonstrates methodolog-

ical solutions to the challenges that arise when researchers want to measure price elasticity in a way that closely corresponds to the price elasticity concept of classic theoretical models of public goods provision. These solutions extend the pioneering work on experimentally measuring the price elasticity of PEB through indirect price variation towards the direct price variation advanced by stated dichotomous choice approaches. Such direct price variation operates without relying on the potentially problematic behavioral equivalence assumption that underpins indirect price approaches. It can also reconcile the surprising contrast between the findings of indirect price approaches that engaging in PEB in consequential experiments is often infinitely inelastic and the more realistic findings of stated preference studies that consistently return finite participation elasticities.

Markets for tradable emission permits provide a favorable empirical opportunity to carry out an assessment of the price elasticity of PEB. 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 average elasticity across the treatment price 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. Given that the experimental evidence draws on an Internet-representative sample of the German population, the finding of a consistently inelastic response at the

extensive margin suggests that using public funds to subsidize a direct carbon price is not an economically meaningful instrument for expanding the set of voluntary contributors in Germany.

To our knowledge, the results reported in this paper represent the first estimates based on consequential decisions and direct price variation on how the price of PEB impacts on individuals' decision to engage in PEB. They also report the first price elasticity estimates for PEB based on direct prices. In contrast to previous findings based on indirect price variation, but in line with stated preference approaches, engaging in voluntary PEB has a finite elasticity. This highlights the potential sensitivity of elasticity measures to the specific manifestation of the price of PEB and the source of its variation as well as the importance of consistency between the theoretical and empirical elasticity concepts. Such differences between direct and indirect price variations potentially constitute a more general phenomenon and merit, in our opinion, further investigation.

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Appendices

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

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[34] in bonus points

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.

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

( ) 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.