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To Give or Not to Give: The Price of Contributing and the Provision of Public Goods
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ABSTRACT

We examine the relationship between the price of giving and the decision to contribute in a framed field experiment (n = 2,440). In a departure from previous research using match rates and rebates, we vary the price of contributing to the public good directly. Treatment groups differ between subjects by the amount of money subjects have to give up in order to provide one unit of the public good. In contrast to earlier results, the theoretical prediction of a clear negative relationship between price and the decision whether to contribute is borne out by the experimental evidence. We estimate the mean elasticity of the probability to contribute as -0.31. The direct price effect is robust across specifications including sociodemographic controls for the highly heterogenous, Internet-representative non-student sample of subjects.

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1 Introduction

The private provision of public goods has been attracting sustained attention from economists for several decades now. A natural and recurrent question within this field has been how the price of giving to the public good affects its supply (see e.g. Vesterlund 2006). Answering this question requires observing variations in the price of giving, and relating those price variations to observed variations in giving decisions. The empirical literature, starting with Feldstein and Taylor (1976) and Feldstein and Clotfelter (1976), has been exploiting observable variations in the marginal income tax rate between households to study the price effect in settings in which contributions to public or charitable causes are tax deductible and, therefore, subsidized.[1] More recently, the focus of empirical research in this area has shifted to gathering evidence from field experiments carried out in a fundraising context. These have provided new estimates of price elasticities of giving (Karlan and List 2007, Eckel and Grossman 2008, Karlan et al. 2011, Huck and Rasul 2011). A major benefit of field experiments is that the researcher is not restricted by given variations in marginal income tax rates. Instead, exogenous variations in the price of giving can be introduced in a controlled manner and independent of subjects' household income. The typical vehicle for such exogenous variations have been changes in 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 (Karlan and List 2007, Eckel and Grossman 2008, Karlan et al. 2011, Huck and Rasul 2011).[2] Converted into theoretically equivalent price variations, the evidence from variations in match ratios forms the basis of our current empirical understanding of the price effect.

Using variations in match ratios as a measure of the price effect in giving decisions offers a number of advantages, as it allows randomized assignment to subjects, is a familiar feature of fundraising, and is easy to implement.[3] It is also theoretically straightforward: 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%).

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

[2] For comparative purposes, Eckel and Grossman (2008) also uses rebate rates as a vehicle.

[3] Even though one drawback is that matches and rebates only allow the experimenter to reduce the price of giving, not to increase it.

At the same time, getting at the price effect indirectly via match ratios also has drawbacks. One important drawback is the assumption implicit in the indirect approach that subjects' response to variations in match ratios can safely be interpreted as those of the theoretically equivalent price variation. The validity of this assumption has been thrown into doubt by recent experimental evidence. For example in the case of contributions to public goods, 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). The introduction of a match leads to a greater effect on giving than the introduction of its theoretically equivalent rebate rate. Similarly, in an experiment involving private goods, Davis and Millner (2005) compare rebates, matches, and direct price variations that should be equivalent on theoretical grounds. They find that there are systematic differences in the quantity responses to these vehicles. This implies that for private goods, "match rate elasticities", i.e. price elasticities derived on the basis of variations in match ratios, are likely to be biased estimators of the price elasticity in a narrow sense. While we are not aware of comparable evidence for public goods, these results show at a minimum that the empirical equivalence of variations in match rates and in prices cannot be taken for granted.

In this paper, we present the design and report on the results of a framed field experiment that provides a *direct* measure of the price effect in a decision whether to contribute or not. The direct price effect arises out of the treatment condition in the experiment: Different subjects are randomly assigned a different amount of money that they give up if they decide to supply one unit of the public good. The observed effect on the probability to contribute therefore closely relates to the notion of the price effect from the theory of the private provision of public goods (Bergstrom et al. 1986, Andreoni 1990). The decision whether to contribute or not presents a useful first target for a study of the direct price effect: While an immediate prediction of theory is that, all else equal, the share of contributors in a population is a decreasing function of the price of contributing, a number of papers have found little field experimental support for the conjecture. Neither variations in match rates (Karlan and List 2007, Eckel and Grossman 2008, Huck and Rasul 2011) nor in rebate rates (Eckel and Grossman 2008) appear to influence subjects' decision whether to contribute.[4] Similarly, in an empirical paper exploiting variation in

[4]However, there is evidence that the presence of a lead donor in itself has a significant positive impact on the

marginal tax rates, Smith et al. (1995) find that the rebate rate does not impact on the decision whether to contribute to a rural health care facility.

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 laboratory-based within-subject dictator game design, 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 (Harrison and List 2004). 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 et al. 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.

The experiment was administered to a non-student population of 2,440 subjects, employing a between-subjects design. The real public good used in the experiment was verified CO_2 emissions reductions, a natural choice since they represent a real physical contribution to a public good, are perfectly uniform and individually traceable. Subjects were randomly assigned to one of fifty treatments, with the experimental price of contributing one metric ton of emissions reductions lying between €2 and €100, depending on the treatment group.

Based on this design, we estimate a direct price effect on the probability to contribute to the public good that is negative and statistically significant: On average, increasing the price for supplying a unit of the public good by €1 decreases the probability that the individual will contribute by around 0.1%. Estimated across all price treatments, the probability to contribute has a price elasticity of -0.31. 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. Our data do not provide evidence for the presence of a gender, age, or a culture effect in terms of either levels or elasticities. We find, however, support

probability that some positive contribution will be given. See Huck and Rasul (2011) for a careful discussion of the possible mechanisms at work.

for the hypothesis that the level of education has a positive role in determining contributions to public goods.[5]

The paper proceeds as follows: We explain the experimental design considerations 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 contributing relies on an experimental design that manipulates the price of giving to a public good. 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.[6] The experimental implementation of the intuition 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 or a charity explored by Kingma (1989), Eckel and Grossman (1996), Karlan and List (2007), Eckel and Grossman (2008), Karlan et al. (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 contribute one unit of the real public good. The real public good are verified CO_2 emissions reductions[7] and the unit is one metric ton. The emissions reduction is realized in the form of the documented and verifiable retirement ("deletion") of an emissions allowance (EUA) under the European Union Emissions Trading Scheme (EU ETS). Retiring one EUA lowers the total ceiling of the Scheme, and hence emissions, by one ton.[8]

[5] The same design can be used to draw conclusions about individuals' willingness to pay for voluntary climate action. See Diederich and Goeschl (2013) for an implementation.

[6] In this model and its variants in the literature, the price of the public good is conventionally normalized to one along with the private good. We provide a formal proof of the proposition that the number of contributors in a sufficiently heterogeneous population decreases in the price of contributing to the public good in the appendix to this paper.

[7] Economists have long noted that voluntary emissions reductions to mitigate climate change constitute a close empirical counterpart to a contribution in a large public goods game (e.g. Nordhaus 1993). An obvious prerequisite is that subjects agree with the economists' characterization. We come to this in the next subsection.

[8] Among several possibilities, the regulatory framework of the EU ETS, regulating the bulk of industrial CO_2 emissions across EU member states, provides the most reliable and transparent technology for real contributions to global greenhouse gas emissions reductions in an experiment. First, retiring EUAs avoids the problem of

Subjects are randomly assigned to one of the fifty different treatment groups, differentiated by the price of contributing. The price of contributing ranges, in increments of €2, from €2 to €100, the upper bound reflecting a current estimate of the maximum marginal cost of emissions reductions per metric ton of CO_2 (Tol 2010). Subjects only decide whether to contribute or not at the given price. They do not learn about others' choices before, during, or after the experiment.

Subjects' choices are implemented under a random incentive system (RIS) (Grether and Plott 1979, Starmer and Sugden 1991, Lee 2008) in order to limit total cost of the experiment. The RIS is between-subjects (BS)(Tversky and Kahneman 1981, Baltussen et al. 2010, Abdellaoui et al. 2011) with odds of one in fifty that the subject's choice (of either cash or contribution) was realized. On the experimental screens, the BS-RIS is framed as a lottery in which the winners' prize choices will be implemented.[9]

Like in most lab experiments, both financial incentives and public good benefits in the present design are "on the house". 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.[10] The latter transaction costs are equivalent to an individual minimum price on the contribution that would be unobservable and therefore out of control of the experimenter. 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

additionality frequently encountered for project-based carbon offsets as the total cap of the EU ETS is binding and enforced. Second, each EUA is uniquely identified by its issue number and hence individually traceable. Third, EUAs are not paper currency and have therefore no curiosity value as a tangible private commodity. Total EU emissions for the trading period 2008-2012, the relevant one for this experiment, were capped at 1.856 billion tons.

[9] Between-subjects (BS) and within-subject (WS) RIS have been subjected to examination for possible biases. While BS introduces noise and decreases risk aversion, there is less evidence of a systematic bias for simple tasks Cubitt et al. (1998), Baltussen et al. (2010). In one example, BS-RIS has been shown to affect behavior in dictator games Sefton (1992) while for ultimatum games, behavior was unaffected Bolle (1990).

[10] For example, the infrastructure of our cooperation partner is not designed to facilitate payments from subjects to the company.

[11] The evidence on a "windfall" (Keeler et al. 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) finds no significant difference in contribution behavior in a standard voluntary contribution mechanism (VCM) in the lab. Harrison (2007) reviews Clark's analysis of the data and identifies a decrease of contributors at the extensive margin by 8% when using house money. **?** 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 et al. (2013) find in a dictator game that

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

2.1 Subjects and procedures

The choice of subjects and the procedures under which the experimental design is implemented constitute a "framed field" setting.[12] The design is administered to a non-student population of 2,440 subjects drawn from the approximately 65,000 Internet panel members of the German section of YouGov and are representative for Germany's Internet using population of voting age.[13] 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 Internet experiment ran in two sessions in May and July 2010.[14] Session 1 lasted from May 25th to June 2nd and generated 1,640 complete observations from 1,817 invitations to the 'baseline' treatment. Session 2 lasted from July 19th to 27th and generated 800 complete observations out of 888 invitations. The recruitment of subjects followed the standard routine in which panel members are invited via an email message to proceed to the poll via a hypertext link. The introductory screen then explained, as common 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).[15]

subjects behave more generously with house money than with own money both in the lab and in the field.

[12] Following the nomenclature of Harrison and List (2004), our design falls short of a "natural field experiment" by virtue of the setting, which is familiar, but not natural, and by virtue of the awareness of subjects that their choices are being observed.

[13] 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.

[14] Prior to the experiment we ran a set of pre-tests and a pilot experiment with 200 economics students at Heidelberg University to test the online implementation and refine the set of texts and questions.

[15] 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.

Following the introductory screen, there was a filter screen to focus on German subjects.[16] Participants then faced a sequence of 10 to 13 computer screens, depending on their decisions.[17]

On average, 49 subjects were randomly assigned to each treatment group, differentiated by the experimental price.[18] 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 a contribution are.[20] Instructions were kept short and simple in order to avoid well-known biases and potential misinterpretations that arise when providing subjects with potentially choice-relevant information about the public good 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 as a winner.[21] The screen then 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.

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

[17] The screens required an answer for each question by entering text or choosing at least one of the options given (including "I don't know" options) before being able to proceed to the subsequent screen. This helps to prevent subjects from "rushing" through a survey.

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

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

[20] When subjects are invited to contribute to give to a liberal political organization (Karlan and List 2007, Karlan et al. 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 take 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. The retirement of EUA issue numbers was verifiable through a public-sector Internet site.

Table 1: Summary statistics of subjects' sociodemographics

Variable	Description	Mean	SD	Obs.
Female	Indicator variable for gender, 1 if female	0.469	0.499	2,354
Age	Subject's age (years)	45.42	14.68	2,352
Years of education	Based on subject's stated highest educational degree	12.27	3.213	2,299
Net income	Midpoint[a] of subject's monthly household net income category (€)	2,556	1,706	1,950
Eastern Germany	Indicator variable for residence in former GDR territory	0.1895	0.392	2354

Notes: [a] In our income approximation, for the 'less than €500' category, we assume €450. For the two categories above €5,000, we assume €8,000 for compatibility with German census data. The remaining categories have widths of €500.

The experiment concluded[22] 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. Table 1 presents summary statistics of the sociodemographics.

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 (**?**Cherry et al. 2004, Harrison et al. 2004). In other words, there is a possibility that subjects perceive an arbitrage opportunity introduced by the experiment, biasing the observable contribution decision. In the present experiment, subjects 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]

[22] Between subjects' choices and the questionnaire, the experiment administered a second choice containing a treatment condition. This paper focuses on the independent first choice only.

[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

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.[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 will censor all responses. Instead, FPC would be driven by subjects' possible perception that field opportunities are available at certain prices (Harrison et al. 2004).

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

the arbitrage margin $\hat{f}_i - e_i$.

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

[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 36).

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

3 Results and Discussion

2,440 subjects completed the experiment with a median completion time of 5 minutes.[28] A total of 382 subjects in the experiment contributed to the public good. 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 answers to open-ended survey questions and were excluded from the subsequent analysis.[29] We observe contributions in each of the fifty price treatments between €2 and €100. In forty-eight treatments, the share of contributors exceeds zero at the 5% level of significance, using a one sided t-test.[30]

The parametric analysis of subjects' discrete choice is based on a probit model. We estimate five specifications of increasing richness. The most parsimonious estimation of the direct price effect has the form

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

with $Y_i = 1$ if subject i chose the contribution to the public good and P_i denoting the cash prize offered to subject i. In several steps, in which additional controls are introduced, we arrive at the final specification of the form

[28] 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.

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

[30] The two prices at which contributions do not exceed zero in statistically significant way are the treatments with a price of contributing of €50 and €56.

$$\Pr(Y_i = 1) = \gamma_0 + \gamma_1 P_i + \gamma_2 P_i^2 + \gamma_2 N_i + \gamma_3 P_i N_i + \varepsilon_i$$

that allows for the possibility of a non-linear price effect and controls for non-price effects driven by a vector N_i of the subject's attributes as well as for interaction effects between the price of contributing and the attributes N_i.

Tables 2 and 3 report the probit coefficient estimates and the marginal effects, respectively, of the five specifications. The first two columns in both tables report on price-only specifications: Column 1 corresponds to model 1 while column 2 estimates a linear and a non-linear price effect. The second three columns augment the price-only model by including sociodemographic attributes and additional controls for experimental session, day, and daytime. Column 3 shows the coefficient estimates of the linear price model with controls for the standard suite of subjects' sociodemographic attributes. Column 4 and 5 report on the results of the final specification above, with column 4 (5) excluding (including) a possible non-linearity of the price effect.

3.1 The Direct Price Effect

Theory predicts that a higher price of contributing will be associated with a lower probability to contribute. Our data confirms this prediction: The marginal effects reported in Table 3 imply that raising the price of the contribution by €1 at the sample mean decreases the propensity to contribute to the public good by approximately 0.1%. The effect has the predicted negative sign and is significant at the 1% level. The effect is also robust: Comparing the magnitude of the linear price effect across specifications (columns 1, 4, and 5), the magnitude of the price effect changes only slightly when allowing for both price and non-price effects. Converting the direct price effect into a measure of elasticity, we calculate the elasticity of the probability of contributing[31] based on column 1 as -0.31 (standard error 0.09).

While in line with theoretical predictions, the evidence generated by direct price variation contrasts somewhat with the reported evidence based on indirect variation. Not all papers on the topic report on how indirect price variation impacts on the decision whether to contribute

[31] 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 (e.g. Miklius et al. (1976), LeClere (1992)).

Table 2: Probit coefficient estimates

	(1)	(2)	(3)	(4)	(5)
Price (€)	-0.0038***	-0.0223***	-0.0040***	-0.0030	-0.0022
	(0.001)	(0.004)	(0.001)	(0.002)	(0.002)
Price squared	–	0.0002***	–	–	0.0002***
		(0.000)			(0.000)
Female	–	–	0.0952	0.0834	0.0808
			(0.076)	(0.076)	(0.076)
Age	–	–	0.0037	0.0038	0.0037
			(0.003)	(0.003)	(0.003)
Years of education	–	–	0.0641***	0.0659***	0.0654***
			(0.011)	(0.011)	(0.011)
Net income (T€)	–	–	-0.0258	-0.0299	-0.0279
			(0.022)	(0.023)	(0.023)
Eastern Germany	–	–	-0.1092	-0.1239	-0.1192
			(0.095)	(0.096)	(0.097)
Price * female	–	–	–	-0.0030	-0.0034
				(0.003)	(0.003)
Price * age	–	–	–	0.0001	0.0001
				(0.000)	(0.000)
Price * years of education	–	–	–	0.0010***	0.0009**
				(0.000)	(0.000)
Price * income	–	–	–	-0.0014*	-0.0012
				(0.001)	(0.001)
Price * Eastern Germany	–	–	–	-0.0006	-0.0012
				(0.003)	(0.003)
Constant	-0.7947***	-0.4904***	-1.7739***	-1.0869***	-1.2419***
	(0.061)	(0.090)	(0.283)	(0.196)	(0.201)
Additional controls	No	No	Yes	Yes	Yes
N	2354	2354	1872	1872	1872
Log-likelihood	-1037.451	-1027.442	-786.483	-781.486	-773.769
χ^2	12.749	32.767	81.359	91.352	106.786
Pseudo R^2	0.006	0.016	0.049	0.055	0.065

Notes: Dependent variable: 1 if subject chose the contribution over the cash award. Standard errors are in parentheses. *** Significant at or below 1% ** Significant at or below 5% * Significant at or below 10%. Main effects of continuous variables in (4) and (5) are evaluated at the sample means. Additional controls include dummies for experimental session, day, and daytime.

Table 3: Marginal effects

	(1)	(2)	(3)	(4)	(5)
Price (€)	-0.0009***	-0.0054***	-0.0009***	-0.0007	-0.0005
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Price squared	–	0.0000***	–	–	0.0000***
		(0.000)			(0.000)
Female (d)	–	–	0.0223	0.0194	0.0186
			(0.018)	(0.018)	(0.018)
Age	–	–	0.0009	0.0009	0.0008
			(0.001)	(0.001)	(0.001)
Years of education	–	–	0.0150***	0.0153***	0.0150***
			(0.003)	(0.003)	(0.003)
Net income (T€)	–	–	-0.0060	-0.0069	-0.0064
			(0.005)	(0.005)	(0.005)
Eastern Germany (d)	–	–	-0.0246	-0.0275	-0.0263
			(0.021)	(0.020)	(0.020)
Price * female	–	–	–	-0.0007	-0.0008
				(0.001)	(0.001)
Price * age	–	–	–	0.0000	0.0000
				(0.000)	(0.000)
Price * years of education	–	–	–	0.0002***	0.0002**
				(0.000)	(0.000)
Price * income	–	–	–	-0.0003*	-0.0003
				(0.000)	(0.000)
Price * Eastern Germany	–	–	–	-0.0001	-0.0003
				(0.001)	(0.001)
Additional controls	No	No	Yes	Yes	Yes
N	2354	2354	1872	1872	1872
Log-likelihood	-1037.451	-1027.442	-786.483	-781.486	-773.769
χ^2	12.749	32.767	81.359	91.352	106.786
Pseudo R^2	0.006	0.016	0.049	0.055	0.065

Notes: Dependent variable: 1 if subject chose the contribution over the cash award. (d) denotes an indicator variable. Additional controls include dummies for experimental session, day, and daytime. Marginal effects are evaluated at the sample means. Standard errors are in parentheses.
*** Significant at or below 1% ** Significant at or below 5% * Significant at or below 10%

(e.g. Eckel and Grossman 2003). Those that do tend to find that variations in match rates or rebates do not have a significant impact on the share of contributors in the population. Karlan and List (2007), Karlan et al. (2011) and Eckel and Grossman (2008) conduct field experiments for political campaign organizations and public broadcasting services, respectively. Even though the experimenters offer match or rebate rates that reduce the price of giving by as much as 66%, response rates in the population do not vary systematically with the indirect price variation. Likewise, Huck and Rasul (2011) examine contributions to an educational program maintained by a large opera theater and do not find an effect on the propensity to contribute when introducing a match.[32]. Smith et al. (1995) examine contributions to rural health care facilities in Montana and do not find a significant effect of the rate of tax rebate on the decision whether to make a charitable contribution.[33]

The difference between the direct price effect on the probability to contribute and the previous evidence based on indirect price variation could be driven by several different factors. One possibility is a bias in reporting evidence: While previous research has stressed that the drivers of whether and of how much to contribute may be different (Smith et al. 1995), some studies do not report separately how the decision whether to contribute is impacted by the variation in the indirect price of giving. Those that report on the contribution decision may do so particularly because of the surprising result that they do not find an effect. A second possibility is that experiments using indirect price variations would have found a price effect in the contribution decision at larger sample sizes. Finally, an explanation could be that indirect and direct price variation are not behaviorally equivalent when subjects decide whether to contribute to a public good (Eckel and Grossman 2003, Davis and Millner 2005, Eckel and Grossman 2008).

Before turning to possibility of FPC as a potential bias, one objection to the result that could be raised regarding the size of the direct price effect is the possibility of an anchoring effect. When subjects are poorly informed or unfamiliar with the good (Green 1992, List and

[32] Huck and Rasul (2011), however, find an effect of introducing a lead donor, pointing to the important confounding effect that arises when matches and lead donors are introduced simultaneously.

[33] Some observers have related this evidence to similar findings on the irrelevance of stake size on behavior in dictator games (e.g. Carpenter et al. 2005). For example, in an artefactual field experiment with an all-or-nothing design similar to ours, Bekkers (2007) exploits variations in the size of the experimental endowment, which range between €6 and €11. He finds that the probability that a subject will donate this amount to a charity is independent of the size of the endowment. A key difference to our experiment is, however, that the recipient of the donation there also receives a larger transfer while in our case, different stake sizes always results in the same physical contribution.

Shogren 1999), higher prices offered might conceivably lead uninformed subjects to infer that the good is more valuable, prompting subjects to choose the public goods contribution. Experimental prices would therefore confound the contribution decision with the result that the true direct price effect would be even greater. 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. We find a non-negative relationship between the propensity to provide the mitigation effort and the "information-weighted" price: Contrary to the hypothesis of the confounding price effect, more familiarity does not change the price elasticity of contributing (for the knowledge variables) or even decreases it (for education, see columns 4 and 5 in Table 3). This resonates with experimental findings that price elasticity does not systematically vary with uncertainty about good characteristics Heffetz and Shaya (2009).

3.2 Non-price controls

There are a number of non-price attributes of subjects that have been examined in the literature as determinants of contributing and that conceivably interact with the price of contributing. These attributes include mainly gender, age, education, income, and 'culture'. Column 3 in Tables 2 and 3 reports the estimated effect of non-price attributes on the probability to contribute while columns 4 and 5 report on the estimated interaction effects.

List (2004) succinctly sums up much of the experimental evidence on the sociodemographic drivers of a failure to contribute in public goods games in his dictum of "young, selfish, and male". In the present experiment, *females* seem to be more inclined to opt for the public good contribution across all tested specifications, but the effect is not significant. The result is in line with the currently equivocal evidence on gender effects in public goods settings where the

evidence on gender differences is less clear-cut than its behavioral salience in areas such as risk taking or competition (see Croson and Gneezy 2009, and references therein). As Andreoni and Vesterlund (2001) point out, however, the lack of a level effect in social dilemmas may mask interaction effects: In a laboratory setting, they find male subjects to be more altruistic than female subjects when the price of giving is low, and vice versa. We therefore test for a possible price-gender interaction term to allow for elasticities to differ between men and women. The estimates in columns 4 and 5, however, yield no evidence for a gender effect in the present setting.

Like gender, *age* has attracted increasing attention as a determinant of behavior in public goods settings (Harbaugh and Krause 2000, List 2004). List (2004) and Carpenter et al. (2008), for example, find that social preferences increase with age in laboratory public goods games and charitable donations experiments. Also, like gender, the age effect is consistently positive but insignificant in all model specifications. Again, we test for a possible interaction effect with the price of contributing, but do not uncover a significant relationship.

In contrast to gender and age, *education* stands out as highly significant across all specifications. As the results in Table 3 show, subjects' propensity to contribute increases by as much as 1% for every year spent in education. Education also stands out for an interaction effect with the price of contributing: Additional years of education are associated with a higher probability of contributing at higher prices. Education therefore makes subjects decision to contribute less price elastic.

Both the presence and strength of the education effect are interesting. Many papers studying pro-social behavior do not report on the educational status of participants. Notable exceptions are List (2004), Karlan (2005), and Bekkers (2007): In three field experiments measuring social preferences reported by List (2004), education is either insignificant or weakly associated with higher contributions. In an experimental study in the context of a Peruvian microcredit program, Karlan (2005) finds that educational attainment is a determinant of observed behavior in a number of archetypical strategic situations such as the trust game, but is not associated with a greater willingness to contribute in public goods games. Bekkers (2007) studies dictator behavior in a survey-based, anonymous, all-or-nothing version of the game. There, educational status is binary (with or without a university degree) and a high status is associated with a significantly

elevated probability of donating.

Pro-social behavior may be acquired through education, but the strong relationship observed in the data may also arise from a different source. One plausible explanation could be that education and the perception of benefits from public goods provision are positively correlated, as is the case for climate policy benefits in the U.S. as survey data indicates (Borick et al. 2011). However, there is less evidence of this type of correlation in EU countries: 89% among those with a high-school degree or less and 92% of those with tertiary education regard climate change as at least "a fairly serious problem" (European Commission 2008). The strong education effect may also be explained by the specific public good used in the experiment: Emission reductions have long-run public good characteristics in a complex climate system. Patience and cognitive ability are therefore likely to matter. A number of empirical studies link cognitive ability and its proxy, education, with lower discount rates when assessing future costs and benefits and with overall stronger forward-looking behavior by individuals (Bettinger and Slonim 2007, Kirby et al. 2005, Parker and Fischhoff 2005). Other studies emphasize the lower cognitive cost to abler individuals of making decisions in complex settings (Peters et al. 2006). Against the background of self-reported income, another explanation is that education is a possible alternative measure of income and wealth. Since both tend to be positively correlated with cognitive ability (Banks and Oldfield 2007), this provides an additional causal channel through which education could enter as a significant explanatory variable.

The effect of *income* is insignificant in every model specification and the interaction effect borderline significant at the 10% level in one specification. While surprising in the context of the tax rebate literature (Auten et al. 2002), income elasticities of contribution close to zero have also been reported in a field experiment on charitable contributions by Eckel and Grossman (2008). However, the authors warn against over-interpreting the result due to the aggregate nature of their income data. In the present experiment, income data is indeed available on an individual level. At the same time, caution is advised as income is self-reported and therefore subject to potential biases, and 482 subjects are excluded that did not report their income. Data speaks against multicollinearity of income and education as explanation for the persistent insignificance of the one and strong significance of the other. The correlation coefficient with education is positive at 0.29, but excluding education from the regression as a robustness check fails to give

rise to a significant income effect.

Previous research has stressed the role of culture as a potential determinant (or not) of contribution decisions in public goods. While some experiments fail to find evidence for cultural difference (e.g. Brandts et al. 2004), two experiments on contribution behavior conducted in Germany (Ockenfels and Weimann 1999, Brosig-Koch et al. 2011) find significant and highly persistent differences between East-German and West-German residents regarding their behavior in a so-called "solidarity game". We test for the presence of significant differences in the contribution decision between subjects located in East and West Germany both in terms of level and in terms of an interaction with the price effect. In both cases, there is no evidence for a significant effect of the place of residence on the contribution decision when considering all subjects.[34]

3.3 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 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 four different ways whether their

[34] However, excluding younger age groups who would have spent their formative years after German reunification, we find a location effect: Subjects resident in Eastern Germany have a 5% lower probability of contributing, with the highest significance ($p = 0.047$) for those aged 33 years or more.

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 prize the subject was assigned to

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 three of four different approaches, the coefficient estimates for the price effect are not affected, in one case they are affected marginally.

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.[35] The question now is whether the inclusion of these subjects bias the estimate of the price effect in column 1 of Tables 2 and 3? 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 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 replicates the significantly negative price effect on the decision to contribute in the full sample (cp. column 1 in Table 2) and compares it to that in the reduced sample. The coefficient of the interaction term is insignificant ($p = 0.69$). Naturally, the overall probability of choosing the reduction is significantly higher if one excludes cash choosing subjects, leading to a significantly positive coefficient on the dummy for the reduced sample. We obtain a price elasticity of probability of -0.33 (standard error 0.089) if computed for the reduced sample only, compared to -0.31 (standard error 0.09) derived for the full sample.

[35] 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: Robustness of the price effect to field price censoring

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

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

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 5 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.[36]

[36] 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 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

Table 6: 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).

Table 6 gives a 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, and not vice versa, then the choice of subjects who anticipate an EUA price below the experimental price may be affected by FPC.[37]

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 given afterwards. 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 the reduced sample is −0.29 (standard error 0.095).

For the final column (4) of Table 5, we split the original full sample into two subsamples, one consisting of subjects whose EUA price estimate exceeds the cash prize and the other of

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.

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

those whose estimates are below the cash prize. Column (4) reports on the results of a direct comparison of contribution choices between the two subsamples with respect to price. The results show that, first, controlling for cash prize, subjects who estimate an EUA price below their cash prize are significantly less likely to contribute than those who estimate a spot price above their cash prize. Second, the contribution choice of the former group is not significantly correlated with price: the interaction term is significantly positive and, regarding magnitude, offsets the significantly negative main effect.[38] The observed effects in this split-sample case are as one would expect them to arise from FPC. The test using a split-sample approach is weak, however, as the result can equally well be generated by reasons other than FPC: First, given the distributions of the cash prize and the price estimate variables, there are only few observations for low prices where the estimate undercuts the experimental price. This inflates the variance at low prices for this group and may prevent detection of a significant price effect. Second, the price estimate reported by the subject may itself not be independent of the choice that the subject has taken. These competing hypotheses cannot be tested against each other, given the data. However, even if there is a FPC bias, it is both small and reinforcing the price effect.

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 giving a the public good and its private provision is a natural subject of interest to economists. Empirical opportunities in the form of exogenous variations in marginal tax rates (for tax deductible contributions), laboratory experiments, and field experiments have provided the basis for important insights into how variations in rebate rates and match ratios affect the probability that individuals will choose to contribute and how much

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

they contribute if they do. Among the many results of this indirect approach to price variation, one finding is that the decision whether to contribute appears to be largely immune to variations in match or rebate rates. While variations in rebate rates and match ratios can be converted into theoretically equivalent price variations, recent experimental evidence has thrown into doubt whether this theoretical equivalence also implies behavioral equivalence. Using price elasticities derived on the basis of their theoretically equivalent match rate or rebate rate elasticities may therefore be problematic.

This paper presents field experimental evidence from an alternative approach to examining the relationship between the price of giving and public goods provision, namely through *direct* price variation: We compare across thousands of subjects how the decision whether to contribute varies with the amount of money that subjects have to give up in order to provide one unit of the public good. The theoretical prediction of a clear negative relationship between price and public goods provision is borne out by our experimental data. There is a negative and robust direct price effect on the probability whether to contribute. We estimate its mean elasticity across the treatment range as -0.31. The direct price effect is robust with respect to a range of controls and with respect to the potential problem of field price censoring. This provides strong evidence that in the present case, making contributing cheaper through, for example, public subsidies has only a modest impact on the probability to contribute.

Among subjects' sociodemographic attributes that we use as controls, education stands out as a key determinant of the decision whether to contribute. Keeping in mind the possible limitations of self-reported income data and the lack of an established education-social preference channel in the literature, the role of education could be due to both cognitive and income or wealth effects. For gender and age, on the other hand, the literature provides reasons for expecting a significant role, but both effects fail to materialize in the experiment.

Given the difference between the evidence on the contribution decision by direct and indirect price variation, an obvious next research step is to directly compare match rates, rebate rates, and direct price changes in the context of public goods, preferably in a field setting. This would be important both in order to confirm independently the nature of the direct price effect and to quantify the differences between these approaches in terms of direction and magnitude.

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A Formal proof of the direct price effect at the extensive margin of contributions

We introduce a unit price for the public good into a variant of Andreoni's (1989, 1990) classical impurely altruistic model in order 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,

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

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

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

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

Substituting for g_i, the problem reduces to

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

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

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

We assume U 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 can thus be written as

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

The third argument in f is the warm glow argument. Taking into account the inequality constraint (3), 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

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

Given (3), 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, (3) holds with equality and thus, $G = G_{-i}$. It follows that

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

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 (4) it follows that

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

if we assume normality for both goods.[39] 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 implies that[40]

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

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)

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!

[39] Note that normality implies that any increase in wealth will always go in consumption of *both* goods.
[40] Note that an increase of δ_i in f^{-1} *ceteris paribus* implies lower demand for x, hence $f_\delta^{-1} < 0$.

B.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."

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[41] 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?

( ) I know the price

( ) Very sure

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

( ) 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.