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The long-term impact of matching and rebate subsidies when public goods are impure: Field experimental evidence from the carbon offsetting market

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1. Introduction

How can we stimulate voluntary contributions to public goods? So far, most experimental and empirical investigations have focused on the provision of pure public goods such as donations to charities. Voluntary contributions to public goods are, however, increasingly attached to conventional private goods. Thus, individuals often make a joint decision on both the consumption of the private and the contribution to the public good. Typical examples include donations to providers of open-source software or green goods which explicitly link public goods contributions to individual harm-related behavior.

As one of the most prominent green goods, carbon offsetting programs provide the opportunity to reduce pollution externalities and therefore diminish one's own contributions to a public bad (Kotchen, 2009). Recent empirical and experimental investigations confirm a positive relationship between the personal perception of individual impacts on climate change, e.g. for detrimental carbon emissions, and the willingness to contribute to climate protection (Löschel et al., 2013; Diederich and Goeschl, 2014). Harding and Rapson (2014) argue that, in contrast to charitable giving, carbon offsets and private consumption are inherently linked and may therefore be subject to potential feedback effects from one to the other. This is why contributions to a public good in the presence of own disamenities may deviate from pure donations being unrelated to private consumption. In particular, it is unclear how financial stimuli such as price rebates or matching grants, which successfully enhance the provision of pure public goods (e.g., Karlan and List, 2007; Meier, 2007; Eckel and Grossman, 2008; Rondeau and List, 2008; Anik et al., 2014), affect contributions in joint decisions on both types of goods. Empirical evidence in the field of green electricity programs indicates that customers typically prefer to register at the minimum participation level (Jacobsen et al., 2012). This raises the question of whether the positive effects of price rebates and matching grants can be transferred to carbon offsetting. Customers might be sensitive to treatments with varying minimum costs of participation in the carbon offsetting program. Recent field experimental investigations on carbon offsetting focus on the effect on different pre-default settings (opt-in vs. opt-out) and offsetting prices (Araña and León, 2012; Löfgren et al., 2012) on the program's effect or study the implications of awareness campaigns (Jacobsen, 2011). However, they do not analyze the effect of financial incentives like price rebates and matching grants.

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In this paper, we examine repeated contributions to a public good that are explicitly linked to the harm-related consumption of a private good. We study carbon offsetting behavior in a large-scale field experiment within the German long-distance bus market. Thereby, we systematically vary the individual payoff structure by introducing different financial stimuli in a randomized controlled trial. In particular, we offer different discounts by introducing a price rebate on carbon offsets of either 25%, 50% or 75%. In addition, we consider different matching schemes that multiply the participants' contributions at a specific rate. We differentiate between matching rates of *1/3:1* (1/3 kg of CO_2 added for every kg offset by the customer), *1:1* and *3:1*. Hence, our design enables us both to compare the impact of these stimuli in contrast to the control group and to study how treatment intensity affects decision behavior. Moreover, by using data on returning customers, we investigate the long-term effects of the different stimuli, both while treatments are in place as well as their persistence after treatment removal.

Previous field studies based on pure public goods like charitable giving decisions found both matching grants (Karlan and List, 2007; Meier, 2007) and price rebates (Eckel and Grossman, 2008) to increase donations.[1] Moreover, in Eckel and Grossman (2008), matching grants were more effective than rebates. Our analysis, based on a contribution decision being related to individual harm-related behavior and not based on a pure donation decision, delivers a more diverse picture. All price rebate schemes immediately increase participation rates in contrast to the baseline scenario, whereas we find weaker effects for matching grants. In terms of treatment intensity, we report a modest price sensitivity with diminishing marginal effects for large interventions. In line with experimental findings from charitable donations by Karlan and List (2007), large match ratios (*3:1*) do not have any additional impact on participation rates, relative to smaller match ratios (*1:1*). We find similar results also for rebates, as a reduction of the price by 75% does not attract more customers than a discount of 50%. From the second booking onwards, most treatment effects vanish. Only the equal (*1:1*) matching scheme continues to lead to higher participation rates when bookers are treated repeatedly. Even after removing the subsidies, we report higher participation rates for customers previously facing a *1:1* matching scheme, confirming its superiority in the long-term. The *1:1* matching scheme is also the only mechanism being able to significantly increase the net contributions of customers in contrast to the control group. Our analysis of repeated booking decisions suggests that price rebates rather serve as an instrument to temporarily increase participation, in particular among non-frequent customers, while a *1:1* matching scheme appears to be more likely to stimulate the long-term involvement of returning customers.

The remainder of the paper is organized as follows. Section 2 describes the offsetting program and the experimental protocol. We present and discuss our results in Section 3, before concluding in the last section.

2. The field experiment

2.1. The offsetting program

For conducting the field experiment, we collaborated with one of the pioneering bus operators in the German long-distance bus market.[2] The company had not offered or participated in any carbon offsetting program before the experiment was launched in October 2013. We introduced the program without any advertising or pre-announcement. It was designed to be a part of the usual online booking system at the company's official website. The booking system allows buying either one-way or return tickets, for up to four passengers at a time. After passengers had chosen their individual trips, the possibility to offset carbon emissions was offered in neutral words, including price (in €) and amount (in kilograms of CO_2) of the compensation.

Based on individual carbon emissions of 47 g CO_2 per passenger kilometer[3] and a price of 17.90 € per ton CO_2 charged by the collaborating offsetting provider, offsets were sold at 8 cents (in €) per 100 passenger kilometers. Passengers were required to make a binary choice by clicking either "yes" or "no". They also had the opportunity to obtain detailed information on the offsetting program on request.[4] After having made their decision, passengers received a list of the items they intended to buy (including their offsetting expenses), were asked to add their personal data (no additional information beyond the usual booking process was requested) and to enter payment details.

The questionnaire data collected after the experiment suggest that the vast majority of passengers were between 16 and 35 years old, and most of them had an academic background. Men and women were equally represented.[5]

2.2. Experimental design and procedure

We introduced different interventions to the offsetting program in three subsequent stages. In stage 1, which also coincided with the introduction of the carbon offsetting program, we varied informational settings by holding relative prices and amounts constant. In stage 2, matching grants and price rebates were introduced in order to hold the informational setting constant. Finally, in stage 3 we reproduced the conditions from stage 1 in order to study decision behavior of passengers over time. After the end of stage 3, the offsetting program continued to exist in a fixed setting. In this paper, we focus on the interventions in stage 2. Moreover, we use data from stage 3 to investigate potential spillover effects after the removal of the treatment.

To study the effects of price rebates and matching schemes on offsetting behavior, we ran a control condition and six treatments where we varied the incentives keeping wording and the general framework as similar as possible. Both types of subsidies were presented as a temporal offer given by the bus company to support individual offsetting contributions. Rebates directly reduced the passenger's offsetting price, whereas matching grants resulted in additional carbon offsets being paid by the company (see translated screenshots in Fig. B1). In particular, we considered three levels of price rebates reducing the costs for offsets by 25% (*r-25%*), 50% (*r-50%*) and 75% (*r-75%*), as well as corresponding matching grants (*1/3:1*, *1:1*, *3:1*). It should be noted that the price per unit CO_2 is identical in *r-50%* and *1:1* (8.95 €/t CO_2) while the amount of CO_2 covered in *1:1* is twice as high as in *r-50%*. Similar considerations hold for *r-25%* and *1/3:1* (13.45 €/t CO_2), and *r-75%* and *3:1* (4.50 €/t CO_2), respectively. In contrast to many experimental settings on charitable donations, individuals did not have the opportunity to adjust their contribution levels independently, but had a binary choice in our experiment. Emission quantities were derived from the underlying travel length. The total amount of CO_2 that can be offset is therefore always higher in the matching treatments than in the price rebate schemes. At the same time, costs of participation are always lower

[1] In related work, Huck and Rasul (2011) and Huck et al. (2015) combine matching schemes with lead donations and show that, in the presence of a lead donor, higher matching rates increase donations including the match but reduce net contributions prior to the match.

[2] In January 2013, Germany lifted its ban on long-distance bus operations. An amendment to the German Passenger Transportation Act (Personenbeförderungsgesetz, PBefG) ended the existing ban on additional scheduled national transport services if a corresponding railway service was already established.

[3] Carbon emissions include both direct carbon emissions from fuel consumption (36 g CO_2/km) and the life-cycle-assessment for the vehicle based on the GEMIS database (IINAS, 2013).

[4] For those interested, the bus service provided the information that greenhouse gas emission reductions were achieved by supporting an energy efficiency project in the public sector dealing with improved household charcoal stoves in Accra/Ghana. Those reductions were certified as voluntary emissions reductions (VER) according to the Gold Standard which is considered to be the most rigorous international certification benchmark for quality and compliance of carbon offsetting program. Using data from an online platform with more than 280 offsetting projects from 97 providers, Conte and Kotchen (2010) show that offsets being labeled as a Gold Standard project allow for a price premium of 30%–65%.

[5] Questionnaires were distributed both in the bus of the collaborating operator, during randomly determined bus rides, and online, using the operator's email list of customers, yielding 403 observations.

Table 1
Summary statistics by control and treatment groups.

Treatment	Control	Match 1/3:1	Rebate 25%	Match 1:1	Rebate 50%	Match 3:1	Rebate 75%	All
Total ticket price (€)	18.33	18.38	18.70	18.85	18.46	18.75	18.67	18.59
	(11.25)	(11.31)	(10.92)	(14.26)	(10.48)	(11.34)	(11.35)	(11.60)
Offset price (share of total ticket price)	0.014	0.013	0.005*	0.013	0.006***	0.013	0.003***	0.009
	(0.006)	(0.004)	(0.171)	(0.006)	(0.002)	(0.006)	(0.001)	(0.064)
CO_2 emissions per booking (kg)	12.6	12.7	13.1	13.0	12.9	12.9	13.0	12.9
	(8.1)	(8.9)	(8.9)	(9.5)	(8.5)	(9.4)	(8.6)	(8.9)
Travel length per booking (km)	267.8	270.6	278.2	276.4	274.7	273.6	276.1	273.9
	(172.2)	(189.7)	(189.7)	(202.7)	(180.2)	(199.7)	(183.4)	(188.4)
Group ticket (1 = yes)	0.10	0.09	0.09	0.09	0.10	0.09	0.11	0.10
	(0.30)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.31)	(0.29)
Return ticket (1 = yes)	0.29	0.29	0.32**	0.30	0.30	0.31	0.31	0.30
	(0.45)	(0.46)	(0.47)	(0.46)	(0.46)	(0.46)	(0.46)	(0.46)
Voucher (1 = yes)	0.019	0.017	0.015	0.023	0.026	0.024	0.024	0.021
	(0.14)	(0.13)	(0.12)	(0.15)	(0.16)	(0.15)	(0.15)	(0.14)
Info button request (1 = yes)	0.023	0.013*	0.014*	0.009***	0.016	0.012**	0.016	0.015
	(0.15)	(0.12)	(0.12)	(0.15)	(0.16)	(0.15)	(0.15)	(0.12)
Female (1 = yes)	0.53	0.56*	0.53	0.54	0.54	0.51	0.54	0.54
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Bookings (# of bookings by same ID)	1.16	1.18	1.15	1.16	1.17	1.15	1.16	1.16
	(0.48)	(0.50)	(0.46)	(0.47)	(0.50)	(0.47)	(0.45)	(0.47)
Observations	1542	1557	1486	1463	1522	1557	1496	10,623

Notes: Means are reported by treatment group, standard deviations in parentheses, asterisks report results of a t-test on differences in means between each treatment group and control, $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

in the rebate treatments. Thus, we do not assume that price rebates and matching schemes are fully equivalent in our case.

The treatment assignment was based on a randomly generated number that was saved as a browser cookie (ID) on the individuals' computers when entering the website for the first time. If individuals interrupted the booking process, returned to the website later and the cookie was not deleted, they remained within the same treatment. This randomization strategy allows us to investigate repeated bookings of returning customers during the entire experiment.[6]

3. Experimental results

Our results are based on 10623 booking decisions in seven varying treatment conditions.[7] Observations are fairly equally distributed across treatments, with about 1500 observations in each setting (see Table 1 for an overview). Summary statistics do not show large differences across treatments. Merely the request for information is less frequent in the matching treatments which might occur due to an increased trust in the company. We observe 9286 individuals purchasing a ticket within the treatment framework for the first time. Moreover, we record 1337 booking decisions from customers returning to the website to book a ticket for a second, or more, time in stage 2. Due to return tickets (30%) and group purchases for more than one individual (10%), the average booking contains 1.46 trips. The travel length on average amounts to 274 km per booking (185 km per trip), resulting in corresponding CO_2 emissions of 12.9 kg per booking. Taking into account rebates, travelers face an average offsetting price of 0.18 € which accounts for about 1% of the average total ticket price per booking.[8]

We start our analysis by focusing on the first booking per ID in order to capture individuals' immediate response to the different treatments (Section 3.1). As offsets are offered in different combinations of prices and quantities, we study effects on both participation rates and contribution levels. Then we extend the discussion to the second and further bookings made by returning customers (Section 3.2) and also analyze the persistence of treatment effects based on bookings after removing the intervention (Section 3.3).

3.1. First bookings

3.1.1. Participation

In this section, we analyze treatment effects with respect to the probability of taking part in the offsetting program, measured by the binary variable *participation*. Across all observations, the average participation rate for first bookings is 30.8% (Table 2). Take-up rates in the different rebate conditions amount to 30.7% (*r-25%*), 35.4% (*r-50%*), and 36.1% (*r-75%*) while they amount to 26.4% (*1/3:1*), 31.0% (*1:1*) and 29.3% (3:1) in the matching schemes (see also Fig. A1). Except for 1/3:1 matching, the participation in all treatments is higher than in the control group (27.0%). However, these differences to the baseline scenario are solely significant for the rebate schemes (*r-25%*: $p < 0.05$, *r-50%* and *r-75%*: $p < 0.01$, MW-tests, Table 2) and for the *1:1* match ($p < 0.05$, MW-test, Table 2).

To further analyze the decision behavior in the first booking, we apply a binary logit model to investigate the effects of treatment assignment on the participation decision. Our main empirical specification includes both a vector of treatment indicators for the respective booking and additional control variables, containing all available information acquired during the booking process such as the amount of CO_2 emissions per booking,[9] the offset price, whether the participant makes use of a voucher during the booking process, whether additional information was requested by means of clicking on the info button, and whether the individual is male or female. An overview of all control variables is given in Table 1. We also provide estimates in the absence of further control variables.

Table 3 reports the estimates for the treatment indicators on the participation decision both in an empirical model with treatment variables only (column 1) and in an expanded model with a set of control variables (column 2). Both model specifications indicate a positive effect of all price rebates schemes on participation compared to the control group (*r-25%*: $p < 0.05$, *r-50%* and *r-75%*: $p < 0.01$). In contrast, behavioral responses to matching grants are rather modest. Only *1:1* matching appears to be clearly effective in enhancing participation across the

[6] In case the individual browser settings had cleared the cookies, individuals were randomly assigned to one of the treatments.

[7] Overall, we recorded 11,258 bookings in stage 2. For the analysis in the paper, we exclude IDs that had been already faced with an offsetting decision in stage 1 (5.2%).

[8] Conte and Jacobsen (2014) study green electricity programs of all utilities in the United States and report average green electricity sales of approximately $7 per customer with an average price premium of 2 cents (USD) per kWh. Participation rates across programs amount to 1.3%.

[9] We applied a series of alternative empirical specifications. In particular, since the value of the control variable *CO_2 emissions per booking* might be influenced by several specific booking features, we split up this variable in the underlying components, i.e. the length of the single trip and whether the ticket was purchased as a return ticket and/or as a group ticket. All these alternative specifications do not affect our main treatment effects and are therefore not reported in the paper.

Table 2
Mean responses by control and treatment groups.

Treatment	Control	Match 1/3:1	Rebate 25%	Match 1:1	Rebate 50%	Match 3:1	Rebate 75%	All
Panel A. First bookings								
Participation (%)	27.0	26.4	30.7**	31.0**	35.4***	29.3	36.1***	30.8
		(0.33)	(−2.16)	(−2.27)	(−4.70)	(−1.38)	(−5.07)	
Resulting offsets per booking								
Without match (kg CO_2)	3.4	3.3	3.9**	3.9**	4.5***	3.7	4.7***	3.9
		(0.33)	(−2.09)	(−2.11)	(−4.58)	(−1.40)	(−5.14)	
Including match (kg CO_2)	3.4	4.4	3.9**	7.8***	4.5***	14.7***	4.7***	6.2
		(−0.98)	(−2.09)	(−5.12)	(−4.58)	(−5.60)	(−5.14)	
Offset payments per booking								
Total payments (€ cents)	6.1	7.8	7.0**	13.8***	8.0***	26.1***	8.4***	11.1
		(−0.93)	(−2.08)	(−5.14)	(−4.58)	(−5.60)	(−5.14)	
By customers (€ cents)	6.1	5.9	5.3	6.9**	4.0	6.5	2.1	5.3
		(0.33)	(−0.65)	(−2.11)	(−1.27)	(−1.40)	(−0.04)	
Observations	1347	1342	1314	1282	1329	1364	1308	9286
Panel B. Second bookings and beyond								
Participation (%)	25.6	24.2	23.3	34.8*	24.4	21.2	24.5	25.4
		(0.34)	(0.53)	(−1.93)	(0.29)	(1.02)	(0.26)	
Resulting offsets per booking								
Without match (kg CO_2)	2.7	2.9	2.4	3.9**	2.5	2.1	2.7	2.7
		(0.28)	(0.54)	(−2.04)	(0.31)	(1.06)	(0.28)	
Including match (kg CO_2)	2.7	3.8	2.4	7.8***	2.5	8.4	2.7	4.3
		(−0.26)	(0.54)	(−3.26)	(0.31)	(−0.21)	(0.28)	
Offset payments per booking								
Total payments (€ cents)	4.9	6.7	4.3	13.8***	4.4	14.8	4.8	7.7
		(−0.21)	(0.54)	(−3.28)	(0.32)	(−0.22)	(0.27)	
By customers (€ cents)	4.9	5.0	3.2	6.9**	2.2	3.7	1.2	3.9
		(0.28)	(0.97)	(−2.04)	(1.31)	(1.06)	(1.58)	
Observations	195	215	172	181	193	193	188	1337

Notes: Means are reported by treatment group. We display the results of a MW-U test between each treatment group and the control group, z-statistics are given in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

different model specifications ($p < 0.01$ in the expanded model). The coefficient for the weaker matching intervention *1/3:1* is even negative, but does not significantly differ from zero. The effect of *3:1* matching is positive, but only significant at the 10% level in the expanded specification. The behavioral responses to price rebates are in line with the findings from Jacobsen et al. (2012) suggesting that customers are especially sensitive to treatments that lower their overall participation costs. While price rebates directly reduce the minimum costs of participation, matching grants only lower the cost of carbon per unit.

We provide average discrete probability effects in Appendix B (Table B1). Point estimates suggest that price rebates increase the customer's tendency to take part in the offsetting campaign in the first booking by 4.3 percentage points for *r-25%*, 8.7 for *r-50%* and 9.4 for *r-75%*, in contrast to the control group. Moreover, *1:1* matching grants increase the propensity to contribute by 4.9 percentage points. The effect for *3:1* matching amounts to 3.1 percentage points. Once again, the coefficient for the *1/3:1* match does not significantly differ from zero.

In a next step, we are interested in the effect of treatment intensity (Table 3, column 2).[10] For the case of matching rates, equal shares (1:1) lead to more offsetting than smaller matches (1/3:1) ($p < 0.01$), whereas large matching rates (*3:1*) do not have any additional impact compared to *1:1* ($p = 0.31$), similar to the findings of Karlan and List (2007) in the context of charitable giving. Similarly, while price rebates of 50% ($p < 0.05$) and 75% ($p < 0.01$) lead to higher participation rates compared to a rebate of 25%, a three-quarter price rebate does not yield any additional effect compared to *r-50%* ($p = 0.67$). Furthermore, estimated discrete probability effects and their confidence intervals show that *3:1* matching and a 75% price rebate do not differ from the corresponding weaker interventions. Hence, the result of Karlan and List (2007), stating that subsidizing public good contributions beyond the equal share does not have additional impact seems to generalize to price rebates.

Our design principally enables us to compare price rebates and matching grants with the same price per unit CO_2 of giving (e.g., *1:1* vs. *r-50%*). Price rebates always lead to higher participation rates than matching grants within pairs (*1/3:1* vs. *r-25%*: $p < 0.05$; *1:1* vs. *r-50%*: $p < 0.05$, *3:1* vs. *r-75%*: $p < 0.01$). However, a comparison between treatment groups embeds further aspects that may limit the comparability of these two classes of interventions. In particular, passengers were not able to adjust offsetting quantities, but faced a binary choice in our experimental setting. Given a decreasing willingness to pay for larger offsetting amounts, as indicated by Jacobsen et al. (2012), systematically higher quantity levels in the matching schemes (see Table 2) decrease their attractiveness. To mitigate this difference between treatments, we include the total amount of CO_2 (including the matched kilograms) in an alternative empirical specification. When controlling for the total amount of CO_2, the coefficients of *1:1* and *3:1* matching increase in both size and significance.[11] While this does not alter our main observations, the differences in participation rates between matches and rebates become weaker and remain significant only for the weakest intervention level (*1/3:1* vs. *r-25%*: $p < 0.05$).

3.1.2. Contributions

In the following, we turn to resulting payment levels. Except for *1/3:1* matching, total average offsetting payments (including matches) are higher in all treatments than in the control group (6.1 cents, in €), being highest in *3:1* matching (26.1 cents) (Table 2). These differences are significant and suggest that all except the *1/3:1* scheme reach the

[10] Throughout the paper, comparisons between treatments are based on post-estimation Wald tests which are carried out after the estimation of the expanded model specification with control variables.

[11] We are aware that controlling for the total amount of CO_2 might not be a fully adequate solution to reduce treatment asymmetry. One potential threat to this approach is multicollinearity. While the correlation between *3:1* and total CO_2 is only 0.56, estimates of the alternative specification might nevertheless slightly overestimate the effect of *3:1* matching. However, given the reported correlation and a willingness to pay for carbon offsets that decreases in quantities, not controlling for the total amount of CO_2 would underestimate the effect of *3:1* matching. Hence, we interpret the coefficients of the matching treatments from our main specification as the lower bound and estimates from this alternative specification as the upper bound of the true treatment effect. Tables of the alternative models are available upon request.

Table 3
Treatment effects, first bookings.

	(1)	(2)	(3)	(4)	(5)	(6)
	Participation	Participation	Payments	Payments	Payments	Payments
			Total	Total	Customer	Customer
Rebate 25%	0.185**	0.207**	5.429**	5.552**	0.287	0.294
	(0.0857)	(0.0870)	(2.525)	(2.433)	(1.323)	(1.258)
Rebate 50%	0.394***	0.416***	11.17***	11.14***	0.230	0.155
	(0.0840)	(0.0851)	(2.455)	(2.355)	(1.230)	(1.169)
Rebate 75%	0.426***	0.451***	12.46***	12.34***	−2.244*	−2.376**
	(0.0842)	(0.0855)	(2.470)	(2.363)	(1.186)	(1.133)
Match 1/3:1	−0.0292	−0.00390	2.265	2.883	−0.580	−0.323
	(0.0872)	(0.0882)	(2.643)	(2.542)	(1.394)	(1.325)
Match 1:1	0.196**	0.235***	15.98***	16.80***	2.830**	3.151**
	(0.0861)	(0.0869)	(2.828)	(2.680)	(1.409)	(1.323)
Match 3:1	0.118	0.148*	32.43***	33.03***	1.655	1.886
	(0.0855)	(0.0864)	(3.287)	(3.152)	(1.366)	(1.297)
Controls	No	Yes	No	Yes	No	Yes
Constant	−0.997***	−0.895***	−42.11***	−51.65***	−13.58***	−18.09***
	(0.0614)	(0.0736)	(2.554)	(3.453)	(1.158)	(1.515)
Log-likelihood	−5707.43	−5640.38	−18,853.66	−18,723.09	−16,622.96	−16,478.29
Observations	9286	9286	9286	9286	9286	9286

Notes: Columns (1) and (2) are binary logit regressions, columns (3)-(6) are Tobit regressions, omitted treatment category is control, the set of controls contains: CO_2 emissions per booking, offset price, voucher (1 = yes), info button request (1 = yes), female (1 = yes), robust standard errors in parentheses, ***p < 0.01, **p < 0.05, *p < 0.1.

goal of increasing the share of carbon neutral transportation services. However, our main interest lies in the payments made by the customers net of subsidies, as the company could increase offsets at own costs in the absence of incentive schemes.[12] Averaged over all first booking decisions, net contributions amount to 5.3 cents per booking. In the rebate conditions, the contribution of passengers equals 5.3 cents (*r-25%*), 4.0 cents (*r-50%*), and 2.1 cents (*r-75%*) and is thus lower than in the control group (6.1 cents, see also Fig. A2). However, these differences are not significant according to a MW-test. Contributions in the matching schemes amount to 5.9 cents (*1/3:1*), 6.9 cents (*1:1*), and 6.5 cents (*3:1*). Hence, merely the *1:1* and the *3:1* matching scheme induce customers to contribute more than in the control setting, but this difference is significant only for the *1:1* match ($p < 0.05$).

Following our empirical strategy for analyzing the participation decision, we conduct a series of Tobit regressions to further investigate these differences (Table 3, columns 3–6). The regression results confirm the superiority of the *1:1* matching scheme, being the only scheme inducing significantly higher net payments compared to the control group ($p < 0.05$, Table 3, columns 5 and 6). While the increase in participation rates in the rebate schemes clearly stimulates the total offsetting payments (Table 3, columns 3 and 4), there is no statistical evidence that net contributions in these schemes differ from those in the control group (columns 5 and 6). For *r-75%*, net contributions are even lower than in the control group (at least $p < 0.1$, Table 3, columns 5 and 6). This effect is intuitive as equalizing net contributions in *r-75%* would require participation rates to quadruple, compared to the control group. Given our results from the previous section, we know that participation rates are far from reaching 100% in *r-75%*.

We now consider the intensity effect of the treatments on net contributions. A price rebate of 75% significantly decreases net contribution levels in contrast to smaller interventions (r-25%: $p < 0.05$, r-50%: $p < 0.01$). The difference between *r-25%* and *r-50%* is not significant in our sample. In line with the observations on participation rates, a *1:1* match leads to higher net contribution than the weaker intervention ($p < 0.01$) and is statistically equivalent to the *3:1* match ($p = 0.33$). Pairwise comparisons between matches and rebates reveal that the latter leads to lower average contribution levels, except for the intervention at the minimum level (*1/3:1* vs. *r-25%*: $p = 0.62$; *1:1* vs. *r-50%*: $p < 0.05$, *3:1* vs. *r-75%*: $p < 0.01$, Table 3, column 6). These observations confirm the findings from Eckel and Grossman (2008) and Davis et al. (2005), which indicate that there is a superiority of matching rates in raising contribution levels within the framework of charitable giving. In relative terms compared to the baseline scenario, we observe the strongest increase in net contributions for 1:1 matching (+13%) and the strongest drop for a rebate of 75% (−66%). At the same time, total payments, including those made by the bus company, rise by 126% for 1:1 matching and by 31% for a rebate of 50%. Despite obvious differences in the choice structure, these values are in line with the previous literature on matching and rebate subsidies (see Table B2 in Appendix B).

3.2. Second bookings and beyond

We use the subsample of repeated bookings to explore potential long-term effects of the different treatments. It is important to point out that, due to our randomization procedure, returning customers were reassigned to the same treatment.[13] Therefore, we are able to investigate how repeated exposure to the treatments affects the decision behavior of passengers. Averaged over all observations, participation rates in repeated bookings (25.4%) are lower than in the first booking decision (30.8%, Table 2). Except for *1:1* matching, we observe a decline in participation rates in all treatments. Participation in the offsetting program exceeds the level of the control treatment (25.6%) only in the *1:1* matching condition (34.8%, Fig. A1). This condition is also the only one with significant treatment effects for second bookings and after ($p < 0.1$, Table 2). All other treatment effects seem to be rather short-lived and disappear in repeated bookings.

Once again, we further investigate these observations in an econometric model in order to obtain a more robust estimate. Additional to the previous specification, we include the current number of individual *recurrences*, indicating the number of times the individual has been exposed to the treatment when making the offsetting decision. While the number of recurrences does not seem to affect the offsetting decision, the estimates confirm the absence of a treatment effect for all treatments except for the equal matching scheme from the second booking onwards (at least $p < 0.1$, Table 4, columns 1

[12] Note that a true efficiency analysis would require capturing all costs and benefits that arise from implementing the different schemes. For simplicity we assume these to be zero and constant across schemes.

[13] We also investigated the effect of treatment assignment on recurrences but the differences do not appear to be significant in our sample. The results are not included in the paper but are available upon request.

Table 4
Treatment effects, second bookings and beyond.

	(1)	(2)	(3)	(4)	(5)	(6)
	Participation	Participation	Payments	Payments	Payments	Payments
			Total	Total	Customer	Customer
Rebate 25%	−0.129	−0.111	−3.233	−3.074	−3.720	−3.790
	(0.244)	(0.254)	(5.825)	(6.089)	(3.209)	(3.338)
Rebate 50%	−0.0687	−0.0410	−2.077	−1.308	−5.048*	−5.037
	(0.235)	(0.263)	(5.574)	(6.478)	(2.986)	(3.448)
Rebate 75%	−0.0625	−0.0881	−1.291	−0.947	−6.757**	−7.009*
	(0.236)	(0.286)	(5.669)	(7.245)	(2.926)	(3.764)
Match 1/3:1	−0.0778	−0.0898	1.808	1.411	−0.317	−0.478
	(0.229)	(0.232)	(5.706)	(5.695)	(3.212)	(3.187)
Match 1:1	0.437*	0.489**	20.45***	20.09***	6.153**	5.867*
	(0.226)	(0.229)	(5.829)	(5.811)	(3.081)	(3.072)
Match 3:1	−0.246	−0.241	12.79*	12.44*	−3.836	−4.118
	(0.241)	(0.243)	(6.865)	(6.907)	(3.229)	(3.258)
Controls	No	Yes	No	Yes	No	Yes
Constant	−1.065***	−0.736**	−35.14***	−33.83***	−13.30***	−13.27***
	(0.164)	(0.352)	(4.713)	(9.584)	(2.516)	(4.816)
Log-likelihood	−751.76	−744.22	−2225.07	−2221.40	−1987.94	−1984.64
Observations	1337	1337	1337	1337	1337	1337

Notes: Columns (1) and (2) are binary logit regressions, columns (3)-(6) are Tobit regressions, omitted treatment category is control, further controls in columns (2), (4), and (6): CO_2 emissions per booking, offset price, voucher (1 = yes), female (1 = yes), recurrences (# of repeated bookings), robust standard errors in parentheses, ***p < 0.01, **p < 0.05, *p < 0.1.

Table 5
Participation, first bookings by customer type.

Treatment	Control	Match 1/3:1	Rebate 25%	Match 1:1	Rebate 50%	Match 3:1	Rebate 75%
One-time customer (total bookings = 1)	0.27	0.26	0.32	0.31	0.36	0.30	0.37
Returning customer (total bookings > 1)	0.26	0.27	0.23	0.31	0.29	0.26	0.27
Diff (returning – one-time customer)	−0.01	0.01	−0.09**	0.00	−0.07*	−0.04	−0.10**
	(−0.20)	(0.32)	(−2.00)	(−0.01)	(−1.87)	(−0.99)	(−2.49)
Observations	1347	1342	1314	1282	1329	1364	1308

Notes: Mann–Whitney z-statistics in parentheses, ***p < 0.01, **p < 0.05, *p < 0.1.

and 2).[14] This observation is in line with the findings of Allcott and Rogers (2014) on waning impacts of behavioral interventions in the long run and may raise concerns about the effects of these contribution schemes for returning customers.

With regard to compensation levels, we get a similar picture. Only individual payments in the *1:1* matching scheme exceed those in the control condition (at least p < 0.1, Table 4, columns 5 and 6) while they are even significantly lower in r-75% (at least p < 0.1) and equally high in the other treatments.

The next step is to analyze whether returning customers behaved differently than one-time bookers already in their first booking decision. Table 5 summarizes participation in the offsetting program when booking for the first time, split into one-time and returning customers. No differences can be detected for both the control group and the matching schemes. In particular, participation rates are equal for both types of customers in their first booking for *1:1* matches. However, it seems that repeated bookers respond differently to price rebates already when these subsidies are given for the first time.

We shed more light onto this question and onto the behavioral dynamics by estimating expanded specifications of our econometric models. We add interactions of treatment effects with dummy variables for both repeated bookings (*2nd +*) and returning customers (*returner*).[15] There is again evidence that the vanishing effect of price rebate schemes is mainly driven by the fact that *returners* react differently to these incentives directly from beginning (see Table B3). In contrast, the vanishing effect for the *3:1* matching appears not to be driven by differences between one-time and repeated bookers, but rather by a decline over subsequent bookings. Remarkably, none of these two different channels affect decision behavior in the *1:1* match.[16]

3.3. Persistence of treatment effects

Finally, we take a look at the individual behavior after removing the incentive schemes (Table 6). In this stage, incentives are not provided, and all customers face the same decision situation. Observed bookings in the post intervention period allow us to study potential spill-over effects of the different stimuli during the period that followed after their removal. Thus, analyzing the persistence of treatment effects reveals important insights for decision makers if interventions are not intended to be implemented permanently. For instance, Jessoe and Rapson (2014) provide evidence that the positive effects of high-frequency information about residential electricity usage given temporary price increases spill over to non-intervention periods in the long run due to the formation of conservation habits. In contrast, Jacobsen (2011) reports only little evidence for the persistence of awareness campaigns.

In the long run, our data suggests that *r-25%* leads to slightly lower participation rates in the offsetting program compared to the control group, while the remaining five treatments are again followed by higher participation rates. Most of these differences, however, appear to be not statistically significant in our sample. The *1:1* matching scheme yields the highest participation rate and the only statistically significant effect

[14] In an alternative specification we interacted treatment dummies with recurrences, but also the interaction terms do not appear to be significant in our sample while treatment effects do not qualitatively change. Moreover, we replaced recurrences with the temporal distance to the first booking and to the last booking, subsequently. But again we did not find these additional controls to be significant or to change our main results. All these additional results are available upon request.

[15] The dummy variable *returner* is set to one for all individuals booking more than once during the experiment and is thus time-invariant within the observations for each individual.

[16] A series of post-estimation Wald tests confirms that also the joint effect of *Match 1:1* and *Match 1:1 × (2nd +)* differs significantly from zero in all specifications except for the model without control variables and *Payments Customer* being the dependent variable (Table B3, column 5), where p = 0.143.

Table 6
Effects after treatment removal.

	(1) Participation	(2) Participation	(3) Payments	(4) Payments
Rebate 25%	−0.211	−0.0627	−2.832	−1.203
	(0.365)	(0.373)	(5.349)	(5.391)
Rebate 50%	0.258	0.408	5.261	6.752
	(0.340)	(0.350)	(5.155)	(5.157)
Rebate 75%	0.297	0.475	5.263	6.647
	(0.338)	(0.346)	(5.025)	(5.036)
Match 1/3:1	0.0443	0.211	1.090	2.856
	(0.339)	(0.349)	(4.998)	(5.035)
Match 1:1	0.420	0.635*	7.758	9.399*
	(0.341)	(0.351)	(5.099)	(5.120)
Match 3:1	0.139	0.319	4.322	6.144
	(0.329)	(0.344)	(5.057)	(5.018)
Controls	No	Yes	No	Yes
Constant	−1.273***	−1.151***	−21.37***	−26.33***
	(0.247)	(0.383)	(4.106)	(6.389)
Log-likelihood	−378.25	−368.67	−1013.36	−1009.11
Observations	685	685	685	685

Notes: First booking decisions per ID after treatment removal, columns (1) and (2) are binary logit regressions, columns (3)–(4) are Tobit regressions, omitted treatment category is control, further controls in columns (2), (4), and (6): CO_2 emissions per booking, offset price, info button request (1 = yes), voucher (1 = yes), female (1 = yes), robust standard errors in parentheses, ***$p < 0.01$, **$p < 0.05$, $p < 0.1$.

($p < 0.10$, Table 6). Hence, the *1:1* matching scheme induces spill-over effects and increases contributions even after its removal. In a related study, Meier (2007) studies the long-run effects of two matching schemes (*1/4:1* and *1/2:1*). He observes decision behavior over a series of years and finds a positive short-term effect for the year in which the matching schemes were in act. In the subsequent year, when the matching schemes were removed, he finds negative effects. Over the full period of four years, including the treatment year, both effects cancel out each other such that no treatment effect remains. The treatment that is most similar to Meier's setting is our *1/3:1* matching scheme where we find no difference in the first decision after treatment removal, compared to the baseline scenario. However, for stronger matching rates, we find positive effects with the strongest results for the *1:1* matching rate in the long-run. This new finding adds to the limited evidence on the persistence of rebates and matching schemes.

4. Conclusion

Forming adequate institutions to enhance the private provision of public goods is a complicated challenge. The joint decision on both the consumption of a private good and the contribution to a public good may provide an opportunity for promoting voluntary contributions. Carbon offsets serve as a prominent application of this concept since they provide the opportunity to displace pollution externalities in connection to an underlying harm-related consumption decision. In this paper, we examine voluntary contributions to a carbon offsetting program based on a large-scale field experiment in the long-distance bus market in Germany. We systematically vary the individual payoff structure and introduce price rebates and matching grants in a randomized manner.

Our results suggest that price rebates are more effective in stimulating short-term participation. We report a modest sensitivity to prices with diminishing marginal effects for large rebates and matches. An equal split of contributions between the traveler and the bus company, in terms of *1:1* matches or a 50% price rebates, enhances the participation in the offsetting program in contrast to the baseline scenario, while larger matches or rebates do not have an additional effect. Hence, the missing impact of increasing stimuli beyond the equal share does not seem to be limited to donation matches (Karlan and List, 2007), but also holds for price rebates in our context. One speculative explanation for this finding is that both larger matching rates and price rebates may provoke skepticism, for instance concerning the quality or the effectiveness of the carbon offset. Our most appealing finding is the dominance of the *1:1* matching scheme in the long-run. While the treatment effects of rebates disappear over time, only the *1:1* matching scheme enhances contributions when bookers are treated repeatedly. Even after removing the subsidies, we report higher participation rates and contribution levels for customers previously facing a *1:1* matching scheme. The outstanding position of the equal matching scheme is somewhat surprising. We speculate that the equal matching scheme may be the only intervention that ascribes a normative character to the offsetting decisions in terms of a social proof, leading bookers to stick to their previous choices.

The dominance of the *1:1* matching scheme may also contribute to the discussion on an acceptable burden sharing rule. The results indicate that the norm of a 50–50 division possibly serves as an acceptable focal point regarding a "fair" burden sharing of carbon emissions between the producer and the consumer. This is in line with findings from the experimental literature showing that a large amount of participants favors an equal split, even in dictator games where a single party determines the outcome (see Engel, 2011 for a meta-study). However, there is evidence that a higher degree of anonymity weakens the norm and leads to a more selfish behavior which may then lead to the hypothesis that people like to be perceived as fair (Andreoni and Bernheim, 2009). Our results indicate that the linkage of public goods contributions to individual harm-related behavior might increase compliance with a 50–50 norm in a matching scheme even if individual decisions are not observed by others. It remains an open question why this burden sharing norm does not hold when it is framed as a rebate as in our case.

Our findings provide new insights into the economics of environmental offsets and contribute to the discussion on how to successfully design interventions in order to promote individual efforts for mitigating greenhouse gas emissions. More generally, our analysis is one of the first randomized field trials to study public good contributions linked to a conventional consumption good. In our experiment, customers have the possibility to offset their externalities directly when they are about to purchase a conventional transportation good. As a matter of course, this sequential decision structure differs from the classic impure public ("green") good setting where the consumer directly chooses between a conventional and a green good, for instance between conventional and shade-grown coffee or between electricity from renewable or non-renewable resources. Nevertheless, the close connection between contributions and own responsibility remains. It may be beneficial to extend this line of research to other contexts and choice structures. This may include decisions where individuals are able to independently determine their own contribution levels when the respective rebate or matching scheme is given, similar to the structure of charitable donation experiments.

Other possible directions for future research include the analysis of information settings on voluntary offsetting contributions. One specific aspect may address the role of feedback, social information or social proof. Recent field-experimental findings by Shang and Croson (2009) in the context of local charities suggest that social information on previous contributions have comparable effects on matching grants or price rebates.

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Appendix A. Supplementary data

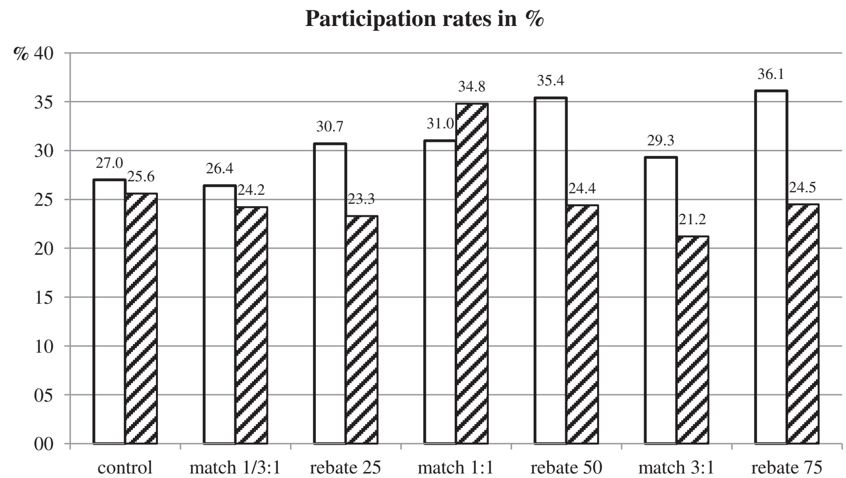

Fig. A1. Participation rates in %. Notes: The first bar represents customers' participation rates for the first booking decision only. The second bar, which is striped, depicts the data for repeated bookings.

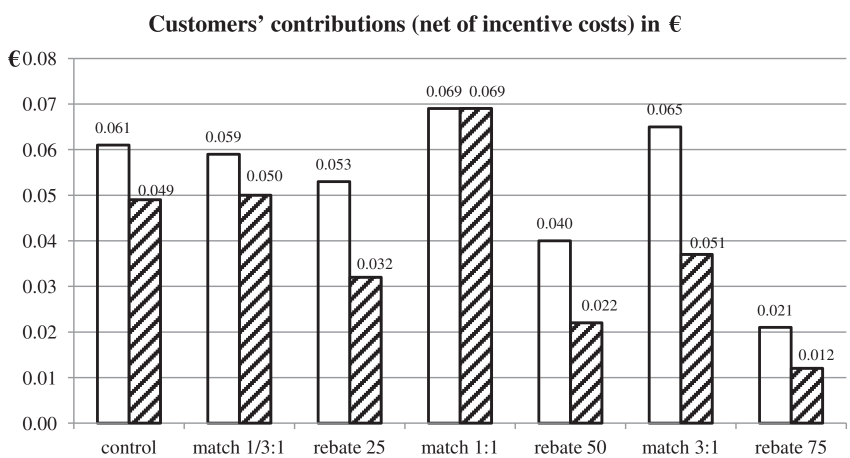

Fig. A2. Customers' contributions (net of incentive costs) in €. Notes: The first bar represents customers' net contributions bookings for the first booking decision only. The second bar, which is striped, depicts the data for repeated bookings.

Appendix B. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2016.01.004.

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