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The power of active choice: Field experimental evidence on repeated contribution decisions to a carbon offsetting program

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1. Introduction

Encouraging individuals to contribute to public goods is often a challenging endeavor. While social and political actors frequently launch initiatives to foster contributions to public goods in various domains, their long-term effects often remain unclear. This is particularly true in situations that lack credible enforcement mechanisms. Since formal institutions such as punishment schemes (e.g., Fehr and Gächter 2000) are often difficult to implement in these settings, the provision of a public good relies exclusively on voluntary contributions. Against this backdrop, recent research has started to focus explicitly on cognitive and behavioral barriers that may hamper individuals to act on their prosocial proclivities, especially when formal institutions are not able to enforce collectively desirable outcomes (Nyborg et al., 2016).

In light of such behavioral barriers, optimizing the design of choice architecture has become a popular practice for stimulating voluntary contributions to public goods, both in business practice and public policy. One well-known instrument in this context is the implementation of a "default option" that "nudges" desired behaviour (Thaler and Sunstein 2008). The effect of defaults has been studied in a broad range of situations, including organ donation (Johnson and Goldstein 2003),

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health care services (Bellman et al., 2001), retirement plans (Beshears et al., 2008), green energy campaigns (Pichert and Katsikopoulos 2008), electricity pricing programs (Fowlie et al., 2017), and carbon offsetting (Araña and León 2012, Löfgren et al., 2012).

A key insight from this literature is that individuals tend to stick to the default option.[1] Individuals join programs more frequently when participation is set as the default option and an opportunity to "opt out" is given, as compared to a setting where non-participation is the default and subjects have the chance to "opt in." Yet does an opt-out default design provide an adequate mechanism for encouraging contributions to a public good? While providing a specific default option may be beneficial to individuals who prefer not to take decisions themselves (Sunstein 2014), the ethical limitations of this choice design have stimulated heated debate (Smith et al., 2013). For example, the European Court of Justice ruled in 2012 that a default opt-in was illegal in the case of travel insurance automatically added to the online booking of passenger flights (European Court of Justice 2012).

In light of ethical concerns, a softer form of intervention may be preferable in certain situations. For example, Sunstein and Reisch (2013) argue that, especially in complex or morally loaded situations, it may be tempting not to make any decision at all. So why not simply require an explicit decision (i.e., by indicating "yes" or "no") to address this problem? In fact, a series of papers has shown that an Active Choice (AC) requirement increases average contributions to public goods compared to a voluntary "opt-in" setting in one-shot decisions (e.g., Johnson and Goldstein 2003, Pichert and Katsikopoulos 2008, Löfgren et al., 2012). However, despite this evidence on the short-term effectiveness of such subtle changes in the choice architecture, at least two important questions related to AC design remain.

First, it is unclear whether these effects hold or fade away over time in recurrent decision-making situations. Few studies have examined the long-term impacts of behavioral interventions, and the empirical findings are mixed. On the one hand, Egebark and Ekström (2016) find persistent reduction in paper use when the default option on university printers is changed to double-sided printing. Altmann and Traxler (2014) show that a simple reminder message to visit the dentist is effective and remains statistically significant over time. On the other hand, Adena and Huck (forthcoming) document the first evidence for long-term negative effects of AC enforcement in the context of donations on long-term ticket purchases at the opera house. In a similar vein, Damgaard and Gravert (2018) find that sending repeated reminders to donate to a charitable cause increases mailing list unsubscribe rates.

Second, little is known about the behavioral channels that cause the observed AC behavior in social dilemma situations. Stutzer et al. (2011) argue that some subjects may initially lack preferences and assume that an AC mechanism can help reflect the subjective value of contribution to public goods and therefore foster the formation of issue-specific preferences. Alternatively, the AC requirement may reduce the "moral wiggle room" that individuals tend to take advantage of (Dana et al., 2007) and may increase the voluntary contributions to a public good due to the guilt associated with actively selecting against making a donation. In this spirit, Andreoni et al. (2017) propose a simple framework for investigating the tendency of subjects to avoid "the ask" in a specific setting. In their field experiment, customers can either enter a supermarket through a door with a solicitor actively asking passersby for donations (such that passing without giving a donation implies a visible "no") or, alternatively, customers can choose a "silent" door where they can pretend not to have seen a passive solicitor. The study shows that subjects passing through the door with more social pressure have higher contribution rates. At the same time, though, subjects seem to be aware of this, leading a lower number of subjects to enter the door with the active solicitor. Andreoni et al. (2017) conclude that subjects try to avoid the "ask" because entering the silent door induces lower levels of guilt when subjects choose not to contribute to the public good. DellaVigna et al. (2012) observe similar behavioral reactions in door-to-door fundraising. In their study, subjects tried to avoid meeting a solicitor by indicating they were "not at home" when the representative of a fundraiser announced her arrival. Given these empirical observations, "ask avoidance" is considered as a strategic self-control mechanism to limit the emotional responses that would otherwise induce donation giving (Trachtman et al., 2015). Introducing an AC design could potentially reveal similar effects as it excludes the possibility of avoiding the "choice" that is inherent to opt-in settings. Explicitly choosing not to contribute to a public good may lead to stronger feelings of guilt than passive behavior that leads to the same result (see e.g., Tangney and Fischer 1995, Hedlin and Sunstein 2016).

We contribute to a better understanding of these two important research questions by studying how an AC requirement impacts repeated decisions on whether to donate to a carbon offsetting program. In a large-scale field experiment, we examine voluntary contributions to a carbon offsetting program during the online purchase of a long-distance bus ticket in two different treatment conditions. In all settings, subjects have the opportunity to offset their carbon emissions. In the control group, subjects can ignore the offer and continue the booking process without indicating an explicit decision. In this case, they will not participate in the program and are thus exposed to a voluntary "opt-in" setting with non-participation as the default option. In the AC condition, individuals are required to explicitly indicate their offsetting decision in order to finalize the purchase of their ticket. If subjects in this setting try to continue the booking process without an explicit

[1] The behavioral economics literature offers a range of explanations for this finding. One potential driver for this effect is inattention. Subjects may simply fail to notice they had a choice or fail to realize that the default option affects their choices (Dhingra et al. 2012). A related explanation refers to inertia and procrastination, because selecting against the default requires some effort (e.g. Sunstein and Reisch 2013). Further explanations point to loss aversion because deviating from the status-quo has to provide a higher utility than the default itself (e.g. Dinner et al. 2011). Alternatively, social conformity may be responsible, as the default may signal appropriate behavior (e.g. McKenzie et al. 2006).

decision (i.e., by clicking either “yes” or “no”), a red frame window pops up with a message prompting participants to indicate their decision, thereby excluding the possibility of “choice avoidance” (see Fig. A1).

With our randomization strategy we were able to both investigate consumers’ first reaction to the different settings and study repeated bookings of returning customers assigned to the same treatment condition. Among other things, our analysis of repeated bookings sheds light on the potential channels underlying the effectiveness of the AC requirement. In particular, we present a simple framework for analyzing explicit and silent free riding, showing that the introduction of an AC requirement provokes behavior similar to “ask avoidance.” Ultimately, we contribute to answering both questions outlined above, summarized by the following findings.

First, we show that the AC requirement increases the share of travelers who offset their carbon emissions in the first booking decision by almost 50%. To the best of our knowledge, this represents the first evidence of the effects of an AC framework in an impure public (or green) good setting (Kotchen 2009) where contributions are linear to the trip distance and hence are explicitly tied to harmful behavior.[2]

Second, by investigating the behavior of returning customers within the same treatment conditions, we show that the total participation rate in the offsetting program in AC remains higher compared to the control group in subsequent bookings. At the same time, the number of recurrences within the booking system is largely unaffected, suggesting that there are no substantial dropouts due to the subtle intervention in our setting.

Third, we identify a robust pattern in repeated decisions. In AC, many subjects remain undecided when returning to the website, even if they were prompted before. The prompt remains effective for returning customers: Prompting customers without a deliberate choice still effectively increases the share of carbon neutral trips, even when they had been prompted before.

Fourth, we provide some insight into the underlying causal channels. Building on Andreoni et al. (2017), we provide a framework that suggests that some subjects will participate in the carbon offsetting program only in the presence of an AC requirement, due to anticipated feelings of guilt. While our experimental design is certainly not able to provide a full picture of all potential behavioral channels that cause the predominance of the AC framework in the long-run, it provides suggestive evidence that a significant share of customers constantly choose to offset only when prompted to decide. This finding is consistent with the “avoiding the ask” explanation posited by DellaVigna et al. (2012) and Andreoni et al. (2017). It suggests anticipated guilt as a causal mechanism, with “choice avoidance” seen as an option for circumventing guilt that would arise from explicit free-riding.

The paper is organized as follows: In Section 2, we focus on the implementation of the carbon offsetting program and our experimental design, and present our hypotheses concerning expected effects. In Section 3, we start with the main results for both first and repeated bookings and then elaborate on the different behavioral channels. Section 4 concludes.

2. The field experiment

In this section, we describe the implementation of the carbon offsetting program (2.1) and the experimental design (2.2.). We derive our hypotheses on the expected effects in Section 2.3.

2.1. The offsetting program

We conducted the field experiment in collaboration with a German long-distance bus operator in October 2013, which coincided with the launch of a carbon offsetting program. The program started without advertising or pre-announcement and was part of the usual online booking system. It allowed passengers to buy one-way or return tickets for up to four passengers at a time. After choosing their bus ticket, participants were asked whether they would like to offset the carbon emissions associated with their trip (in kilograms of CO_2) for a given price (in €) (see translated screenshot in Fig. A1). The offsets were sold at 8 cents for 100 passenger-kilometers.[3] Details concerning the carbon offsetting program could be easily accessed by clicking on a hyperlink, thus minimizing information costs.[4] In the final online cart, passengers obtained a list

[2] In previous field experiment studies on carbon offsetting and default setting, individuals offsetting costs were not fully associated with the underlying trip. Löfgren et al. (2012) study decision behavior of air travelers to a scientific conference in Sweden in 2008. The underlying offsetting costs amount to €10 for flights from within Europe and €40 for flights from outside Europe. In a similar study, Araña and León (2012) consider fixed offsetting prices of €10, €20, €40 and €60 when investigating the choice behavior of air travelers to scientific conventions and conferences in Gran Canaria in 2009–11.

[3] Including both the 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) individual carbon emissions amounted to 47 g CO_2 per passenger kilometer. The collaborating offsetting provider charged a price of €17.90 per ton CO_2.

[4] The bus service provided the information that reductions in greenhouse gas emissions were achieved by supporting a public sector energy efficiency project that improved household charcoal stoves in Accra/Ghana. Those reductions were certified as voluntary emissions reductions (VER) according to the Gold Standard, which is often considered the most rigorous international certification benchmark for quality and compliance of voluntary carbon offsetting programs. Using online data from more than 280 offsetting projects from 97 providers, Conte and Kotchen (2010) show that offsets labelled as Gold Standard project obtain a price premium of 30-65%.

of items they intended to buy (including their offsetting expenses). They then added their personal data and entered their payment details. No further information, apart from the usual booking data, was requested. After the experiment, we ran a short survey among customers of the bus company.[5] The elicited data reveals that a vast majority of the passengers were between 16 and 35 years old. Most of them had an academic background. Men and women were equally represented.

2.2. Experimental design

In both the control and the treatment (i.e., AC) group, individuals had an opportunity to offset their carbon emissions. After selecting their trip, they were presented with an identical screen in both the settings that allowed them to participate in the carbon offsetting program by choosing "yes" (see Fig. A1). Similarly, in both conditions they could decline the offer by clicking "no."

In the control group, customers were allowed to skip the offsetting decision by choosing the "continue" button at the bottom of the page without previously clicking "yes" or "no." These individuals without an explicit decision in the control group did not participate in the carbon offsetting program. In other words, the control group corresponds to an opt-in design with non-participation as the default.

In the AC group, individuals were shown exactly the same initial screen displaying "yes," "no," and "continue" buttons. However, if subjects in that setting tried to click on the "continue" button without indicating an explicit decision, a red frame popped up with a message asking them to make a decision ("please select whether you would like to offset your emissions") (Fig. A1, lower part).

Throughout the experiment, we refer to individuals who indicated an explicit decision on the initial screen as *deliberate participants* (i.e., those who clicked "yes"). In the control, the remaining individuals were non-participants by default. In the AC, a greater variety of outcomes is possible: we can differentiate between *deliberate non-participants* (who clicked "no"), *prompted non-participants* (who clicked "no" after having seen the pop-up frame), *deliberate participants* (who clicked "yes") and *prompted participants* (who clicked "yes" after having seen the pop-up). When we mention *participants* without qualification, we refer to the pooled group of both deliberate and prompted participants.

We assigned individuals randomly to the two treatment conditions based on a browser cookie (ID) saved on the individual's device when accessing the website for the first time.[6] The same cookie was used to reassign the individuals to the same treatment condition, not only when restarting and finishing an earlier booking process, but also when re-visiting the website for a subsequent ticket order. Given this randomization strategy, we were able to investigate the effect of repeated exposure to the same treatment for the case of returning customers making subsequent ticket purchases.[7]

2.3. Expected responses toward the active choice design

In this section, we provide a simple framework for deriving the expected responses to an AC design in our experimental setting. The framework is based on Andreoni et al. (2017), who elaborate a tendency to avoid "the ask" in the context of charitable giving. In their setting, customers can either enter the supermarket through a door with a solicitor actively asking passersby for donations. In our setting, "the ask" cannot be avoided and is always present (see Fig. A1). However, the possibility to move on without giving an explicit answer and thereby avoiding "the choice" differs across the treatments, potentially leading to similar effects: Passing the solicitor without a donation implies an active "no" and thus comes close to an AC treatment, while passing a "silent" door without a donation resembles passive non-participation in an "opt-in" setting. In the following, we present a modification of the Andreoni et al. (2017) framework that matches our AC setting.

Imagine an online customer is given an opportunity to offset her emissions from bus travel by paying a small fee. Presuming a level of introspection, she may imagine how she would feel if she made an explicit decision, i.e., if she clicked "yes" or "no." She may also evaluate how she would feel if she instead chose not to make an explicit decision, i.e., by clicking on "continue" in order to proceed with the booking process, thereby circumventing the question. Finally, she considers the cost of each path and chooses the path with the highest utility ex ante.

Let u_{dec*} be the utility from making the optimal explicit decision on program participation ($part$), i.e., to offset the emissions from the trip ($part = 1$) or not ($part = 0$), conditional on taking an explicit decision at all. Giving an explicit answer provokes costs. If the answer is "yes" ($part = 1$), the online customer takes part in the offsetting program, thereby contributing to the public good. If the answer is "no" ($part = 0$), she saves the participation costs but suffers from an implicit cost of guilt from free riding on others' contributions to the public good.

[5] We elicited answers both online, using the company's social networks and offline, distributing paper-based questionnaires in buses during randomly selected bus rides. The survey was framed as a general customer survey. In total, we obtained 403 observations.

[6] We conducted several separated experimental interventions to study different types of behavioral interventions with the same bus operator. The data for this paper are exclusively taken from the first part of the program. The subsequent treatments on (spillover-) effects of matching grants and price rebates are reported in Kesternich et al. (2016). In the analysis conducted in Kesternich et al. (2016) we excluded all IDs that had been already faced with an offsetting decision in the first stage (see footnote 7 in Kesternich et al. (2016)).

[7] If the cookie had been deleted between bookings or if individuals were using different devices, subsequent bookings would appear in the data as first time bookings. This means that true differences between first and repeated bookings may be somewhat higher than that recorded in our experiment.

Passive non-participation (by clicking on the "continue" button) is different from explicit non-participation as it comes with reduced moral responsibility and hence allows for moral "wiggle room" (Dana et al., 2007), potentially leading to weaker feelings of guilt (see e.g., Tangney and Fischer 1995). As a prominent example, Hedlin and Sunstein (2016) find that participants actively choosing against participating in a green energy program feel guiltier about not contributing than those in a setting with non-green defaults. Hence, let u_o be the utility from passive non-participation. Moreover, we posit that avoiding an answer by strategically circumventing the question comes with some fix cost c in the form of mental discomfort from self-deception.

Depending on the customer's preferences for giving, tolerance for guilt, and discomfort from self-deception, she may choose to make the decision to offset her emissions (*deliberate participant*), to make the decision to not offset her emissions (*deliberate non-participant*), or to ignore the offer and "avoid" making an explicit decision. The latter "choice avoiders" are those with sufficiently low costs from self-deception. When forced to decide, they may either prefer to participate (*prompted participant*) or not to participate (*prompted non-participant*). All four types are mutually exclusive with respect to the parameters (corresponding types in Andreoni et al. (2017) in parenthesis):

- *deliberate participants* (alias "passive givers"): those who deliberately decide to offset in both treatments, defined by

$$u_{dec^*} > u_0 - c, \text{ and } u_{dec(part=1)} > u_{dec(part=0)} \tag{1}$$

- *prompted participants* (alias "giving avoiders"): those who avoid to make a decision if possible, but participate when prompted to decide in AC, defined by

$$u_{dec^*} < u_0 - c, \text{ and } u_{dec(part=1)} > u_{dec(part=0)} \tag{2}$$

- *deliberate non-participants* (alias "passive nongivers"): those who deliberately decide not to offset in both treatments, defined by

$$u_{dec^*} > u_0 - c, \text{ and } u_{dec(part=0)} > u_{dec(part=1)} \tag{3}$$

- *prompted non-participants* (alias "saying no avoiders"): those who avoid to make a decision if possible and choose not to participate when prompted in AC, defined by

$$u_{dec^*} < u_0 - c, \text{ and } u_{dec(part=0)} > u_{dec(part=1)} \tag{4}$$

This simple framework describes one-shot decisions. In repeated bookings, previously prompted subjects have the chance to update their information concerning the choice options, which might lead to different behavior. First of all, learning about the AC requirement may change the expected utility as the presence of the red frame might be perceived as unpleasant, leading to a general switch towards deliberate decisions in later rounds. Moreover, deciding against participation in the presence of the red frame is likely to come with even stronger guilt than clicking "no" immediately, while we do not expect additional guilt from saying yes in the presence of the red frame. In effect, we expect that the switch towards deliberate decisions is more pronounced for *prompted non-participants* than for *prompted participants* (see Appendix 2 for a formal extension of the above framework).

In summary, based on the above framework we expect three distinct effects of the introduction of an AC requirement, due to anticipated feelings of guilt: (1) In AC, overall participation rates should be higher compared to the control group as some subjects will participate only when forced to decide. (2) The behavioral pattern of strategic "choice avoidance" should persist in subsequent bookings. (3) The switch from prompted non-participation to deliberate non-participation is more pronounced than the switch from prompted participation to deliberate participation. While our theoretical framework provides a simple structure for the introduction of an AC requirement, it is certainly not able to provide a full picture of all the behavioral channels that may be at work (e.g. preference formation, limited memory or inattention). We therefore start our results section with the assessment of the general effectiveness of AC before turning the discussion towards the role of anticipated guilt and further potential behavioral channels.

3. Main results

In this section, we use data from both the first and the repeated bookings to confirm the general effectiveness of AC in contrast to the control group, in the short term (3.1) and in repeated interactions (3.2). In the subsequent discussion, we shed light on the transition dynamics between different bookings (3.3) (i.e., following individuals over time, mainly based on a within treatment comparison) before discussing the underlying behavioral channels for the observed differences (3.4). In the following, by "first bookings" we mean all initial reactions to the treatments during the experiment. That is, this term comprises both one-time customers (i.e., those customers who only do one booking during the experiment) as well as the first bookings of repeat customers (i.e., those who do multiple bookings during the experiment).

In total, we observe 11285 booking decisions with 5661 observations in the treatment and 5624 observations in the control group.[8] In the experimental framework, 9733 observations result from the first booking decisions while the

[8] For our analysis, we removed observations in which the booking process was not completed (644 observations, 5.3%) from the raw data (12245 observations). Focusing on potential treatment differences, we do not observe the AC treatment increasing the share of uncompleted bookings in the first

Table 1
Summary statistics by control and treatment group.

	1st booking			2nd booking and beyond		
	Control	AC	All	Control	AC	All
Total ticket price (€)	18.85	19.12	18.99	16.58	16.90	16.74
	(12.29)	(11.82)	(12.05)	(9.12)	(9.47)	(9.29)
Offset price (share of total ticket price)	0.013	0.013	0.013	0.013	0.013	0.013
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
CO_2 emissions per booking (kg)	13.4	13.4	13.4	11.4	11.6	11.5
	(9.40)	(9.26)	(9.33)	(7.60)	(7.48)	(7.54)
Travel length per booking (km)	284.0	285.7	284.8	242.6	247.2	244.9
	(199.9)	(196.9)	(198.4)	(161.6)	(159.0)	(160.3)
Group ticket (1 = yes)	0.09	0.10	0.10	0.05	0.07	0.06
	(0.29)	(0.30)	(0.30)	(0.22)	(0.25)	(0.24)
Return ticket (1 = yes)	0.31	0.31	0.31	0.22	0.23	0.22
	(0.46)	(0.46)	(0.46)	(0.41)	(0.42)	(0.42)
Voucher (1 = yes)	0.01	0.01	0.01	0.02	0.02	0.02
	(0.11)	(0.12)	(0.12)	(0.13)	(0.15)	(0.14)
Info button request (1 = yes)	0.04	0.06***	0.05	0.01	0.01	0.01
	(0.20)	(0.23)	(0.22)	(0.07)	(0.11)	(0.09)
Female (1 = yes)	0.52	0.53	0.52	0.50	0.57**	0.53
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Observations	4838	4895	9733	786	766	1552

Notes: Means and proportions (with standard deviations in parentheses) are reported both for the 1st booking (columns 2–4) and the 2nd booking and beyond (columns 5–7). For both subsets, we report data by the treatment groups (Control and AC) and for the entire subsample (All). We show the results of a t-test on equality of means or of a chi-square test on equality of proportions of the respective variable of interest (column 1) between the two independent samples Control and AC. Asterisks indicate significant differences in means or proportions between Control and AC, $^{***}p < 0.01$, $^{**}p < 0.05$. We indicate significant differences between Control and AC by adding asterisks to column 3 or column 6, respectively. Reading Example: In first bookings, there is a significantly higher share of info button requests in AC compared to Control (0.06 vs. 0.04, $p < 0.01$). A correction for multiple hypotheses testing, as suggested by List et al. (2016), reveals that the difference in *info button request* persists in the first booking. This also holds for the share of *females* in the second booking and beyond.

remaining 1552 observations stem from subsequent bookings (i.e., 2nd to 5th bookings). Returning rates are almost identical in both treatments ($p = 0.85$, chi-square test): In the control group, 12.3% of the customers return to the website during the experimental phase; in AC, 12.2% of the individuals come back to perform additional bookings.

3.1. First booking decision

In the first booking decisions ($N = 4838$ in Control and $N = 4895$ in AC), the average travel distance is 285 km per booking, corresponding to CO_2 emissions of 13.4 kg and to an offsetting price of €0.25 on average (1.3% of the total ticket price) (Table 1, left panel). Summary statistics do not reveal large differences in the observable characteristics between the two settings in the first booking decision. As expected, the request for more information on the offsetting program is slightly higher in the AC treatment but overall very low in both the groups (AC: 6%, Control: 4%, $p<0.01$, chi-square test).

In line with our expectation, we observe a significantly higher share of travelers who offset their carbon emissions in the AC group (26.5%, $N = 1298$) compared to the control group (17.7%, $N = 857$) (Fig. 1, upper left panel) ($p < 0.01$, chi-square test). This difference can be primarily explained by the share of prompted participants (8.5%, $N = 416$) which adds to the share of deliberate participants (18.0%, $N = 882$) in the AC treatment. As described earlier, *deliberate participants* explicitly click "yes" on the first screen. *Prompted participants* are those individuals who decide to participate in the carbon offsetting program after having seen the pop-up frame – which can only be observed in the AC. We find statistically indistinguishable shares of deliberate participation in the AC treatment (18.0%) and in the control group (17.7%) ($p = 0.70$, chi-square test). This finding further underscores the success of our randomization strategy since the decision screen, up to this point, was identical for both treatments. In control, the remaining subjects (82.3%, $N = 3982$) did not participate in the program.[9] In AC, we obtain a greater variety of outcomes: 33.3% ($N = 1631$) of all participants deliberately chose not to participate. The remaining 48.7% ($N = 2382$) did not deliberately indicate their participation decision. When asked to decide, this fraction

booking decision (control: 6.4%, AC: 4.1%). Then we removed observations that were entered in the online form by a booking agency (163 observations) or that had missing values with respect to the *Info button request* (2 observations), or that already had participated in the trial phase (125 observations). We also removed observations from IDs beyond the fifth booking decision to avoid our explanatory variable that captures the number of recurrences to contain a few observations with very high values which might affect our regression analysis (27 observations). Finally, we removed observations from three IDs which were assigned to different treatments (6 observations). The final dataset contains 11285 observations.

[9] Note that the recorded data does not allow one to determine whether non-participation was an explicit choice (by clicking "no") or an implicit choice (by clicking "continue" without providing an active decision) for the control group.

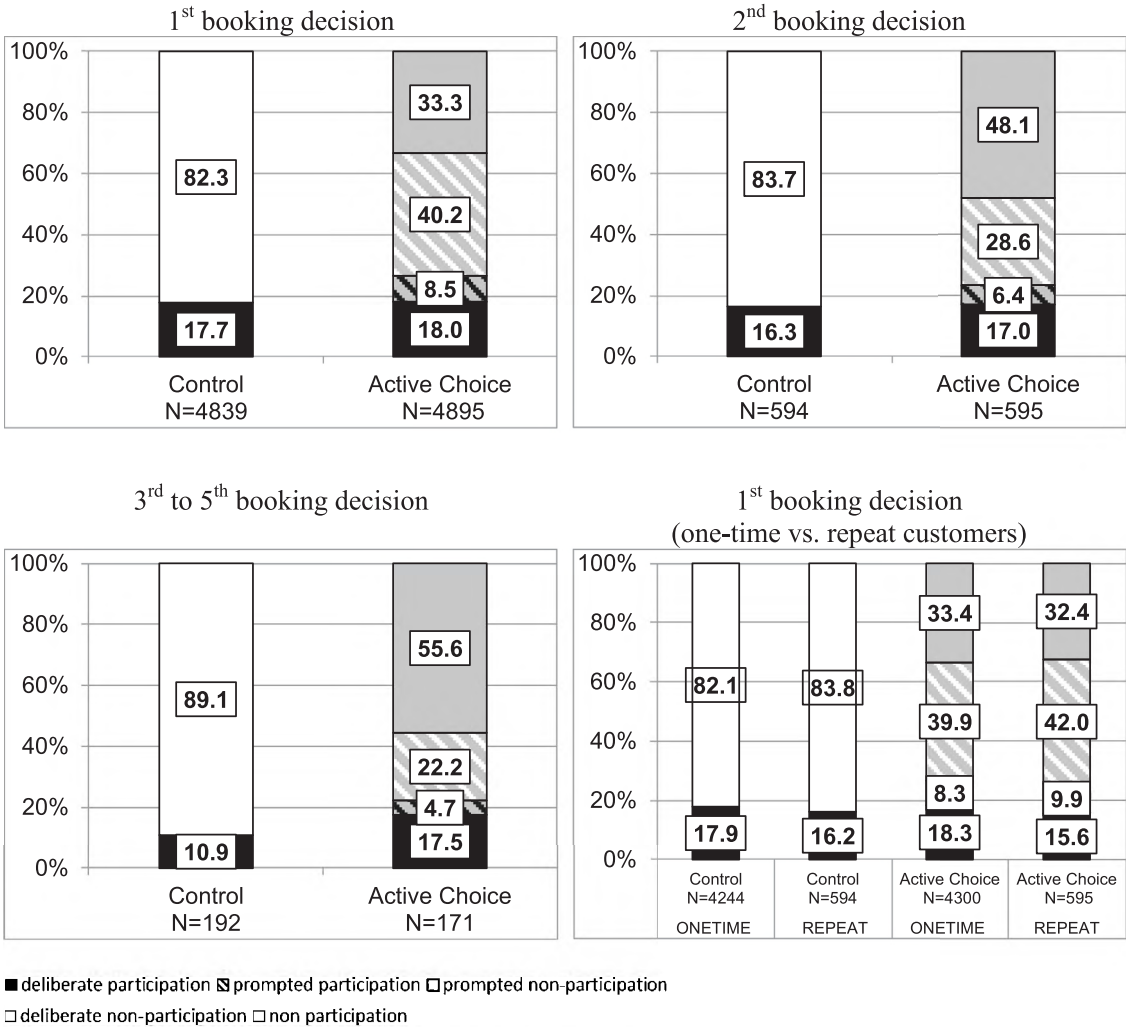

Fig. 1. Participation in first and repeated bookings.
Notes: In the control group we only observe whether individuals decided to participate in the carbon offsetting program or not. Since all participants in that group actively indicated their willingness to contribute to the carbon offsetting program by clicking "yes," we describe their choice as "deliberate participation" – as we do for the AC. All other observations in the Control group are labeled as "non-participation." In the AC, we are able to provide more differentiated results. For the participants, we are able to distinguish between deliberate (i.e., clicking "yes" or "no" on the initial screen) and non-deliberate choices (i.e., clicking after having seen the pop-up frame).The overall share of participants is therefore composed of both deliberate and prompted participants. Accordingly, we observe deliberate non-participants and prompted non-participants who jointly form the share of individuals not taking part in the offsetting program.

splits into prompted non-participants (40.2%, $N = 1966$) and prompted participants (8.5%, $N = 416$). Therefore, the AC treatment increases participation by roughly 50% (AC: 26.5%, control: 17.7%, $p < 0.01$, chi-square test) in contrast to the control group in the first booking decision.

The statistical significance of the treatment effect (i.e., the total difference between AC and control) is confirmed in a series of binary Logit models that investigate differences in overall participation rates (i.e., pooling deliberate and prompted participation) between the two treatment conditions (see Table 2). Both in an empirical model with the treatment indicator as a single explanatory variable only (column (1)) and in an expanded model with a set of further control variables (column (2)), we find significant positive effects of the AC treatment. The econometric results predict participation in the offsetting program to increase by about 9 percentage points when the choice architecture requires an AC.[10]

[10] One might argue that the effect of group bookings could differ from that of individual bookings since the booking group member might be forced to decide in the absence of other travelers without knowing their preference. Excluding those who purchased a group ticket ($n = 955$) does not however change our main findings, neither does restricting the regression analysis to the subsample of group bookings. For the latter case, the AC treatment increases participation rates by 11.7% (without control variables) or 11.8% (with control variables).

Table 2
Effect of active choice on program participation in the first booking.

	(1) Participation All first bookings	(2) Participation All first bookings	(3) Participation Only onetime bookers	(4) Participation Only first bookings of repeat customers
Active choice	0.088***	0.089***	0.088***	0.099***
	(0.008)	(0.008)	(0.009)	(0.023)
Controls	No	Yes	Yes	Yes
Observations	9733	9733	8544	1189

Notes: Mean discrete probability effects for treatment indicators are calculated after the ML estimation of a binary Logit model, dependent variable: participation (1 = the individual participated in the carbon offsetting program, either deliberately or after the prompt), explanatory variables: Active Choice (1 = the individual was assigned to the AC treatment), further control variables: CO_2 emissions per booking, offset price, voucher (1 = yes), female (1 = yes), standard errors in parentheses, $^{***}p < 0.01$.

One might expect that initial reactions (i.e., first bookings) to the program may differ for those who rarely book tickets compared to those who are frequent customers. We therefore distinguish between one-time customers ($N = 8544$ bookings, ONETIME) and the first bookings of repeat customers ($N = 1189$ first bookings, REPEAT) (Fig. 1, lower right panel).

In the control group, 17.9% ($N = 761$) of all the one-time bookers ($N = 4244$) and 16.2% ($N = 96$) of all the repeat customers ($N = 594$) opt for the program in their first booking ($p = 0.29$, chi-square test). In AC, 18.3% ($N = 789$) of all one-time customers ($N = 4300$) and 15.6% ($N = 93$) of all repeat customers deliberately choose to participate in their first booking. That is, in their first booking, repeat travelers are slightly less inclined to participate deliberately, but the differences are not statistically significant ($p = 0.11$, chi-square test). In contrast, we observe slightly more prompted participants in the group of repeat customers (9.9%, $N = 59$) compared to one-time customers (8.3%, $N = 357$). Again, this difference is not statistically significant ($p = 0.19$, chi-square test). In sum, when comparing total participation rates between these two types in AC, the mild differences cancel out and lead to similar overall participation rates in both groups: 26.6% ($N = 1146$) of all one-time customers and 25.5% ($N = 152$) of all repeat customers decide to join the program in their first booking decision ($p = 0.64$, chi-square test). Our econometric estimates reveal a significant treatment effect in AC compared to the control group for both one-time customers (8.8%) and repeat customers in the first booking decision (9.9%) (Table 2, columns (3) and (4)).[11] This analysis confirms our main result for frequent and infrequent bookers alike: the AC requirement significantly increases the participation in both groups by approximately 50% in the first booking in contradistinction to the control group.

This immediate positive effect is in line with previous empirical evidence on the introduction of an AC design in the context of voluntary contributions to a public good. Both in the case of organ donations (Johnson and Goldstein 2003) and retirement saving plans (Carroll et al., 2009) participation rates in AC are found to be significantly higher compared to an opt-in design. Löfgren et al. (2012), analyzing the effects on contributions to a carbon offsetting program, also find positive but insignificant effects for AC compared to opt-in. The authors explain the missing significance with the characteristics of the subject pool, mainly consisting of environmental economists traveling to scientific conferences and being experienced in such choice situations.

3.2. Repeated bookings

In this section, we investigate whether the AC requirement is sufficient to sustain higher contribution levels in repeated bookings. Our randomization procedure assigns repeat customers to the same treatment, which allows us to investigate their "follow-up" behavior and to explore the effects of repeated exposure.

Focusing on the second booking decision, we find that participation in AC remains higher compared to the control treatment (23.4% vs. 16.3%, Fig. 1, upper right panel, $p < 0.01$, chi-square test, $N = 1189$). While the share of deliberate participants is similar across both treatments (16.3% in control vs. 17.0%) the effect of the prompt on the treated (+6.4%) is still remarkable and not much smaller than in the first booking decision (+8.5%). Going beyond the second booking, the overall participation rates in the AC treatment, aggregated over the third to fifth booking, remain fairly stable, while contributions in the control group decline sharply (see Fig. 1, lower left panel: participation in AC 22.2% vs. 10.9% in control, $p < 0.01$, chi-square test, $N = 363$).

A set of Logit regressions confirms the overall treatment effect in repeated bookings (Table 3A). For our econometric model, we pool all observations and we add an explanatory variable that counts the current number of recurrences per ID (*recurrence #*), i.e., taking the value zero for the first booking, the value one for the first recurrence (i.e., the second booking), the value two for the second recurrence per ID and so forth. In addition, we include a corresponding interaction effect capturing the joint effect of the treatment variable and the booking count variable (*Active Choice x recurrence #*). Both in

[11] In a further robustness check we include a dummy variable *onetime* in our specification in column (2), Table 2. The estimated coefficient of this dummy – also in a further specification where we interact our treatment indicator with *onetime* – does not differ significantly from zero in our sample.

Table 3
Effect of active choice on program participation in repeated bookings.

A. ML estimates in binary Logit models			
	(1) Participation All bookers 1st to 5th booking	(2) Participation Only repeat customers 1st to 5th booking	(3) Participation Only repeat customers 2nd to 5th booking
Active Choice	0.509***	0.511***	0.448***
	(0.050)	(0.149)	(0.150)
Recurrence #	−0.135*	−0.139*	
	(0.077)	(0.073)	
Active Choice x Recurrence #	0.088	0.099	
	(0.126)	(0.126)	
Re-recurrence #			−0.400**
			(0.166)
Active Choice x Re-recurrence #			0.408
			(0.268)
Total bookings	−0.059	−0.042	0.005
	(0.058)	(0.104)	(0.111)
Constant	−1.408***	−1.489***	−1.556***
	(0.101)	(0.314)	(0.343)
Controls	yes	yes	yes
Observations	11285	2741	1552

Notes: ML estimation of a binary Logit model, dependent variable: participation (1 = the individual participated in the carbon offsetting program, either deliberately or after the prompt), explanatory variables: Active Choice (1 = the individual was assigned to the AC treatment), Recurrence # (no. of recurrence of the corresponding ID within the experiment, counting from 0 to 4), for the econometric specification in model 3 (only repeat customers, 2nd to 5th booking) we use recurrences beyond 1, labeled as "re-recurrences." Total bookings = total number of bookings per ID during the experiment, further control variables: CO_2 emissions per booking, offset price, voucher (1 = yes), female (1 = yes), standard errors (clustered for IDs) in parentheses, $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B. Estimated average probabilities and discrete probability effects of AC on program participation at different values of repeated bookings			
Dependent variable: Participation	(1) Estimated average probability for Active Choice = 0	(2) Estimated average probability for Active Choice = 1	(2)-(1) Estimated average discrete probability effect
Recurrence # = 1	0.159	0.239	0.080
Recurrence # = 2	0.141	0.215	0.073
Recurrence # = 3	0.126	0.193	0.067
Recurrence # = 4	0.112	0.173	0.061

Notes: Estimated discrete probability effects of the AC treatment on participation in the carbon offsetting program at different values of "recurrence #." Basis: estimated parameters in column (1) (All bookers, 1st to 5th booking) of Table 3, part A.

a specification where we include all customers (column (1)) ($p < 0.01$) and specifications where we exclude one-time customers (columns (2) and (3)) ($p < 0.01$) we report a significant effect of AC on total participation rates. We find a negative effect of the booking count variable on program participation in all specifications ($p < 0.1$ or $p < 0.05$). The estimated coefficient of the interaction term (*Active Choice x recurrence #*) is positive but does not statistically differ from zero in our sample. It should be noted, as pointed out by Ai and Norton (2003) and further discussed by Greene (2010), that for nonlinear models such as Logit models, the estimated parameter of the interaction term does not reflect the true estimated interaction effect and therefore may be misleading. Rather, the interaction effect depends upon the values of all explanatory variables. Following Greene (2010) and as applied by Ziegler (2017), the interaction effect is therefore assessed by comparing discrete probability effects of AC on program participation for different values of *recurrence #* (i.e., 1st to 5th booking) obtained for the estimated parameters shown in Table 3A, column (1). For the first recurrence, we report an estimated average discrete probability effect of 8.0 percentage points (Table 3B). This effect decreases over more repetitions but still amounts to 6.1 percentage points in the fourth recurrence.

We further compare return rates between the control and the AC group. One might suspect that subjects in AC could have been annoyed by the need to make a decision and thus might have returned to the website less frequently. However, the returning rates of 12.3% and 12.2%, respectively, do not differ between the two treatments ($p = 0.85$, chi-square test).

To summarize our findings on repeated bookings, we find a declining trend in participation rates both in the control and the treatment group over repeated bookings, but the difference in levels between both groups persists. Most importantly, as depicted in Fig. 1, a substantial share of the overall treatment effect in repeated bookings stems from *prompted participation*. Whether this stable effectiveness of the prompt is mainly attributable to individuals who face "repeated prompts" in their

Table 4
Transition dynamics between first and second booking.

A. Control group

1st booking	2nd booking		
	non-participation	deliberate participation	Obs
non-participation	477 (95.9%)	20 (4.1%)	497 (100%)
deliberate participation	20 (17.1%)	97 (82.9%)	117 (100%)
Obs	497	117	594

B. AC group

1st booking	2nd booking				
	Deliberate non-participation	Prompted non-participation	Prompted participation	Deliberate participation	Obs
Deliberate non-participation	141 (73.1%)	45 (23.3%)	0 (0.0%)	7 (3.6%)	193 (100%)
Prompted non-participation	124 (49.6%)	112 (44.8%)	6 (2.4%)	8 (3.2%)	250 (100%)
Prompted participation	6 (10.2%)	9 (15.3%)	29 (49.2%)	15 (25.4%)	59 (100%)
Peliberate participation	15 (16.1%)	4 (4.3%)	3 (3.2%)	71 (76.3%)	93 (100%)
Obs	286	170	38	101	595

subsequent bookings or to individuals who initially decided deliberately and then, in a later booking, faced the prompt for the first time, is the core of our analysis in the following section.

3.3. Transition dynamics

To shed more light on the mechanisms that led to declining contributions in the control group while keeping overall contributions more stable in AC, we now compare the decision dynamics between the first and the second booking in two transition matrices. In this section we therefore only look at repeat customers. We show the transition dynamics from the first to the second booking in the control group (upper panel) and the AC group (lower panel) in Table 4.

We start our discussion with the analysis of deliberate participation. In the control group, looking at first round deliberate participants ($N = 117$), we find that the large majority (82.9%, $N = 97$) remain with their decision in the second booking (Table 4A). At the same time, there is a stronger shift from deliberate participation to non-participation (17.1%) than from non-participation to deliberate participation (4.1%).[12] This picture is in line with the observed decline in contributions in the control group over repeated bookings.

In AC, we find similar levels of stability. Of the first-round deliberate participants ($N = 93$), 76.3% ($N = 71$) deliberately participate in the second booking (Table 4B, lower right cell) and only 6% ($N = 30$) of the remaining subjects ($N = 502$) become deliberate participants in the second round. Moreover, we also observe prompted participation. Here, 49.2% ($N = 29$) of the first round prompted participants stick to their first round behavior and only participate after seeing another prompt, while only 25.4% ($N = 15$) deliberately take part in the offsetting program in the second round. For first-round prompted non-participants, the picture is slightly different. Here, we observe that 49.6% ($N = 124$) of all first-round prompted non-participants ($N = 250$) switch to deliberate non-participation in the second round without seeing another prompt. The difference in the "switch" towards respective deliberate decisions between the two groups is also highly significant (25.4% vs. 49.6%, $p < 0.01$, chi-square test). Hence, analyzing transition patterns confirms our initial expectations that a repeated prompt remains effective for returning customers, even if they have been "prompted" before, and that prompted non-participation disappears faster than prompted participation.

3.4. Discussion

In this section we discuss our experimental results in the light of our theoretical assumption that anticipated feelings of guilt are a primary influencing factor, while also considering further potential behavioral channels. The above findings corroborate our explanatory framework: We both detect deliberate participants and deliberate non-participants who actively indicate their decisions in our experiment. At the same time, we also observe subjects who exhibit "choice avoidance" – that is, who try to move on without making a decision. When forced to decide, they finally either contribute to the program (prompted participants) or not (prompted non-participants). In particular, all three hypotheses derived from our framework are confirmed: Introducing an AC framework increases carbon offsetting immediately and also remains effective over

[12] This difference in relative terms does not (yet) lead to differences in absolute terms ($N = 20$ vs. $N = 20$), but if we go a step further and take into consideration the transition dynamics in the third round this picture gets more pronounced. 15.8% of second booking deliberate participants become non-participants in the third round, while only 0.9% of non-participants become deliberate participants.

subsequent choices. Analyzing transition patterns, we find that the positive effect in repeated decisions largely stems from subjects who had seen the prompt before, pointing towards strategic "choice avoidance." Finally, we observe a stronger decline in prompted non-participation as compared to prompted participation. That is, in line with our theoretical framework, saying "no" in the presence of the highlighted AC request may be perceived as even less pleasant as saying "no" in the more neutral screen prior to the prompt.

We are fully aware that our data do not provide unequivocal evidence of "choice avoidance" behavior. Hence, in this section we discuss the robustness of our findings while also considering two further behavioral channels. First, as discussed by Sunstein (2014) and others, the AC design might contribute to a learning process that helps to guide the development of preferences among individuals uninformed about the choice. As Stutzer et al. (2011) argue, even if the individuals fully understand the cost of contributing to a public good, the benefits often remain unclear or are difficult to assess. As a result, individuals might want to remain ignorant rather than make a decision about engaging in a prosocial activity. Along these lines, an AC design may prompt individuals to engage in reflection and form a subjective valuation of the offsetting program when facing this option for the first time, thereby providing a better basis for subsequent decision-making.

While our data on first bookings are in line with this mechanism of action, the persistent effectiveness of the prompt in repeated bookings cast doubt upon its explanatory power, as all these subjects had to decide on their participation in the offsetting program in the first booking decision. Of course, subjects may have forgotten about this option (or their preference for it) when returning for their next booking. If we generally assume that attitudes toward the program among returning customers tend to become weaker over time one might expect that the effect of the prompt should be weaker among customers who return quickly. Put differently, the effect of the prompt should increase with the time period between the first two bookings. However, as depicted in Fig. A2, the share of all prompted subjects, i.e., including both prompted participants and non-participants, does not increase in relation to the time gap between the first two bookings. It is also relevant to note that while the results show a drop in prompted decisions from 48.7% in the first booking decision to 35.0% in the second booking decision, the share of subjects that need a prompt to decide remains meaningful (Fig. 1, upper left and right panel). Even after the fifth booking we still observe about 25% of the subjects without a deliberate decision (Fig. 1, lower left panel).

Second, related to the previous argument, the AC requirement may simply drive individuals toward an option they would have failed to notice otherwise, e.g., due to inattention, leading them to stay with the default of non-participation in the opt-in setting (e.g., Hedlin and Sunstein 2016). For instance, strong time pressure has been found to reduce the cognitive capacity of individuals (Mullainathan and Shafir 2013). While the resulting cognitive scarcity may result in increased focus on some tasks, it will typically lead to a neglect of others. In our setting, it seems plausible that booking the actual ticket is the main task that could receive increased attention given time pressure, with ancillary concerns such as a carbon offsetting program receiving less attention. Even though the offsetting program was prominently placed in the booking system (see Fig. A1), subjects might have failed to notice their options in this regard. Following this argument, customers with pronounced time constraints might be more likely to overlook this option, even if they had already seen it before. To address this possibility, we analyze a time count that starts as soon as the customer has entered the full passenger name and stops when the customer tries to leave the subsequent decision screen (as depicted in Fig. A1), by clicking "yes," "no," or "continue." When comparing deliberate and prompted decisions, this time count is almost identical during first bookings. In particular, those trying to move on without a decision did not spend less time up to this point as those who deliberately made a decision. Note that inattentive individuals could be expected to be quicker even in the absence of time pressure, as they would not spend time reading the offsetting option and thinking about how to react to this offer. However, nothing like this can be observed in our experiment. If anything, prompted subjects even took more time before seeing the pop-up frame compared to the deliberate decision-makers, both in the first booking decision (83 vs. 79 s) and in subsequent bookings (59 vs. 45 s, see also Fig. A3). Lastly, we compare customers who clicked on the "additional information" button to receive more detail about the carbon offsetting program before their final participation decision. It is safe to assume that individuals who clicked this button were aware of the offsetting option and not suffering from inattention. Interestingly, among those who received additional information, the share of customers trying to move on without indicating a decision is still high (42.3%) and only slightly below the share of prompted individuals who did not follow the link (49.1%) during the first booking decision.

In conclusion, these additional analyses provide suggestive evidence that the observed behavior is not dominated by confounding effects. Rather, the persistently large share of missing decisions in AC, the persistent effectiveness of the prompt, and the decreasing share of prompted non-participation all lend support to the hypothesis that people intentionally try to avoid giving an answer due to anticipated feelings of guilt. We are fully aware that – without imposing further assumptions, e.g., on preference formation, we cannot clearly reject the impact of alternative channels in our setting. Hence, the true effect of strategic "choice avoidance" may differ from the observed treatment effect.

4. Conclusion

In many economic settings, public goods tend to remain underprovided. Numerous factors may hamper individuals to act on their good intentions and behave in a prosocial manner, including incentives for free riding and cognitive or behavioral

barriers. It has been shown that simple nudges such as altering the default option in a purchase process can help to successfully address these barriers. Since individuals tend to remain with the default option, contribution rates are higher when participation is the default and subjects have the option to "opt out," as compared to a setting where non-participation is the default and subjects have the option to "opt in" for contributing to a public good.

Given the important debate on the ethical concerns posed by default options (Smith et al., 2013), it may be preferable to employ softer interventions to a given choice architecture. While the literature seems to confirm that asking individuals to make a choice increases average contributions in the case of public goods, many questions remain unanswered. In particular, little is known about behavioral reactions in AC situations. Most notably, while AC choice architecture tends to exhibit a degree of effectiveness in the short run, little research has been conducted to date on whether these effects hold or fade away over time in repeated decisions.

In this paper, we analyze how the introduction of an AC requirement influences repeated decision-making in the framework of a carbon offsetting program. We show that the AC requirement increases the share of customers who offset their carbon emissions in the first booking decision by almost 50%. To the best of our knowledge, we provide the very first evidence of how an AC framework affects a green or impure public good setting when contributions are linear on trip distance and hence explicitly tied to harm-related behavior.

Investigating the behavior of returning customers within the same treatment conditions, we show that the treatment effects of the AC setting do not fade away over time but lead to behavioral differences in subsequent decisions. At the same time, the number of recurrences is largely unaffected, suggesting that there are no substantial dropouts due to this subtle intervention. Analyzing repeat customers enables us to shed light on the underlying behavioral channels. Prompting those without a deliberate choice to indicate their decision still effectively increases the share of carbon neutral trips. An important finding is that a substantial share of customers choose to offset only when prompted. This holds true not only for first time customers, but also for returning customers. The repeated prompt is effective even if the returning customers had seen the prompt before.

While our experimental design is certainly not able to provide a full picture of the underlying behavioral channels that lead to higher contributions in the AC framework, it does offer suggestive evidence that a significant share of customers constantly attempts to avoid a decision in repeated bookings. This finding is consistent with the "avoiding the ask" argument suggested by DellaVigna et al. (2012) and Andreoni et al. (2017) and offers a new facet to the idea of strategic "ask avoidance" as means of reducing the feeling of guilt.

From a policy perspective, our results indicate that requiring an AC can have strong effects. This form of intervention thus represents an alternative to opt-out settings, which are subject to legal challenges. Our results contribute to the debate on how to "nudge" pro-social behavior – for example, to promote environmental policy goals (List and Price 2016). In particular, we demonstrate that requiring individuals to make an active decision on whether to offset their carbon emissions has a positive effect on participation not only in the short-term but also in repeated interactions. During our experiment, a total amount of 32.2t CO_2 was offset (19.3t in AC, 12.9t in Control). The AC design increased the amount of emissions avoided by 6.5t CO_2 compared to an opt-in scenario – without incurring additional monetary cost to the bus company once implemented.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2019.02.001.

Appendix 1: Figures and tables

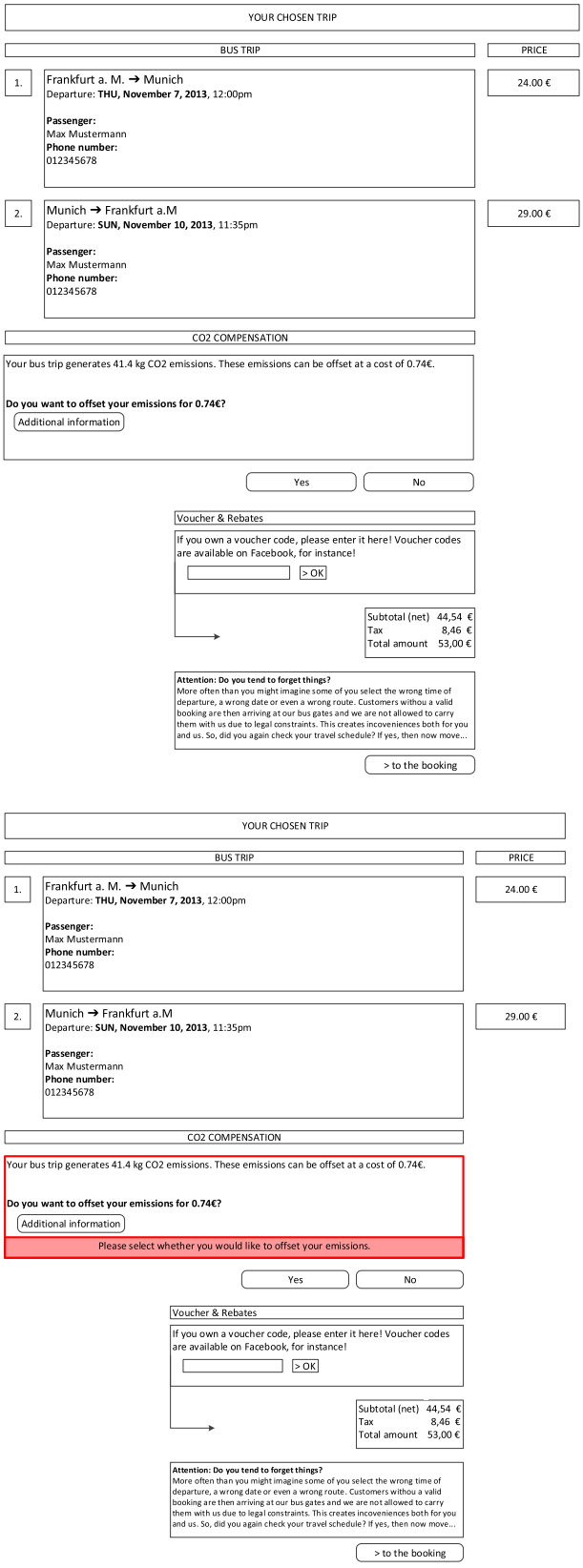

Fig. A1. Translated screenshot of initial screen (both AC and Control) (upper part) and second screen (pop-up) for AC (lower part).

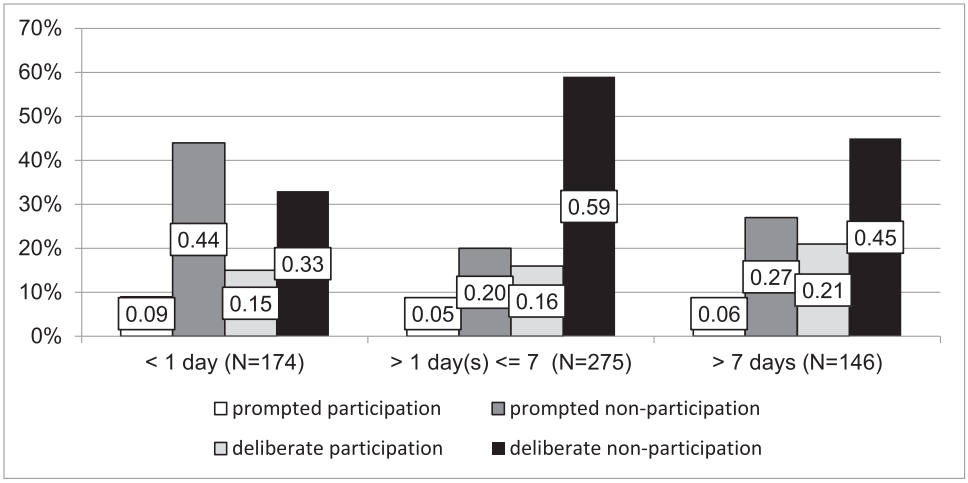

Fig. A2. Participation decision in the second booking depending on temporal distance between the first and the second booking.
Notes: The table shows decision behavior (e.g. share of deliberate participation, deliberate non-participation, prompted participation, prompted non-participation) for returning customers in the second booking decision depending on the temporal distance between the first two bookings. The bars on the left side show decision behavior for individuals who order their first two bookings within one day. The bars in the center show decision behavior for those who returned to the website within more than one but less than 7 days to order the second ticket. The bars on the right side capture decisions for those customers who returned for their second booking more than 7 days after their first booking.

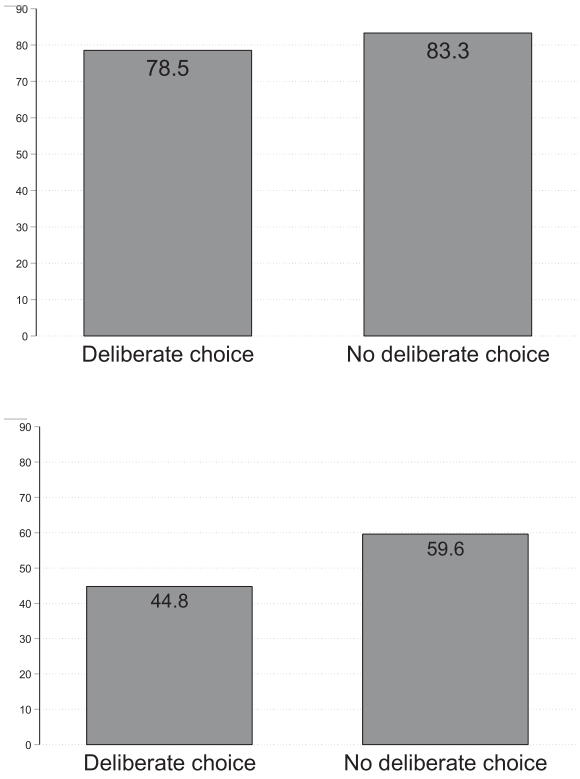

Fig. A3. Time spent (in sec.) before trying to leave the decision screen.
Notes: Top graph: first time booking decision, bottom graph: second booking decision and beyond. The time measure starts as soon as the customer enters the full passenger name and stops when the passenger tries to leave the subsequent decision screen (by clicking "yes," "no," or "continue").

Appendix 2: Expected responses toward the active choice design in repeated bookings

Deliberate participants and non-participants do not learn about the Active Choice requirement. Hence, their choice problem remains identical across rounds. Previously prompted subjects, however, learn about the Active Choice requirement and may consider this new information in subsequent bookings. More specifically, their choice situation will change as follows:

i. So far, we implicitly assumed that subjects expect to move on in the booking process with probability $p_0=1$ when not providing an answer. After seeing the prompt, subjects update the probability of being able to move on without giving an answer to a smaller probability p (with $0 < p < 1$).
ii. Moreover, after seeing the prompt, subjects are aware that, with probability $1 - p$, they are asked to make a choice in the presence of a red frame, inducing utility $u_{dec^*,red}$. This utility is likely to be lower compared to a decision without the red frame, due to two elements. First, the mere presence of the red frame may be perceived as unpleasant as it reminds subjects that they did not give an answer, independent of the later choice. Second, the red frame may increase the guilt from deciding against participation, inducing additional disutility for prompted non-participation compared to clicking "no" immediately. In summary, we assume that the red frame results in a differential reduction in utility, amounting to δ_{np} for prompted non-participation and to δ_p for prompted participation, with $\delta_{np} > \delta_p > 0$:

$$u_{dec^*,red} = u_{dec^*} - \delta_p \quad \text{if } u_{dec(part=1)} > u_{dec(part=0)}$$
$$u_{dec^*,red} = u_{dec^*} - \delta_{np} \quad \text{if } u_{dec(part=0)} > u_{dec(part=1)}$$

Incorporating the additional parameters yields the following type definitions for previously prompted subjects:

– *deliberate participants* (alias "passive givers"):

$$u_{dec^*} > pu_0 + (1-p)(u_{dec^*} - \delta_p) - c, \text{ and } u_{dec(part=1)} > u_{dec(part=0)} \tag{1b}$$

– *prompted participants* (alias "giving avoiders"):

$$u_{dec^*} < pu_0 + (1-p)(u_{dec^*} - \delta_p) - c, \text{ and } u_{dec(part=1)} > u_{dec(part=0)} \tag{2b}$$

– *deliberate non-participants* (alias "passive nongivers"):

$$u_{dec^*} > pu_0 + (1-p)\left(u_{dec^*} - \delta_{np}\right) - c, \text{ and } u_{dec(part=0)} > u_{dec(part=1)} \tag{3b}$$

– *prompted non-participants* (alias "saying no avoiders"):

$$u_{dec^*} < pu_o + (1-p)\left(u_{dec^*} - \delta_{np}\right) - c, \text{ and } u_{dec(part=0)} > u_{dec(part=1)} \tag{4b}$$

Based on these inequalities, our framework suggests the following changes in behavior when going beyond one-shot decisions:

1. *Prompted participants*: Comparing Eqs. (2) and (2b) reveals that the right hand side is decreased by $(c + \delta_p)\frac{(1-p)}{p}$ for previously prompted subjects, reducing the expected utility of not giving an answer. Hence, the higher c and δ_p, and the lower p, the more likely a switch to deliberate participation in subsequent decisions.
2. *Prompted non-participants:* Comparing Eqs. (4) and (4b) reveals that the right hand side is decreased by $(c + \delta_{np})\frac{(1-p)}{p}$ for previously prompted subjects, reducing the expected utility of not giving an answer. Hence, the higher c and δ_p, and the lower p, the more likely a switch to deliberate non-participation in subsequent decisions.
3. Comparing both groups, as $\delta_{np} > \delta_p$ due to additional guilt from saying "no" in the presence of a red frame, there should be more switches from prompted non-participation to deliberate non-participation than from prompted participation to deliberate participation.

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