Transportation Research Part D 133 (2024) 104289

Contents lists available at ScienceDirect

Transportation Research Part D

journal homepage: www.elsevier.com/locate/trd

Can social comparisons and moral appeals encourage low-emission transport use?☆

Johannes Gessner[a,*], Wolfgang Habla[b], Ulrich J. Wagner[c,d]

[a] *Graduate School of Economic and Social Sciences, University of Mannheim, L7, 3-5, 68161 Mannheim, Germany*
[b] *School of Business, Baden-Wuerttemberg Cooperative State University Villingen-Schwenningen, Friedrich-Ebert-Str. 30, 78054 Villingen-Schwenningen, Germany*
[c] *Department of Economics, University of Mannheim, L7, 3-5, 68161 Mannheim, Germany*
[d] *ZEW - Leibniz Centre for European Economic Research, L7, 1, 68161 Mannheim, Germany*

ARTICLE INFO

JEL classification:
C93
D04
D91
L91

Keywords:
Mobility budget
Randomized experiment
Nudging
Social comparison
Moral appeal
Habit formation

ABSTRACT

Because company cars add to corporate CO_2 footprints, companies are beginning to replace cars with mobility budgets that employees can use for leisure and commuting trips. This study examines whether nudges can encourage sustainable travel in such a subsidized setting. We conduct a field experiment with 341 employees of a large German company. Observing expenditure items charged to the mobility budget, we test if social comparisons and a climate-related moral appeal induce a shift towards low-emissions transport modes. We find that simultaneous application of both nudges causes a reduction in car use, particularly taxi and ride sharing, as well as substitution towards micromobility, but not public transport. The social comparison alone is not effective, and the treatment effects of the combined nudge vanish in the second half of the treatment period. Survey evidence suggests that these results are driven by a minority that complies with the communicated social norm.

1. Introduction

In Europe, many companies allow their employees to use company-owned cars for both business-related and personal travel (Copenhagen Economics, 2010). Thanks to favorable tax rules, such company cars have been a very popular fringe benefit, but their CO_2 emissions make it difficult for companies to reduce their carbon footprint. According to an EU-wide survey, 26% of companies providing company cars consider offering a so-called mobility budget as an alternative, i.e., a monthly or annual budget that employees can flexibly spend on a broad variety of transport modes available on the market (Arval Mobility Observatory, 2023). Implementing a mobility budget for CO_2 abatement purposes gives rise to a trade-off. On one hand, the benefit should provide mobility services equivalent to those of a company car. This means that car-based transportation such as car sharing, rental cars, and taxis, cannot be excluded from the menu of mobility options. On the other hand, emissions abatement hinges on employees using modes other than cars, particularly public transportation. Solving this trade-off is not trivial, because restricting or disincentivizing car use might drive employees to revert back to the company car or even change employers. In this paper, we study whether "green nudges" (Carlsson et al., 2021) can encourage the switch towards more climate-friendly modes of transport in this setting.

☆ This study was registered with the AEA RCT registry (AEARCTR-0008470) and approved by the Ethics Committee at the University of Mannheim (number 58/2021).

* Corresponding author.
E-mail addresses: johannes-gessner@gmx.net (J. Gessner), wolfgang.habla@dhbw.de (W. Habla), ulrich.wagner@uni-mannheim.de (U.J. Wagner).

https://doi.org/10.1016/j.trd.2024.104289
Received 5 January 2024; Received in revised form 4 June 2024; Accepted 6 June 2024
Available online 4 July 2024
1361-9209/

We conducted a randomized controlled trial that tested the effectiveness of nudging subjects into more sustainable transport mode choices within a mobility budget scheme implemented at a large German company. Since mobility budgets are regarded as more flexible and more sustainable alternatives to company cars, their popularity is bound to increase in the future. However, academic research so far provides little guidance on how the design of mobility budgets could steer participants towards sustainable mobility choices. By contributing the first causal evidence on the effectiveness of nudges within a mobility budget, our paper takes an important first step towards filling this gap. An advantage of our research design is that we observe a large share of individual transport mode choices, including public transportation, car sharing, rental cars, and ride-hailing and ride-sharing services. This allows us to study substitution between different modes of transport and sets our paper apart from previous research in this domain which mostly used data on a single transport mode only (see, e.g., Kormos et al., 2014; Kristal and Whillans, 2020; Gravert and Collentine, 2021). In contrast to previous studies observing transportation behavior across multiple transport modes (Cellina et al., 2019; Hintermann et al., 2024; Götz et al., 2023), participants in our study were not aware that they were participating in a field experiment.

The subjects in our experiment received bi-weekly e-mail messages containing either a nudge (treatment) or general information about the mobility budget (control) over a period of eight weeks. All treated subjects were nudged via a social comparison and half of them additionally via a moral appeal to reduce emissions from individual transportation choices. Both treatments promised to induce substitution away from car-related mobility: Social comparisons convey a *descriptive norm*, i.e., the behavior adopted by the majority in a relevant peer group. This should be effective in the context of the mobility budget if participants benchmark their transportation choices against those of their co-workers. Moral appeals communicate information about *injunctive norms*, i.e., shared standards of acceptable group or societal behavior, and may change individual behavior if participants wish to comply with the communicated expectations of the peer group or have an intrinsic motivation to do something about the moral issue at hand (in this case, mitigating climate change). By combining information about *injunctive norms* and *descriptive norms*, the second treatment conveys a *social norm* (Bicchieri, 2005) for the use of low-emission transport modes.

In the *social comparison* treatment, we informed participants of the mobility budget program about their share of public transportation expenditures as compared to the respective share of a peer group. In the second treatment, the message additionally contained (i) information on the CO_2 emissions savings of public transportation relative to car use, (ii) information on the necessity to combat climate change, and (iii) a moral appeal to use public transportation (and other low-emissions transport modes) whenever possible to effectively combat climate change. Since the information contained in this treatment goes beyond injunctive and descriptive norms, we refer to this treatment as the *strong social norm* treatment (as in Ferraro et al., 2011).

The remainder of this paper is structured as follows. Section 2 reviews the related literature. Section 3 describes the setup for the experiment. Section 4 presents the estimated effects of both treatment arms on the use of three transport mode categories and investigates treatment heterogeneity with respect to time, employee characteristics, and modal choice within transport categories. In Section 5, we discuss potential channels through which the two treatments could have worked, drawing on data from various surveys. Section 6 concludes.

2. Literature review

Nudges, in parts of the transportation literature also referred to as "soft interventions" (e.g., Steg, 2003), are often employed to counteract deviations from rational behavior, such as bounded rationality or cognitive biases (Thaler and Sunstein, 2009). In the transportation context, as pointed out by Mattauch et al. (2016), such nudges can target biases that have been shown to induce sub-optimal choices of transportation modes at the *individual level* (e.g., Innocenti et al., 2013; Larcom et al., 2017; Lattarulo et al., 2019; Moody and Zhao, 2019; Andor et al., 2020a).[1] In addition, nudges are becoming increasingly popular as a substitute for regulating environmental externalities. Such "green nudges" (Carlsson et al., 2021) can be employed to alter transportation choices that are *collectively* sub-optimal. Drawing on research in economics, transportation, psychology, and behavioral sciences, this study analyzes whether "green nudges" can help to mitigate externalities from fossil fuel-based mobility, addressing several research gaps in the literature.

First, few studies in the transportation sector analyze causal effects of behavioral interventions using randomized controlled trials (cf. Garcia-Sierra et al., 2015). The available experimental evidence is mostly based on self-reported (stated) choice data gathered via surveys or lab experiments (e.g., Avineri and Waygood, 2013; Filippini et al., 2021; Piras et al., 2021; Bao and Lim, 2022). Rare exceptions are Cellina et al. (2019), Gravert and Collentine (2021), Kristal and Whillans (2020) and Götz et al. (2023) who analyze real-world transportation behavior (revealed preferences). We contribute to filling this gap by running a field experiment at a large German company where we are able to track employees' mode choice and spending within a mobility budget.

Second, while nudges have been studied in countless field experiments across a broad range of settings, including some in transportation, our study is the first to examine the joint impact of social comparisons combined with a moral appeal in a randomized controlled trial in the transportation context.[2] A social comparison communicates a descriptive norm, i.e., a characterization of the factual behavior of a peer group. According to Cialdini et al. (1990), subjects tend to conform to the behavior of their peers,

[1] For comprehensive reviews on behavioral aspects in transportation, see Graham-Rowe et al. (2011), Avineri (2012), Metcalfe and Dolan (2012), Garcia-Sierra et al. (2015), and Semenescu et al. (2020).

[2] Simultaneous and independent research by Götz et al. (2023) analyzes the effect of a mobile application that communicates a moral appeal to reduce CO_2 emissions from individual mobility choices. While the social comparison is one of many "gamification features" that participants can select in their experiment, our study administers moral appeal and social comparison to *all* participants in the respective treatment groups.

either because they consider the observed behavior as a signal for individually optimal behavior in a given situation, or because they have a preference for conformity. Social comparisons, the most frequently evaluated type of "green nudge", have proven effective at reinforcing pro-environmental behavior (Farrow et al., 2017), particularly in the area of energy and water conservation by households (e.g., Schultz et al., 2007; Nolan et al., 2008; Ferraro et al., 2011; Ferraro and Price, 2013; Allcott and Rogers, 2014; Allcott and Kessler, 2019; Andor et al., 2020b; Brick et al., 2023). In transportation research, social comparisons have been found to reduce (self-reported) car usage (Kormos et al., 2014), while having no effects on decisions to car-pool (Kristal and Whillans, 2020) or use public transit (Gravert and Collentine, 2021). In view of the weak performance of nudges in the transportation context, Gravert and Collentine (2021) conjecture that "switching transport options comes at a higher effort and even monetary cost" compared to other environmental domains.

This motivates our design choice to combine information about peer group behavior with a moral appeal to reduce transport-related CO_2 emissions. As moral appeals signal socially approved behavior, they should provide stronger nudges. In fact, moral appeals have been shown to enhance energy and water conservation efforts (Ferraro et al., 2011; Ferraro and Price, 2013; Ito et al., 2018), and to be effective in domains other than conservation (Bursztyn et al., 2019; Bott et al., 2020). Our study is perhaps most closely related to Ferraro et al. (2011) and Ferraro and Price (2013) who experimentally evaluate a social comparison combined with a moral appeal in the context of water conservation. Ours is the first paper to test the effects of such a "strong social norm treatment" on observed individual transportation choices. Our paper is also related to a stated choice experiment by Piras et al. (2021), showing that injunctive norm messages increase participants' intention to use public transportation and active modes of transport instead of passenger cars, while descriptive norm messages affected only the intention to use active modes of transport.

Our paper also contributes to the literature on interventions to improve environmental outcomes in a corporate setting. Other papers have studied, e.g., the use of paper and printing at universities (Egebark and Ekström, 2016), fuel-efficient behavior by airline captains (Gosnell et al., 2020) or truck drivers (Hoffmann and Thommes, 2020), the commuting behavior of university employees (Rosenfield et al., 2020), and the electricity consumption of a bank (Fanghella et al., 2022).

3. Experimental setup

This section describes the experimental environment, the selection of the experimental sample, the randomization procedure, and the setup of the experiment.

3.1. The experimental environment

The present study was carried out in collaboration with a large company that has several business locations in Germany. Our partner company offers a company car to approximately 50% of its employees, including employees who would not need a car for business-related travel. The company provides the vehicle and pays for insurance, maintenance, and fuel. Private car use is explicitly allowed in exchange for monthly deductions from the employees' pre-tax salary. In 2020, the company introduced a mobility budget as an alternative to a company car. After a pilot run at two business locations, the program was rolled out at all German business locations of the company in April 2021. Employees eligible for a company car could choose to give up that eligibility for 24 months and commit to participating in the mobility budget program during that time. Participants receive an annual budget to cover their mobility expenses, amounting to €2400 for full-time employees and at least €1400 for part-time employees. The budgetary year April 2021–March 2022 defines the time frame for our experiment. At the beginning of that period, the company communicated it would donate all funds remaining at the end of the budgetary year to a reforestation project, emphasizing the implied carbon sequestration but also benefits to the local communities and wildlife.

The mobility budget can be spent on commuting and leisure trips (expenses for business trips are reimbursed separately) and admits the use of a broad range of means of transportation (listed in the next paragraph). The program intends to offer attractive substitutes for the mobility services provided by a company car. It therefore allows the budget to be used on trips during the employee's vacation, including outside Germany, and it partly pays for mobility expenditures of the employee's family members. The budget is implemented as a reimbursement scheme whereby participants pay for expenditures out of their own pocket and subsequently claim reimbursement by entering the trip details (day and time of the trip, location where the trip was booked, trip fare, transport category) into the company's expense tool. For tax compliance purposes, employees are asked to hand in their tickets and receipts as soon as possible and not after the end of the calendar year.[3] Thanks to this documentation requirement, we observe most of the participants' travel activity in terms of the transport mode used, the date of the transaction, and the expenditures made. However, we do not observe the distance, duration, point of departure, and destination of their journeys.

Eligible expenditures can be subdivided into three categories. *Public transport* includes all short-distance ("local") and long-distance public transportation tickets for single and multiple rides, as well as monthly commutation tickets. While the program also reimburses annual commutation tickets, we do not include these expenditures in the public transport category unless otherwise noted, as their period of validity does not match the timing of the interventions we describe below.[4] *Car-related transport* includes ride-hailing and ride-sharing services (e.g., taxis, UBER, etc.), car sharing and rental cars. Fuel expenditures for rental cars are not

[3] Expenditures for all transport modes except for public transportation are taxed as non-cash benefits to the employee and thus must be handed in before the end of the corresponding calendar year. Participants were encouraged to also hand in expenditures for public transportation on time.

[4] The budget can also be used for annual rail cards that provide a discount on train fares ("BahnCard25", "BahnCard50"). We are not able to distinguish them from tickets for long-distance public transport.

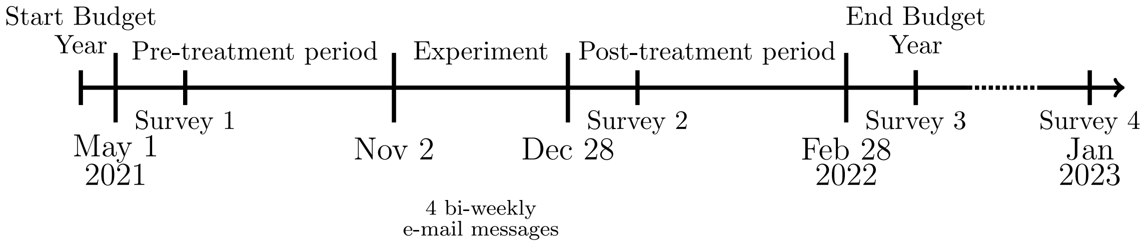


Fig. 1. Timeline of the experiment.

reimbursable though. *Micromobility* is the residual category and comprises electric scooters and electric kick scooters (henceforth referred to as e-scooters), bike sharing, bike subscriptions and bike repairs.

In the expenditure data of the mobility budget, we cannot observe individual public transit rides covered by commutation tickets, walking, or use of vehicles not covered by the mobility budget, such as private cars, bicycles, or motorbikes. In our surveys, 82% of respondents (see survey question Q3.1 in Appendix D) stated that they regularly use transport modes outside of the mobility budget (39% use a private car or their partner's company car, 2% use a motorbike or similar and 64% use a bicycle or similar; see Appendix D, question Q3.2). The appendix with all supplementary material can be found online.

3.2. Implementation

Timeline. Prior to the start of the program on April 1st, 2021, our partner company invited employees that were eligible for a new company car to sign up for the mobility budget instead. All employees who expressed an interest in the program were enrolled. Of those 463 participants, roughly 80% had not made use of the company car offer before. 21 subjects joined the program after April 1st (but prior to the treatment period) because they had not yet been eligible to join the program on the start date.

Fig. 1 displays the timeline for our interventions and data collection. The experiment lasted from November 2nd to December 28th (eight weeks) and was thus fully contained within the first budget year (April 1st, 2021 to March 31st, 2022). We conducted four surveys to elicit additional data from the participants. Our questionnaires were added as a separate section to semi-annual surveys that our partner company conducted among all participants of the new mobility budget scheme. At the end of the main survey, participants could either leave or voluntarily continue with a questionnaire intended for scientific research on the mobility budget. The baseline survey took place at the end of June 2021. We conducted further surveys shortly after the end of the interventions (the midline survey) as well as at the end of the first budget year in April 2022 (the first endline survey). A second endline survey was conducted one year after the experiment, in January 2023. For the analysis, we define the *pre-treatment period* to last from May 1st until November 1st, 2021, and the *post-treatment period* to last from December 28th, 2021 until February 28th, 2022. Because spending at the beginning and the end of the first budget year is very atypical, April 2021 and March 2022 are excluded from the analysis sample. Tables E.4–E.5 in the appendix show that results are robust to this exclusion.

Randomization. In mid-October 2021, the company provided us with data on spending by all 463 program participants between April 1st and September 30th, 2021. Based on this dataset, we randomly assigned participants into control and treatment groups in the last week of October 2021. To ensure that participants with differing expenditure levels and commuting options were equally represented across groups, we stratified the sample between urban vs. rural business locations and across quartiles of public transport expenditures and total expenditures (excluding expenditures incurred by household members) during the pre-treatment period.[5]

Panel A of Table 1 shows the number of participants assigned to the different groups at the time of randomization (the full sample). Before the analysis of the experiment, two participants were excluded because they had opted out of the mobility budget before the start of the experiment. Additionally, we removed 33 inactive participants from our sample who had not used the mobility budget before November 2nd, 2021. We excluded 87 subjects who bought an annual public transport ticket at some point during the budget year, because a large share of public transport use for this group is covered by their annual ticket and is hence unobservable to us. As shown in Table 1, inactive users and annual public transport ticket holders are distributed evenly over the three treatment arms. We retain 341 employees in the main analysis sample: 110 in the control group, 115 in the social comparison treatment, and 116 in the strong social norm treatment (social comparison plus moral appeal). Before aggregating expenditure items for each participant at the monthly level,[6] we drop expenditures made outside Germany (most likely incurred during vacation) and expenditures made by the employee's family members.

[5] In total, we have 34 strata. Urban locations of work are those classified as densely populated areas according to Eurostat's "Degree of Urbanization" (DEGURBA) classification for "Local Administrative Units". Rural refers to intermediate density areas. Two additional strata based on the degree of urbanization were assigned only to those participants who had not handed in any expenditures before October 1st, 2021.

[6] We aggregate expenditures for each week from Tuesday to Monday, assign these weekly observations to the corresponding Sunday, and assign these Sundays to the corresponding month.

Table 1
Time-invariant variables and pre-treatment mobility.

	Control		SC		(SC + MA)		
Panel A: Full sample							
N	150		156		157		
thereof inactive users	11		12		12		
thereof annual ticket holders	29		29		29		
	Mean	SD	Mean	SD	Mean	SD	Test
Panel B: Experiment sample							
Total expenditures	113	91	123	90	123	84	F = 0.471
thereof PT expenditures	56	58	66	61	66	56	F = 0.977
thereof CT expenditures	46	79	48	76	47	69	F = 0.008
thereof MT expenditures	10	20	9	26	11	22	F = 0.102
Total use count	4	4	5	6	5	4	F = 0.852
thereof PT use count	3	3	3	3	3	3	F = 1.863
thereof Car use count	0.9	2	2	4	1	3	F = 1.588
thereof MT use count	0.6	2	0.5	3	0.5	1	F = 0.069
% PT users	95%		91%		92%		$\chi^2 = 0.917$
% CT users	61%		62%		68%		$\chi^2 = 1.533$
% MT users	50%		37%		47%		$\chi^2 = 4.708^*$
% Gender male	55%		50%		62%		$\chi^2 = 3.705$
% Urban	45%		46%		41%		$\chi^2 = 0.869$
Annual budget	2343	174	2351	190	2316	224	F = 1.014
Age	43	11	43	12	41	12	F = 0.906
% High career level	54%		49%		43%		$\chi^2 = 2.515$
N	110		115		116		

Statistical significance markers: * p < 0.1; ** p < 0.05; *** p < 0.01.
Notes: The full sample corresponds to the sample included in the randomization. In the experiment sample, inactive users and annual PT ticket users are removed. Control abbreviates the control group. SC abbreviates the social comparison treatment. SC + MA abbreviates the strong social norm treatment. PT abbreviates public transport (excluding annual public transportation tickets), CT abbreviates car-related transportation and MT abbreviates micromobility. Total expenditures exclude expenditures made by household members of the participant and expenditures made outside Germany. Expenditures give the monthly average expenditures for the corresponding transport mode for the time period May 1st–November 1st, 2021. Count gives the average monthly number of expenditure items. Users indicate whether an individual has used the corresponding transport mode at least once. % Gender male gives the share of male participants. % Urban gives the share of employees that work at a location classified as urban. Annual budget gives the average annual mobility budget. Age gives the average age. % High career level gives the share of participants on a high career level (according to the company's career level specifications). To test whether the variable of interest varies significantly with the treatment group assigned, a χ^2-test is used for categorical, and an F-test for numerical variables.

3.3. Descriptive statistics

We assess the balance on time-invariant and pre-treatment covariates across the control and treatment groups in Panel B of Table 1. The differences between groups along the covariates of interest are not jointly significant at the 5%-level. Only the differences in the share of micromobility users are jointly significant at the 10%-level.[7] Public transport expenditures were slightly (but not significantly) higher in the treatment groups as compared to the control group. In the econometric analysis, we control for such differences via employee fixed effects or by including controls for employee pre-treatment mobility use across the three transport modes.

Table 2 summarizes the monthly averages of individual expenditures and budgeted items, respectively, for car-related transport, public transport and micromobility over the whole observation period, i.e., from May 1st, 2021 until February 28th, 2022. Monthly average expenditures over this period amount to €55.32 for public transport and €40.39 for car-related transport. Including micromobility, total expenditures amount to €104.39. On average, participants submitted 2.60 expenditure items for public transport and 1.10 for car-related mobility. Adding micromobility brings the total to four items in an average month. Despite the low average, there is considerable variation across participants, with the maximum usage amounting to about one expenditure item *per day*. At the beginning of treatment, the average participant had spent 43% of their annual budget. The average participant spent only €1516 during the entire year. This is less than two thirds of the full budget. Only 24% of participants used their full budget.[8]

Fig. 2 summarizes the participants' average propensity to use the three main transportation categories (Panel (a)) and their respective expenditure shares (Panel (b)) during the pre-treatment period. Almost all participants booked at least one public transport ticket, about two thirds made use of car-related transport, but less than half used micromobility options. Most of the budget was spent on public transport (52%), followed by car-related mobility (39%) and micromobility (8%). Panel (c) plots the average monthly expenditures for a more disaggregate modal classification. With respect to public transportation, the average participant spent €25.30 per month on local vs. €37.40 on long-distance public transportation. Monthly expenditures on car-related mobility are split

[7] Micromobility was included into the main analysis upon request by a reviewer in an earlier submission, and had thus not been included in balance assessments at the time of randomization.

[8] It is likely that usage would have been higher in the absence of the COVID-19 pandemic. We consider full usage when less than 1% of the budget remained by April 4th, when we received the final data.

Table 2
Average monthly transportation expenditures.

	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total expenditures [EUR]	341	104.39	66.06	0.00	45.73	98.07	154.73	238.75
thereof PT Exp.	341	55.32	48.00	0.00	14.78	43.90	88.09	231.56
thereof CT Exp.	341	40.39	54.73	0.00	0.00	14.20	58.17	220.73
thereof MT Exp.	341	8.67	18.80	0.00	0.00	0.00	10.93	178.90
Total use count	341	4.10	3.97	0.00	1.40	3.00	5.70	31.20
thereof PT use count	341	2.60	2.67	0.00	0.60	1.60	3.80	14.90
thereof CT use count	341	1.10	2.25	0.00	0.00	0.40	1.00	17.30
thereof MT use count	341	0.41	1.44	0.00	0.00	0.00	0.30	20.90

Notes: The average monthly use is calculated by summing expenditures and expenditure item counts for a participant for the period May 1st, 2021–February 28th, 2022. PT abbreviates public transport. CT abbreviates car-related transport, including, e.g., taxi or UBER rides, car sharing and rental cars. MT abbreviates micromobility, e.g. bike sharing, rental and repairs or shared e-scooters. We include only expenditure items within Germany and exclude expenditure items of family members. The maximum monthly averages can exceed €200, as we exclude April 2021 and March 2022.

almost evenly between car sharing (€16.30), rental cars (€17.20) and taxis (€13.40, including ride-hailing services such as UBER and shuttle pooling). Note that expenditures for micromobility account for less than 10% of overall pre-treatment expenditures. Micromobility expenditures are mostly made for bike sharing, bike rentals and repairs (€8.55 per month, in contrast to €1.58 for electric scooters), which is the more costly category per expenditure item.

Only employees eligible for a company car can sign up for the mobility budget. We thus compare the average participant in the mobility budget with the average employee eligible for a company car (see Appendix Table E.1). The former is younger, more likely to work at an urban business location and less likely to live in a household with access to a car than the latter. Moreover, participants are similar to non-participants who are eligible for but do not have a company car. This suggests that employees selecting into the mobility budget might not have chosen a company car in the absence of the program.

3.4. The interventions

The interventions consisted of a series of e-mail messages. Messages were written in English (the second company language besides German) and sent to the participants' company e-mail addresses. Overall, every participant received four e-mails, in bi-weekly intervals between Tuesday, November 2nd, and Tuesday, December 14th, 2021. Both treated and control subjects received e-mails. The subject line was the same for all groups and read "Information about *[name of the mobility budget program]* ". Since the sender was the company's team managing the mobility budget, participants had strong incentives to read the e-mails, and hence we interpret the effects reported below as average treatment effects rather than intent-to-treat effects. While the exact wording of all e-mail messages is relegated to Appendix A, we shall summarize below their most important contents. We will discuss potential channels through which the treatments could have altered behavior in Section 5.

All e-mails were comprised of two paragraphs and were very similar in length. We sent an e-mail ("placebo e-mail") to the control group because the literature has shown that e-mails and other types of messages can serve as a reminder for feasible options that subjects have forgotten about (see, e.g., Allcott and Rogers, 2014; Sonntag and Zizzo, 2015; Castleman and Page, 2015; Karlan et al., 2016; Habla and Muller, 2021). The first paragraph of the placebo e-mail thanked the employees for participating in the mobility budget scheme and provided some information on the scheme itself, e.g., that the budget can be used to pay for different transport modes. It mentioned explicitly that this includes public transportation. The second paragraph provided further information about the program, unrelated to the participant's transport mode choice and use of the budget.

Social comparison treatment. The social comparison group received an e-mail that compared, in the first paragraph,

1. the share of public transport expenditures in the participant's cumulative reimbursements (spanning the time period April 1st, 2021, up to either October 1st for the first two e-mails, November 1st for the third e-mail, and December 1st, 2021, for the last e-mail), and
2. the average share of public transport expenditures across participants at all business sites in either rural or urban areas, depending on whether the employee worked at a business location classified as urban or rural. Since urban areas tend to offer better access to public transportation (Nobis and Herget, 2020), comparing participants working at business locations with a similar degree of urbanization should make the peer group comparison more relevant.

The second paragraph was identical to the second paragraph of the placebo e-mail, so as to ensure similar total length.

Strong social norm treatment. The second treatment group received an e-mail with the same social comparison text as sent to the social comparison group in the first paragraph, but the second paragraph contained a moral appeal instead of the very general text that the control group and the social comparison group received. Adding the moral appeal to the social comparison treatment adds an explicit injunctive norm to the descriptive norm conveyed by the social comparison, together invoking a social norm (Bicchieri, 2005) for using low-emissions transport modes. Additionally, to study whether norm-based interventions can affect transportation behavior, the strong social norm treatment is the more promising intervention, as previous research shows that combining a moral appeal with a social comparison leads to bigger and more persistent treatment effects than a moral appeal in isolation (Ferraro

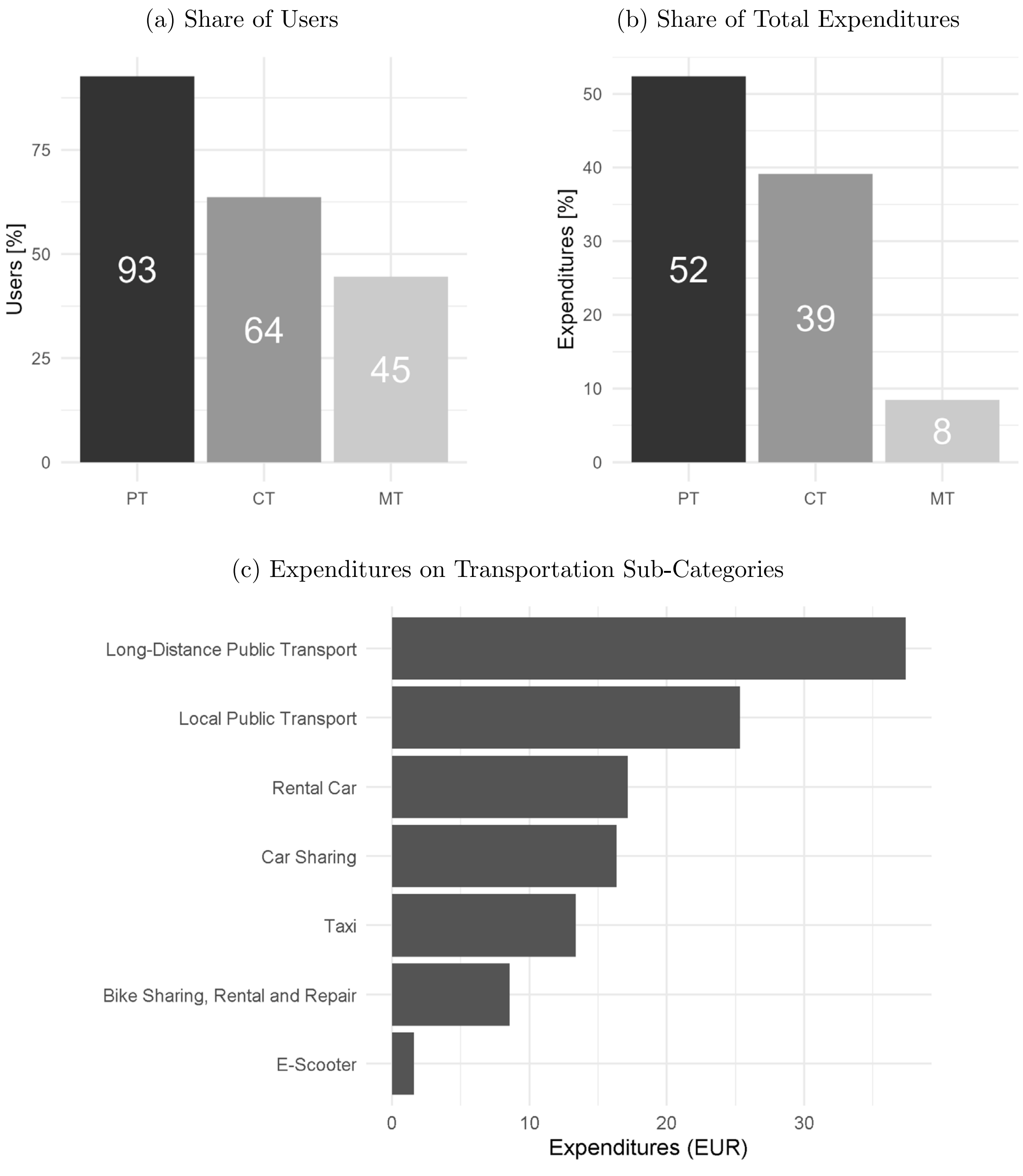


Fig. 2. Pre-treatment usage of the mobility budget. Notes: Users [%] gives the share of participants who used the transport mode at least once during the pre-treatment period. Expenditures [%] gives the share of expenditures for one transport mode during the pre-treatment period, relative to the sum over the three transport modes included. Expenditures (EUR) gives the average monthly pre-treatment expenditures for the sub-categories making up the transport modes included in Panels (a) and (b). PT abbreviates public transportation, CT abbreviates car-related transportation, and MT abbreviates micromobility. The pre-treatment expenditures made by family members (€15.30 on average) are excluded in all panels and the subsequent analysis.

et al., 2011). The moral appeal was framed in the context of climate change. Following the norm activation model by Schwartz (1977), the moral appeal comprised three parts, which were meant to increase awareness for a "state of need" for environmental protection, for "actions which could relieve the need" and the "own ability to provide some relief":

1. a sentence highlighting the necessity and urgency of mitigating climate change (*"scientific evidence gathered by the United Nations emphasizes that immediate and large-scale efforts to mitigate climate change are needed"*),
2. information about the participant's ability to reduce her transport-related CO_2 emissions by changing transport modes (*"traveling one kilometer by public transportation causes only between 20 and 60% of CO_2 emissions released when traveling the same distance by car"*), and
3. the moral appeal to use low-emissions transport modes like public transportation (*"in order to combat climate change, you should use public transportation or other low-emissions transport modes whenever possible"*).

4. Results

We consider expenditures and use of car-related transport, public transport, and micromobility as our main outcome variables. We first show trends in these outcomes, followed by econometric estimates of treatment effects. After discussing heterogeneous treatment effects over time, individuals and transport mode subcategories, we provide an interpretation of our results at the end of this section.

4.1. Trends

Fig. 3 shows the average monthly expenditures on different transport modes, as well as the average propensity to use them at least once in a given month, by treatment arm. The diagrams show increased travel activity over the summer months and in autumn (school holidays are not synchronized across the German federal states, Länder). Furthermore, travel behavior across all groups tracks the course of the COVID-19 pandemic, which severely affected Germany at the beginning of 2021 and again after October 2021, entailing lockdowns as well as travel restrictions. Employees at our partner company were allowed to work from home during the entire observation period, and fear of infection likely limited their disposition towards public transportation. Observing travel behavior in the randomized control group allows us to tease apart the impact of the treatments on transport choices from variation driven by the pandemic or seasonal preferences (Hudde, 2023).

The empirical strategy employed in the next subsection relies on a parallel-trends assumption. In Fig. 3, we see that most outcome variables follow parallel trends across the three treatment arms during the pre-treatment period (Figure E.1 in Appendix E shows additional evidence). It is apparent that monthly average expenditures for the three transport modes are quite volatile. Part of this is driven by the impact of few but exceptionally expensive expenditure items. For example, the highest monthly expenditure for car-related transport per individual in the pre-treatment period was over €1000 (Figure B.1), about one fifth of total monthly expenditures in this category (Table 2).

4.2. Regression analysis and main results

We employ regression analysis to estimate causal treatment effects on outcome Y_{it} of employee i in month t. We use the term "extensive margin" to indicate changes in the propensity that an average participant uses a certain mode of transport, which is equal to changes in the share of users per month. By contrast, "intensive margin" refers to how much the average participant uses a certain transport mode (in terms of expenditures per month). We start with the extensive margin and then discuss the intensive margin. Both outcomes are analyzed using a difference-in-differences approach with month fixed effects ω_t and employee fixed effects c_i, controlling for common time trends and unobserved individual heterogeneity.

We estimate the following equation as a linear probability model to recover extensive-margin reactions to our treatment:

$$Y_{it} = \sum_{j\in\{1,2\}} \left[\beta_1^j T_i^j \times \tau_t + \beta_2^j T_i^j \times \rho_t\right] + \omega_t + c_i + \epsilon_{it} \tag{1}$$

In this specification, Y_{it} is an indicator for whether participant i has used the corresponding transport mode in month t; τ_t and ρ_t are indicator variables for the treatment and post-treatment period, respectively; T_i^j is an indicator for subject i being in treatment arm $j \in \{1 = \text{social comparison}, 2 = \text{strong social norm}\}$; and ϵ_{it} is an error term.

To assess the effect along the intensive margin, we estimate the following equation for $\tilde{Y}_{it}$, the monthly expenditures per transport mode,

$$\tilde{Y}_{it} = \exp\left(\sum_{j\in\{1,2\}} \left[\beta_1^j T_i^j \times \tau_t + \beta_2^j T_i^j \times \rho_t\right] + \omega_t + c_i\right) + \epsilon_{it} \tag{2}$$

using a Poisson pseudo-maximum-likelihood estimator (Santos Silva and Tenreyro, 2006; Bergé, 2018). The estimated treatment effects are proportional to the baseline consumption of the control group, which is well-suited to describe changes in consumption patterns. The estimator reduces the influence of outliers on monthly expenditures (e.g., renting a car for a vacation). Because it is robust to large shares of zero-observations (Santos Silva and Tenreyro, 2011), it has been recommended in settings where those observations preclude log-transformations of the outcome variable (Chen and Roth, 2023; Norton, 2022).

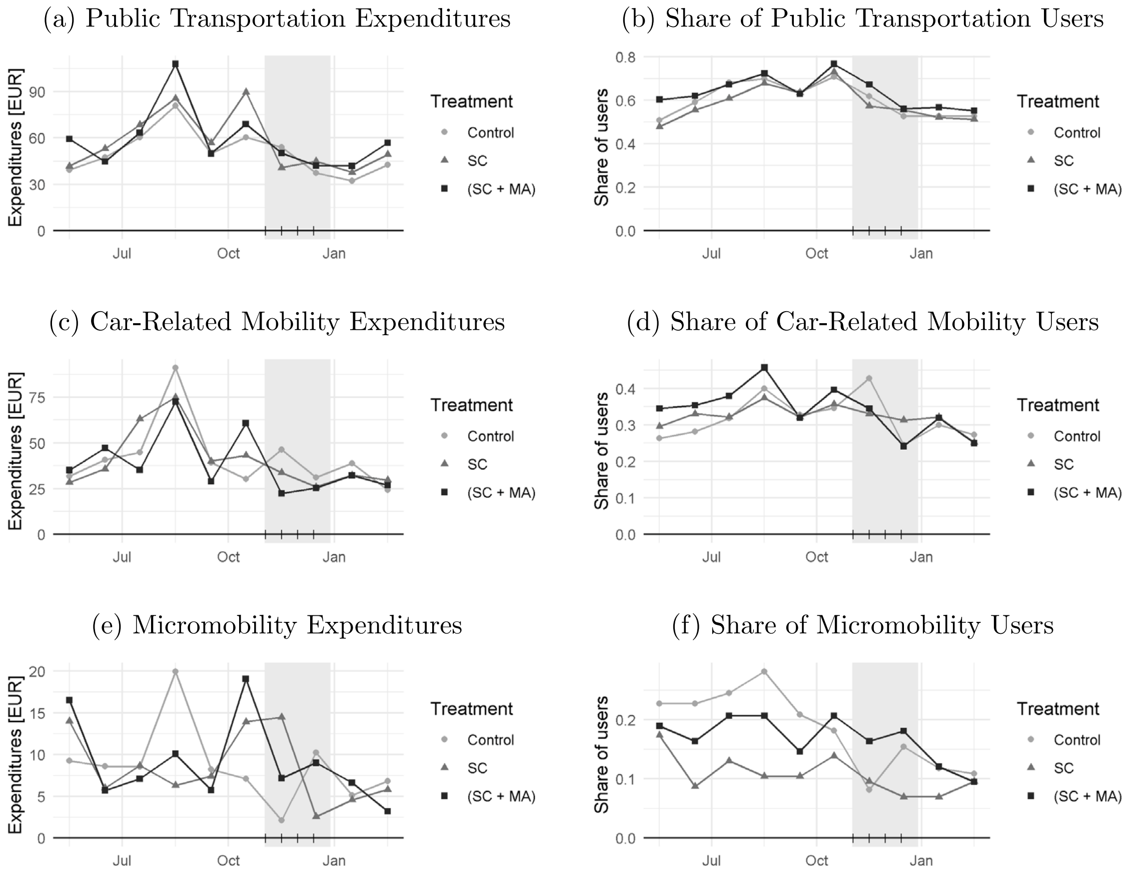


Fig. 3. Trends in average monthly mobility expenditures across treatment groups. Notes: SC abbreviates social comparison. SC + MA abbreviates strong social norm. Panels (a), (c) and (e) depict the average monthly expenditures for the transport mode in the corresponding treatment arm. Panels (b), (d) and (f) illustrate the share of participants who used the transport mode at least once in a given month as a percentage of the size of the treatment arm. The gray area indicates the treatment period. The four marks on the x-axis indicate the dates on which the e-mails were sent.

Fig. 4 summarizes the treatment effects during the treatment period from our preferred specifications in Eqs. (1) and (2). In Panel (a), we report extensive-margin responses to the interventions. The average treatment effect on the propensity to use public transport is not significantly different from zero for both treatments during the intervention. By contrast, the strong social norm treatment reduced the propensity to use car-related transport during the treatment period by almost 10%-points (significant at the 5%-level), while there is no significant effect of the social comparison alone. Simultaneously, the share of micromobility users in the strong social norm group increased by 10%-points (significant at the 1%-level). In our main specification, we also find significant effects of the social comparison treatment on the share of micromobility users (increase by 7%-points, significant at the 5%-level). However, this result is not robust to alternative econometric specifications.

Panel (b) of Fig. 4 depicts the estimated average treatment effects on expenditures by transportation mode. While the point estimates for car transportation and micromobility have the same sign as the extensive-margin estimates, the average treatment effect on expenditures is not significantly different from zero in our preferred specification for any transport mode across both treatments. We thus cannot reject the hypothesis that our treatment did not change mobility expenditures for any of the three transport modes considered.

During the two months following our treatment period, we do not observe any significant effects of our treatment on the three transport modes considered, except for a significant positive effect of the social comparison treatment on the propensity to use micromobility (increase by 7%-points, significant at the 5% level). However, this result is not robust to alternative specifications (Appendix Tables E.2 and E.3 report all treatment effects estimated for the intervention and post-intervention period).

Because the expenditure data are highly volatile and, by construction, sensitive to outliers in travel activity such as a one-time car rental for a long vacation, we analyze the number of booked expenditure items per month as an alternative measure of intensive-margin responses. The results, reported in Appendix Table E.6, show no significant effects of either intervention on public transit use during or after the treatment period. For car-related expenditure item counts, we estimate significant (at the 5%-level) and large negative effects in the treatment period for *both* treatments (and not only for the strong social norm treatment as before). No significant effects are detected in the post-treatment period. Reductions in the number of booked car-related mobility items were

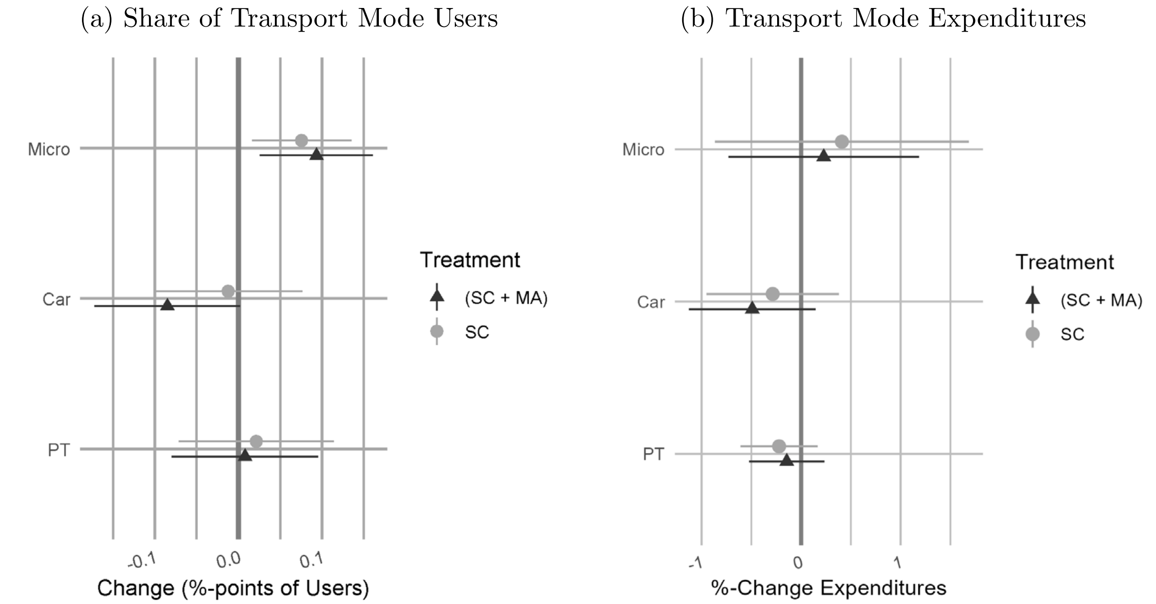


Fig. 4. Average treatment effects on monthly use: Main results. Notes: SC abbreviates social comparison. SC + MA abbreviates strong social norm. Panel (a) depicts the average treatment effects from column (1) in Appendix Table E.2. Panel (b) depicts the average treatment effects from column (1) in Appendix Table E.3. PT is short for public transportation, Micro is short for micromobility, Car is short for car transportation. Coefficient estimates and 95% confidence intervals are displayed. Standard errors are clustered by participant.

larger for the strong social norm treatment than for the social comparison treatment alone, amounting to up to 63% of the control group average, or a reduction by 0.6 expenditure items per month. For micromobility, we estimate significant (1%-level) positive effects for the strong social norm treatment. Coefficients for the social comparison treatment and for the post-treatment period are again insignificant. The estimated increase of 93% amounts to 0.6 additional expenditure items for micromobility. These effects are substantially larger than the corresponding coefficients observed for transportation expenditures.

Further robustness checks. We investigate the robustness of our main results to changing the composition of the analysis sample, modifying the econometric model, and to including expenditures made outside Germany.

Table E.2 in the appendix documents that the effect of the strong social norm treatment on the use indicator is robust across specifications, with similar point estimates and statistical significance at the 10%-level or better. Only when all employee-level controls are dropped in model (3) do the treatment effects lose statistical significance. Restricting the sample to potentially more attentive participants (those who took part in at least one post-treatment survey) does not yield a larger treatment effect.

In the social comparison treatment, a statistically significant coefficient on micromobility use is not robust to alternative controls for pre-treatment differences between employees. As Table 1 and Figure E.1 show, there are meaningful differences in the propensity to use micromobility between the social comparison group and the control group. We thus refrain from presenting this finding as a main result.

With respect to the expenditure outcomes, Appendix Table E.3 shows that the estimated coefficients remain insignificant (at the 5%-level) across all specifications both during and after the treatment period.

As a further robustness check, we re-estimate the above regressions using data on the full observation period, including the months of April 2021 and March 2022, which were omitted in the main analysis. The magnitude and significance of the treatment effects is robust to the inclusion of the two months, as we report in Appendix Tables E.4 and E.5.

Our preferred specification slightly deviates from the pre-analysis plan with respect to employee controls and sample restrictions. In an exact implementation of the pre-analysis plan, Appendix C produces regression results for all outcomes analyzed above without controlling for individual fixed effects or pre-treatment mobility on weekly, monthly and treatment-period outcomes, with a completely unrestricted sample. The key difference is that controlling for cross-sectional heterogeneity in the regressions – be it via employee fixed effects or controls for pre-treatment mobility – improves the precision of the estimated treatment effects. This is the main reason why we find no significant treatment effects in the regressions of the pre-analysis plan.

4.3. Heterogeneous treatment effects

This subsection analyzes heterogeneous treatment effects over time, for different groups of users, and for the individual transport modes. Given our previous results, we focus on the extensive margin.

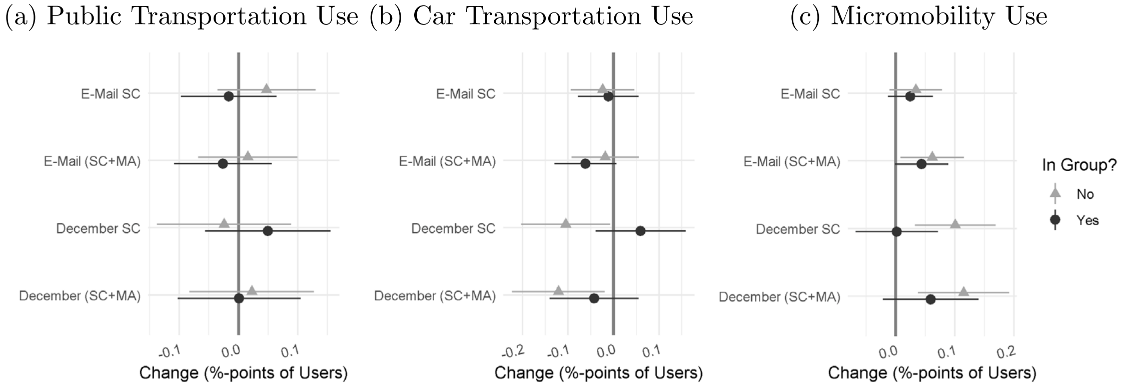


Fig. 5. Heterogeneous treatment effects over time. Notes: Treatment effects are estimated according to our main specification for the Linear Probability Model (Section 4.2). SC abbreviates social comparison. SC + MA abbreviates strong social norm. All regressions are estimated separately for the time periods of interest. E-Mail is an identifier for the use of a particular transport mode during a week in which an intervention e-mail was sent, aggregated on the monthly level. December is an identifier for the use of a particular transport mode during the month December. Coefficient estimates and 95% confidence intervals are displayed. Standard errors are clustered by participant. See Appendix Table E.7 for complete results.

Temporal heterogeneity. To examine the time dimension, we estimated treatment effects separately for (i) weeks in which the e-mails were sent vs. weeks in which no e-mails were sent, and (ii) the first vs. the second month of treatment (December). The results are shown in Fig. 5. Point estimates for weeks with an email are similar to those in weeks without emails. The lack of statistically significant differences for any of the three transport categories rules out a pattern of "action and backsliding" observed in other contexts (Allcott and Rogers, 2014).

In contrast, meaningful differences arise between the first and second treatment month for car-related transport (Panel (b)) and micromobility (Panel (c)). The treatment response occurs mainly during the first month of treatment. While the response goes in the same direction for both treatment arms, it is stronger and more persistent in the strong social norm treatment, resulting in a larger average treatment effect than in the social comparison treatment.

Heterogeneity across employees. We investigate the heterogeneity of treatment effects between (i) participants receiving a "strong" vs. a "weak" social comparison and (ii) participants working at urban vs. rural business sites. With respect to (i), we construct a measure of treatment intensity based on our treatment messages. We refer to a "weak" social comparison as receiving the information that a participant's expenditure share for public transport is higher than the average in her peer group. A "weak treatment" is defined as receiving a weak social comparison in at least three out of four treatment messages. Conversely, a "strong treatment" is defined as receiving a weak comparison at most once. For this distinction to work, we dropped 13 subjects receiving two weak comparisons.[9]

Fig. 6 displays estimated treatment effects and 95% confidence intervals for the different groups. For public transit use, depicted in Panel (a), we find that participants receiving a strong treatment increase usage by 15%-points whereas those receiving a weak treatment reduce usage by 14%-points. This behavior is consistent with the so-called "boomerang effect" of social comparisons documented in previous research (Schultz et al., 2007). Adding the moral appeal in the strong social norm treatment mitigates the boomerang effect.

For car-related transport, depicted in Panel (b), the strong social norm treatment induces a significant reduction in usage by 13%-points among strongly treated subjects, but none in the weak treatment group. Similar for micromobility, where the significant increases among strongly treated subjects seem to be driving the average treatment effects in the main results (in Panel (c)).

Perhaps surprisingly, we do not find meaningful differences between participants working at urban vs. rural business locations. One explanation for this is that access to public transportation varies more with the participants' place of *residence* than with their place of *work*. Unfortunately, we do not observe the place of residence in our data.

Heterogeneity between transport modes. Fig. 7 depicts average treatment effects for each of the transport modes included in the broader categories analyzed so far. The treatments have no significant impact on neither local nor long-distance public transport (Panel (a)). For car-related transport (Panel (b)), the strong social norm treatment has weakly significant monthly treatment effects (at the 10%-level) for the category "Taxi", which contains taxi rides, UBER rides and the use of other ride-hailing and ride-sharing services. The effect has the same sign and roughly the same magnitude as the effect for total car-related transport use (the strong social norm treatment reduced the probability of using taxis by 12%-points).

Analogous to the reduction in the propensity to use taxi rides, we find a weakly significant (at the 10%-level) increase by 9%-points in the propensity to use e-scooters in the strong social norm group.

[9] In the first treatment message, expenditures for long-distance trains had erroneously been omitted in the calculated expenditure share for public transportation. This mistake was corrected from the second e-mail onward. We should thus expect that some participants change their treatment intensity once, which 96 out of 341 participants did.

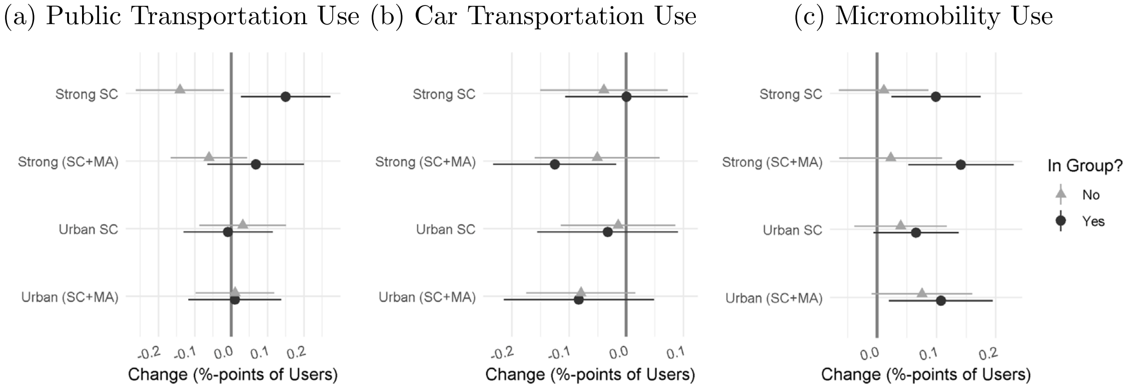


Fig. 6. Treatment effect heterogeneity across sub-groups. Notes: Treatment effects are estimated according to our main specification for the Linear Probability Model (Section 4.2). SC abbreviates social comparison. SC + MA abbreviates strong social norm. All regressions are estimated separately for the sub-groups of interest. Strong is an identifier for participants who received 3 or more e-mails with a social comparison stating that they had spent a smaller share of their expenditures on public transportation than their peer group. Urban is an identifier for participants working at a business location in an urban area. Coefficient estimates and 95% confidence intervals are displayed. Standard errors are clustered by participant. See Appendix Table E.8 for complete results.

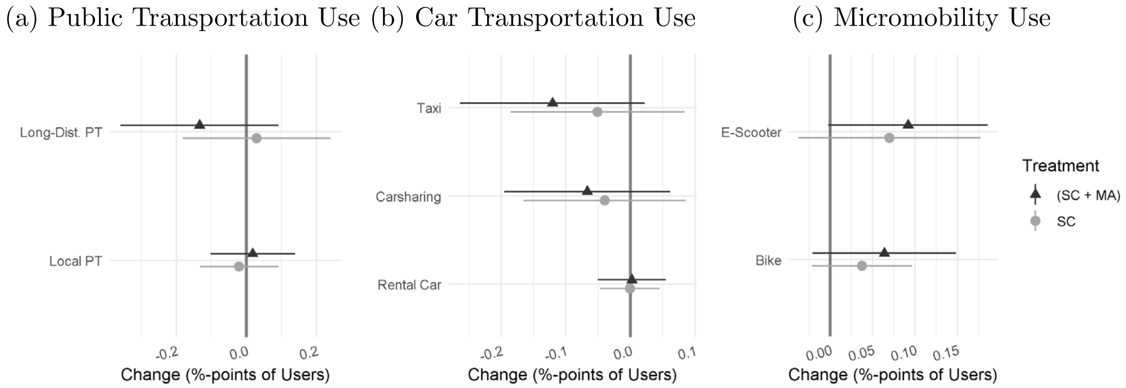


Fig. 7. Treatment effects for transport mode sub-categories. Notes: Treatment effects are estimated according to our main specification for the Linear Probability Model (Section 4.2). SC abbreviates social comparison. SC + MA abbreviates strong social norm. All regressions are estimated separately for the transport modes of interest. Local PT is an identifier for monthly use of local public transport. Long-Dist. PT is an identifier for monthly use of long-distance public transport. Taxi is an identifier for monthly use of taxis, shuttle pooling, ride-sharing or ride-hailing services. Carsharing is an identifier for monthly use of carsharing services. Rental Car is an identifier for monthly use of rental cars. Bike is an identifier for monthly use of bike sharing and subscription services, as well as bike repairs. E-scooter is an identifier for monthly use of e-scooter sharing services. Coefficient estimates and 95% confidence intervals are displayed. Standard errors are clustered by participant. See Appendix Table E.9 for complete results.

4.4. Summary and interpretation of results

Our analysis has yielded the main results that (i) none of the treatments affected the use of public transportation, and (ii) the combined treatment of social norm and moral appeal shifted users from car-related travel towards micromobility.

Several explanations for the lack of an effect on public transport use come to mind. On one hand, frequent users of public transport would not have much scope to further increase public transport use. On the other hand, infrequent users might have been deterred from using public transport more frequently due to the onset of a new wave of COVID-19 infections occurring in late October 2021, shortly before the beginning of the experiment. An explanation supported by our heterogeneity analysis would be that the boomerang effect of the social comparison treatment drives both frequent and infrequent users of public transport to revert to average usage. In contrast, the notion that public transport is simply not a good enough substitute for more emission-intensive modes seems to be refuted by evidence from our final survey, in which more than 80% of participants indicated that they do consider public transportation as a substitute for car-related transportation (Appendix D, Q4.1–Q4.3). This casts a spotlight on the discrepancy between stated and revealed preferences for public transport.

Regarding car-related travel, we find robust evidence that combining a moral appeal with a social comparison reduced the usage propensity by 10%-points compared to the control group average. Moreover, the share of individuals using micromobility increased by 10%-points, suggesting that micromobility is a reasonably good substitute for some car-related trips. This explanation is corroborated by results from our heterogeneity analysis, showing that the reduction in the share of car-transport users is driven by fewer taxi and UBER rides. The typical taxi ride might have closer (and more) substitutes than, e.g., a rental car. Especially for within-city mobility, e-scooters, the mode driving the increase in the share of micromobility users, may be a viable substitute for some taxi rides. In support of that, 52% of participants in our last survey indicated that they consider micromobility as a substitute for taxi rides (Appendix D, Q4.1). Competing explanations for the reduction in car-related transport use would be a reduction in overall trips or substitution to outside options such as a private car or a private bike. Both explanations would imply a reduction in the total use of the mobility budget, but this is not supported by our data. Appendix Table E.10 shows that the propensity to make use of the budget was not affected by any of the treatments.

After normalizing by the standard deviation, the estimated effects correspond to $d = 0.21$ and $d = 0.30$ for car-related transport and micromobility, respectively, which can be compared to estimates from the few previous studies on social comparisons in the transportation domain (Kristal and Whillans, 2020; Gravert and Collentine, 2021; Götz et al., 2023). Those estimates were not significantly different from zero and would reject effect sizes such as ours with 95% confidence. Notwithstanding this, we argue that the larger effect sizes are credible in our setting for three reasons. First, the larger effect we find is derived from an intervention combining a social comparison with a moral appeal.[10] In consonance with previous work, we find that the social comparison alone is not sufficient to change transportation behavior. That adding a moral appeal can generate much larger effects than a social comparison in isolation has previously been shown in the context of water conservation (Ferraro and Price, 2013). Second, large effects are plausible because transportation choices in the mobility budget can be very flexibly adjusted in response to treatment. Lock-in effects that could arise when buying a car or an annual public transportation ticket are less relevant for participants of the mobility budget. Third, the strong social norm treatment is effective in our setting as it links subjects' transportation behavior back to a pre-existing injunctive norm shared by large parts of the sample. In the baseline survey, 44% of respondents expressed the belief that their social environment (e.g., colleagues) expects them to act environmentally friendly (Appendix D, Q1.2).

Our experimental design does not allow us to assess the moral appeal separately from the social comparison. Based on the heterogeneity analysis, we conjecture that the social comparison moderates the effect of the moral appeal in our setting. For both car-related transport and micromobility, the effect of the strong social norm treatment is driven by participants who are informed that their peer group spends a larger share of their budget on public transport. This is in line with expectations, since this group is informed that their peer group behaves more environmentally friendly on average and should thus perceive a stronger moral obligation to change their transportation behavior.

The effects observed during the treatment period disappear in the post-treatment period. This result confirms findings obtained in similar experimental settings with messages delivered through emails or apps, in which little or no habit formation was observed (see, e.g., Calzolari and Nardotto, 2017), or in which the effects fade over time due to a lack of engagement on the side of the participants (see, e.g., Fosgaard et al., 2021; Enlund et al., 2023). In addition, our main results are driven by reactions during the first month of the treatment period. This further supports the notion that participants habituated to our e-mail messages, making the effects of our treatment rather short-lived.

5. Potential channels underlying the results

This section presents evidence from two endline surveys (April 2022, n = 236; January 2023, n = 200) speaking to the mechanisms driving our results. All users of the mobility budget were invited to participate in both surveys, but participation was voluntary.

5.1. Stated reactions to our treatment messages

In the first endline survey, we asked 50% of survey participants how they would change their *public transportation* use in response to a *weak social comparison* message, i.e., the communicated share of individual public transport expenditures was already above the communicated peer group average. The remaining 50% of the survey sample were asked about their reactions to a *weak social comparison* message combined with a *moral appeal* (Appendix D, Q3.3). As shown in Fig. 8(a), very few participants stated that they would reduce their public transport use; 10% indicated that they would increase their public transport use in response to the social comparison treatment, and 33% said so for the strong social norm treatment. This is in line with the experimental finding that the combined treatment is more powerful in changing people's behavior, and it demonstrates that most respondents are not willing or able to change public transport use even under the combined treatment. Across treatments, 25% of survey participants stated that they would increase their use of public transport, yet this intention is not borne out in any significant treatment effect on public transport use in the field experiment (Tables E.2 and E.3 in the appendix). This evidence is consistent with an intention-behavior gap.

In the second survey, we asked all participants how they would change their *car-related travel* in response to a *strong social comparison* message combined with a *moral appeal* (Appendix D, Q4.4). As shown in Fig. 8(b), this strong treatment message did not change the intended use of car-related transport for 75% of survey participants, while 18% indicated that they would decrease and 7% indicated that they would increase their use of car-related transport. Interestingly, compared to public transport use, an even higher share of respondents stated that they would not change their behavior. The experimental treatment effect on car-related transport use could thus be driven by the reactions of relatively few participants.

[10] The mobile application used in the study by Götz et al. (2023) does this in an incomplete way. While a moral appeal featured prominently on the page shown when opening the app, participants could additionally select into gamification features, including a social comparison.

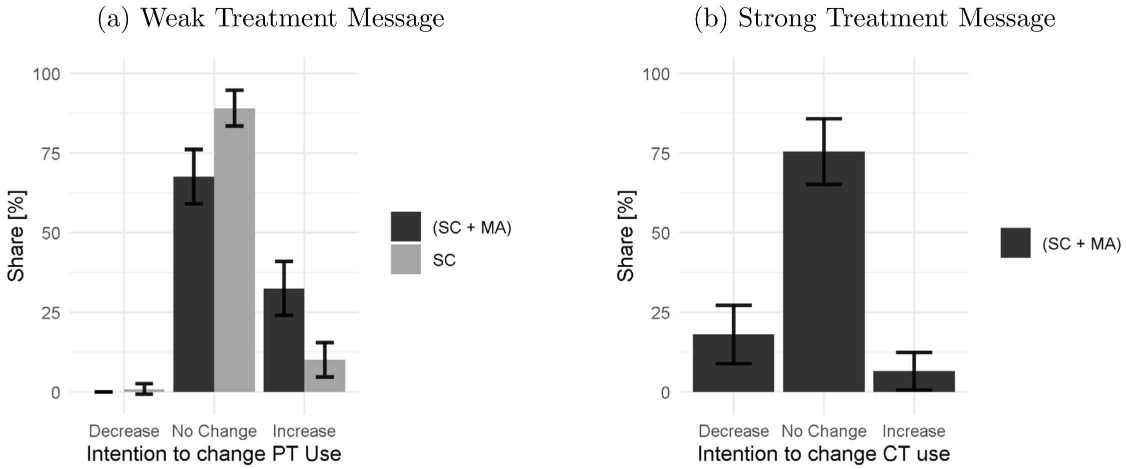


Fig. 8. Stated reaction to the treatment messages. Notes: Stated reactions to our treatment messages in terms of public transport use in a survey in April 2022 and in terms of car-related transport use in a survey in January 2023. SC abbreviates social comparison. SC + MA abbreviates strong social norm. PT abbreviates public transport. CT abbreviates car-related transport. In the 2022 survey, we assessed reactions to a weak social comparison treatment, informing participants that they already outperform their peers, in one half of the sample (randomly assigned) and to a strong social norm treatment with a weak social comparison in the other half. The exact wording of the question asking for stated reactions to the weak social comparison messages can be found in Appendix D, Q3.3. In the 2023 survey, we assigned the strong social norm treatment to all participants. The exact wording of the question asking for stated reactions to the strong social comparison message can be found in Appendix D, Q4.4. 95% confidence intervals of the means are displayed.

5.2. Channels

In the endline survey in April 2022 (January 2023), we asked participants who stated that they would *increase* their use of public transport (*decrease* their use of car-related transport) to indicate their agreement with a number of potential reasons for their stated reaction. We interpret agreement with these reasons as suggestive evidence for the channels behind the indicated reactions (and, ultimately, also the observed reactions). Fig. 9 summarizes the degree to which subjects agree with the various reasons. In what follows, we discuss the different channels in detail.

Social comparison treatment. The social comparison treatment could have influenced behavior through several channels.

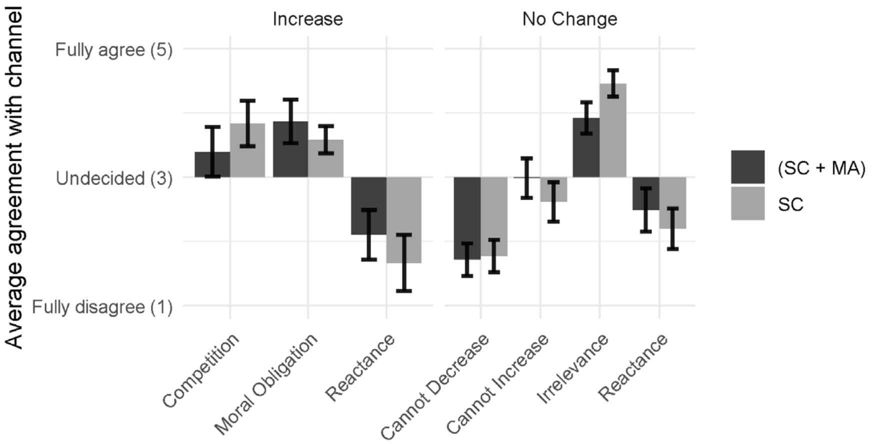
First, participants might think that the behavior of their co-workers conveys information about optimal behavior. Co-workers have similar qualifications and interests as the participants themselves and might thus be a relevant peer group for **social learning** (we highlight the relevant channels in bold font to facilitate navigation between text and figures). Fig. 9(b) shows that, on average, participants were undecided whether social learning played a role for their decision to reduce car-related transport use. Social learning is not plausible as a channel behind the observed reactions to the weak treatment in Panel (a), as it would imply a *reduction* in the use of public transport. Almost no participant indicated to reduce public transport use (Fig. 8(a)).

Second, the social comparison could have worked because people care about their status and consumption relative to others (e.g., Solnick and Hemenway, 2005), which we subsume under **competition**. If this holds true in our setting, the intervention encouraged public transport use (Fig. 9(a)) and thus discouraged car-related transport use (Fig. 9(b)) through the social comparison. Participants receiving only the social comparison agreed that competition with their peers was a motivating factor to increase the use of public transport (see Panel (a)). However, this is no longer the case once we add the moral appeal (see Panels (a) and (b)).

Third, participants in both treatment groups might have perceived the social comparison as a threat to their behavioral freedom, especially when adding the moral appeal. Participants who felt this way might also feel compelled to vindicate their freedom by doing the opposite of what the treatment message had asked them to do. This motivational state is called psychological **reactance** (Van den Bos, 2007). However, for both treatments, participants on average disagreed that reactance influenced their decision, as shown in Panels (a) and (b) of Fig. 9.

Strong social norm treatment. The moral appeal could have been reinforced by impure altruism (or "warm-glow" utility, see, e.g., Andreoni, 1990). This concept refers to a mixture of both altruistic and egoistic desires to help others (in this context, to avert damages from climate change). Both the social norm and impure altruism would be perceived as a **moral obligation** to use low-emissions transport modes. Participants who stated that they would increase public transport use (decrease car-related transport use) on average agreed that they felt a moral obligation to do so, as is shown in Fig. 9. However, we believe that participants would not be truthfully reporting compliance with a social norm in the corporate setting we study. Thus, we will try to disentangle social norms, impure altruism and attitudes towards the environment using a more indirect approach described in Section 5.3 below.

(a) Reasons for Intention to Change Public Transport Use, Weak Treatment



(b) Reasons for Intention to Change Car-related Transport Use, Strong Treatment

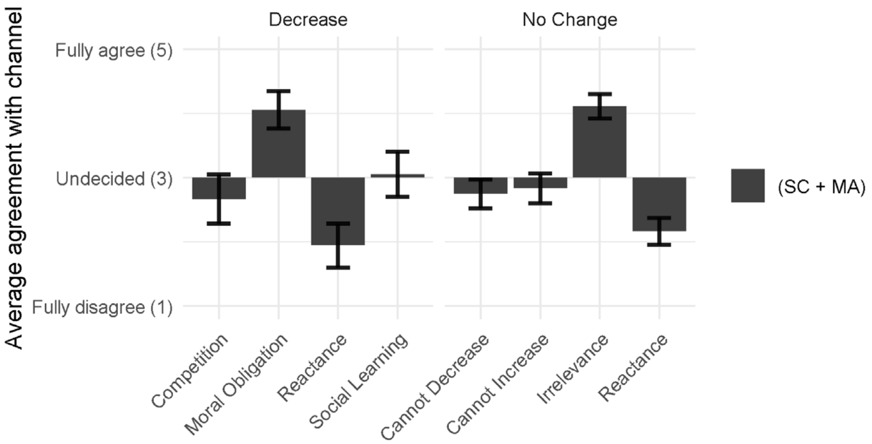


Fig. 9. Reasons for the stated reactions to the treatment messages. Notes: Agreement with different reasons for the indicated reaction to the treatment messages social comparison and strong social norm in surveys in April 2022 and January 2023. SC abbreviates social comparison. SC + MA abbreviates strong social norm. We split the sample according to the indicated change in the use of public transport in 2022 and the indicated change in the use of car-related transport in 2023. The exact wording of the questions can be found in Appendix D, Q3.3 and Q4.4. 95% confidence intervals of the means are displayed. To those increasing their use of public transport in 2022 (respectively decreasing their use of car-related transport in 2023), the following reasons were provided: *"(i) I feel offended by the message. I will use public transportation (the transport modes mentioned above) as much as I please. (ii) I feel morally obliged to use public transportation more (the transport modes mentioned above less) often. (iii) The fact that I am using public transportation more than my colleagues motivates me to use this transport mode even more (My colleagues are ahead of me, so I should use the transport modes mentioned above less often)"*. We label the first reason **Reactance**, the second **Moral Obligation**, and the third **Competition**. For the strong treatment message used in the 2023 survey, **Social Learning** was added as another hypothesized channel: *"(iv) Usually, my colleagues make good decisions. If they use public transportation more often than I do, I should reconsider my transportation behavior"*. To those not changing their use of public transport in 2022 (respectively not changing their use of car-related transport in 2023), the following reasons were provided: *"(i) I feel offended by the message. I will use public transportation (the transport modes mentioned above) as much as I please. (ii) The message is irrelevant for my transport mode choice. (iii) Although I would like to increase my use of public transportation (the transport modes mentioned above), it is impossible for me. (iv) Although I would like to decrease my use of public transportation (the transport modes mentioned above), it is impossible for me"*. We label the first reason **Reactance** (as before), the second **Irrelevance**, the third **Cannot Increase**, and the fourth **Cannot Decrease**.

Additional channels for compliance with the norm. In addition to the channels described above, our treatment messages could have influenced behavior as follows.

First, the treatment messages could have contained new information, at least to some participants, on their share of public transport expenditures, inducing them to re-optimize their transport mode choice. We expected that this channel would be irrelevant

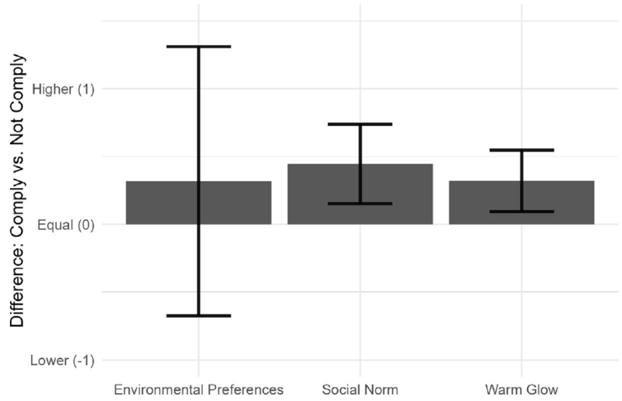


Fig. 10. Differences between individuals complying vs. not complying with the social norm. Notes: On the y-axis, pre-existing differences in preferences between individuals who indicated that they would either increase their use of public transport or decrease their use of car-related transport for moral reasons in response to the strong social norm treatment message are displayed. The exact wording of the underlying questions can be found in Appendix D, Q3.3 and Q4.4. On the x-axis, differences are displayed for (1) environmental preferences, (2) the perception of a social norm for environmentally-friendly behavior in Appendix D, Q2.1, item 3, (3) the perception of a "warm glow" from acting environmentally-friendly in Q2.1, item 4. (1) displays the difference in average pro-environmental preferences, as measured by 6 questions from the New Ecological Paradigm (NEP) scale. Participants indicated their agreement on a 5-point Likert scale. The scores from these questions are summed up to create the NEP scale. Our reduced scale ranges from 6 (for those providing the least environmentally-friendly answer in all questions) to 30. The exact wording of the questions on the NEP scale can be found in Appendix D, Q2.2.

in our experiment, as participants could access information about their expenditures for different transport modes at any time online or in a mobile application. However, a non-negligible share of 38% of respondents in our endline survey indicated that they would not have known their own public transport expenditures without receiving the information provided in the intervention e-mails (Appendix D, Q3.7).

Second, the wording of the moral appeal treatment could have provided new information about the relative emissions intensity of public vs. car-related transportation, or about climate change and the urgent need for action against it. Our surveys provide some evidence that this was not the case; 95% of respondents in our baseline survey stated that they believe that climate change is already happening, and 94% correctly ranked cars and public transportation in terms of their CO_2 emissions (Appendix D, Q1.3 and Q1.1, respectively).

Third, as the messages were sent by the participants' employer, messenger effects (Dolan et al., 2012) could also have played a role: Had the messages been sent by another source, participants might have reacted differently. However, in the endline survey, 93% of respondents stated that the fact that the messages were sent by their employer did not alter their reactions (Appendix D, Q3.6).

Channels explaining a nil effect. Participants who had indicated that they would *not* change their use of public transport (or car-related transport) were asked whether they agreed with any of the following reasons for that reaction: **reactance**, an **inability to change the use of public transport in either direction**, and **irrelevance** of the treatment message for participants' mode choice. Fig. 9 shows that the average participant agreed that the message was irrelevant for their transport mode choice, and that reactance would not influence their choice. Respondents were, on average, also undecided about whether they could increase their use of public transport (Panel (a)) or decrease their use of car-related transport (Panel (c)) in response to the strong social norm treatment. This confirms that, at least for some participants, there is limited scope to change their current travel behavior.

5.3. Disentangling social norms, warm glow and environmental preferences

To disentangle the different channels that could lead to a perceived moral obligation for environmentally-friendly transportation behavior, we test whether participants who stated to change their transportation behavior due to a perceived moral obligation agreed more strongly with (i) having pro-environmental preferences, (ii) perceiving a social norm for environmentally-friendly behavior, and (iii) perceiving a "warm glow" from acting environmentally-friendly.[11] Fig. 10 shows that participants who felt morally obliged to change their transportation behavior on average agreed more with perceiving a **social norm** for environmentally-friendly behavior. Furthermore, participants on average agreed more to feeling a **warm glow** from acting environmentally-friendly. The difference in terms of **pro-environmental preferences**, as measured by a subset of six questions from the New Ecological Paradigm Scale (Dunlap et al., 2000), is not significant.

[11] Note that the questions about preferences were asked in our midline survey, before eliciting reactions to our treatment messages in the endline surveys.

6. Conclusion

We find that a social comparison alone does not lead to a significant change in travel behavior within the mobility budget scheme. This stands in stark contrast to results obtained by studies in areas other than transportation, where social comparisons did have persistent effects. Explanations for why social comparisons failed to achieve the desired effects in our setting include (i) boomerang effects (counteracting effects for different parts of the sample), (ii) disregard for the transportation choices of one's colleagues, (iii) strong habits that are difficult to change, and (iv) lack of appropriate adjustment margins in our specific setting.

When combining the social comparison with a moral appeal, we do find significantly altered mobility behavior. This treatment decreased the propensity to use car-related travel by 10%-points, increased the propensity to use micromobility by the same share, but left the propensity to use public transport unchanged. Achieving a similar reduction of car-related trips among private car owners would require a fuel price increase of around 25%, assuming a relatively high short-run price elasticity of 0.43 (Litman, 2022). While the mobility budget differs in important ways from private car ownership, this comparison suggests that behavioral interventions can provide relatively strong leverage for reducing car-related travel.

We note slight deviations from the pre-analysis plan. Most importantly, our regression analysis controls for cross-sectional heterogeneity by including either employee fixed effects or pre-treatment mobility choices. This notably increases the precision of the estimated treatment effects in our relatively small sample. When conducting the analysis exactly as specified in the pre-analysis plan, the estimated treatment effects are not statistically significant at conventional levels.

The result that subjects did not substitute towards public transit is striking. From a policy perspective, it is important to know whether this generalizes to other settings. We believe that this is not necessarily the case because, in our field experiment, several factors coincided which were conducive to finding a null-result. First, the onset of a new wave of the COVID-19 pandemic during the treatment period likely deterred some subjects from using public transit to minimize infection risks. Second, employees were not randomly assigned to the mobility budget. We conjecture that participants in the mobility budget likely used public transport more frequently than non-participants, already before the intervention. This kind of self-selection may have left little scope for further intensifying public transit use. Third, for some participants public transport options may have been either unavailable or poor substitutes for car-related mobility.

That a moral appeal combined with a social comparison can drive mobility choices away from car-based transportation is highly policy relevant due to its impact on CO_2 emissions. The upside of this is that participants pondered their mobility options and some decided to contribute to reducing emissions by forgoing car-based trips. This result is in line with a recent meta-analysis on the effectiveness of behavioral interventions at reducing car use (Semenescu et al., 2020), which finds that interventions targeting social, cultural and moral norms are the most effective. The downside is that those changes were short-lived. The estimated treatment effects were driven by reactions in the first treatment month. To achieve longer-lasting effects, the strong nudge would have to be complemented with incentives for a permanent switch to climate-friendly modes of transport. For example, as shown by Brandon et al. (2022), the long-term effectiveness of nudges in the domain of residential energy consumption is mainly driven by the adoption of long-lived energy-efficient technology. Such "commitment technologies" could be easily enforced in settings like the one studied in this paper, e.g., by reimbursing only expenditures for climate-friendly transport modes. However, such features were deliberately excluded in this experiment.

Evidence from our endline surveys points to an intention-behavior gap, as is common for many issues related to the environment and climate change. It further suggests that, while the average participant considered the transportation behavior of their peers and moral appeals to combat climate change as irrelevant for their transport mode choice, some participants did feel morally obliged to comply with the social norm communicated by the moral appeal and the social comparison. Thus, social-norm-based interventions in the transportation domain may only be able to induce behavioral change among a minority.

Our partner company decided not to continue the norm-based appeals after the experimental period. Given the lack of persistent treatment effects, we would not advise companies aiming at inducing long-term behavioral change among users of corporate mobility benefits to work with norm-based appeals. However, in settings where a short-lived treatment effect is sufficient, norm-based appeals could be a promising intervention, e.g., to induce the uptake of new corporate mobility benefits. A promising approach to reducing emissions would be for companies to more actively induce their employees to surrender their company car and opt into a mobility budget instead. Previous research found several nudges to be ineffective in inducing the uptake of less emission-intensive transport technologies (Allcott and Kessler, 2019; Kristal and Whillans, 2020). Searching for successful behavioral interventions in this domain remains an interesting avenue for future research.

CRediT authorship contribution statement

Johannes Gessner: Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Wolfgang Habla:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Investigation, Funding acquisition, Conceptualization. **Ulrich J. Wagner:** Writing – review & editing, Supervision, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

We are grateful to the cooperating company for assisting in this research endeavor. We also thank three anonymous referees, Anomitro Chatterjee, Lorenz Götte, Kathrine von Graevenitz, Beat Hintermann, Martin Kesternich, and Madeline Werthschulte, as well as participants at the 27th Annual Conference of the European Association of Environmental and Resource Economists, the Mannheim Conference on Energy and the Environment 2022, the 8th CRC TR 224 Workshop for Young Researchers and at various seminars at the universities of Mannheim and Heidelberg for valuable comments and suggestions. Support by the German Research Foundation (DFG) through CRC TR 224 (Project B07) and MISTRA Carbon Exit is gratefully acknowledged. Wagner received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (Grant agreement No. 865181).

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.trd.2024.104289.

References

Allcott, Hunt, Kessler, Judd B., 2019. The welfare effects of nudges: A case study of energy use social comparisons. Am. Econ. J.: Appl. Econ. 11 (1), 236–276, URL https://www.aeaweb.org/articles?id=10.1257/app.20170328.

Allcott, Hunt, Rogers, Todd, 2014. The short-run and long-run effects of behavioral interventions: Experimental evidence from energy conservation. Amer. Econ. Rev. 104 (10), 3003–3037, URL https://www.aeaweb.org/articles?id=10.1257/aer.104.10.3003.

Andor, Mark A., Gerster, Andreas, Gillingham, Kenneth T., Horvath, Marco, 2020a. Running a car costs much more than people think — Stalling the uptake of green travel. Nature 580, 1–27, URL https://doi.org/10.1038/d41586-020-01118-w.

Andor, Mark A., Gerster, Andreas, Peters, Jörg, Schmidt, Christoph M., 2020b. Social norms and energy conservation beyond the US. J. Environ. Econ. Manage. 103, 102351, URL https://www.sciencedirect.com/science/article/pii/S0095069620300747.

Andreoni, James, 1990. Impure altruism and donations to public goods: A theory of warm-glow giving. Econ. J. 100 (401), 464–477, URL http://www.jstor.org/stable/2234133.

Arval Mobility Observatory, 2023. Mobility and Fleet Barometer 2023. Technical Report, Arval, Arval Mobility Observatory, Paris.

Avineri, Erel, 2012. On the use and potential of behavioural economics from the perspective of transport and climate change. J. Transp. Geogr. 24, 512–521, URL https://www.sciencedirect.com/science/article/pii/S0966692312000646.

Avineri, Erel, Waygood, E.O.D., 2013. Applying valence framing to enhance the effect of information on transport-related carbon dioxide emissions. Transp. Res. A: Policy Pract. 48, 31–38, URL https://www.sciencedirect.com/science/article/pii/S0965856412001425, Psychology of Sustainable Travel Behavior.

Bao, Helen X.H., Lim, Yi, 2022. Behavioural interventions for micro-mobility adoption: Low-hanging fruits or hard nuts to crack? Transp. Res. F: Traffic Psychol. Behav. 84, 423–441, URL https://www.sciencedirect.com/science/article/pii/S1369847821002898.

Bergé, Laurent, 2018. Efficient Estimation of Maximum Likelihood Models with Multiple Fixed-Effects: The R Package FENmlm. CREA Discussion Papers, Vol. 13, URL https://EconPapers.repec.org/RePEc:luc:wpaper:18-13.

Bicchieri, Cristina, 2005. The Grammar of Society: The Nature and Dynamics of Social Norms. Cambridge University Press, Cambridge, URL https://www.cambridge.org/core/books/grammar-of-society/2B063E9C9621C2340DEFB2BE15B3AEA5.

Bott, Kristina M., Cappelen, Alexander W., Sørensen, Erik Ø., Tungodden, Bertil, 2020. You've got mail: A randomized field experiment on tax evasion. Manage. Sci. 66 (7), 2801–2819, arXiv:https://doi.org/10.1287/mnsc.2019.3390.

Brandon, Alec, Ferraro, Paul, List, John A., Metcalfe, Robert, Price, Michael, Rundhammer, Florian, 2022. Do the Effects of Nudges Persist? Theory and Evidence from 38 Natural Field Experiments. NBER Working Paper w23277, URL https://www.nber.org/papers/w23277.

Brick, Kerri, De Martino, Samantha, Visser, Martine, 2023. Behavioural nudges for water conservation in unequal settings: Experimental evidence from cape town. J. Environ. Econ. Manage. 121, 102852, URL https://www.sciencedirect.com/science/article/pii/S0095069623000700.

Bursztyn, Leonardo, Fiorin, Stefano, Gottlieb, Daniel, Kanz, Martin, 2019. Moral incentives in credit card debt repayment: Evidence from a field experiment. J. Polit. Econ. 127 (4), 1641–1683, URL https://doi.org/10.1086/701605.

Calzolari, Giacomo, Nardotto, Mattia, 2017. Effective reminders. Manage. Sci. 63 (9), 2915–2932, arXiv:https://doi.org/10.1287/mnsc.2016.2499.

Carlsson, Fredrik, Gravert, Christina, Johansson-Stenman, Olof, Kurz, Verena, 2021. The use of green nudges as an environmental policy instrument. Rev. Environ. Econ. Policy 15 (2), 216–237, URL https://doi.org/10.1086/715524.

Castleman, Benjamin L., Page, Lindsay C., 2015. Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? J. Econ. Behav. Organ. 115, 144–160, URL https://doi.org/10.1016/j.jebo.2014.12.008.

Cellina, Francesca, Bucher, Dominik, Mangili, Francesca, Veiga Simão, José, Rudel, Roman, Raubal, Martin, 2019. A large-scale, app-based behaviour change experiment persuading sustainable mobility patterns: Methods, results and lessons learnt. Sustainability 11 (9), URL https://www.mdpi.com/2071-1050/11/9/2674.

Chen, Jiafeng, Roth, Jonathan, 2023. Log-like? Identified ATEs defined with zero-valued outcomes are (arbitrarily) scale-dependent. arXiv preprint arXiv:2212.06080.

Cialdini, Robert B., Reno, Raymond R., Kallgren, Carl A., 1990. A focus theory of normative conduct: Recycling the concept of norms to reduce littering in public places. J. Pers. Soc. Psychol. 58 (6), 1015–1026, URL https://doi.org/10.1037/0022-3514.58.6.1015.

Copenhagen Economics, 2010. Company Car Taxation: Subsidies, Welfare and Environment. Taxation Papers 22, European Commission and Directorate-General for Taxation and Customs Union, URL https://data.europa.eu/doi/10.2778/13821.

Dolan, Paul, Hallsworth, Michael, Halpern, David, King, Dominic, Metcalfe, Robert, Vlaev, Ivo, 2012. Influencing behaviour: The mindspace way. J. Econ. Psychol. 33 (1), 264–277, URL https://www.sciencedirect.com/science/article/pii/S0167487011001668.

Dunlap, Riley E., van Liere, Kent D., Mertig, Angela G., Jones, Robert Emmet, 2000. New trends in measuring environmental attitudes: Measuring endorsement of the new ecological paradigm: A revised NEP scale. J. Soc. Issues 56 (3), 425–442, URL https://doi.org/10.1111/0022-4537.00176.

Egebark, Johan, Ekström, Mathias, 2016. Can indifference make the world greener? J. Environ. Econ. Manage. 76, 1–13, URL https://www.sciencedirect.com/science/article/pii/S009506961500090X.

Enlund, Jakob, Andersson, David, Carlsson, Fredrik, 2023. Individual carbon footprint reduction: Evidence from pro-environmental users of a carbon calculator. Environ. Resour. Econ. 86, 433–467, URL https://doi.org/10.1007/s10640-023-00800-7.

Fanghella, Valeria, D'Adda, Giovanna, Tavoni, Massimo, 2022. Evaluating the impact of technological renovation and competition on energy consumption in the workplace. J. Environ. Econ. Manage. 114, URL https://www.sciencedirect.com/science/article/pii/S0095069622000390.

Farrow, Katherine, Grolleau, Gilles, Ibanez, Lisette, 2017. Social norms and pro-environmental behavior: A review of the evidence. Ecol. Econ. 140, 1–13, URL https://www.sciencedirect.com/science/article/pii/S0921800915301543.

Ferraro, Paul J., Miranda, Juan Jose, Price, Michael K., 2011. The persistence of treatment effects with norm-based policy instruments: Evidence from a randomized environmental policy experiment. Am. Econ. Rev.: Pap. Proc. 101 (3), 318–322, URL https://www.aeaweb.org/articles?id=10.1257/aer.101.3.318.

Ferraro, Paul J., Price, Michael K., 2013. Using nonpecuniary strategies to influence behavior: Evidence from a large-scale field experiment. Rev. Econ. Stat. 95 (1), 64–73, arXiv:https://direct.mit.edu/rest/article-pdf/95/1/64/1917150/rest_a_00344.pdf.

Filippini, Massimo, Kumar, Nilkanth, Srinivasan, Suchita, 2021. Nudging adoption of electric vehicles: Evidence from an information-based intervention in nepal. Transp. Res. D 97, 102951, URL https://www.sciencedirect.com/science/article/pii/S1361920921002492.

Fosgaard, Toke Reinholt, Pizzo, Alice, Sadoff, Sally, 2021. Do People Respond to the Climate Impact of Their Behavior? The Effect of Carbon Footprint Information on Grocery Purchases. IFRO Working Paper 2021/05, University of Copenhagen, Department of Food and Resource Economics (IFRO), Copenhagen, URL http://hdl.handle.net/10419/235145.

Garcia-Sierra, Marta, van den Bergh, Jeroen C.J.M., Miralles-Guasch, Carme, 2015. Behavioural economics, travel behaviour and environmental-transport policy. Transp. Res. D 41, 288–305, URL https://doi.org/10.1016/j.trd.2015.09.023.

Gosnell, Greer K., List, John A., Metcalfe, Robert D., 2020. The impact of management practices on employee productivity: A field experiment with airline captains. J. Polit. Econ. 128 (4), 1195–1233, arXiv:https://doi.org/10.1086/705375.

Götz, Alexander, Marinica, Ioana, Mosetti, Luca, Schubert, Renate, 2023. Do mobile applications foster sustainable mobility? Evidence from a field experiment. Available at SSRN, URL http://dx.doi.org/10.2139/ssrn.4310386.

Graham-Rowe, Ella, Skippon, Stephen, Gardner, Benjamin, Abraham, Charles, 2011. Can we reduce car use and, if so, how? A review of available evidence. Transp. Res. A 45 (5), 401–418, URL https://www.sciencedirect.com/science/article/pii/S0965856411000309.

Gravert, Christina, Collentine, Linus Olsson, 2021. When nudges aren't enough: Norms, incentives and habit formation in public transport usage. J. Econ. Behav. Organ. 190, 1–14, URL https://www.sciencedirect.com/science/article/pii/S0167268121003000.

Habla, Wolfgang, Muller, Paul, 2021. Experimental evidence of limited attention at the gym. Exp. Econ. 24 (4), 1156–1184, URL https://doi.org/10.1007/s10683-020-09693-5.

Hintermann, Beat, Schoeman, Beaumont, Molloy, Joseph, Götschi, Thomas, Castro, Alberto, Tchervenkov, Christopher, Tomic, Uros, Axhausen, Kay W., 2024. Pigovian Transport Pricing in Practice. WWZ Working Paper, No. 2021/11, URL https://hdl.handle.net/10419/296574.

Hoffmann, Christin, Thommes, Kirsten, 2020. Can digital feedback increase employee performance and energy efficiency in firms? Evidence from a field experiment. J. Econ. Behav. Organ. 180 (C), 49–65, URL https://ideas.repec.org/a/eee/jeborg/v180y2020icp49-65.html.

Hudde, Ansgar, 2023. It's the mobility culture, stupid! winter conditions strongly reduce bicycle usage in german cities, but not in dutch ones. J. Transp. Geogr. 106, 103503, URL https://www.sciencedirect.com/science/article/pii/S0966692322002265.

Innocenti, Alessandro, Lattarulo, Patrizia, Pazienza, Maria Grazia, 2013. Car stickiness: Heuristics and biases in travel choice. Transp. Policy 25, 158–168, URL https://www.sciencedirect.com/science/article/pii/S0967070X12001680.

Ito, Koichiro, Ida, Takanori, Tanaka, Makoto, 2018. Moral suasion and economic incentives: Field experimental evidence from energy demand. Am. Econ. J.: Econ. Policy 10 (1), 240–267, URL https://www.aeaweb.org/articles?id=10.1257/pol.20160093.

Karlan, Dean, Morten, Melanie, Zinman, Jonathan, 2016. A personal touch in text messaging can improve microloan repayment. Behav. Sci. Policy 1 (2), 25–31, URL https://doi.org/10.1177/237946151500100204.

Kormos, Christine, Gifford, Robert, Brown, Erinn, 2014. The influence of descriptive social norm information on sustainable transportation behavior: A field experiment. Environ. Behav. 47 (5), 479–501, URL https://doi.org/10.1177/0013916513520416.

Kristal, Ariella S., Whillans, Ashley V., 2020. What we can learn from five naturalistic field experiments that failed to shift commuter behaviour. Nat. Hum. Behav. 4 (2), 169–176, URL https://doi.org/10.1038/s41562-019-0795-z.

Larcom, Shaun, Rauch, Ferdinand, Willems, Tim, 2017. The benefits of forced experimentation: Striking evidence from the London underground network. Q. J. Econ. 132 (4), 2019–2055, URL https://doi.org/10.1093/qje/qjx020.

Lattarulo, Patrizia, Masucci, Valentino, Pazienza, Maria Grazia, 2019. Resistance to change: Car use and routines. Transp. Policy 74, 63–72, URL https://doi.org/10.1016/j.tranpol.2018.11.013.

Litman, Todd, 2022. Understanding Transport Demands and Elasticities - How Prices and Other Factors Affect Travel Behavior. Victoria, BC, Canada, Victoria Transport Policy Institute, URL https://www.vtpi.org/elasticities.pdf.

Mattauch, Linus, Ridgway, Monica, Creutzig, Felix, 2016. Happy or liberal? Making sense of behavior in transport policy design. Transp. Res. D 45, 64–83, URL https://www.sciencedirect.com/science/article/pii/S136192091500111X, Special Issue on Climate Change and Transport.

Metcalfe, Robert D., Dolan, Paul, 2012. Behavioural economics and its implications for transport. J. Transp. Geogr. 24, 503–511, URL https://www.sciencedirect.com/science/article/pii/S0966692312000245.

Moody, Joanna, Zhao, Jinhua, 2019. Car pride and its bidirectional relations with car ownership: Case studies in New York City and Houston. Transp. Res. A 124 (C), 334–353, URL https://ideas.repec.org/a/eee/transa/v124y2019icp334-353.html.

Nobis, Claudia, Herget, Melanie, 2020. Mobilität in ländlichen räumen: Betrachtungen aus sicht der verkehrswende und der gleichwertigkeit von lebensverhältnissen. Int. Verkehrswesen 72 (4), 2–5, URL https://elib.dlr.de/141855/.

Nolan, Jessica M., Schultz, P. Wesley, Cialdini, Robert B., Goldstein, Noah J., Griskevicius, Vladas, 2008. Normative social influence is underdetected. Pers. Soc. Psychol. Bull. 34 (7), 913–923, URL https://doi.org/10.1177/0146167208316691.

Norton, Edward C., 2022. The inverse hyperbolic sine transformation and retransformed marginal effects. Stata J. 22 (3), 702–712, URL https://doi.org/10.1177/1536867X221124553.

Piras, Francesco, Manca, Sara, Sottile, Eleonora, Meloni, Italo, Mosca, Oriana, Tuveri, Giovanni, Fornara, Ferdinando, 2021. Can persuasive normative messages encourage sustainable transportation usage? Transp. Res. F: Traffic Psychol. Behav. 83, 304–322, URL https://www.sciencedirect.com/science/article/pii/S1369847821002473.

Rosenfield, Adam, Attanucci, John P., Zhao, Jinhua, 2020. A randomized controlled trial in travel demand management. Transportation 47 (4), 1907–1932, URL https://doi.org/10.1007/s11116-019-10023-9.

Santos Silva, João M.C., Tenreyro, Silvana, 2006. The log of gravity. Rev. Econ. Stat. 88 (4), 641–658, URL https://doi.org/10.1162/rest.88.4.641.

Santos Silva, João M.C., Tenreyro, Silvana, 2011. Further simulation evidence on the performance of the Poisson pseudo-maximum likelihood estimator. Econom. Lett. 112 (2), 220–222, URL https://www.sciencedirect.com/science/article/pii/S0165176511001741.

Schultz, P. Wesley, Nolan, Jessica M., Cialdini, Robert B., Goldstein, Noah J., Griskevicius, Vladas, 2007. The constructive, destructive, and reconstructive power of social norms. Psychol. Sci. 18 (5), 429–434, URL https://doi.org/10.1111/j.1467-9280.2007.01917.x.

Schwartz, Shalom H., 1977. Normative influences on altruism. In: Berkowitz, Leonard (Ed.), Advances in Experimental Social Psychology. Vol. 10, Academic Press, pp. 221–279, URL https://www.sciencedirect.com/science/article/pii/S0065260108603585.

Semenescu, Alin, Gavreliuc, Alin, Sârbescu, Paul, 2020. 30 Years of soft interventions to reduce car use – A systematic review and meta-analysis. Transp. Res. D 85, 102397, URL https://www.sciencedirect.com/science/article/pii/S1361920920305848.

Solnick, Sara J., Hemenway, David, 2005. Are positional concerns stronger in some domains than in others? Amer. Econ. Rev. 95 (2), 147–151, URL https://www.aeaweb.org/articles?id=10.1257/000282805774669925.

Sonntag, Axel, Zizzo, Daniel John, 2015. On reminder effects, drop-outs and dominance: Evidence from an online experiment on charitable giving. PLoS One 10 (8), 1–17, URL https://doi.org/10.1371/journal.pone.0134705.

Steg, Linda, 2003. Factors influencing the acceptability and effectiveness of transport pricing. In: Schade, J., Schlag, B. (Eds.), Acceptability of Transport Pricing Strategies. Emerald Group Publishing Limited, Leeds, pp. 187–202, URL https://doi.org/10.1108/9781786359506-012.

Thaler, Richard, Sunstein, Cass, 2009. Nudge: Improving Decisions About Health, Wealth, and Happiness. Penguin Books, New York.

Van den Bos, Gary R. (Ed.), 2007. APA Dictionary of Psychology. American Psychological Association.