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Abstract

Across five field experiments with employees of a large organization (N=68, 915), we examined whether standard behavioral science interventions (i.e., 'nudges') successfully reduced single occupancy vehicle commutes. In Study 1-2, we sent letters and emails that included standard nudges to increase carpooling. These interventions failed to meaningfully increase carpool sign-up or usage. In Studies 3-5, we examined the efficacy of two other well-established behaviorally-informed interventions: non-cash incentives and personalized travel plans. Again, we found no meaningful effects. Across studies the effect sizes were d=[-0.01, 0.05], with a meta-analytic effect size of d = 0.02, 95CI [-0.02, 0.06]. Equivalence testing reveals that the effect size of four of the studies is statistically equivalent to zero (p < 0.0001). Our paper presents the first set of highly powered behaviorally informed experiments on these outcomes and highlights the importance of publishing null results to build cumulative knowledge about how to encourage sustainable travel.

Significance Statement

Millions of people commute to work daily by driving in single-occupancy vehicles (SOVs). This behavior contributes to climate change and undermines commuters' health and happiness. To encourage healthier and more sustainable commutes, we conducted a series of experiments to increase carpooling and transit use. We worked with an employer with over 75,000 employees to conduct five behaviorally informed field experiments all which failed to significantly shift commuter behavior. These findings speak against the utility of employing simple, low-cost interventions to change commuting behavior. More broadly, our findings show that when policy makers are trying to tackle an entrenched habitual behavior like driving alone, relying on nudges may not be enough. Instead, stronger incentives (like cash or policies) are likely necessary.

/body

Twenty-four percent of global energy-related CO_2 emissions result from transportation (*1*). Drastic reductions in emissions must take place in this decade to avoid expensive and potentially catastrophic climatic events (*2*). A primary source of emissions within the control of individual decision-makers (vs. corporate actors) is the use of single occupancy vehicles (SOV).

Most individuals commute to work every day: Americans spend an average 200 hours a year commuting (*3*). Seventy-six percent of Americans drive alone to work in their cars (*4*).

Car commuters have significantly higher levels of self-reported stress as compared to train commuters (*5*) and report lower levels of life satisfaction (*6*). In fact, driving to work is associated with an increased risk of developing high blood pressure and having a higher body mass index (*7*), putting citizens at an increased risk for cardiovascular death (8).

To reduce congestion, pollution, and increase citizens' health, it is critical to encourage individuals to engage in alternative forms of commuting, such as carpooling or taking transit. Due to far reaching success in other policy domains, 'nudges' have gained attention as a potential policy lever to reduce the use of SOVs and encourage alternative forms of transportation (*9, 10*).

Nudges are modifications to the decision-making environment that are noncoercive, easy to avoid, and do not change economic incentives (*11*). Nudges are considered a cost-effective policy tool, and are effective at shaping a diverse set of citizens' behaviors, such as tax compliance, energy reduction, obtaining vaccinations and retirement planning (see *12* for cost-effectiveness calculations). Attesting to the wide-spread use of nudges as a policy tool, as of 2018, there were over two-hundred "nudge units" in governments and institutions around the world (*13*).

Some recent review papers have highlighted the role of psychological barriers in preventing people from shifting away from SOV use to carpooling or other alternative forms of transportation (*9, 10*). These psychological barriers include 1) incorrectly low assumptions about how many people are available for carpooling, 2) difficulty of coordinating rides, 3) failure to recognize the potential savings of alternative modes of commuting, 4) a lack of certainty about best public transit routes, and 5) ingrained commuting habits around driving alone.

To the best of our knowledge, no rigorous causal research exists that tests the efficacy of interventions that target these psychological barriers. We therefore conducted a series of field experiments to test the impact of behaviorally-informed interventions designed to overcome critical psychological barriers known to prevent transportation mode shifting (*9, 10*).

Our key outcomes of interest include any shift away from driving alone, which includes carpooling, taking public transit, or active commuting (i.e. biking or walking). We tested standard, well-researched behavioral science interventions that have been effective in many other contexts (*12*) including: reducing friction costs, loss framing, testimonials, and non-cash incentives. Across studies, participants did not know that they were in a study or that their behavior was being recorded, which is critical given that individuals often act differently when they know their behavior is being monitored (*14*). Indeed, naturalistic field experiments are considered the "gold standard" of empirical evidence (*14*).

Our experiments therefore provide the strongest test to date of whether simple nudges shift commuter behavior. In Study 1 (N=54,887) we tested whether reducing friction costs, highlighting savings (gain framing), and providing testimonials could increase employees' propensity to sign up to a centralized employee carpooling scheme. In Study 2 (N=871), we tested whether providing personalized recommendations about how employees could shift their

commuting behavior and highlighting opportunity costs (i.e. making explicit how they could use the money they save on splitting gas costs on vacations) would effectively encourage carpooling. In Study 3 (N=7,564), we attempted to increase the use of public transportation by offering a free bus trial. In Study 4 (N=4,732), we followed up with participants in the treatment group from Study 3, who did not take advantage of the free trial they were offered. Building on the psychology of loss aversion, where people respond more powerfully to losses than to gains (*15*), we emphasized the monetary equivalent that employees missed out on by not taking the bus during the free trial. In Study 5 (N=1,095), we tested the impact of personalized travel plans (PTPs) that provided information of various routes, transit schedules, travel discounts, and carpool matches tailored to each employee. See Table 1 for more details on the studies, barriers, and interventions.

Our field site was an airport outside with over 75,000 employees outside of a major city. Forty-nine percent of employees reported commuting by SOVs at the time of the study, as indicated by a report commissioned by the employer from an external research firm. The report also suggested that 61% of SOV commuters (approximately 22,000 employees) reported they would consider carpooling, and 41% of SOV drivers stated they would if they could find someone with a similar commute (which translates to 15,000 potential carpool registrants). The site's infrastructure made it an ideal location for our study: 1) the airport was highly connected to frequent subway, train, and bus infrastructure, 2) there was a pre-existing carpool service that used an algorithm to match employees with colleagues with similar commutes, and 3) carpoolers had access to a 24-7 emergency ride home and priority parking.

Results

Post-hoc power calculations revealed that we were powered at the 95% level to detect small effect sizes (d = 0.30) and at 80% to detect very small effect sizes from d = [0.01, 0.13]. Despite our high statistical power to detect small effects, none of our psychologically informed messages or incentives effectively shifted behavior. See Figure 1 for a meta-analysis of the effect sizes observed across studies.

There was one study where an intermediate outcome measure (online registrations) was significantly different from zero using null hypothesis testing, but we failed to reject the null hypothesis for the subsequent four studies. For those studies, we therefore conducted two one-sided tests (TOST) to test for equivalence of the treatment effect to zero (*16*). Since the smallest effect size of interest we could detect with 90% certainty was d = 0.15, we used that as the limit for our equivalence test. We found significant results using TOST for all four studies where we failed to reject the null using null hypothesis testing, leading us to conclude that the observed effects were statistically equivalent to zero ($p < 0.0001$, *Table S1*). We now proceed to document the results for each study separately. Where t-tests are reported, they are all two-sided tests.

Study 1. Carpooling recruitment. Employees were randomly assigned to receive a letter or no letter. Those who received a letter received one of three behaviorally-informed letters. The first was a standard letter, which included behavioral insights such as reducing friction costs, gain framing, and information to minimize anticipated regret. The second had the same behaviorally informed messaging as the standard letter, but also highlighted the registration link with a clear "call to action" to register. The final letter had the behaviorally informed messaging, along with testimonials of successful carpoolers who saved money and enjoyed the experience. Employees

who received a letter were more likely to register for the carpooling system than those who did not (0.22% compared with 0.05%, $t_{(17,614)} = 4.2628$, $p < .05$). However, the effects of the letters were small (all letters vs. no letters $d = 0.055$, 95% CI [0.04, 0.07]).[1] In raw numbers, 33 individuals who received letters signed up for the carpooling scheme (out of 14,987), while 20 individuals who did not receive letters signed up for the scheme (out of 39,900). Only seven people from this study became active members (who activated their carpooling unit and thus became eligible to use the benefits for carpoolers, such as the priority parking and emergency ride home). Four of those seven newly active members were in the control condition.

Study 2. Carpooling activation. Employees who were registered to the carpooling system, but who were not active members (i.e. did not declare that they were part of an active carpool and were not receiving the benefits) were sent one of three emails ($N = 871$). The first email, which we used as a control condition, was a reminder to use the system to try to find a match and become an active carpooler. The second email provided three potential matches of registered users with similar commutes. The third email was similar to the second email but also highlighted the opportunity cost of not carpooling. All emails included a link for employees to log into the system to find potential carpool partners. Around one-third of participants in each email group opened the email, with no differences between conditions. This was expected as the subject line did not differ across conditions (control = 30.53%, matching email = 35.82%, matching and opportunity cost salience = 28.55%; $F_{(2,997)} = 0.859$, $p = 0.424$). However, there were also no differences between conditions in the proportion of employees who clicked through the link (control = 9.15%, matching email = 9.97%, matching and opportunity cost salience = 9.63%; $F_{(2,997)} = 0.064$, $p = 0.939$). The effect of the matching email approximated zero ($d =$ 0.00, 95% CI [-0.15, 0.15])[2]. Only one individual out of the whole sample became an active carpooler.

Study 3. Free bus trial. Employees who were not bus users and who lived along bus routes were randomly assigned to two conditions. Employees in the first condition received a letter informing them about the bus routes near them as well as how they could purchase discounted transit cards through their employer. Employees in the second condition received that same letter, along with vouchers for seven days of a free bus trial. While 103 (out of 7,330) individuals in the treatment group used their free trial vouchers, there was no statistically significant difference between conditions in the purchase of subsidized transit cards (1.45% in control, 1.31% in treatment, $p = 0.484$). The effect size was negligible ($d = -0.01$, 95%CI[-0.06, 0.03]).

Study 4. Follow-up feedback for bus trial. The loss-aversion follow-up letter had no impact on the purchase of transit cards (1.07% in the control, 1% in the treatment, $p = 0.797$). The effect size was negligible ($d = -0.01$, 95%CI[-0.06, 0.05]).

Study 5. Personalized travel plan. We found no effect of delivering a personalized travel plan (PTP) from their home address to the employer worksite address on commuting behavior ($N =$

[1] Note: we collapsed across the three treatment conditions, since there were no differences across them.

[2] Note: we collapsed across the two matching email conditions, since there were no differences across them.

1,095). This travel plan included personalized information regarding potential carpooling matches, bus/train routes and times and transit pass discounts, and bike routes. We also provided the option of signing up a one-on-one session with the airport commuter team, but only 21 individuals signed up. The primary outcome measure was the difference in number of SOV trips (compared to a baseline survey). We could not monitor observable behavior and had to rely on self-report. Although self-reporting is not without risks of measurement error, the participants in this trial had no incentive to be less than truthful, and the behavior (commuting) is a regularly repeated behavior that is easily recalled. Furthermore, we cross-validated the self-report measures with objective secondary measures, including the number of people who registered for carpooling and the number of people who purchased discounted transit passes.

The first self-reported outcome measure was the number of days in the past 5 days an individual drove an SOV to work (a number between 0-5), and the second outcome measure was the frequency of times they drove an SOV to work in the past month (intervals: never, once or twice, 3-5 times, 6-10 times, more than 10 times but not every day, or every day). The effect size for number of days driven in the past five says was negligible (d = 0.01, 95% CI [-0.13, 0.14]). Furthermore, we failed to significantly shift individuals to reduce the frequency interval with which they commuted to work by SOV the previous month. We found no significant differences between the two conditions in terms of carpool registration rates or transit pass purchases.

Discussion

We conducted an extremely high-powered set of naturalistic field experiments that were based on the best insights from behavioral science. We tested a letter and email campaign to increase carpooling, and through our interventions, we significantly increased the proportion of employees who registered for a carpooling service; however, the effect size was tiny. Moreover, this increase in registrations had no downstream impact on the behavior we were trying to shift; though 15,000 letters were sent, only 33 employees registered for the carpool service and a total of only three employees who received the letters became active carpoolers one month later. We also tested an email campaign to encourage inactive registered carpooling users to become active members. Once again, our study had no meaningful impact. We subsequently tested, with a different population, the impact of a one-week free bus trial, and observed no improvement in registration for discounted bus passes. Finally, we evaluated the impact of a personalized travel plan. This travel plan did not reduce the number of times that individuals reported driving on their own to work, even when paired with a one-on-one intensive travel planning meeting.

From our studies, there are a number of inferences we can make, some specific to this context, and others that are generalizable to the use of behaviorally informed interventions to increase carpooling and other alternative forms of commuting (i.e., transit use). This is a context in which a parking space, worth thousands of dollars a year, is provided to employees free of cost (employers pay anywhere from $1,500 - $7,500 per year per spot). Therefore, nudges may be more likely to succeed in contexts where individuals bear the full cost of driving to and parking SOVs at work. This argument is consistent with findings demonstrating the stickiness of SOV commuting behavior when incentives to encourage public transit or active commuting are offered in the context of an employer that offers free parking (*17*). There are also initial instances of success of personalized travel plans when free parking is taken away (*18*).

Our findings also corroborate the pervasive nature of the intention-action gap (for a review see (*19*)). The intention-action gap accounts for the difference between individuals' stated

intentions to perform a particular behavior and their likelihood of actually following through. This gap can be particularly detrimental when it comes to adopting environmental behaviors, whereby individuals espouse pro-environmental attitudes, yet fail to translate those attitudes into action (*20, 21*). Our own partners' internal research suggested that there were close to 15,000 potential employees who were open to registering to carpool if they could find someone to carpool with. However, when we notified employees of the first step to carpooling (registration), fewer than 1% of contacted employees signed up. Even fewer employees proceeded to actively carpool in the months following the trial. Employees also stated they would be more likely to take use public transit if they had discounted travel, but fewer than 1.5% of contacted employees in our bus trial shifted their behavior when notified of subsidized passes. It therefore appears that we cannot rely on employees' self-reports of perceived barriers to carpooling and transit use. Future research should target other barriers that could prevent employees from carpooling that employees might be more reticent to admit on self-report surveys, such as hesitation to talk with an employee they do not know (*22*) or strong positive emotional connection to their cars (*23*).

Overall, these interventions have limited practical significance, failing to meaningfully change driving behavior. Publishing null results, or results of interventions with very weak success, is particularly important because it enables science to self-correct (*24*) and prevents researchers from wasting time and resources repeating the failed efforts of others (*25*). Publishing null results is particularly important in the context of policy-relevant research, to ensure that policy makers do not waste time and money pursuing solutions to pressing challenges that are unlikely to yield favorable outcomes. Thus, results from our studies echo the need for further rigorous evaluations to be conducted for similar programs in the future.

Our findings show that even when addressing many of the psychological factors that employees and researchers believe prevent sustainable travel behavior, mode shift is difficult to achieve. This is true even when deploying nudges that have been shown to be successful in other domains. The ingrained nature of driving behavior that is connected to individuals' perceptions of freedom and autonomy (*26*) and status and power (*27*), makes it very difficult to change.

Furthermore, so long as infrastructure and communal norms favor SOV use, it is unlikely that nudges alone will result in meaningful mode shift away from SOVs. These data suggest that the primary initial focus in many cases should be on land use and pricing policy, employer regulation and policies, and making improvements to transit service and active transportation infrastructure. After such measures are taken, nudges may be an appropriate and cost-effective tool for effecting commuter mode shift away from SOV use.

Interventions that are more heavy-handed than liberty-preserving nudges (*28*), such as congestion charges, parking bans, public transportation infrastructure investment and other mandates, may be necessary to achieve a significant change in commuting behavior. Similar arguments have been made in discussions around shifting smoking (*29*) and carbon emissions (*30*). When heavy-handed approaches are used, the best correlational research suggests that they are effective at reducing driving (*31-33*). For example, dynamic tolling, road pricing and congestion charges, and enhancing the quality of public transportation have been shown to shift driving behavior and reduce pollution (see (*34*) for a review). Furthermore, recent evidence highlights the "cost of soft paternalism," such that under many circumstances, nudges crowd-out support for more effective substantive policies (*35*). For example, when Hagmann and colleagues (*35*) presented policymakers with the option of a nudge to reduce carbon emissions or a carbon tax, when the nudge was presented first, it reduced support for the more effective carbon tax.

Our failure to shift behavior change in this context may shed light on the settings in which nudges are more or less likely to succeed. This research is concerned with a collective action problem, such that, in this context, the behavior in question is, objectively viewed, the most "rational" of available commute choices in terms of time and individuals' monetary costs. The focal behavior in this research also requires people to make a change to a behavior they do every day, rather than a one-time change, which is when nudges have often been shown to be effective. We hope these results lead to more robust theoretical and empirical developments on the limitations of nudges, in relation to commuter behavior and also more generally.

Our evidence may suggest a potential boundary condition for the effectiveness of nudges, that if further developed, could lead to the development of a more sophisticated and nuanced theory about when nudges are likely to be effective. Recent literature (*12*) suggests that nudges are cost efficient in most domains, such as a $100 return per dollar spent on increasing retirement savings (*36*) compared with a $1.24 return per dollar spend on U.S. tax incentives (*37*), or a 1.53 increase in students enrolled in university per $1,000 spent, compared to a negligible increase due to tax credits (*38, 39*). Yet, this review considered only a limited number of domains, specifically ones where the target behavior was consistent with an individual's self-interest (e.g. saving for their own retirement) and where the nudge helped the individual perform a one-off beneficial action from which they directly reaped the benefit for months and years to come (e.g. the decision to enroll in a retirement savings plan). As a result, these efficiencies might not be true across all policy domains. Energy reduction shares important properties with sustainable travel (e.g. changing behavior to mitigate climate change or decrease energy demand). While social comparison letters (*40*) have been shown to be more cost effective than discounts, incentives or education (*41*), a recent paper finds that many of the energy savings for this nudge persisted even after the original tenants moved; indicating that much of the energy reduction was not achieved by changing habitual behavior, but rather through a one-off capital improvement to the home (*42*). These results further point to the possibility that shifting commuting behavior might require fundamentally changing one's habits through purchasing energy efficient cars or not purchasing cars at all. We hope that these results lead to more robust theoretical and empirical developments on the limitations of nudges both in relation to commuter behavior and more broadly.

Methods

Sample characteristics: In an internal employee travel survey, conducted by an external qualitative research firm on behalf of the employer, the majority of employees reported having commutes that are under 45 minutes (59%), while only 24% reported have commutes of over an hour or more.

Study 1. Carpooling recruitment: The organization also had a sizeable number of registered members (8,000) and active carpoolers (2,000) increasing the probability that employees who signed up or who used the platform would successfully find a match.

Participants. To determine our sample, first, we excluded all employees living in the city center with close proximity to public transit, as well as employees living along commuter train and bus routes. We also excluded addresses with multiple employees, because it seemed likely that they may be informally carpooling already and it would be hard to isolate the treatment effect. After those exclusions we were left with a sample of 54,931 eligible employees. We only had the

budget to send 15,000 flyers to airport employees. We randomly assigned 15,000 of the 54,931 eligible employees into one of four conditions: a no-action control group, call to action letter treatment, modelling behavior treatment (testimonial) letter, and a treatment letter that addressed perceived barriers to carpooling (testimonials about shift patterns and distance).

Procedure. We mailed letters to the 15,000 individuals in the treatment group. The first letter was merely informative about the carpooling scheme, the benefits to the users, and the fact that thousands of employees were registered. The next letter was the same as the first, but had a call to action for employees to register, with the website featured in a large font in the header of the letter. The final condition consisted of the same information as the first letter, but featured testimonials and photos of two employees who were successful in finding carpoolers who had similar shift times and came from the same area, and who saved hundreds of dollars a year on carpooling. This final letter addressed the employees' self-reported perceived barriers to carpooling, as well as modelled behavior. Two months later, we checked the records from the employers' online carpooling service to see if individuals from the treatment or control groups registered to the program. We found that 13 individuals in the treatment groups and 31 individuals in the control group had registered between the time we had randomized them into their condition and the time the letters were in fact sent, therefore those 44 individuals were excluded from the analysis.

Study 2. Carpooling activation. *Participants.* We randomly assigned 928 registered members who specified their route to work, but were inactive members of the carpooling scheme, to one of three conditions: control, matching (with details on up to three people who they've been matched with based on location and shift times), and matching and opportunity cost salience.

Procedure. We emailed participants and four weeks later measured the number of participants in registered sharing group.

Study 3. Free bus trial. *Participants*. Employees who lived in the districts where the bus lines ran, were the only employee in their household, and were not registered bus users were included in the trial. This left us with a sample of 7,564 participants.

Procedure. The company survey revealed that the main barrier to taking public transport was the lack of nearby services and the fact that taking public transport would take longer than driving. We therefore used this opportunity to advertise new routes to the employees living in communities with new services being introduced. The survey revealed that employees who used public transport emphasized that it saved them money compared to driving, was easier than driving and meant they could go straight into the terminal buildings instead of having to park outside the terminals and take an additional bus. Based on this survey data, we tested a free bus trial to provide employees who had never taken the bus before an opportunity to experience the benefits. Letters were sent to all eligible employees informing them of the bus services in their area, the low cost of commuting by bus, the amount of money they would save by registering, and the convenience of the bus. Our sample was comprised of employees living in two different towns with new bus routes. The prices from each town were different (based on distance from the airport) and therefore when we randomized, we stratified by town. Half of those who received the letter also received one week's worth of free bus tickets to be used during a

specified free trial week. We collected individual-level data on who used the free ticket. We collected condition-level data on registration for a transit pass one month later.

The economics of transit passes. Transit passes are discounted travel tickets that employees are eligible to purchase. These transit passes significantly reduce the price of travel on certain forms of public transport. For example, a round-trip ticket on the one of the bus services is $8/day, whereas the discounted transit pass is $32/month (and the regular transit pass for non-employees is $112/month). If an individual in that neighborhood intends to take the bus more than four days in a month, it makes sense to buy the transit pass. For the other service advertised in the trial, a round-trip ticket is $22/day, whereas the discounted transit pass is $117/month (and the regular transit pass for non-employees is $130/month). If an individual in that area intends to take the bus more than five days in a month, it makes economic sense to buy the transit pass.

Study 4. Follow-up feedback for bus trial. *Participants*. Since only 103 individuals participated in the free-trial, the rest of the employees in the original treatment group were included for the feedback trial (N = 4,936).

Procedure. Half of the employees who received the free trial but did not take advantage of it were sent a follow-up letter making salient the amount of money they "lost" by not participating in the free trial. It then told them the benefits of registering for a discounted transit pass. One month later, we linked subsequent registration to condition.

Study 5. Personalized travel plan. *Participants*. All employees based at the airport administrative headquarters were asked by senior leaders to fill out a travel survey in January 2016. Our sample were those who commuted to work by SOV at least one day in the month preceding the survey. The final eligible sample for this study was 1,095.

Procedure. All eligible participants were sent an email from the airport's commuter team with a PDF attachment of their personalized travel plan (PTP). The PTP included bespoke information of carpooling matches, different public transportation routes, and cycling routes that individual could take to work. It included travel discounts employees were eligible for, along with the estimated travel time and cost for each option. These emails included the option for interested individuals to sign up for a one-on-one personalized travel plan session. One month after the last commuter plan session was administered, we sent a follow-up survey (similar to the original) to see how commuting behavior changed (specifically to detect a reduction in SOV use).

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revisions. Both of the authors approved the final version of the manuscript for submission. **Competing interests:** Authors declare no competing interests. **Data and materials availability.** Coarsened data, R code, and modified materials are available at https://osf.io/39rja/. Data and materials have been modified to protect the identity of the partner organization and its employees. **Ethics.** The Behavioral Insights Team provided ethical guidelines, reviewed a protocol and approved the study. People often act different when they know their behavior is being studied. Thus, participants did not know that their responses were being recorded. Informed consent was waived by Behavioral Insight Team's ethics review committee and was also agreed to by the field site partner.

Fig. 1. Internal meta-analysis demonstrating negligible effects of the five studies.

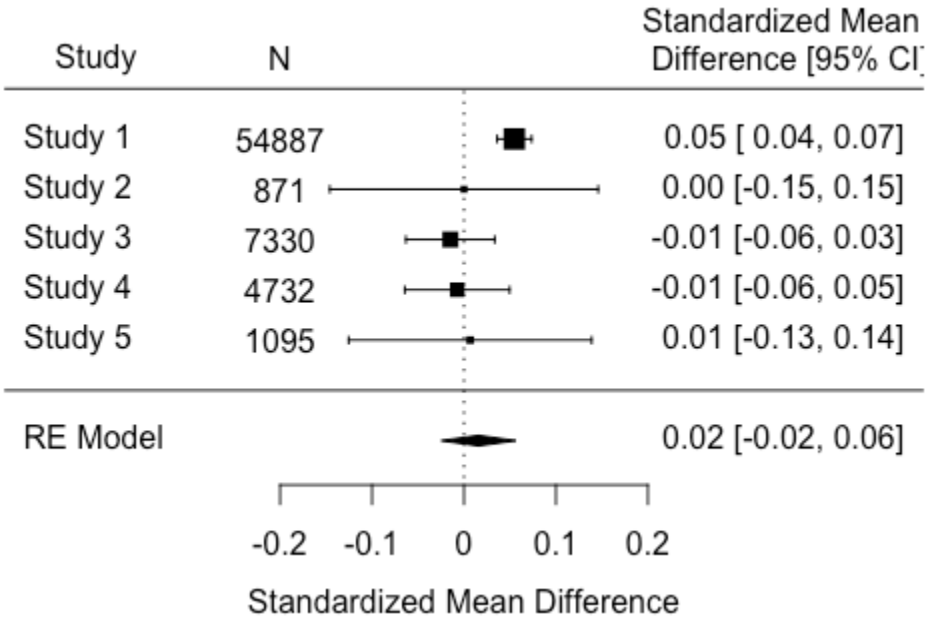

Table 1. Summary of interventions

Behavior	Psychological barriers	Psychologically informed intervention strategies	Interventions
Increase carpooling	Lack of awareness of the scheme, of potential matches, of the savings, and of the opportunity cost. Misinformation about prevalence of potential matches Anticipated regret in case of emergency Misinformation about prevalence of potential matches	Make savings salient Testimonials addressing perceived barriers/correct misinformation Testimonials modeling behavior Messenger effects Make desired action easy/reduce friction cost Induces reciprocity Highlight opportunity costs	Sending letters to increase carpooling registration: ● Control (no letter) ● Standard letter ● Call to action letter ● Testimonial letter) Sending emails to registered carpoolers to become active members ● Control email ● Matching email ● Matching email + opportunity cost made salient
Increase bus use	Negative perceptions of public transit Ambiguity aversion Status quo bias	Free trial to help overcome negative perceptions and reduce uncertainty Highlights monetary equivalence of the incentive Exploits loss aversion	Offering a one-week free bus trial to increase bus use ● Letter with route and discount information ● Letter + offer of a one-week free trial Sending follow-up letters to those who did not partake in the free bus trial to increase bus use ● Control (no follow-up letter) ● Follow-up letter
SOV reduction through personalized travel plan	Lack of information	Provides personalized information Includes travel discounts Induces reciprocity	Emailing a personalized travel plan (PTP) with tailored journey information and information about discounted travel products ● Control (no PTP) ● PTP

Table SI.

	Study 2*	Study 3**	Study 4***	Study 5****
TOST results: t-value lower bound t-value upper bound p-value lower bound p-value upper bound degrees of freedom	 2.02 -2.02 .0022 .0022 475.63	 6.61 -5.43 2e-11 3e-8 4749.33	 5.41 -4.90 3e-8 5e-7 4707.53	 2.12 -2.31 0.017 0.011 541.66
TOST confidence interval: lower bound 90% CI: upper bound 90% CI:	 -0.038 0.038	 -0.003 0.006	 -0.004 0.005	 -0.247 0.219
NHST confidence interval: lower bound 95% CI: upper bound 95% CI:	 -0.046 0.046	 -0.004 0.007	 -0.005 0.006	 -0.292 0.264
Equivalence test	significant	significant	significant	significant
Null hypothesis test	non-significant	non-significant	non-significant	non-significant

TOST results based on equivalence bounds of d = [-0.15, 0.15], alpha = 0.05 (*17*).

*Study 2: Based on the equivalence test and the null-hypothesis test combined, we can conclude that the observed effect is statistically not different from zero and statistically equivalent to zero.

**Study 3: Based on the equivalence test and the null-hypothesis test combined, we can conclude that the observed effect is statistically not different from zero and statistically equivalent to zero.

***Study 4: Based on the equivalence test and the null-hypothesis test combined, we can conclude that the observed effect is statistically not different from zero and statistically equivalent to zero.

****Study 5: Based on the equivalence test and the null-hypothesis test combined, we can conclude that the observed effect is statistically not different from zero and statistically equivalent to zero.