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Using behavioral insights to incentivize cycling: Results from a field experiment☆

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ABSTRACT

Motivating active transport is a health and environmental policy priority, and plays an important role in achieving the necessary shift toward a sustainable transport system. Financial incentives to promote cycling are used in many countries, but very few studies document causal effects. Using a randomized controlled trial in the field, we provide causal evidence of the effect of different types of economics incentives on cycling activity in Norway. Participants' mobility is monitored through an innovative mobile app that registers travel behavior automatically. Results show that both a flat rate and a conditional lottery motivate people to cycle more. Compared to the control group, participants who received an economic incentive cycled 36% more and 18% more often. The conditional lottery appears to be an effective and economically efficient solution and the only treatment with a lasting effect after the incentives were removed.

1. Introduction

Transport choices have large impacts on our health. The transport sector is a major contributor to global warming, but it can also bring positive health and environmental effects for the individual and society, when walking and cycling are encouraged (Hemmingsson et al., 2009; Finkelstein et al., 2008). Active transport[1] can help many to reach an adequate level of daily physical activity, and it thus represent a potential solution against the consequences of a sedentary lifestyle (e.g. diabetes, obesity) (Saunders et al., 2013; Fishman et al., 2015). Increasing the shares of active transport is not only beneficial

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[1] Active transport is intended here as walking and cycling.

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for public health, but it is a crucial step toward achieving a sustainable transport system, as it directly contributes to reduce congestion, energy use and CO2 emissions. Motivating cycling then becomes a health and environmental policy priority, at least in densely populated areas.

Understanding how to motivate people to cycle and walk more in an efficient and effective way is not obvious. Economic incentives are widely used to motivate individual behavior, for example in the workplace, but are also known to sometimes backfire, crowding out intrinsic motivation associated with the behavior of interest (Gneezy et al., 2011). Financial incentives have been used to promote cycling in several European countries. Various policies and schemes have reached the headlines of many newspapers.[2] However, only few academic studies document any causal effect of such interventions, and those generally focus on giving free bikes, not on rewarding actual use (Martin et al., 2012). Understanding which type of incentive works best and for whom, is extremely important since transport behavior is heavily habit dependent. Hence, it is a behavior hard to change, but it holds the potential for long lasting effects. A vast body of research can be found on either the psychological determinants of mode choice (Hamre and Buehler, 2014; De Ceunynck et al., 2019) or on the effectiveness of economic incentives (Patel et al., 2018; Charness and Gneezy, 2009; Volpp et al., 2008a; 2008b; Kullgren et al., 2013). There is a lack of controlled studies combining the two literatures that can disentangle how incentives work for active transport. Moreover, practical knowledge about how to operationalize these insights into effective policy measures to promote sustainable transport choices is also lacking.

Using insights from behavioral economics paired with new mobile technology we aim at filling these gaps. This study tests people's responsiveness to monetary incentives for cycling, using a Randomized Controlled Trial (RCT) in the field. From Kahneman and Tversky (1979)'s seminal paper on prospect theory, we learn that people prefer lotteries with high rewards and low probability compared to low riskless gains. Our experiment provides evidence of a causal relationship between the type of monetary incentive and the amount of cycling, allowing us to study the effect of a small, riskless and flat rate for all, and the possibility for few to win a large sum of money through lotteries. To the best of our knowledge no scholar has tested these two incentives using a RCT in the field, especially not in the transport sector and for cycling. In addition, we draw from recent literature (Haisley et al., 2012; Zeelenberg and Pieters, 2004), and we design one of the lotteries as to potentially evoke individuals' regret aversion (Loomes and Sugden, 1982) to strengthen its motivational effect. From a policy perspective, an important advantage of the lottery lies in its efficiency: its costs are fixed, and a fraction of the spending compared to a flat rate subsidy or tax deduction.

Within the field of transport research, travel data are traditionally measured through self-reported surveys, which have problems such as inaccuracies and biases due to rounding, respondent burden, and under-reporting (Witlox, 2007). Great potential for improvement was brought about by the widespread distribution of smart phones and the development of app technology, which allows for passive tracking. In this study, participants' trips are monitored through an automatic mode detection app, Sense.DAT, which provides reliable quantitative data about time, distance and transport mode for every trip.[3] The app is self-learning and tracks movements using the mobile-network, wifi-networks and GPS data. Users can view, approve and, if needed, correct each trip through a user-friendly interface. Travel behavior is automatically registered, and accurate and complete travel diaries are obtained without incurring in biases common to self-reported data.

Norway offers the perfect setting for our experiment. Norway is considered a very environmental friendly country, as it is at the forefront in various policy areas, such as electrifying its car fleet and banning cars in the city center of its capital. Nevertheless, its cycling shares are low compared to other (comparable) countries such as Sweden, Denmark and the Netherlands. Moreover, Norway has a big potential to increase cycling shares in urban areas given that about half of the trips registered in the Norwegian National Travel Survey 2013/2014 are short car trips (Hjorthol et al., 2014). In 2015, the Norwegian socialist party suggested the implementation of economic incentives, such as tax deductions, to stimulate cycling, but the proposal faced opposition because of the problems associated with monitoring and budget.

We ran a 5-weeks RCT in the field during the fall of 2018 in the three largest cities in Norway. The experiment was designed with 3 treatment arms and one control group. All participants were incentivized to download and use the app by evenly revising and correcting modes, if necessary. Participants in the control group did not receive any further incentive, while participants in the 3 treatment groups could receive monetary compensation based on the amount of cycling registered. In the pay per km (PPK) treatment, participants received 2 Norwegian Kroner - NOK (about EUR 20 cents) for every km cycled.[4] Participants in the "lottery (L)" treatment gained one lottery ticket for every km cycled. At the end of the experimental period one ticket was drawn and one person would win NOK 9000 (EUR 900). The third treatment group was faced with a "conditional lottery (CL)" aimed at exploiting regret aversion. As in the L treatment, participants gained one lottery ticket per km cycled. However, in the CL group, the winner of the lottery would actually win the money only conditionally on having cycled during one randomly extracted day. In other words, the winner of the lottery would lose the prize (and be notified) if he or she had not cycled the randomly extracted day.

[2] See for example: https://edition.cnn.com/travel/article/netherlands-cycling/index.html and https://www.independent.co.uk/travel/news-and-advice/bari-italy-cycle-work-bike-commute-pilot-scheme-payment-a8777116.html and https://www.reuters.com/article/us-france-bicycles/france-experiments-with-paying-people-to-cycle-to-work-idUSKBN0ED1O120140602.

[3] A study on the accuracy of the (previous version) of this app estimated an overall success rate in mode choice detection of 82% (Thomas et al., 2018), see more: https://www.dat.nl/en/solutions/mobility-patterns-from-mobile-phone-data.

[4] This amount was chosen based on results from a pilot run in 2017 and reported in the pre-registration protocols, and it is roughly in line with what other European countries have used as tax deduction or subsidy.

This paper presents the results from 475 people that downloaded the app and registered transport activities during September 2018. Compared to a control group, being exposed to a monetary incentive based on the amount of km cycled, resulted in a significant increase in cycling activity: we estimated an average effect of 2.4 km (36%) and 1.6 days (18%). The strongest effect is found in the PPK treatment, where participants registered 48% km more than the control group (about 3 km more per day). The CL treatment shows a similar effect (increase of 36%) for km cycled, and the strongest effect in terms of amount of days cycled: an increase of 24% (2 more days cycled out of the 15 available) compared to the control group. Additionally, we find interesting gender differences and heterogeneous effects for people with different cycling habits, but we do not find strong evidence that the induced extra cycling substituted car trips or other modes. Both when looking at the post experimental period[5] and when restricting the sample to those who registered activity everyday, we see that only the CL group still shows a significant effect. In light of these results and the fact that the lottery-based interventions are cheaper, we conclude that a (conditional) lottery is more efficient at motivating cycling than a flat rate. However, more research is needed to study long terms as our study was constrained in time because of the worsening of the weather conditions.

The article is organized as follows. Relevant related literature is summarized in Section 2, while Section 3 reports details about hypotheses, experimental design, and recruitment procedures. Section 4 describes the survey and app data. Section 5 presents the results for the two main outcome variables: km cycled and number of days with cycling activity and additional results. The paper ends with Conclusions. Internal and external validity, robustness checks and relevant documents, such as instructions and recruitment texts, are reported in the Appendices 1.

2. Related literature

Transport researchers have found strong correlation between bicycle infrastructures and cycling shares, but very few studies have raised or answered the question of causality. When it comes to evaluating bicycle infrastructure improvement, Cleaveland and Douma (2009) conclude that the build it and they will come theory is not always applicable and context factors can play a more important role than infrastructure itself (see also Barnes et al. (2006) for a discussion on direction of causality). Self-reported data shows that cultural, environmental factors and safety perception are important predictors for cycling activity (De Ceunynck et al., 2019). To objectively measure cycling activity before and after an infrastructure improvement, most studies use on sites count measures. Count studies can sometimes fail to separate between increased activity or routes redirection toward new infrastructure (Marqués et al., 2015; Gårder et al., 1998; Rissel et al., 2015).

Looking at relevant factors for cycling to work, the literature deem infrastructure as one crucial element, together with good access to bicycle parking and shower facilities at work (Buehler, 2012). Improving bike-specific infrastructure is important, but Hamre and Buehler (2014) show that benefits for public transportation, walking, and cycling are associated with an increased likelihood to commute by these modes only when free car parking in not present. Finally, a combination of daily payment, infrastructure improvements and trip-end facilities can be the most effective policy to shift people from car to bike commuting, according to Wardman et al. (2007). Based on both revealed preferences and stated preferences data, the authors predict that payment of two British pounds per day, could result in doubling of the proportion of cycling to work.

Financial incentives have been shown to potentially have a large role in promoting walking and cycling, based on a review of empirical evidence (Martin et al., 2012). Even though incentives have been implemented in several European countries to boost cycling[6] only few studies have evaluated the causal effect of such incentives and most results rely self-reported data. A RCT in Sweden found a significant increase in the amount of cycling when obese women were exposed to a moderate intensity program with free bikes (Hemmingsson et al., 2009), while payment contingent on exercise level (walking, running, etc.) positively affected older adults in the USA (Finkelstein et al., 2008). Two uncontrolled studies providing free bikes found that the Danish Bikebusters and the Australian Cycle100 schemes led to significant increases in the proportion of trips made by bicycle (from 9% to 28% in Bikebusters), although both involved selected participants (Martin et al., 2012). A subvention program directed to e-bikes in Norway, was found to almost double bike shares (cycling as a proportion of all traveled kilometers) for people replacing their normal bike, with an e-bike.[7]

Financial incentives have also proven effective for other healthy behaviors, for example in habit formation for exercise at the gym (Charness and Gneezy, 2009). Insights from behavioral economics and social psychology have been successfully used for improving healthy habitual behavior using lotteries, social pressure and regret aversion. Lotteries that evoked regret aversion or created social pressure provided a stronger effect than direct payment (with the same expected value) for increasing the number of completed health surveys in the workplace (Haisley et al., 2012). Lotteries and regret aversion were also found effective in a weight loss programs (Volpp et al., 2008a; Glanz et al., 2019; Kullgren et al., 2013), while lottery-based

[5] The post experimental period consists of one week, where participants were encouraged to register all activity and were *not* incentivized specifically to cycle.

[6] Countries such as the Netherlands, Denmark, Germany, Belgium and Britain have bike-to-work schemes with incentives spanning from tax deduction, payments per kilometer and financial support for buying bicycles.

[7] Institute of Transport Economics (TØI) report 1498/2016 (in Norwegian). See executive summary in English https://www.toi.no/publications/effect-of-subvention-program-for-electric-bicycle-in-oslo-on-bicycle-use-transport-distribution-and-co2-emissions-article33886-29.html.

incentive scheme, together with a reminder system, lowered the amount of misdirections of the blood-thinning medicines (Volpp et al., 2008b).

The lack of strong empirical evidence identifying the causal effect of incentives for active transport can be associated with the difficulty of obtaining objective data. Most studies rely on self-reported data, which are known to present several problems, such as recall bias and are therefore less reliable than objective data. This paper contributes to the literature by providing causal relationships between economic incentives and objectively registered data using new mobile sensing technology. Monetary incentives for sustainable and active transport can be both easy and effective policy instruments, as they fall between the regulatory instrument like taxes penalizing car use, which create a lot of opposition, and informative or behavioral "nudging" techniques, which can have limited impact in isolation (Martin et al., 2012).

3. Method

3.1. Experimental design

The current study is based on a 5 weeks randomized controlled trial in the field held during the fall of 2018. The study was organized in 3 time periods: a *calibration period* of about 1 week, from the moment they downloaded the app (first possible date was 27th of August) until the 3rd of September, when the experimental period started. The *experimental period* consisted of 15 days excluding weekends (3 working weeks between the 3rd and the 21st of September). Finally, 10 days were allocated for a short *post period* of observation (21st-30th September). The timing of the experiment was dictated by the end of holiday season (mid August), while its end was constrained by the worsening of weather conditions, which generally imply a quite short cycling season in Norway.

The RCT is designed with 3 treatment arms and one control group. All groups received a small incentive to use the app for the duration of the experiment to avoid drop out: the possibility of winning NOK 1000 upon registration of all transport activity for one month. Registration happened automatically, so participants only needed to install the app and revise/correct mode tracking. The control group received no further instructions.

The three treatments received incentives linked to the amount of kilometers cycled during the experimental period. In the "pay per kilometer" (PPK) group, participants were told they would receive NOK 2 for every kilometer cycled registered with the app during the experimental period. In the "Lottery" (L) treatment, participant would gain one lottery ticket for each kilometer cycled registered with the app. At the end of the experiment one lottery ticket would be drawn and one winner would receive NOK 9000. Finally the last group is the "conditional lottery (CL)" treatment which was design to intensify motivation by evoking regret aversion. The instructions for this group were identical to the "lottery" one, but with an additional feature: the risk of loosing all the money. We implemented this by randomly drawing one day out of the 15 experimental days. If the winner of the lottery registered at least one cycling trip in that particular day, the winner will receive the money. While if the lottery winner did not registered any cycling during the selected day, he or she would receive an email with information that despite winning the lottery they will unfortunately receive no prize. Another winner would be drawn and checked against the same day and so on. Translated instructions are reported in Appendix.

It is important to note that the treatments were not designed to have the same expected earnings, as the monetary prices were capped by Norwegian taxation laws and project budget constraints. For the PPK treatment, we opted to give a quite high flat rate - NOK 2 per km cycled - in order to provide empirical insights with a realistic fare: This amount is in line with what other European countries have used as tax deduction or subsidy, and it had revealed a significant effect in a pilot run in 2017, which was used for power calculation. For participants in the lottery treatments, the value of a km cycled was not as clear. The lottery's expected value was not possible to compute without ambiguity, as participants could not observe other peoples' cycling activity nor they knew the exact number of other participants in the experiment group. However, instructions revealed that there were approximately 150 people in their treatment group. Based on the average km cycled in the lottery treatment (8.3 km per day for 15 days), an additional km traveled is worth approximately NOK 0.48 in expectation. A similar number holds for the CL group, with the complication of the second step, i.e. the randomly extracted day. Given that this value is less than a quarter of what participants in the PPK treatment could earn, we do not directly compare estimates between treatments, but we limit our results to comparisons with the control group. This issue does not negatively impact our results. In fact, it implies that our findings constitute lower bound estimates.

This study was pre-registered in The American Economic Association's registry for randomized controlled trials (AEA RCT registry) where experimental details were provided and power calculations and calibrations were based on a pilot that we run in the spring 2017.[8]

3.2. Hypotheses

The current article aims at studying the effect of different payment schemes for active transport by testing the following hypotheses.

HP1: *monetary incentives increase cycling activity*;

8 https://www.socialscienceregistry.org/trials/2462/history/33257.

HP2: *the possibility of winning a lottery stimulate cycling activity as much as, or more than a flat rate for all*;
HP3: *adding regret aversion evoking features to a lottery strengthen its effect, by incentivizing people to cycle more often*.

3.3. Recruitment and practicalities

Participants were recruited from three major Norwegian cities (Oslo, Trondheim and Bergen) through different media. The majority was recruited through Facebook (67%). The rest was recruited through e-mails to members of the Norway's Automobile Federation (NAF) (18%), e-mail lists at the Institute of Transport Economics (TØI) (13%) and the University of Oslo (UiO) (3%). The study was also advertized through an online local newspaper, were people could sign up if interested through the facebook link. The recruitment started the 21 of August 2018. The email and the Facebook posts contained an invitation to a 15 min survey with the possibility of winning NOK 5000 for filling it. The recruitment texts were slightly different for the different recruitment channels. The email directed to NAF was quite general: its main focus was the reporting of their everyday travel (see Appendix F.3 for translated text). The email directed to the university students explicitly mentioned the word experiment and the fact that they could be paid to cycle (see Appendix F.2 for translated text).[9] On Facebook, we created a post targeted for people who were likely to have a bike and cycled (see Appendix F.1 for translated text).

All participants filled out an online questionnaire and were asked to download the mobile app Sense.DAT on their phones. The survey contained questions about basic socioeconomic information, people's commuting habits, cycling experience and their physical activity. These questions were used to filter people in order to have a sample of participants that worked or studied, lived less than 20 km from their work place, had access to a (e-)bike, and had a phone compatible with the app. The selected participants were then randomized into the experiment's control and treatment groups and received preliminary information about the app and instructions (attached in Appendix). Randomization was done using stratification over city of residence and level of self reported cycling in order to have balanced groups around variables that are likely to affect the response to treatment. After reading the experimental instructions, the participants had to answer a series of control question created to ensure comprehension of the experiment.

People who answered the survey and fulfilled the criteria, were contacted again through an email a few days later with information about how to download the app, log-in credentials and experiment instructions. Participants were informed that the experimental period was starting the 3rd of September, but they were encouraged to download the app and activate it immediately after receiving the email to allow for some calibration.[10] A total of 675 people provided informed consent and were invited to download the app. All participants received a reminding e-mail on the 2nd of September that the experiment was staring the next day and a thank you email when the registering period was over. Before and throughout of the experiment, participants could communicate with the researchers through email, in case something was unclear or they had problems with the app.

At the end of the experiment everyone was contacted with a short survey where they were informed on how much they had won (PPK treatment and lottery winners only) and asked to provide information (name, address etc) to receive the payment through a universal gift card that would be sent home.[11] To allow for people who did not want to reveal personal information or did not want to receive the money we gave them the possibility to donate the gained money to a good cause (but this was not anticipated before the experiment).

The total spending for the experiment was NOK 49 000 (circa EUR 5 100, 2018 exc. rate). Average payment for those in the PPK treatment was about NOK 303 (min NOK 10 and max NOK 1 366, total about NOK 30 000). For each of the lotteries treatment one person won NOK 9000. No one lost the lottery in the CL treatment as the first drawn winner also cycled the randomly drawn day. One person won the lottery of NOK 1 000 meant to motivate everyone (including the control group) to register travel activity. About 35% of participants in the PPK treatment wished to donate their earnings. A total of NOK 8200 was then donated to the Norwegian Church Aid, a well-known charity organization in Norway.

4. Data

Out of the 675 people who were invited to download the app, 486 people have download it and 483 registered at least one trip during the experimental period.

4.1. Survey

The sample is composed of 58% men with an age ranging from 20 to 69 years old with an average and median of 40 years old. The majority of participants (59%) lives in Oslo, while 26% in Trondheim and 14% in Bergen. Most of them have

[9] The reason behind this is that these students were part of the subject pool used to recruit for laboratory economic experiment at the University of Oslo and were used to expect compensation for showing up.

[10] The app could learn favorite places and favorite modes of transport especially if people were to correct wrong sensed trips.

[11] The gift card is extremely simple to use, it is valid for 5 years and it is spendable all over Norway in more than 5000 shops. See more: https://www.presentkort.no/gavekort/.

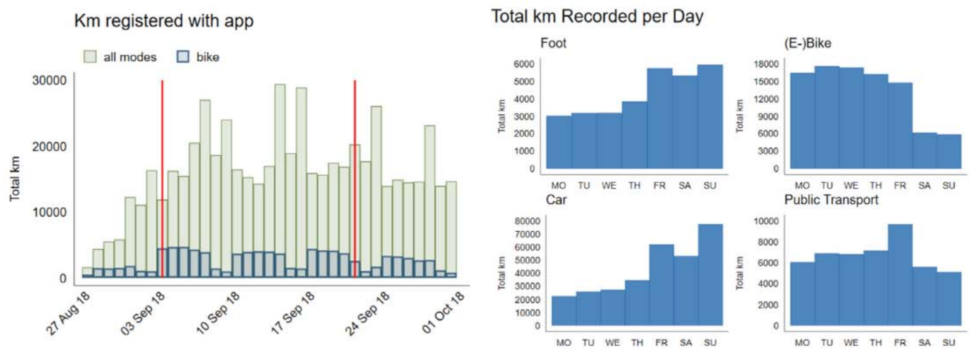

Fig. 1. Left panel: Km registered by the app per day of the week. Right panel: total km registered by the app per day for all transport modes and for bike trips only for all 475 people. The two red vertical lines indicate when the experimental period started and ended. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

a job, while 8% are students. The majority of people (56%) report their income being between NOK 300 and 700 thousand and hold a university degree. More than 40% do not have children, while the rest have between 1 and 3 children.

Participants needed to have access to a bike or electric bike: almost 90% owned a bike and about 30% owned or had access to an electric bike. In addition, about 64% of the sample also owned a car, but the most common commuting mode (in August-September) was (e-)bike for 75% of respondents. Moreover, almost 60% of respondents stated that they cycle all year around, a number that is higher than the norm in Norway. The majority of the people in our sample also stated their commuting distance to be less than 4 km and 72% of them has a high frequency (more often than four times per hour) public transport stop less than 500 m from their home. Participants reported an average of 3.5 trips per day (all modes), of which almost 50% are done to and from work or school, 12% in caring activity, 16% for grocery shopping or similar errands, 8% leisure and 5% for training activity. Women travel slightly more than men (3.7 trips vs 3.3) and on average, they run more errands and caring trips, while men do more work and training trips.

4.2. App

In this study, we used an automatic mode detection app, Sense.DAT, which provides quantitative data about time, distance and transport mode for every trip taken outside the home. Sense.DAT is an implementation of the Mobidot platform, which was also used in Thomas et al. (2018) and Geurs et al. (2015). These studies show an overall success rate in mode choice detection of 82%. The accuracy depends on trip length - higher for longer trips - as well as trip detection rates.

On average, participants used the app for 28.3 days and registered about 7 trips per day (all modes and all periods included). About 30% of the registered trips were made by bike or e-bike, 27% by car, 29% by foot and 10% using public transport. We observed the possibility for one trip to be broken up in multiple trips when stopping for too long, for example for a red light or to change transport mode. For this reason, our analysis relies on the amount of km registered instead than the number of trips.

Fig. 1 shows the sum of km registered by the app per day for all modes and for bikes. Cycling activity is much lower during the weekends indicating that most people use this mode in their daily commute (Fig. 1). In contrast, walking and car use is highest during the weekends - from Friday to Sunday. Data collected through the app show that the majority of trips are made toward home and to and from work or school. These are the easiest destinations to register for the app as participants could save “home” and ”work” locations.

Participants could easily monitor what the app was registering: number of trips, km travelled, a map showing where they started and ended each trip and the automatically detected transport mode. Participants could then confirm trip sensing or correct the transport mode if necessary. Correcting the transport mode was encouraged in order to help the app “learn”, especially during the calibration period. Participants across treatments corrected on average 9% of the registered trips. As expected, this share was a little higher (12%) during the calibration period and declined over time (11% in the experimental period and 8% in the post experimental treatment). We observe very small differences between correction rates across treatments. The highest rate of mode correction is observed for the control group and PPK group (10%), while the lotteries treatments have on average 8% (L group) and 9% (CL group).

The possibility of overriding the automatic registration of travel modes may create distortions in the results. In the instructions, we explicitly advise participants against cheating and inform them that they will automatically be excluded from the study if any cheating activity was detected. We check for potential cheating behaviour by comparing correction rates across treatments (see Table 5 in Appendix). During the experimental period, we observe that 53% of corrected trips, were changed *to* (e-)bike,[12] 20% to public transport, 13% to car and 7% to foot. Looking at the correction rates across treatments,

[12] A reason behind the higher correction rates for the (e-)bike category compared to other modes, is that the app did not separate between bike and e-bike. The majority of such corrections stems from changes from bike to e-bike. We disregard this difference as participants were not required to separate between bike and e-bike nor the incentives were dependent on that.

Table 1
OLS estimation results, outcome variable km cycled.

	(1) Km cycled	(2) Km cycled	(3) Km cycled	(4) Km cycled
Treatments	2.385***	2.409***		
	(0.642)	(0.610)		
PPK			3.159***	2.889***
			(0.871)	(0.840)
L			1.697*	1.702*
			(0.837)	(0.812)
CL			2.365**	2.670***
			(0.810)	(0.782)
City		0.347		0.347
		(0.214)		(0.217)
Women		-1.846**		-1.872**
		(0.580)		(0.580)
Age		0.0752*		0.0725*
		(0.0360)		(0.0356)
Income		0.258		0.262
		(0.279)		(0.276)
Number of kids		0.213		0.221
		(0.295)		(0.293)
Social media		2.989***		2.974***
		(0.629)		(0.634)
Constant	6.616***	0.143	6.616***	0.239
	(0.517)	(1.368)	(0.518)	(1.381)
Observations	475	475	475	475
Adjusted R^2	0.022	0.091	0.023	0.092

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Robust standard errors in parentheses.

we do not find important differences that would create problems for the validity of the results. (E-)Bike is the most "changed to" mode in all treatments and the differences between control and treatment are small.

5. Results

In this section, we analyze the results for a sample of 475 people that registered activity during the experimental period.[13] In particular, we have 131 people in the control group, 107 people in the pay per km (PPK) group, 118 people in the lottery group and 122 people in the conditional lottery (CL) group. Following the hypothesis we consider two main outcome variables: km cycled per day and total number of days cycled, out of the 15 possible experimental days. We further explore the data looking at patterns within subpopulations such as gender and commuting mode reported in the survey. The main part of the analysis is also performed on the subsample of people who have used the app everyday of the experimental period (306 people) as robustness test (see Appendix E).

5.1. Km cycled

Following our hypothesis we first test the effect of any monetary incentives comparing the average amount of km cycled per day between the control group and all treatment groups together. On average, people in the control group have cycled 6.6 km per day (standard deviation 5.9), while those who were exposed to a monetary incentive have registered on average 9 km per day, 36% increase (with standard deviation 7.1 km), confirming HP1. This difference is highly statistically significant according to both parametric and non-parametric tests (Wilcoxon rank-sum test reports a p-value of 0.0001).[14]

Model (1) and (2) of Table 1 shows results from a OLS regression where the dummy variable treatment takes value one if the person is part of one of the three treatments and zero if it was in the control group. The average effect of receiving a monetary compensation linked to the amount of km cycled is 2.4 km per day and results are robust to control variables such as city of residence, gender, age, income levels, number of children and whether they were recruited from social media. On average women cycle less than men and people who were recruited through social media cycle more.

Focusing on the effect for each treatment, all incentives had an impact on the distribution of total km cycled per person (Fig. 8). Fig. 2 plots the average km cycled per day in the different treatments and control groups. Hypothesis 2 is not confirmed, as the lottery incentives did not produce a larger effect than a flat payment for all. In particular, those who were

[13] With respect to Fig. 10 there are 3 people missing because they did not registered any activity during the experimental period, but used the app only during a few days before.

[14] Throughout the result section we report non-parametric test results given the non-normal distribution of the outcome variable of interest as shown in Fig. 8. However, t-test are also performed and no difference was found.

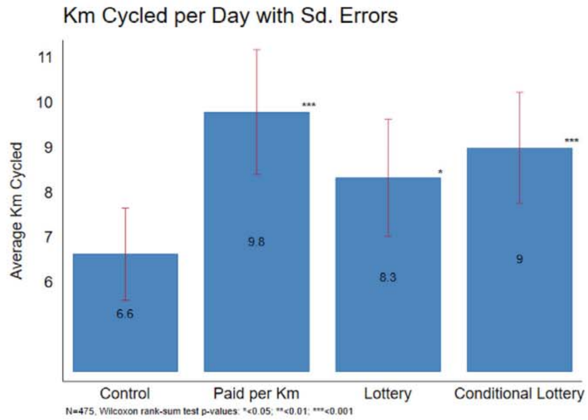

Fig. 2. Average km cycled per day in each treatment with standard errors. Stars reported on top of the bars refers to Wilcoxon non parametric test run between each treatment and the control group.

paid NOK 2 per km, cycled an average of 9.8 km per day, while those who could win NOK 9000 through a lottery cycled 8.3 km and those who where exposed to the conditional lottery treatment registered 9 km per day.

Non-parametric Wilcoxon rank-sum test were run for each treatment group compared to the control group, with individuals as unit of observation.[15] Results from such tests show that participants in the PPK Treatment (p-value=0.0001), the Lottery treatment (p-value=0.033) and the CL Treatment (p-value=0.0005) have cycled significantly more than people in the control group. More precisely, participants in the PPK treatment have cycled on average 3.2 km more per day than the control group, which is an increase of 48%. Those in the lottery treatment, cycled 1.7 km (26%) more per day than the control group, and people in the CL treatment scored an increase of 36% (2.4 km).

Similar conclusions are found with a linear regression in Table 1 in model (3) and (4). All models have as dependent variable the total amount of km cycled per person per day during the experimental period, so that results are directly comparable with Fig. 2. Model (4) include relevant control variables such as city of residence, gender, age and income as control. Results are robust to model specification and gender seem to be correlated with amount of km cycled, with women cycling about 1.9km less than men, while those who were recruited through social media cycle an average of 3 km more. Table 12 (Model (1) and (2)) in the Appendix E reports similar results when including only people who have registered activity everyday in the experimental period. The level of significance is reduced especially for the PPK treatment, the effect for the simple lottery it is not significant anymore, while the CL group still shows significant effects.

5.2. Days cycled

We test Hypothesis 3 by using the variable "number of days cycled" as dependent variable. We hypothesized based on the literature, that making the lottery conditional on cycling activity would influence people to cycle more often by triggering regret aversion. In particular, we designed a treatment where people had the possibility of winning the lottery, but then lose all the money if they did not cycle on a randomly drawn day. Fig. 3 shows the average number of days cycled per treatment group. Participants in the control group cycled an average of 8.8 days out of the 15 possible days.

The largest effect is recorded for the CL treatment confirming HP3, where participants cycled an average of 2.1 days (24%) more than the control group. Participants in the PPK treatment cycled av average of 1.96 days (22%) more than the control group and those in the lottery group 0.9 km (10%) more. Non parametric tests (Wilcoxon rank-sum test) show that participants in the PPK (p-value=0.0003) and the CL treatment (p-value=0.0003) have cycled significantly more days than people in the control group. A smaller effect is found for the lottery group (p-value=0.11), the effect is not significantly different from the control group according to non parametric tests.

Similar conclusions are found with linear regression models in Table 2, where the dependent variable is the number of days cycled during the experimental period.[16] Models (1)-(2) test the effect of being exposed to any monetary compensation compared to the control group, while Models (3)-(4) looks at the effect for each treatment. Models (2) and (4) include as control relevant variables such as city of residence, gender, age, income and recruitment source. The average effect of being exposed to a monetary incentive is 1.6 day difference out of 15 possible days. When looking at each treatment, the largest impact is found for the CL treatment with an effect size between 2 and 2.2 days. Both results are robust to model specification. The city of residence seem to be correlated with the amount of km cycled. This could be due to differences in weather or to pre-existing differences in cycling levels, which have been documented in a Norwegian study (Lunke et al., 2018). As in Table 1, people who were recruited through social media cycle more: almost 2 days more than the others.

[15] T-tests provide very similar results.

[16] Similar results are found with a ordered Logit model.

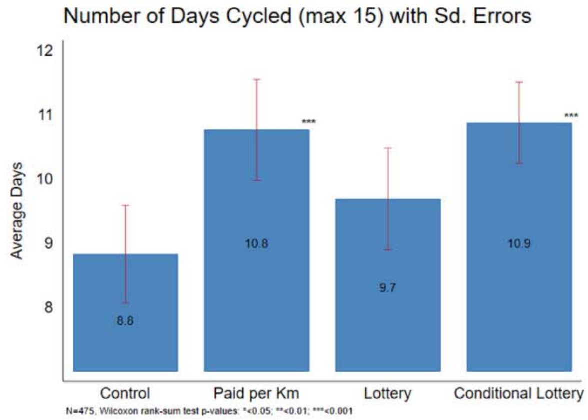

Fig. 3. Average number of days cycled in each treatment with standard errors. Stars reported on top of the bars refers to Wilcoxon non parametric test run between each treatment and the control group.

Table 2
OLS estimation results, outcome variable number of days cycled.

	(1)	(2)	(3)	(4)
	N. days	N. days	N. days	N. days
Treatments	1.609***	1.680***		
	(0.440)	(0.430)		
PPK			1.934***	1.913***
			(0.551)	(0.532)
L			0.861	0.929
			(0.554)	(0.542)
CL			2.045***	2.206***
			(0.500)	(0.487)
City		0.492***		0.500***
		(0.133)		(0.133)
Women		0.00182		-0.0561
		(0.382)		(0.384)
Age		-0.00174		-0.00301
		(0.0211)		(0.0209)
Income		0.0647		0.0655
		(0.170)		(0.168)
Number of kids		0.0429		0.0521
		(0.174)		(0.172)
Social media		1.963***		1.960***
		(0.402)		(0.403)
Constant	8.823***	6.202***	8.823***	6.240***
	(0.383)	(1.041)	(0.384)	(1.036)
Observations	475	475	475	475
Adjusted R^2	0.028	0.089	0.036	0.099

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Robust standard errors in parentheses.

The effect for PPK and CL is robust to including only people who registered activity everyday, and the simple lottery treatment becomes weakly significant (See Appendix E, Table 12).

5.3. Additional results

To understand the results in more details, we further analyze the data looking at subpopulations within our sample and how pre-existing characteristics of the population, recorded during the pre-experimental survey, correlates with the outcome variables. Moreover, we test whether the effect of the treatments is lasting even when people are not longer payed to cycle, and whether the experiment has affected participants other transport mode choice.

5.3.1. Gender differences

Fig. 4 shows that women in our sample cycle on average less than men in terms of km (7.4 vs. 9.25 km per day), but not in terms of number of days cycled (c.ca 10 days for both men and women). Women display a stronger and more uniform response to treatment than men when it comes to km cycled (Fig. 4, left panel). Compared to men in the control group, men cycle statistically significantly (95% level or higher) more only in the PPK treatment: 46% km more (Wilcoxon rank-sum

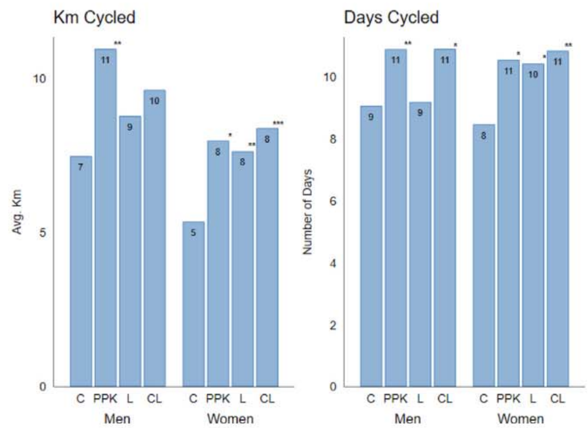

Fig. 4. Left panel: Average km cycled per day by gender and treatment. Right panel: Number of days cycled by gender and treatment. Stars reported on top of the bars refers to Wilcoxon non parametric test run between each treatment and the control group for the relevant subgroup, more precisely:* <0.05; ** <0.01; *** <0.001.

p-value=0.0011).[17] In contrast, compared to women in the control group, women cycled 49% km more (Wilcoxon rank-sum *p*-value=0.012) in the PPK group, 43% (*p*-value=0.0045) in the L group and and 57% (*p*-value=0.0009) more in the CL group.

Looking at the number of days cycled (Fig. 4, right panel), men and women in the control group cycled of 9 and 8.5 days respectively, and they responded quite similarly to monetary incentives except for the lottery treatment. Compared to men in the control group, men cycled statistically significantly more in the PPK treatment: 20% km more (Wilcoxon rank-sum *p*-value=0.0059) and in the CL group 20% more (Wilcoxon rank-sum *p*-value=0.0131).[18] Compared to women in the control group, women cycled 25% km more (Wilcoxon rank-sum *p*-value=0.026) in the PPK group, 23% (*p*-value=0.03) in the L group and and 28% (*p*-value=0.006) more in the CL group.

Table 10 in Appendix D shows regression results with the interaction between treatment and gender for km cycled per day and number of days cycled confirming the results highlighted by the non-parametric tests.

5.3.2. Pre-experimental level of cycling

In this section, we use self reported amount of cycling in the pre-experimental survey to investigate differences between people's response to treatment for those who already cycled a lot and those who cycled occasionally or never. This variable was also used as a strata for the randomization process during initial allocation to treatment groups. The variable "cycle often" takes value one if participants declared to use their bike as transport mode more than once a week and zero otherwise.[19] A total of 475 people answered this question, with 408 people cycling more than once a week, and 67 once a week or less. Therefore it is important to account for the number of observation when drawing conclusion for this analysis. We thus include non-parametric tests for all the treatments jointly versus the control group in the text below.

Fig. 5 shows the amount of km cycled per treatment group for those who used to cycle often and for those who did not (left panel) and the number of days cycled (right panel). Those who said they cycle often, cycled on average 9.2 km per day in the experimental period (average across treatments) and 10.6 days out of 15, while those who cycled less often averaged 3.2 km per day over 6.1 days.

Those who used to cycle more than once a week, cycled on average 23% more when they were part of one of the treatment groups compared to the control group (Kruskal-Wallis equality of population test significant *p*-value=0.0017). In particular, those in the PPK treatment cycled 2.6 km more (Wilcoxon rank-sum *p*-value=0.001, N=94) than the control group (N=105) and those in the CL group 2 km more (Wilcoxon rank-sum *p*-value=0.0097, N=102). Those in the Lottery group cycled the same amount.[20]

Those who said they used to cycle less than once a week, cycled on average 3.2 km (244%) more when incentivized through one of the treatment (Kruskal-Wallis test significant *p*-value=0.0028). This effect is non-significant for participants in the PPK group.[21] While, those in the Lottery group cycled 4.1 km more than the control group (Wilcoxon rank-sum *p*-value=0.047, N=10) and those in the CL group 2.43 km more (Wilcoxon rank-sum *p*-value=0.0001, N=19).

[17] Compared to men in the control, men cycled 17% more in the L group (*p*-value=0.52) and 29% more in the CL treatment (*p*-value=0.052).

[18] Compared to men in the control, men in the L group cycled 1% more (*p*-value=0.81).

[19] The original question was: "How often do you use your bike as a mean of transport this time of the year?" and the answer could be "more than 4 times a week", "from two to four times a week", "once a week", "from one to three times a month", "less often", "never".

[20] People in the Lottery treatment cycled 0.7 km more and Wilcoxon rank-sum test shows no significant difference (*p*-value=0.658, N=107).

[21] PPK with low pre-level of cycling (N=13) cycled 3.1 km more than the control group (N=25), but this difference is not statistically significant according to Wilcoxon rank-sum tests (*p*-value=0.611).

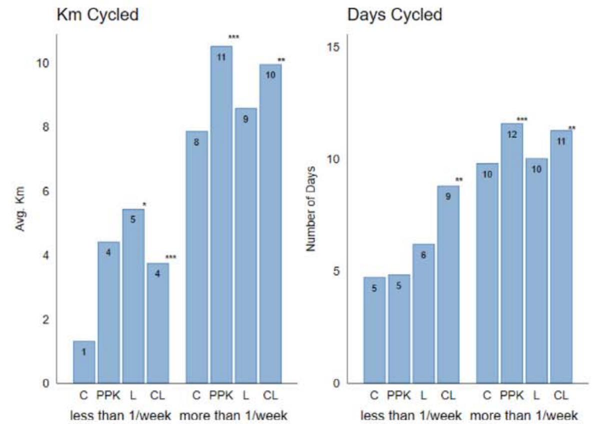

Fig. 5. Left panel: Average km cycled per day by level of cycling and treatment. Right panel: Number of days cycled by level of cycling and treatment. Stars reported on top of the bars refers to Wilcoxon non parametric test run between each treatment and the control group for the relevant subgroup, more precisely:* <0.05; ** <0.01; *** <0.001.

In terms of number of days, those who used to cycle more than once a week, cycled on average 12% more often when they were part of one of the treatment groups compared to the control group (Kruskal–Wallis test *p*-value=0.002). In particular, those in the PPK treatment cycled 1.8 days more than their peers in the control group (Wilcoxon rank-sum *p*-value=0.001). Those in the Lottery group show no difference with the control group,[22] while those in the CL group cycled 1.5 days more (Wilcoxon rank-sum *p*-value=0.009).

Those who said they used to cycle less than once per week, cycled on average 40% more often when incentivized through one of the treatment (Kruskal-Wallis *p*-value=0.018). This effect is only significant for participants in the CL group: they cycled 4 days (86%) more than their peers in the control group (Wilcoxon rank-sum *p*-value=0.0015).[23]

Table 11 in Appendix D shows regression results with the interaction between treatment and level of cycling for km cycled per day and number of days cycled confirming the results highlighted by the non-parametric tests.

5.3.3. Post-experimental period effect

All participants, including those in the control group, were incentivized with a small lottery to keep using the app until the end of September 2018. In this section we analyze the post treatment observations. In order to compare working days as done in the main analysis we exclude weekends and select days between the 24th and 30th September.

The amount of km cycled per day is on average 1.8 km lower in the post treatment period than in the experimental one. Non parametric tests comparing each treatment group across periods reveal a statistically significant lower amount of km was cycled in the post treatment period, at 95% or higher confidence. One possible explanation is seasonality as the weather in Norway tend to worsen around the end of September with likelihood for rain and even snow increasing quickly. See Fig. 9 in the Appendix for an overview over the amount of km cycled over all periods.

Fig. 6 shows post treatment observations for 431 people who used the app both in the experimental period and in the post treatment period. The left panel shows the amount of km cycled per day per treatment groups, while the right panel shows the number of days cycled. Non parametric tests shows that only people in the CL treatment cycled significantly more km than the control group (Wilcoxon rank-sum test *p*-value=0.012).[24] When it comes to number of days cycled (Fig. 6 right panel), we find no significant difference between the control group and the treatment groups, even though the trend is at least visually increasing in treatments.

Table 3 confirms these results showing linear regression estimated for the post treatment period for the main outcome variables: km cycled per day and number of days cycled. Column (1) shows that the only statistically significant effect is found for the CL treatment.

5.3.4. Transport mode change

To further understand the effect of the experiment we look at the amount of km that were registered with the app in the experimental period for the other transport modes in a similar fashion as we did for the km cycled.

Fig. 7 shows the total amount of kilometer travelled with car, public transport[25] and walked per person averaged over treatment. In addition, the bottom right panel shows the total amount of km registered with all the modes of transport

[22] 0.2 days more than the control group. Wilcoxon rank-sum *p*-value=0.6.

[23] Participants in the PPK treatment cycled 0.2 days more than the control group cycled, and those in the Lottery group 1.5 days more than their peers in the control group (Wilcoxon rank-sum test *p*-value=0.79), but the difference is not significant (Wilcoxon rank-sum test *p*-value=0.594).

[24] Wilcoxon rank-sum test report a *p*-value of 0.067 for the lottery group and a *p*-value of 0.093 for the PPK group compared to the control.

[25] Public transport include train trips as well.

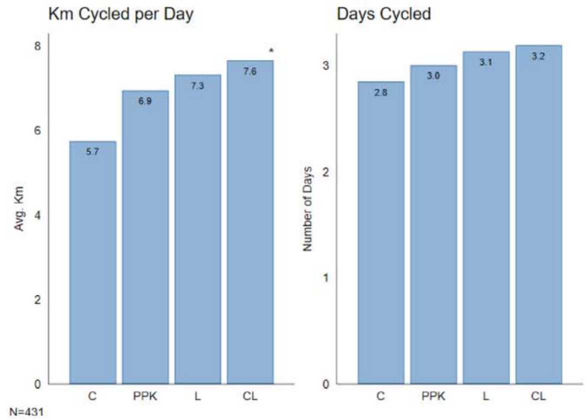

Fig. 6. Average km cycled per day (left panel) and number of days cycled (right panel) in the post-experimental period (5 days). Stars reported on top of the bars refers to Wilcoxon non parametric test run between each treatment and the control group, more precisely:* <0.05; ** <0.01; *** <0.001.

Table 3
OLS estimation results post experimental period.

	Km cycled	Km cycled	N. days	N. days
PPK	1.197	0.843	0.155	0.124
	(0.924)	(0.896)	(0.229)	(0.223)
L	1.574	1.405	0.283	0.220
	(0.951)	(0.923)	(0.221)	(0.220)
CL	1.911*	1.968*	0.341	0.291
	(0.883)	(0.850)	(0.213)	(0.209)
City		-0.0260		-0.00600
		(0.233)		(0.0581)
Women		-1.182		0.275
		(0.678)		(0.162)
Age		0.0619		-0.00547
		(0.0458)		(0.00875)
Income		0.313		0.113
		(0.331)		(0.0715)
Number of kids		0.353		0.0799
		(0.313)		(0.0707)
Social media		2.987***		0.656***
		(0.643)		(0.163)
Constant	5.737***	-0.112	2.845***	1.933***
	(0.600)	(1.573)	(0.153)	(0.458)
Observations	431	431	431	431
Adjusted R^2	0.004	0.057	-0.000	0.041

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.
Robust standard errors in parentheses.

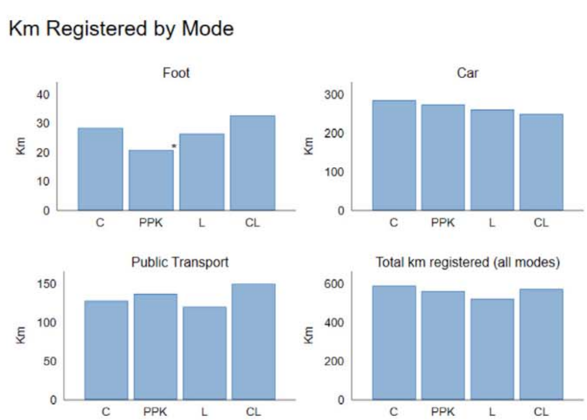

Fig. 7. Km registered during the experimental period by transport mode.

Table 4
OLS estimation results for other transport modes.

	Km registered			
	Foot	Car	Public Transport	Total
PPK	-7.122**	-10.49	11.26	-32.83
	(2.648)	(39.33)	(26.91)	(61.11)
L	-1.868	-24.58	-5.082	-69.92
	(4.112)	(41.39)	(20.65)	(63.02)
CL	3.933	-28.12	21.12	-11.48
	(6.381)	(39.06)	(23.80)	(61.88)
Women	-0.205	-36.64	-3.061	-85.68*
	(3.169)	(28.02)	(18.29)	(40.43)
City	-2.476**	3.188	-11.15	-21.64
	(0.807)	(10.95)	(7.567)	(14.63)
Age	-0.354	-0.541	0.0419	-0.285
	(0.200)	(1.843)	(0.958)	(2.298)
Income	4.225	32.86**	1.669	67.85***
	(2.245)	(12.21)	(7.211)	(18.38)
Number of kids	-2.234	-9.538	-16.92*	-19.41
	(1.505)	(11.80)	(7.890)	(18.43)
Constant	35.27***	198.5*	175.3***	444.6***
	(6.888)	(88.78)	(46.95)	(110.6)
Observations	466	434	403	475
Adjusted R^2	0.016	0.010	0.001	0.043

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$Robust standard errors in parentheses.

including plane, boats, train, car, bike, public transport and walked. We observe some variation between treatment groups and run non-parametric tests and regressions for each mode, as done in the main analysis.

Results are shown in Table 4. The only statistically significant difference is found between the control group and the PPK treatment for km walked. This effect is negative and it is found both through non-parametric test and regressions (Table 4, column 1).[26] In conclusion it seems participants did not substitute other modes with bike, but they increased their cycling activity when incentivized.

It is important to note that previous experience with the app have revealed that tracked walking trips can be quite unreliable, since they are much shorter than the other transport mode and the app would have needed a specific calibration to correctly track them. Moreover, the instructions for participants in the treatment groups indicated that participants were going to be paid only based on the amount of cycling, and therefore it is likely that they focused on registering and correcting bike trips more than other modes. Looking at the metadata from the app, we notice participants in the control group corrected more trips on average than people in the treatment groups, because they corrected trips for all transport modes. Looking only at bike trips, in fact, participants in the treatment groups corrected more trips than those in the control indicating a higher attention to only bike mode.

6. Conclusions

This article's main contribution is to provide causal evidence on the effect of different types of economic incentives for cycling using objective data registered through an automatic mode detection mobile app. We ran a Randomized Controlled Trial (RCT) in the field to test the effect a small riskless flat rate for all (mimicking a subsidy or tax deduction) and to types of risky lotteries. We find that a conditional lottery designed to evoke regret aversion can be an effective and economically efficient solution.

A total of 475 people, residing in three major cities in Norway, participated in the experiment by downloading an app and registering cycling activities. On average, participants used the app for 28.3 days and registered about 7 trips per day (all modes and all periods included). Results shows that participants exposed to economic incentives significantly increased their cycling activity, measured in both km cycled and amount of days with non-zero cycling activity, compared to a control group. On average, people in the control group cycled 6.6 km per day, while those who were exposed to a monetary incentive have registered on average 9 km per day (average effect 36%). The strongest effect was found in the "pay per km" (PPK) treatment, where participants registered 48% more km cycled than the control group. A risky lottery alone did not provide a larger incentive that a small flat rate, but it had a similar effect (36%) to the PPK when a regret-triggering condition was added to it.[27] In terms of amount of days cycled, the conditional lottery provide the highest effect with an increase of 24% (2 more days cycled, out of the 15 available), while similar results are found in the PPK group (about 22% increase).

[26] Hp tests between the control and PPK treatment reports a p-value=0.044.

[27] Note that the lottery had a much lower expected value than the PPK treatment, hence the focus is not placed on the direct comparison between the two treatments, but between each treatment and a common control group.

An additional interesting finding is the difference in treatment response between men and women. Women in the control group, cycle on average less km than men, but a similar number of days. When exposed to economic incentives, women respond more homogeneously, meaning equally strong, to both PPK and lotteries. In contrast, men respond strongly to PPK, while seems less affected by the lotteries.

Looking at the external validity of our results, we find that our sample may suffer from some self selection given that the recruiting process was voluntary, and one of the participation criteria was to have access to a bike (see more in Section Appendix C). Comparing our control group to official numbers from the Norwegian National Transport Survey, we find higher cycling shares in our sample. However, official figures often refer to the entire year, while our experiment was run in September (one of the month with highest cycling activity) and these numbers are based on surveys, which are known to often under-report cycling and walking activity. We check for internal validity by running balance tests and looking at attrition rates. We find no indication to doubt the fact that the randomization was successful, as multiple factors correlated with the outcome variables are well balanced between groups (Table 6).

One limitation of having a sample with higher share of cycling compared to the population, is that we cannot know whether the estimated effect is generalizable to people with lower cycling activity. On the one hand, we can think that people who do not own a bike or that do not already cycle will not easily move into cycling with a small economic incentive. On the other hand, those with a low level of cycling have a much larger margin for improvement and a larger health effect. From our explorative analysis (Section 5.3), we find that the effect for those who didn't cycle often before the experiment is much larger, reaching effect sizes higher than 200%. However, these results are based on few observations and therefore have weak or no statistical significance. It is therefore important for future research to study in more detail how subpopulations, such as gender and people with different level of cycling, respond to economic incentives.

One issue that warrants discussion is cheating behavior as it can be a potential threat when monetary incentives are involved. The app automatically detects travel modes, hence it greatly reduces the likelihood of misreporting activity compared to a pure self-reported approach. Still, participants could (and were indeed encouraged to) check and correct their trips to ensure the app had guessed correctly. We therefore investigated the meta-data from the app to see if there were any differences in correcting behavior between any of the groups. Only 9% of all the trip were corrected by the users. The comparison showed that the number of corrected trips and the change pattern (from - to travel mode) did not differ significantly across treatment groups and the number of changes decreased over time, possibly as the app learned individual travel habit.

The mode change effects from the interventions were not clear. It seemed that most of the increased cycling appeared from new cycling activity and not from replacement of existing travel with other modes, at least not from non-sustainable modes. From a transport policy perspective, a successful measure should shift transport from non-sustainable to sustainable transport modes. However, from a public health perspective any increase in cycling as a form of active mobility is beneficial, and the largest gains are seen among the least active individuals (Oja et al., 2011). The monetary incentives tested here, and in particular the lottery interventions, were most effective among those who cycled little at baseline.

To conclude, this article shows that monetary incentives are effective at increasing the amount of cycling, when the intervention is carefully administered. The pay per km approach seemed to be as effective or more than the lottery approaches in the short term. However, this effect disappears when we look at the post treatment period: the conditional lottery is the only one with a lasting effect. From a policy perspective, the conditional lottery was the most efficient instrument: it provided comparable effect sizes and a total cost that was fixed and more than 3 times lower than the flat rate. If the interventions were to be scaled up to a large audience, these cost differences would be further accentuated. Based on these results, we believe that the implementation of monetary incentives among a wider public should be encouraged and further explored.

Declaration of Competing Interest

The authors have no financial or personal relationships with other people or organizations that could inappropriately influence (bias) this work.

Appendices. A Additional figures and tables

The following table reports correction rates across treatments for the automatic mode detection app

Table 5
Correction rates across treatments, percentage.

Transport mode	Treatments				Total
corrected *to*	C	PPK	L	CL	
(E-)bike	49	56	50	58	53
Car	12	16	14	10	13
Foot	7	6	8	9	7
Public Transport	27	16	19	18	20
Train	1	1	3	2	2
Other	4	4	7	3	5
Total	100	100	100	100	100

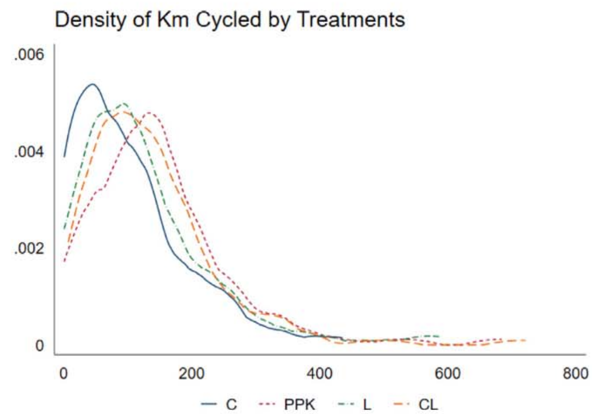

Fig. 8. Kernel density distribution for km cycled during the experimental period (15 days) by treatments. All three incentives had an impact shifting the distributions further to the right compared to that of the control group (solid line).The mean number of km cycled in the control group is 99,while the median is 86 km. The PPK treatment shows the biggest shift(short-dashed line) bringing its mean to 147 km.

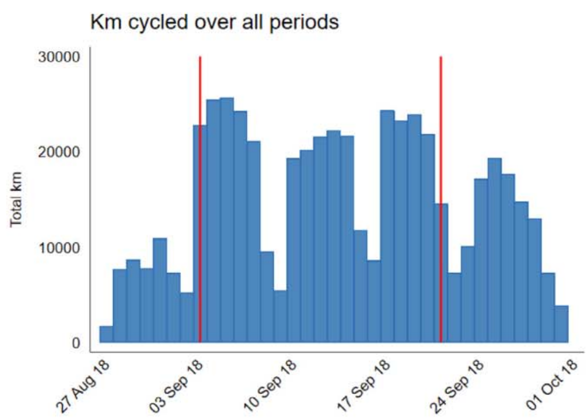

Fig. 9. Total amount of km cycled over all periods. The two red vertical lines indicate the experimental period, where participants in the treatment group were incentivized to cycle. While all groups were incentivized to register all activity with the app until the 30th of September. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Appendix B. Internal validity

Possible threats to the internal validity of this experiment can be linked to problems with randomization and attrition. As described in Section 3.3, the randomization of survey respondents into treatment was done using stratification over city of residence and level of self reported cycling, to ensure balanced groups around variables that are likely to be correlated with the treatment. We performed multiple balance tests on self-reported information gathered from the pre-experimental survey to check whether the control and treatment groups are balanced. Table 6 reports the results of these balance tests[28] and shows no particular indication that large and statistically significant differences occurred.

[28] Each test was chosen based on the type of variable using as resource https://stats.idre.ucla.edu/other/mult-pkg/whatstat/.

Table 6
Balance tests between treatments and control.

Variable	Treatment (4 levels)	Test
City of residence - 4 levels (strata)	*P*-value= 0.280	Kruskal Wallis
Cycling level - 2 levels (strata)	*P*-value= 0.094	Chi2
Age - continuous	*P*-value= 0.293	ANOVA
Gender - 2 levels	*P*-value= 0.186	Chi2
Income - 6 levels ordinal	*P*-value= 0.833	Kruskal Wallis
Education - 4 levels ordinal	*P*-value= 0.107	Kruskal Wallis
Commuting mode - 7 levels	*P*-value= 0.249	Kruskal Wallis
Cycling all year - 2 levels	*P*-value= 0.522	Chi2
Access to e-bike - 4 levels	*P*-value= 0.518	Kruskal Wallis
Recruitment from Social media - 2 levels	*P*-value= 0.982	Chi2

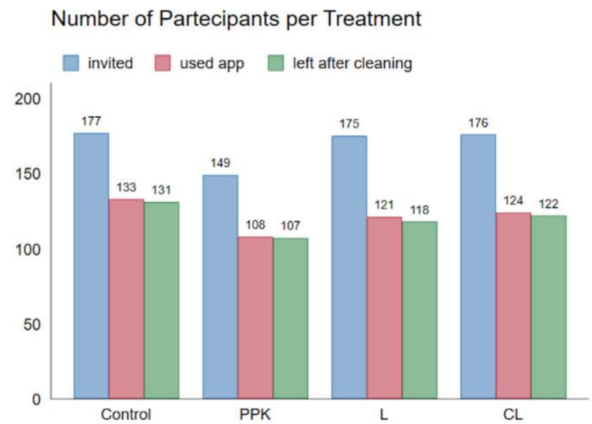

Fig. 10. Number of people per treatment at the different stages of the experiment.

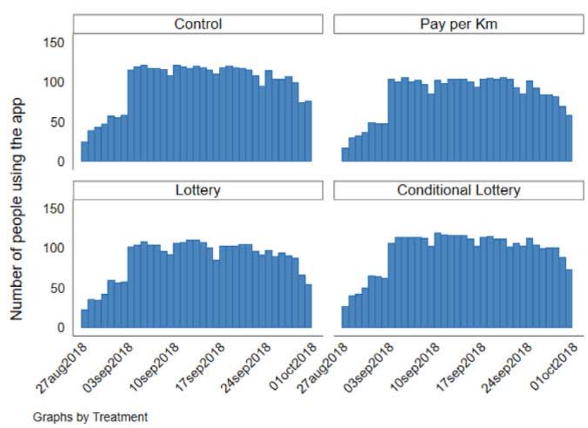

Fig. 11. Number of people using the app per treatment during the experiment.

A threat to randomization can occur if people could choose the treatment they end up in or were exposed to other treatments. While participants recruited by email had a unique link to access the survey and hence could only answer once, those accessing the experiment through Facebook could potentially answer the survey multiple times. Nevertheless, only one app could be downloaded and accessed per registered email and per mobile phone. Furthermore, the access code for the app were sent manually by the researchers checking for double names or emails. Eleven people accessed the survey more than once, probably due to internet connection problems, and hence could have read instructions from different treatments. Of these, three people were randomly assigned to the same treatment twice by chance. To avoid problems, we excluded from the analysis the eight people who could have seen two different treatment.

Fig. 10 shows the total number of people per treatment who were invited and participated in the experiment. A total of 677 people answered the survey and were then randomly allocated in the four groups (blue bars). Of those, a total of 486 people downloaded the app (red bars). After cleaning, a few people have been excluded for the analysis because they either did not understand the instructions, or contacted us and asked to be deleted or, because they accessed the survey twice and were exposed to more than one treatment. After this data cleaning process we ended up with 478 people randomly divided in 4 treatments (green bars of Fig. 10).

Finally, an issue that is important for the internal validity of the experiment is attrition. Participants' dropout after assignment to treatment can be an important weakness, especially for field experiment where the researcher does not have complete control. Drop outs during the course of the experiment can create imbalance between control and treatment groups with respect to participants' characteristics. The experiment internal validity is threatened if those who remain in the treatment groups are different from those who remain in the control group, especially if this difference is correlated with the outcome of interest. In order to avoid this issue, we incentivized the control group as well as the treatment introducing a small lottery for those who used the app the entire duration of the experiment.

From Table 6, we have seen that the sample is well balanced between treatment and control groups in terms of relevant observables. Hence, even if there was attrition, it did not create important imbalances on observable characteristics between treatment and control groups and hence the probability of attrition bias is low. Moreover, Fig. 11 shows that the total number of people registering trips with the app was not reduced within the experimental period, but that drop outs occurred only the last few days of the post-experimental period (after the 21st of September 2018).

We calculated attrition rates for each treatment and control groups as the number of people that stopped using the app before the 21st of September divided by the total number of people who have downloaded the app at the beginning of the experiment.[29] The attrition rate for the control group is 9% as 12 stopped using the app before the 21st of September. The PPK treatment has an attirition rate of 1.9% (2 people), while the lottery has 9.9% (12 people) and CL group a 7.3% (9 people). While the attrition rate between the control and the lotteries treatments is very similar, the attrition rate for the PPK treatment is lower.

To further assess whether attrition plays a role in our experiment, we test for differences in the means of the main observables (the same variables as in Table 6) for those who drop out and those who completed the experiment. Such tests reported no statistical difference implying that those who left were not significantly different from those who remained.

Appendix C. External validity

In this section, we consider issues with external validity, looking at our sample representativeness compared to official statistics from Oslo City Council and the National Transport Survey (NTS 2013/2014). For simplicity and because the majority of the participants are living in Oslo, we are comparing only Oslo residents. Representativeness of the sample in terms of basic socioeconomic traits is reported in the Appendix D.1.

The experimental sample is likely to be self selected in at least two ways. People who answered the survey may have a particular interest in cycling, since the recruiting is based on voluntary participation and the majority of the sample was exposed to a recruiting text explicitly mentioned "cycling" and they had to have access to a bike. Hence, it is possible that the experiment participants are people who particularly like to cycle and do it more than the average. For this reason, we compare our sample's mode share with that of the Oslo residents reported by the National Transport Survey (NTS 2013/2014) in Table 7. The mode share is calculated in terms of number of trips.[30]

Table 7 reports official numbers from the National Transport Survey (second column) and from the Oslo city council (third column), while data from the experimental sample are reported in the fourth (app data) and fifth column (survey data). Looking at the share of cycling, official numbers reports about 6% in share, while the app recorded around 30% of the trips to be done by bike and self reported data suggest even more skewed figure for cycling shares reaching 46%. However, these numbers are calculated for the whole year, while our sample covers only the months of late August and September. The National Transport Survey provides modal share also per season: in Oslo, cycling shares reach 10 and 11% in summer and autumn respectively (Lunke et al., 2018). Compared with official numbers for the same season, the registered share of cycling by app are 3 times larger. However, when considering only people in the control group the cycling shares are around 20%, while it is between 24 and 31% for those in the treatments groups.

Table 7
Transport mode share in Oslo.

Mode	Oslo (NTS 2013-14)	Oslo (City Council 2018)	App	Survey
(e)Bike	6% (11% summer)	6%	30%	46%
Walk	35%	28%	31%	21%
Public Transport	27%	35%	13%	17%
Car	32%	30%	23%	15%
Other	0%	0.8%	3%	0.6%

[29] For assessing attrition we are considering all the 486 people who downloaded the app.

[30] There are different ways of calculating modal splits. The most used one is to sum the number of trips by transport mode and divide it by the total number of trips. Alternatively one can use the amount of km traveled with each mode and calculate shares based on the total amount of km. We use trips to match the National Transport Survey (NTS 2013/2014) and Oslo Cicty council data registry.

Table 8
Population distribution by age groups and gender (2018).

	Women		Men	
Age	Oslo population	Experimental sample	Oslo population	Experimental sample
18–24	9%	5%	8%	5%
25–39	30%	47%	30%	41%
40–54	19%	40%	21%	44%
55–69	13%	8%	13%	10%
N	337 663	129	335 806	149

Note: The data relative to the Oslo population are reported only for the age groups included in the experiment, hence the percentages do not sum to 100. Source: Oslo City Council http://statistikkbanken.oslo.kommune.no/webview/.

Table 9
Population distribution by income groups and gender (2016).

	Women		Men	
Income (NOK)	Oslo population	Exp. sample	Oslo population	Exp. sample
$<$ 100K	11%	6%	8%	4%
100-299K	16%	9%	14%	4%
300-499K	29%	17%	24%	14%
500-699K	27%	45%	23%	34%
$>$ 700K	17%	21%	31%	42%

Source: Oslo City Council http://statistikkbanken.oslo.kommune.no/webview/.

Source: the National Transport Survey (NTS 2013/2014) and Oslo City Council database, Table: travel mode share, everyday travel in Oslo (2009- 2018).

When it comes to the external validity it is difficult to assess whether people that have lower or no cycling activity would react very differently from those considered in this article. On the one hand, we can think that people who do not own a bike or that cannot cycle will not easily move into cycling with a small economic incentive. On the other hand those who can bike, but don't often do so, have a larger margin to improve their cycling so the effect of incentives could be even larger, as we do find in our sample. In addition, an increase in the number of cyclists -both experienced and novice cyclists - may have multiple positive effects. It may help create a new cycling culture by shifting the image of cyclist from "mamil" (middle aged man in lycra) to something suitable for everyone in everyday mobility, as well as contribute to the safety in numbers effect (Fyhri et al., 2017).

C.1. Socioeconomic factors

We compare our experimental sample with official statistics from Oslo City Council. Table 8 shows the share of Oslo residents by gender and age. The experimental sample is over represented by people between 25 and 54 years old, while it is under-represented for the youngest and oldest group. In addition, women are younger than men in our sample. The experimental sample is also richer than the average Oslo person, with a strong over-representation in the income group over 500 thousand Norwegian Kroner (Table 9).

Appendix D. Additional results: subgroups

D.1. Gender

Table 10 shows regression results with the interaction between treatment and gender for the two outcome variables of interest: km cycled per day and number of days cycled. The first three lines of the table indicate treatment effect estimations for men (each treatment compared to the control group). While the effect of monetary incentives for men is (weakly) significantly different from zero only in the PPK treatment for km cycled (column) (1)), and different from zero in PPK and the conditional lottery (column (2)). The coefficient "Women" (forth line) shows the difference between men and women in the control group: on average women cycle 2.76 km less then men per day. The interaction between "treatment x Women" is the difference between men and women in each treatment (non-significant). While the linear combination coefficients reported in the lower part of Table 10 show the treatment effect for women: they are statistically significant for all three of them and for both outcome variables. Table 10 confirms thus the non parametric tests reported in the additional results, showing that women respond to all treatments more homogenously than men.

Table 10
OLS estimation results.

	(1)	(2)
	Km cycled	N. days
PPK	2.714*	1.660*
	(1.180)	(0.671)
L	1.060	0.106
	(1.211)	(0.717)
CL	1.966	1.934**
	(1.201)	(0.658)
Women	-2.761**	-0.876
	(0.973)	(0.785)
PPK × Women	0.406	0.599
	(1.647)	(1.124)
L × Women	1.581	2.029
	(1.606)	(1.104)
CL × Women	1.557	0.696
	(1.524)	(0.991)
City	✓	✓
Age	✓	✓
Income	✓	✓
Number of kids	✓	✓
Social media	✓	✓
Constant	0.530	6.488***
	(1.402)	(1.058)
Linear Combinations		
PPK+ PPK ×Women	3.120**	2.259*
	(1.143)	(0.889)
L+ L×Women	2.641**	2.135*
	(1.007)	(0.830)
CL+ CL×Women	3.522***	2.631***
	(0.959)	(0.738)
Observations	475	475
Adjusted R^2	0.088	0.100

* $p < .05$, ** $p < .01$, *** $p < .001$. Robust standard errors in parentheses.

Table 11
OLS estimation results.

	(1)	(2)
	Km cycled	N. days
PPK	3.006	0.369
	(1.706)	(1.415)
L	3.472	1.530
	(1.808)	(1.629)
CL	2.324***	4.075***
	(0.621)	(1.048)
Cycle often	5.459***	4.511***
	(0.689)	(0.845)
PPK×Cycle often	-0.563	1.396
	(1.938)	(1.514)
L×Cycle often	-2.619	-1.228
	(2.002)	(1.725)
CL×Cycle often	0.0584	-2.520*
	(1.069)	(1.174)
Women	✓	✓
City	✓	✓
Age	✓	✓
Income	✓	✓
Number of kids	✓	✓
Social media	✓	✓
Constant	-2.860*	3.647**
	(1.278)	(1.213)
Linear Combinations		
PPK+PPK×Cycle often	2.44**	1.765**
	(0.906)	(0.516)
L+L×Cycle often	0.854	0.302
	(0.879)	(0.555)
CL+CL×Cycle often	2.382**	1.555**
	(0.892)	(0.514)
Observations	475	475
Adjusted R^2	0.144	0.199

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Robust standard errors in parentheses.

D.2. Level of cycling

Table 11 shows regression results with the interaction between treatment and cycling level for the two outcome variables: km cycled per day and number of days cycled. The first three lines of the table indicate treatment effect estimations for those who cycled little before the experiment (each treatment compared to the control group). Looking at column (1), we notice that the effect for those who cycle little is quite large in each treatment (PKK 3 km, L 3.5 and CL 2.3), but only the CL treatment is significantly different from zero. This is probably due to the low number of participants with a low level of cycling. For the number of days (col (2)), the effect sizes are much lower except for the CL group. The coefficient "Cycle Often" (forth line) shows the difference between the two groups in the control group: on average those with higher pre-level of cycling, cycle 5.3 km more then men per day and 4.5 days more. The interaction between "treatment X Cycle often" represent the difference between the groups in each treatment (mostly non-significant). While the linear combination coefficients reported in the lower part of Table 11 show the treatment effect for people with high pre-level of cycling: they are statistically significant for PPK and CL groups for both outcome variables. Table 11 confirms thus the non parametric tests reported in the additional results.

Table 12
OLS estimation results.

	(1)	(2)	(3)	(4)
	Km cycled	Km cycled	N. days	N. days
PPK	2.422*	2.098	1.810**	1.820**
	(1.107)	(1.066)	(0.670)	(0.606)
L	2.146	2.067	1.424*	1.505*
	(1.192)	(1.146)	(0.674)	(0.635)
CL	2.731*	2.772**	2.580***	2.680***
	(1.108)	(1.037)	(0.582)	(0.545)
City		0.581*		0.786***
		(0.288)		(0.136)
Women		-2.033*		-0.268
		(0.789)		(0.427)
Age		0.0608		-0.0190
		(0.0490)		(0.0264)
Income		0.367		-0.147
		(0.376)		(0.192)
Number of kids		0.246		0.165
		(0.369)		(0.182)
Social media		3.245***		2.116***
		(0.844)		(0.458)
Constant	7.471***	0.584	9.519***	7.670***
	(0.734)	(1.874)	(0.490)	(1.106)
Observations	306	306	306	306
Adjusted R^2	0.013	0.079	0.051	0.185

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$ Robust standard errors in parentheses.

Appendix E. Robustness check: only completed

As robustness check we run the same regression as in the main results, but only with people who have registered activity everyday during the experimental period. About 35% of people have not registered trips every single day. Possible reasons are that they did not leave the house, forgot to take their phone with them, or they did not have GPS and WIFI activated.

The results found for this sample are similar to the ones in the main analysis (Table 2), but the treatment PPK is no longer significant for the amount of km cycled when including the controls (model (2)). another difference with the main analysis is that for the outcome variable "number of days" (model (3) and (4)), the lottery treatment becomes significant at the 95% level.

Appendix F. Supporting information: recruitment text

We herby attach a translated copy of the recruitment text for the different sources and a screenshot of the original Facebook post used for the recruiting (Fig. 12).

Fig. 12. Screen shot of the original Facebook post.

F.1. Facebook

The facebook post reported: "Do you cycle or walk in your everyday life? Answer our survey to help research and even win some money"

Once people clicked on the link, they were redirected to a website page hosted by the Transport Economic Institute (TØI), with the following text:

Do you cycle or walk in your everyday life?

...rarely or often, it doesn't matter...We want to hear from you!
TØI is conducting a study on urban cycling and walking. We investigate what affects people's everyday mobility. More knowledge on this topic may helps the authorities to plan the city you live in. The study is funded by the Norwegian Research Council.

We need travel data from you! We collect data through a survey and a travel app called Sense.DAT. The app is very easy to use. We are interested in people who only want to answer the survey and you who also want to use the app. It takes about 10--15 min to respond. The survey is for all people over 18 who live in or around the city of:

- *Oslo*
- *Bergen*
- *Stavanger*
- *Trondheim*

Everyone who participates in the survey has the possibility of winning a gift card of NOK 5 000. And by using the app you can win more!

CLICK HERE TO START THE SURVEY

Participation is voluntary and you can withdraw at any time. We process information about you based on your consent. The investigation is reported to the Privacy Authority for Research, NSD - Norwegian Center for Research Data AS. Here you can find more information about the app and our support page.

F.2. University of Oslo students email

Hi

TØI is conducting an economic experiment on urban cycling. We investigate what affects people's everyday journeys. This knowledge will help the authorities to plan the city. The study is funded bythe Research Council. You receive this email because you have previously participated in an experiment at the Economic Institute (UiO). If you have already responded or do not wish to participate, please disregard this request.

Would you like to make some money while riding your bike?

We collect data through a survey and use of a travel app called Sense.DAT, which is very easy to use. You get more information in the questionnaire.

Everyone who participates in the survey is involved in the drawing of a gift certificate of NOK 5,000. And if you want to use the app you can win more!

Follow this link to get to the survey: [link]

It takes about 10--15 min to respond. Deadline is Friday, August 31st.

Participation is voluntary and you can withdraw at any time. We process information about you based on your consent. The investigation is reported to the Privacy Ombudsman for Research, NSD - Norwegian Center for Research Data AS.

Âš If you have questions about the study, or would like to use your rights, contact researcher Alice Ciccone (aci@toi.no) at the Institute of Transport Economics (TØI).

F.3. NAF email

How do you travel in everyday life?

The Institute of Transport Economics (TØI) will conduct a study to gain new knowledge about how people choose to travel in everyday life and test good solutions to meet the city's growing transport needs.

We want to collect data through a questionnaire and use an app for a period between August and October 2018. We are interested both in those who only want to answer the questionnaire and those who also want to use the travel app. The app is very easy to *use and once you have downloaded / installed, it automatically records your trips. The questionnaire gives you more information about the app and how to download it.*

The Norwegian Automobile Association (NAF), which is the country's largest consumer organization, would like to contribute to knowledge building and therefore supports the study. You have been randomly pulled from NAF's membership register to participate in the investigation.

By answering our survey, you are involved in the drawing of a gift certificate of NOK 5,000. It takes about 10 min to respond to the form. The deadline is August 31st.

You can take the survey by clicking on the link:

If you have questions about the study, or would like to make use of your rights, contact project manager Aslak Fyhri (af@toi.no) at the Department of Transport Economics (TØI). The project is funded by the Research Council of Norway. Participation is voluntary and you can withdraw at any time. We process information about you based on your consent. The investigation is reported to the Privacy Ombudsman for Research, NSD - Norwegian Center for Research Data AS.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.06.011.

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