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Test-drives & information might not boost actual battery electric vehicle uptake?

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ABSTRACT

Many scholars have suggested driving experiences and information campaigns to promote battery electric vehicle (BEV) uptake. Especially, when BEVs are powered with green energy, they reduce transport emissions, which is vital for mitigating climate change. Nevertheless, BEVs are still adopted far less than conventional cars, which motivates studying whether information on and experience with BEVs changes household car fleet compositions through the adoption of BEVs in a randomised control trial (RCT). A large random sample (N = 4,149) of conventional vehicle (CV) drivers was randomly assigned to one of three conditions: a comprehensive information sheet on BEVs, the same information plus a multiple day-long test drive with a BEV, and a control group receiving no intervention. For the first time, information and test-driving were assessed side-by-side to measure effects on actual BEV adoption. The results do not point towards substantial effects of these interventions after around 18 months. Larger samples or faster growing BEV markets would be needed to find (possible) significant effects.

1. Introduction

To hold the disastrous effects of climate change at bay, all sectors need to decarbonise (Intergovernmental Panel on Climate Change, 2018). Around a quarter of European CO_2 emissions stem from the transport sector and road transport is its biggest contributor (International Energy Agency, 2018). The transport sector is perceived as *more difficult* to decarbonise, due to factors such as the sheer scale of transport flows, the fixed nature of transport infrastructure with its long planning horizons, the entrenched political and economic interests in it and the complicated inter-dependencies with human lifestyle choices (Stern et al., 2006). Multiple car dependencies (Mattioli et al., 2020) reinforce the problem and the associated carbon lock-in (Unruh, 2000). Nonetheless, individual consumer behaviour can play a vital role in reducing emissions, even in slow-changing sectors such as transport (Herberz et al., 2020). For example, consumers could adopt more efficient vehicles, such as Battery Electric Vehicles (BEVs), which are solely powered by electric energy stored in an on-board battery that is recharged from the grid, instead of conventional vehicles (CVs) powered by fossil fuels. The question how to effectively incentivise this consumer behaviour is the main question of this paper. To answer this, I empirically estimate the causal treatment effects of exposure to electric vehicle driving and information about BEVs on actual adoption of BEVs.

This paper explores one option for encouraging conventional car consumers to engage in transport decarbonising through the uptake of battery-electric vehicles instead of CVs. As they do not have to give up cars with their associated benefits (e.g., convenience, luggage space and privacy as depicted e.g., in Gärling and Schuitema, 2007), this change is assumed to be easier than avoiding travel or using active modes or mass public transit. Nonetheless, BEV uptake is much lower than conventional vehicle uptake, especially in

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areas without strong policies to promote BEV adoption (e.g., Hardman, 2019; Narassimhan and Johnson, 2018; Wang et al., 2019; Lieven, 2015; Sierzchula et al., 2014; Brückmann et al., 2021). The transport sector's negative externalities (for an overview see e.g., Sovacool and Kim, 2020) and BEVs additional positive externalities, such as reduced urban noise emissions (e.g., Campello-Vicente et al., 2017) and local air pollution (e.g., Requia et al., 2018), motivate potential governmental interventions. The (IEA, 2021) estimates that an 18-fold increase in BEV uptake is needed by 2030 to reach *net-zero* by 2050. Many scholars argue BEV trialling and information are key elements to their wide uptake (e.g., Herberz et al., 2020; Ye et al., 2021; Wang et al., 2018; Kim et al., 2019; Thøgersen and Ebsen, 2019; Sierzchula et al., 2014; Santos and Davies, 2020; Carley et al., 2019), often argued to follow diffusion of innovation theory (Rogers, 2003) or theory of planned behaviour (Ajzen, 1991). Here, the effectiveness of this two potential low-intrusive policy options are tested in a field-experiment.

This paper reviews previous studies with test-drives and argues, why some improvements are needed, before an original field-experiment is presented. For the latter, I randomly assign a large random sample of CV user to two interventions (information or trialling) regarding BEVs and a control group. Around 1.5 years after receiving the treatments, both groups were compared with each other and the control group on the main dependent variable, actual BEV uptake. Here, the main outcome of interest is whether participants actual have a BEV in their household. As this study assesses actual BEV uptake it goes beyond previous studies, which only assess stated intention to adopt BEVs. This is the first study ever using an RCT to study actual BEV adoption. In contrast to stated-preferences, revealed preferences (here: actual adopting a BEV) are associated much higher costs both in terms of actual financial costs and those of the forgone alternatives. While the results on BEV uptake of both treatments are positive, a larger sample or longer time-frame is needed to observe the effects of randomised test-driving periods on actual BEV adoption.

The scientific community is advised to use this paper as the learning experience on how to implement such a study that I recommend repeating similarly. Taken together, it might be too early to close the debate whether BEV information and trialling are important policy measures to increase BEV uptake and more research, employing causal identification strategies like this, is needed.

The paper is organised as follows: The next section discusses the background and theoretical expectation; thereafter, the case as well as data and methods are presented; after the results are presented, they are discussed and the last section concludes this article.

2. Background

The adoption of new technologies is often slow in the beginning, as new technologies need behavioural change (Haustein and Jensen, 2018). Green, or more precisely, energy-efficient low-carbon technological innovations tend to be associated with a higher upfront initial price and lower running costs (Hagman et al., 2017). An excellent example in the area of transportation is the electric vehicle. The current uptake of BEVs is still lower than the uptake of conventional vehicles. In the European Union, only 5.4% of new registrations in 2020 were BEVs (Transport&Environment, 2021). So BEV's technological availability alone is insufficient for decarbonising transport. They need to be adopted and used by consumers (Carley et al., 2019; DellaValle and Zubaryeva, 2019) to work towards this goal.

Low BEV uptake can be attributed, inter alia, to the lack of knowledge (e.g., Long et al., 2019; Wang et al., 2018). Other obstacles to BEV adoption are e.g. low driving ranges, long recharging times, little access to charging infrastructure, high purchase prices, discouraging dealerships, and many more, which are systematically discussed elsewhere (see e.g., Rezvani et al., 2015; Coffman et al., 2017; Liao et al., 2017; Li et al., 2017; Pettifor et al., 2017; Hardman et al., 2018; Daramy-Williams et al., 2019; Kumar and Alok, 2020; Rajper and Albrecht, 2020; Singh et al., 2020; Wicki et al., 2022). Here the focus is on the lack of information (e.g., on lower running costs, charging infrastructure, acceleration etc.) and on misperceptions (Axsen et al., 2017), e.g., about BEVs' usability (Rajper and Albrecht, 2020). Therefore it is necessary to fill knowledge gaps among CV users (Haustein and Jensen, 2018; Rezvani et al., 2015). For the adoption and use of BEVs, it is vital that consumers know the advances in BEV technology and charging infrastructure (Haustein et al., 2021). Almeida Neves et al. (2019) stress the importance of communicating the reduced environmental impacts from BEVs to potential consumers and Carley et al. (2013) stresses the need for educational campaigns targeting the reduced running costs of electric vehicles. Taken together, many scholars stress the importance to fill knowledge gaps through information in order to accelerate BEV uptake.

A very specific form of information is direct experience, as it provides multi-sensory information. One form of direct experience are vehicle trials (i.e. test-drives). Trials might increase consumers' knowledge about BEVs and reduce prior uncertainties about the new technologies and their ease of use, benefits and drawbacks (Thøgersen and Ebsen, 2019). Following *diffusion of innovation theory* (Rogers, 2003), trialability has a direct influence on BEV adoption decision (Carley et al., 2019). The *theory of psychological distance* (Liberman et al., 2007) can explain that trials can make previously distant concepts more psychologically close through hands-on experience. Taken together, using these different channels, the literature on innovations finds *t*rialability to be a strong determinant of the speed of innovation spread (Gärling and Thøgersen, 2001; Petschnig et al., 2014; Rogers, 2003; Carley et al., 2019). Trialability is described as experiencing the vehicle in (daily) use (Schlüter and Weyer, 2019). Experience reduces prior perceived complexity (Tomic et al., 2020) in other new, energy-efficient technology contexts, too. Therefore, trialability has a vital role for adopting (and intention to adopt) high-involvement purchases, such as cars (Molesworth and Suortti, 2002). Possibilities to experience electric vehicles through test-drives are strong leverages for adoption through car consumers (Ensslen et al., 2016). The provision of experience with a previously unfamiliar technology makes purchasing more likely among consumers with stronger environmental motives (Labeye et al., 2016; Schmalfuß et al., 2017). Experience is directly related to a higher likelihood to purchase (Kim et al., 2019). While Li et al. (2017) acknowledge that previous results on the impact of experience are mixed, see also Table 1, Li et al. (2017) confirm that generally, experience is understood as positively influencing BEV purchase (intentions).

Table 1
Previous BEV test-drive experiments (all stated-preference studies).

Study	n (test-drivers)	Duration test-drive	Sample Selection	Control group	Area	Car type (if provided)	Dependent Variable	Results
Bühler et al. (2014)	79	6 months	>1200 applicants, 80 selected	no	GER (Berlin)	Mini E	willingness to buy	insig. pos.
Degirmenci and Breitner (2017)	167	15 min ≈ 6 km, accompanied	invitation through social networks, than random sampling	no	GER (Hanover)	small passenger city car	attitudes on EVs	sig. pos.
Franke and Krems (2013)	79	3 months	print and online media advertising yielding > 1000 applicants	no	GER Berlin)	Mini E	range preferences	sig. decreased
Graham-Rowe et al. (2012)	20 BEV (+20 PHEV)	7 days	ICE drivers willing to participate in transportation research	PHEV users	UK (Berkshire, Hampshire, Surrey)	Mitsubishi i-MiEV, Citroen C1 EVIE	–	(qualitative)
Hinnüber et al. (2019)	114	10 min, accompanied	(not explicitly stated)	no	GER (NRW, Münster)	Volkswagen E-UP and E-Golf	willingness to buy	sig. pos.
Jensen et al. (2013)	369	3 months	press announcement, selection upon certain criteria (e.g. home-charging possibilities)	no	DK	Mitsubishi ImiEV, Citroën C-Zero, Peugeot I-on, Citroën C1 EVIE	vehicle choice	more range wanted
Labeye et al. (2016)	36	6 months	>900 applicants, 50 chosen	no	FRA (Paris)	Mini E	range management	improved
Schmalfuß et al. (2017)	30	24 h	print and online media advertising, than selection	no	GER	Mini E	purchase intention	marginal sig., small pos.
Skippon and Garwood (2011)	58	10 miles or over-night ($n = 20$)	Eon workers in the UK	no	UK	Mitsubishi i-MiEV	–	no before/after comparison
Skippon et al. (2016)	393 (1/2 in EVs)	36 h	advertising and volunteer pool	yes, conventional car	UK	medium family hatchback	purchase intention	sig. neg.

Table reports published studies with BEV test-drive trials from 2010–2020. The main inclusion criteria for reporting in this table were BEV trials without BEV owners, were participants drove the BEVs. Both studies with within-subject and between-subject design were included. The column "Dependent Variable" reports the outcome of previous studies, and if multiple outcomes were assessed, the outcome closest to this study. "Results" depicts the associated results from the BEV driving experience.

The following paragraphs give a focused overview of the state of knowledge on the role of information and test-drives on BEV uptake. While e.g., trialling congestion charges are a prominent example of the positive causal effects of trialling on acceptance in transport studies (e.g., Schuitema et al., 2010), the broader literature on the uptake of new technologies through experience cannot be transferred easily to BEVs. BEVs, or more generally, cars offer specific perceived advantages (e.g., transport, privacy, luggage space, status and emotional factors) (Gärling and Schuitema, 2007) that other technologies do not necessarily have. Therefore, I selectively review studies on electric vehicles with a focus on causal effects of information and experience. Drawing on these previous studies, I argue how methodical decisions might lead to inconclusive results and that we need to focus on the interventions' effects on actual adoption (revealed preferences), instead of solely intention to adopt (stated preferences).

Previous research on whether more information on BEVs indeed leads to higher uptake of BEVs is relatively scarce, even though information campaigns are considered important non-pecuniary policy measures (e.g., Brückmann and Bernauer, 2020; Yang et al., 2019). While the available literature finds positive correlations between knowledge and purchase intentions (e.g., Kim et al., 2019), it is mostly non-experimental and therefore cannot examine causal effects of information. Providing information about fuel-costs and total-cost-of-ownership in a survey experiment lead to higher interest in purchasing different kinds of EVs, at least for some consumer groups (Dumortier et al., 2015). Therefore, many scholars call for further research on providing detailed information for emerging, energy-saving technologies, with high up-front costs and low running costs, such as BEVs (e.g. Dumortier et al., 2015; Thøgersen and Ebsen, 2019; Wang et al., 2018; Liu et al., 2020; Hinnüber et al., 2019). However, no study has yet analysed actual BEV adoption instead of BEV adoption intention after such treatments.

Similar to information, previous studies examining how direct experience with BEVs, could change consumers' preferences, are also rare and so far somewhat inconclusive (Daramy-Williams et al., 2019). Previous studies exploring the relationship between BEV driving experience and BEV adoption can be separated into two different strands: First, studies that enable test-driving and studies analysing actual adopters. The latter is by definition, always non-experimental, as the processes that leads to the adoption of a BEV is already over when this population is studied. An overview of both types of studies can be found in a comprehensive review by Daramy-Williams et al. (2019). I focus on the first strand of literature and exclusively review only studies that use an experimental component and provide actual test-driving experiences with BEVs. The latter means that participants had to drive the cars themselves in order to be included in this review. This excludes studies like (Roberson and Helveston, 2020), where participants were only in the passenger seats.

For an overview of experimental studies with test-drives, see Table 1. Usually, in previous studies with BEV trialling sample size was relatively small, between 20 and 369, mean 105 (see the second column of Table 1). This means that researchers expected and therefore powered to observe large treatment effect. In most previous studies, participants were invited to join the experiment through mobility-research participant pools or through advertisements of the study (see the fourth column of Table 1). Therefore, some doubt that these previous studies analysed mainstream users (Daramy-Williams et al., 2019). In terms of study design and analysis, most studies relied on an intervention design, comparing the same respondents (within-subjects) before and after the trial (see the fifth column of Table 1), and usually no control group was used (with the notable exemption of Skippon et al. (2016)). The lack of control groups is a reoccurring problem in transport research (see also e.g., Tørnblad et al., 2014). The last two columns of Table 1 explore the main dependent variable and display the heterogeneous results obtained.

Methodological, the present study is closest to Skippon et al. (2016) through the use of randomisation and the use of a control group (see column 4 Table 1). They compare the effect of random assignment to the experimental or control group. Members of the experimental group drove a BEV, while members of the control group drove a conventional car (Skippon et al., 2016).

The main differences from this study are the study populations, and how the control groups were treated (here: not at all, see Section 4) and which dependent variables are assessed. All previous studies assessed only intention to adopt (stated preferences), not actual adoption (revealed preferences). So far, all studies assess behavioural intention (stated preferences), as a close antecedent of behaviour (e.g. BEV adoption). This argument follows the Theory of Planned Behaviour (Ajzen, 1991). Observing this prior lack of revealed preference studies, She et al. (2017) encourages further research with actual experience using revealed preferences. This study uses revealed preferences, even though the decisions in SP and RP studies are likely to be determined by similar factors (Schuitema et al., 2013). However, SP studies are usually biased due to gaps between stated preferences and actual behaviour. Behavioural intentions might have been found to be the best available proxy for behaviour (Ajzen, 1991; Ajzen et al., 2011), there remains a substantial gap between intention and actual behaviour. Therefore, RP studies are usually favoured by policymakers (Carlsson, 2010).

Based on these previous findings, the abundant claims in previous research, and the postulation that behavioural intention leads to behaviour (Ajzen et al., 2011; Ajzen, 1991), I expect the following hypotheses to hold true:

Hypothesis 1 In this experiment it is expected that both treatments (information alone and information plus test-drive) positively influence the purchase of BEVs in contrast to the control group.

Hypothesis 2 The effects of the information plus test-drive treatment are expected to have a larger, positive effect on the revealed preferences (actual BEV adoption) than the information treatment.

Information can positively influence purchase intention when the popular claim that there is little knowledge of BEVs holds. Similarly, test-driving will increase the probability of adopting BEVs if the test-drive provides a new, decisive experience. Trialling's expected positive effects are in line with the observation that dealerships actively encourage potential car buyers to test-drive prior to purchase. Moreover, in some jurisdictions[1] electric vehicle test-drives for the mass-public are already a policy measure in place. According to previous research (e.g. Barth et al., 2016) experience can have more positive effects than only knowledge provision about BEVs regarding the intention to use an EV. Therefore, the test-drive with information treatment is expected to result in a stronger positive effect on actual adoption behaviour than information alone, too.

3. Case

The case for this experiment is Switzerland, a wealthy country located centrally in (Western) Europe. This case is one in which the effects of information and exposure are medium likely to affect BEV. Some factors are positively related to BEV uptake like high environmentally concerned population, and a relatively cheap renewable energy mix. At the same time, low rates of single-family detached home ownership and few policy incentives for electric vehicle adoption are inhibiting quick mass-uptake of BEVs. This section discusses factors supporting and inhibiting BEV uptake in Switzerland.

Switzerland is a small country with fast adoption of new technologies (Herberz et al., 2020). Swiss inhabitants on average hold high levels of environmental concern (Tranter and Booth, 2015; Franzen and Vogl, 2013), also compared to other countries, which can constitute a stronger social norm to reduce transport energy use and emissions. In Switzerland, there is an increasing charging infrastructure network as well as generally short travel distances (Herberz et al., 2020). These distances are within BEV range 99% of the time (Melliger et al., 2018). In addition, Switzerland has excellent public transport access (Petersen, 2016), also in rural areas. This leads to one of the highest rates of public transport usage in Western Europe and North America, measured as the number of annual public transport trips per capita (BFS, 2020a). This demonstrates that convenient modes of sustainable transport are taken up by the Swiss as well as that trips beyond BEV range could be (partially) achieved via public transport instead of conventional cars.

Another positive factor for BEV uptake is high income (IMF, 2018) as well as low electricity prices, that enable operating BEVs at low costs. Swiss electricity prices are among the lowest in Europe (Global Petrol Prices, 2021), which makes driving BEVs cheaper. Furthermore, energy is generated mostly from renewable sources. As of 2019, 58 % of electric energy in Switzerland were from renewables, mostly hydro-power and solar, excluding nuclear, which is also in the Swiss electricity mix (SFOE, 2020). Swiss citizens support renewable energy to phase out this nuclear energy (Plum et al., 2019).

The uptake of BEVs is negatively influenced through the high share of people living in flats and rented housing (Eurostat, 2021), as current BEV adopters strongly prefer home-charging (Hardman et al., 2018; Patt et al., 2019). Home charging is convenient and easier to be achieved for those owning the place their car parks. So far, there are only very little policies in Switzerland in place that aim at increasing BEV uptake. The absence of policies is usually related to lower shares of BEV uptake (e.g. Barton and Schütte, 2017; Sierzchula et al., 2014; Hardman, 2019; Narassimhan and Johnson, 2018). Additionally, some previous research has identified that the absence of BEV manufacturers, as in Switzerland, tends to be negatively correlated with electric vehicle uptake (Sierzchula et al., 2014; Meunier and Ponssard, 2020).

By the end of 2020, not even 1% of the stock of cars in Switzerland has been battery-electric (BFS, 2020b). This shows the potential for further uptake. While Switzerland might appear as a most-likely case for BEV adoption through high income, short journeys and cheap electricity coupled with environmental concern, the absence of policies and home recharging opportunities for renters and apartment-dwellers, might make it a less-likely *middle case*. In terms of vehicle age, an indicator for fleet-turnover,

[1] For example, the German state of Baden-Württemberg offers free BEV trialling opportunities for its citizens, see www.eauto-ausprobieren.de, last checked: March 12, 2021 20:19.

Swiss cars are on average 9 years old (FSO, 2021), which is similar to the European Union's average of 11.5 years (ACEA, 2021). Switzerland is therefore in between Luxembourg, with the youngest cars in Europe (6.5 years) and Estonia, Lithuania, and Romania, with the oldest fleets (> 16 years) (ACEA, 2021). Overall, this is to say that Switzerland is neither a best nor a worst case scenario.

For this study, only a part of German-speaking Switzerland (Aargau, Schwyz, Zug and Zurich) was selected, conditioned on the cantons' proximity to the test-site for the experimental treatment and same language for survey facilitation. These areas consist of both rural and urban areas, and each canton has slightly different vehicle taxation policies — however none has direct subsidies for BEV purchases. The cantons selected include Zug, with the highest (698) and Zurich (484) with the third-lowest shares of registered vehicles per 1000 inhabitants among all Swiss cantons (SFOE, 2021). As the sample of car holders is roughly similar to the Swiss micro census on mobility and transport, the findings from this study could also be relevant to other German-speaking parts of Switzerland. Transfer to other parts of Switzerland, with their different in vehicle cultures (Filippini and Wekhof, 2021), and abroad are possible when contextual factors are considered.

4. Data and methods

4.1. Sampling

This research relies on original panel data, derived from Swiss authorities. For this study, the postal addresses of 20,000 CV drivers (that personally had no BEV registered) were transferred from each canton's car registry to the research team. 5'000 registered car holders from each of the Swiss cantons of Aargau, Schwyz, Zug, and Zurich were invited by postal mail to take part in a survey. This data-transfer followed local data protection law.

The decision to only invite car-holders followed the assumption, that travel behaviour is usually habitualised (e.g., Schlich and Axhausen, 2003; Kurz et al., 2015) and therefore it is of most interest to see how the experimental interventions changed these path dependencies. Also, it provided a possibility to select people without BEVs. Moreover, inviting car holders aimed at inviting people with driving licences, which would be needed for the test-drives. As there are no sampling frames for people with driving licences available in Switzerland, sampling from registered car-holders served as a proxy.

Earlier research has shown that sampling from pure online samples can be skewed, e.g., with respect to younger and more technophile respondents. To maximise take-up by different consumer groups, a dual-mode survey (online and "pen-and-paper"), was offered and served as a baseline survey. At the beginning of the baseline survey, all participants gave their informed consent to take part in this study, which was approved in full by the institution's ethics commission (EK 2017-N-85).

Of these 20,000 invitees, 4,149 took part in the dual-mode baseline survey. To encourage participation, two postal reminders were sent out. The survey was not incentivised and it took around 30 min to complete. Following (Poortinga et al., 2004), socio-demographic covariates are advised as transport energy use is dependent on socio-demographic characteristics. Therefore, all the main covariates, such as socio-demographic variables, car usage, and preferences, attitudinal scales and (electric vehicle) policy preferences were assessed in this baseline survey. These control variables were also assessed to analyse which consumer groups take up test-drives.

4.2. Experimental design

During the baseline survey (at its very end) the randomised-control trial (RCT) started. All participants were randomly and entirely independent of any previous data entries assigned to one of three experimental treatment groups, see Fig. 1. Due to the substantial investment in respondents time, consensus for a (potential) test-drive with an unspecified type of car was already sought for at the end of the first (baseline) survey. It was not mentioned the cars were electric (or of any specific drive-train). However, it is possible that respondents might have guessed, that the test-drive were with an electric vehicle, as the baseline survey already had multiple items on BEVs. Those who objected this initial treatment offer were re-randomised into the other treatment group or into the control group. The estimation procedure, will take this into account, see below. Consent for entering the second and third panel wave was sought at the very end of the baseline survey (wave 1). Between the first and the second survey wave, the treatment was administered, if it included test-drives. The information treatment (alone), a comprehensive overview about BEV facts, was distributed with the second survey to the relevant group. This paper reports on the last survey wave, follow up survey 2. In this survey, household car fleet composition was examined for all respondents. The original survey instrument is available online: https://osf.io/7ap8z/?view_only=bfdda4be26d94ea19e1d11b4be0cf3f8. The main dependent variable of interest is whether, a BEV was present in respondents household at the end of the second follow up survey.

The second follow up survey took place between October and November, 2020. This survey was online-only, due to the COVID-19 situation with unforeseeable consequences with regard to contamination fears among those who might have received a print version, and the unforeseeable (home) office situation of the research team. In total 1,908 full and partial responses were collected in wave 3. Out of these, 597 had initially been assigned to the treatment "test-drive with information" and 180 have received it and took part in the last survey wave. With a power of 80% I can detect effect sizes of around 7% on the 90% level of significance. 669 participants were assigned to the information treatment, as well as, 642 to the control group. After re-allocating those who did not want to test-drive or who could not get a spot for the treatment with test-drives, 807 received the information treatment and 921 were in the control treatment.

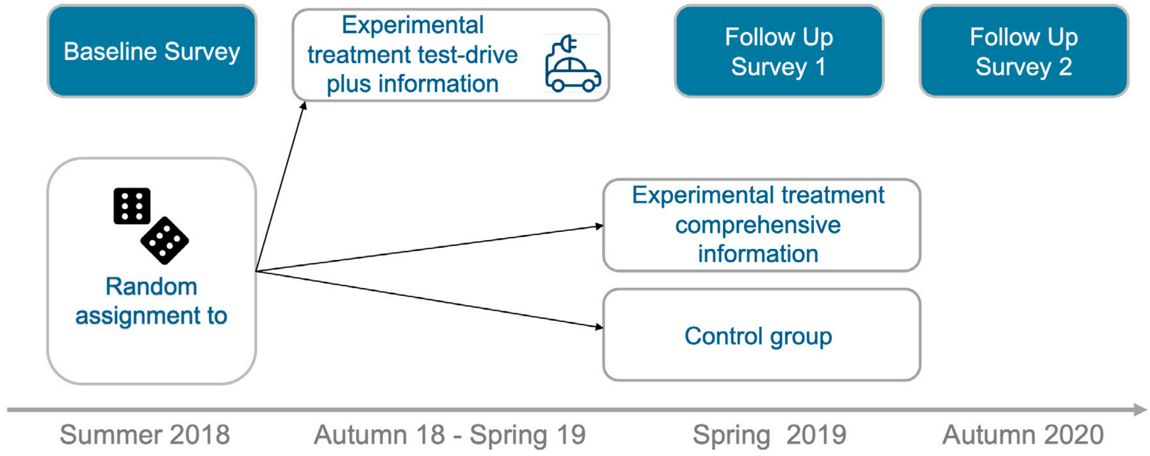

Fig. 1. Study overview. Within the baseline survey, all participants were randomised into one of three groups. Test-drive treatment started already before the other two treatments, as the time needed to deliver many test-drives was long, due to limited number of cars available. The information (only) treatment was delivered at the same time, as the control group, as it was administered directly with the first follow up survey. All respondents were invited for the last follow up survey at the same time.

4.3. Treatments

The information treatment, that was utilised in both experimental treatments, is motivated by different factors that were previously identified as misperceptions and barriers to BEV adoption. The information consisted of information regarding characteristics car-buyers typically pay attention to. They included financial, environmental and practical information. The original information treatment can be found in Supplementary Information 1.

The financial aspect in the information treatment is motivated by the fact, that the higher purchase price for BEVs is often seen as an obstacle of purchase and consumers associate these vehicles with being not aligned to their financial motives (Herberz et al., 2020). Partly, this could be explained by the under-estimation of (higher) CV running costs (Andor et al., 2020) and neglecting or non-awareness of the reduced BEV running costs (Biresselioglu et al., 2018). The information sheet stated, that while BEVs are more expensive to purchase, they are cheaper than CVs to run.

Environmental information was mostly centred around the fewer energy losses of BEVs in contrast to CV and regarding reduced CO2 emissions (Hoekstra, 2019) over the whole life cycle, especially when BEVs are powered with renewables. Lastly, reduced noise and emissions from braking, through regenerative breaking, were mentioned.

Practical information included a rough estimate of the range of average 2019 BEV models, of around 300 km. This was included to keep participants informed on advancements in battery technology and in order to reduce range anxiety, which was previously found to be a major barrier to the adoption of BEVs (e.g., Melliger et al., 2018; Rauh et al., 2015). Similarly, information on charging time and a map with public charging stations, as well as websites with this information, were included. This information was also included as it was expected to reduce range-anxiety and demonstrate preceding network growth. Moreover, it maybe had the potential to change participants' perception of recharging infrastructure abundance. This information also enabled behavioural adaption with regard to the test-vehicle (for respondents who received the test-drive with information treatment).

The treatment including test-drives was an actual multi-day test-driving period with one of seven different 2018/2019 available BEVs. The test-drives took place from November 2018 to Mai 2019, which intentionally included the winter and allow electric vehicle usage in colder temperatures. The allocation of BEVs was random, except when participants had important specific needs e.g., maximum size restrictions to fit the BEV in their garage. The test-drive started at the study's institutional garage, where respondents got a random BEV and verbal introduction into that specific car as well as the aforementioned information sheet. Subjects drove accompanied by a student research assistant and were allowed to ask questions on the vehicle operation, before starting their unaccompanied two to three days test-driving period. (Invitations stated test-drive for 48 h, however, the research assistants arranged times on demand. This included some shorter ones, if above 24 h, and longer ones over the weekends and public holidays.) Expenses for travelling privately to do the test-driving opportunity were lump sum reimbursed with CHF 20.

4.4. Method

In the baseline survey, assignment to any of the treatments or into the control group was random — however, the actual treatment received varied with participants interest in the test-drive. Participation in the test-drive treatments was voluntary. When respondent's indicated unwillingness in the baseline survey, a test-drive was never offered to them. I performed additional analysis regarding, who was willing to accept the offer for a test-drive in Supplementary Information 3.1. Reasons for unwillingness to be invited into a test-drive was not systematically assessed, however from information in an open text-field at the end of the survey, I

Table 2
Descriptive statistics: Households with BEVs in wave 3 by treatment received.

Treatment received	Test with Info	Information	Control group
Number of households with BEVs	8	24	25
Mean	0.044	0.029	0.027
Standard deviation	0.207	0.169	0.163
Observations in group	180	807	921

learned that some respondents felt uncomfortable to pick-up and drive a car in the city of Zurich and/or that they felt too old and want to give up driving. Note that, in 2018, Swiss licence holders above 70 years of age had to get a bi-annual medical check up to continue driving. In 2019, the age for this procedure was elevated to 75 (Swiss Federal Council, 2018, 2017).

Test-drive appointments were made in a random order only with those from the assigned treatment group, who indicated a possibility that they might take part in a test-drive when offered. However, some did not take it up and at some point, we ran out of possible spots for the rather lengthy (on average 198 km and 56 h) test-drive opportunities.

The voluntarily nature of the experimental treatment assignment is likely to lead to imbalances between those who are willing to test-drive an unknown vehicle and those, who do not wish to do so, also after the consent to be invited to the test-drive. This imbalance is accounted for in this study. Therefore the intention to treat (ITT) and Local Average Treatment Effects (LATE) (Angrist et al., 1996) are estimated. ITT serves as a lower-bound estimate of the treatment effect, as it is the ordinary least squares (OLS) regression of the assigned treatment on the dependent variable (BEV in household at second follow up survey). Here I use the baseline assigned treatment (before the sentiment towards the test-drive was assessed among those initially assigned to it) as an instrumental variable (IV) to instrument the (later) received treatment. This takes into account that some do not want to or can comply with the treatment. Note, the instrumented, independent variable is treatment received. That means, that treatment (i.e. information or information and test-drive) was received. It is still possible that some did not read the provided information or that test-drives violated the protocol by letting other people than themselves drive.

The model used is depicted below. The outcome Y is a binary indicator, that is 1 (0 otherwise) for a BEV in household at the last survey wave. Each participant is denoted with i. $\boldsymbol{X}$ refers to a vector of socio-demographic covariates, that can be empty. Treatment_1 is a dummy for information with test-drive, while the Treatment_2 - dummy reflects the information treatment. Therefore, the omitted category, where both Treatment_1 and Treatment_2 equal 0, is the control group. Both of these treatment indicators need to be instrumented to estimate LATEs.

$$Y_i = \delta^{ITT}\text{Treatment}_1 + \gamma^{ITT}\text{Treatment}_2 + \beta_i \boldsymbol{X} + \epsilon_i, \quad (1)$$

Measuring the effect of the treatment, for those who ended up receiving a specific treatment, is referred to as LATE. Here treatment indicators are instrumented with prior assignment of treatment. These are obtained using IV regressions. The first stages are as follows:

$$\text{realisedTreatment}_1 = \psi\text{Treatment}_1 + \gamma\text{Treatment}_2 + \omega_i \boldsymbol{X} + \mu_i, \quad (2)$$

$$\text{realisedTreatment}_2 = \psi\text{Treatment}_1 + \gamma\text{Treatment}_2 + \omega_i \boldsymbol{X} + \mu_i, \quad (3)$$

and the second stage

$$Y_i = \delta^{LATE}\widehat{\text{realisedTreatment}}_1 + \gamma^{LATE}\widehat{\text{realisedTreatment}}_2 + \beta_i \boldsymbol{X} + \epsilon_i. \quad (4)$$

The choice of the specific functional form of linear regression, for a binary outcome, is due to the "forbidden regression problem" (Wooldridge, 2010, p.236), that arises when non-linear functions of endogenous independent variables are replaced "with the same nonlinear function of fitted values from a first-stage estimation", potentially leading to inconsistent estimates. All regressions use robust standard-errors and IV also uses the small-sample correction and are performed using Stata 15. An alternative method, matching, is discussed in Supplementary Information 3.2.

5. Results

5.1. Experimental result

While Table 2 provides descriptive results, Fig. 2 displays the main results of the between-subject design used to elicit the treatment effects on BEV uptake. The upper part shows the OLS results, the intention to treatment (ITT), without control variables, while the lower part displays the IV results, the Local Average Treatment effect (LATE). Here, no control variables are used. The main effects are positive, but very small and insignificant. The OLS point estimates for "Test with Info" and "Info" are .017 and .0035, respectively. For the IV regression, they are .053 and .0036, respectively, confirming that OLS can serve as a lower bound. The intention to treatment for the test-drive with information case is basically the same as the *"treatment"* of receiving a survey question in the first survey wave regrading if one would take part in a test-drive when offered (without the mentioning it would be with a BEV). The OLS estimate can be interpreted as the (small and insignificant) causal effect of offering test-drives to all.

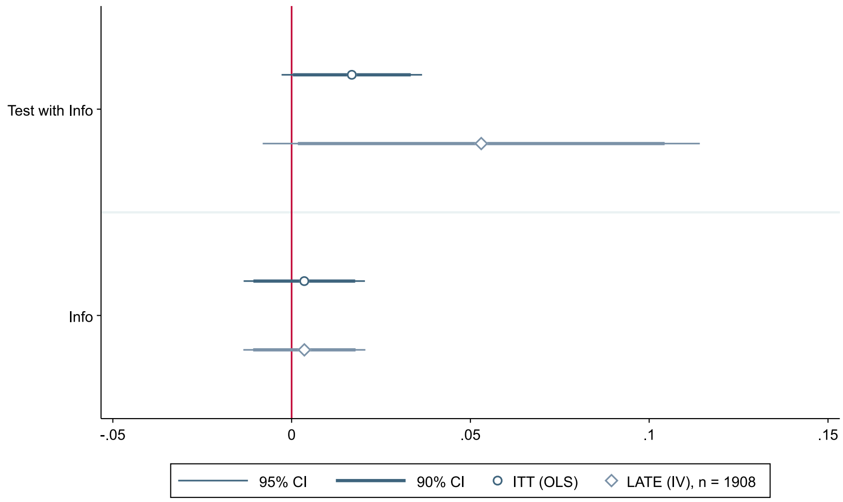

Fig. 2. Overall main experimental effects of both treatments on BEV adoption (linear probability of stated BEV in household in wave 3), without any control variables. Both regressions use a linear specification (linear probability model) with robust standard errors. The upper values represent results from ordinary least squares (OLS) regressions (darker lines with circles), that represent the intention to treat (ITT). Below, the instrumental variable (IV) estimates are displayed (lighter lines with diamonds), showing the Local Average Treatment Effects (LATE) estimates. Instrumental variable regressions use small sample correction. The (red) vertical line at zero represents the control group. Stronger horizontal lines indicate 90%-confidence intervals (CIs) and thinner lines 95%-CIs. A table representing this graph can be found in Table SI 5.

Practically identical values for the coefficient for the information treatment in both, the OLS and IV in case, are by design. Those that were already assigned to this treatment before new respondents were randomised from the other assigned treatment (with test-drives) condition stayed in this group — the overlay is high. These main results are very similar to the alternative specification using matching in Supplementary Information 3.2.

5.2. BEV ownership before and during the field-experiment

There are a few outliers in the data, that require a more in-depth analysis. In the first (baseline) survey wave some people had BEVs in their households. 25 out of 4,149 stated they had a BEV in their household in the first wave. This does not violate the sampling, as they might have acquired the BEV after sampling by the car registries or a BEV in their household was registered on someone else (e.g. on another household member or on their company). While this share of BEVs (0.6%) in wave 1 is higher than the share in the general Swiss car fleet at that time, it is conceivable that who just purchased a new vehicle or who had a new technology battery-electric vehicle might have been more inclined in taking part in a survey labelled as “Mobility behaviour and future mobility politics”. While the wording in the survey was carefully designed to avoid confusion of BEVs (always explained as “a car, exclusively powered with energy stored in a battery” with other plug-in vehicles, it is impossible to fully rule this possibility of respondents’ confusion out. This information on having a BEV did not alter the randomisation as it was completely neglected in the randomisation. Unluckily, these 25 respondents are not equally distributed among the three intention to treatment groups. 14 (1.04%) were assigned to test-drive with information, 6 (0.41%) to information treatment and 5 (0.37%) assigned to the control group.

When inviting those willing to do a test-drive to the actual test-drive three persons declined to test-drive, because, they said they already owned a BEV (this revealed another BEV owner, who did not state or have (indistinguishable) the BEV in the first wave). While the research team was instructed not to reveal the drive-train of the test vehicles, the team obeyed the subjects’ wish and abstained from encouraging them to take the test-drive, they had been invited to. As all others who did not take up the test-drive, they were randomised again into either control or the other treatment group (information only). However, through data gathered during the third survey wave, especially through asking about when the (latest) BEV was acquired, this data from previous waves can be enhanced and several robustness checks will be presented, here.

Fig. 3 presents estimates from six different samples, that exclude those who had BEVs within the study time-frame. The first specification repeats the main specification from Fig. 2 for comparison. The second specification excludes those, who state in the first survey wave that they had owned a BEV prior to the first wave (but did not have it anymore). The third specification excludes those who had a BEV in their household fleet during wave 1. The fourth specification excludes the same subjects as specification [3] and additionally, one person, that stated they had a BEV when invited to the test-drive. The fifth specification excludes everyone who stated in the third wave, that they previously had a BEV. The sixth specification uses the in wave 3 stated purchase date of a BEV (that is still in the household, this is the only purchase date asked in the survey), compares it with the date this person completed the first survey wave and excludes everyone, who already had that BEV in the household during their first survey wave. Lastly, specification [7] excludes all previous exclusions ([2,3,4,5,6]) jointly. While the direction and (in)significance of results does not

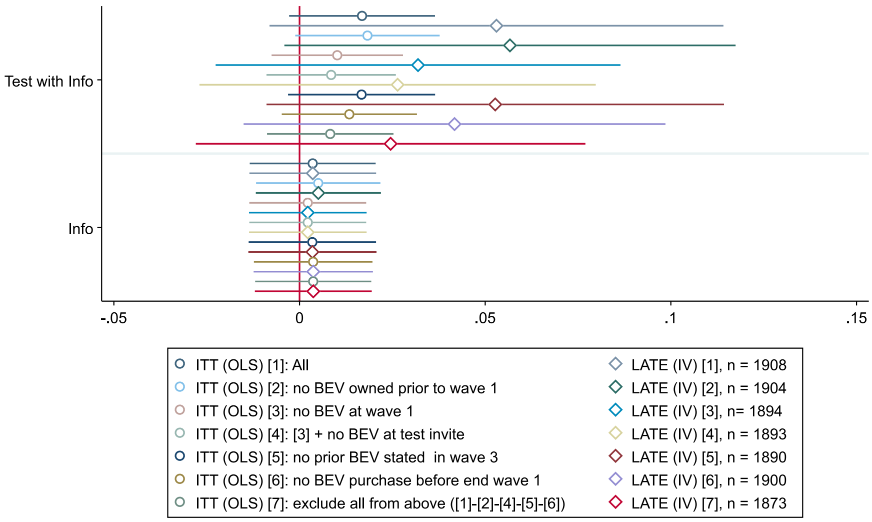

Fig. 3. Main experimental effects of both treatments on BEV adoption (linear probability of stated BEV in household in wave 3), no controls, excluding by pre-experimental BEV ownership. Specification 1 is equal to the specification displayed in Fig. 2: All regressions use the same linear specification (linear probability model) with robust standard errors. The upper values represent results from ordinary least squares (OLS) regressions (darker lines with circles), that represent the intention to treat (ITT). Below, the instrumental variable (IV) estimates are displayed (lighter lines with diamonds), showing the Local Average Treatment Effects (LATE) estimates. Instrumental variable regressions use small sample correction. The (red) vertical line at zero represents the control group. Stronger horizontal lines indicate 90%-confidence intervals (CIs) and thinner lines 95%-CIs. Specification [2] excludes all who stated, they had a BEV in the first survey wave, specification [3] excludes additionally those who declined the test-drive invitation as they had a BEV. Specification [4] additionally excludes those declining a test-drive because they had a BEV. Specification [5] excludes all that state they previously had a BEV (but do not have it anymore) in wave 3. Specification [6] uses the purchase date indicated in wave 3 and excludes those with purchase date prior to end of baseline survey (wave 1). Specification [7] jointly excludes all those excluded in any of the previous specifications. This figure is represented in Table SI 6.

change, point estimates for the test-drive with information treatment changed substantially, especially in the IV specification. The largest point estimate (specification 2) is .032 and the smallest point estimate (from specification [7]) is .025, which is a reduction by more than 20%.

5.2.1. Results with covariates

There are different sets of variables, that are usually found to predict BEV uptake (intentions), beyond information and experience. Following recent revealed and stated preference studies (e.g., Brückmann et al., 2021; Herberz et al., 2020; Tamor et al., 2015; Plötz et al., 2014; McCollum et al., 2017; Coffman et al., 2017; Liao et al., 2017) socio-demographic and mobility-related covariates are selected to allow more precise estimation of treatment effects. The inclusion of covariates, does not substantially alter the results. Again, the standard errors are too big to conclude about the experimental treatment effects. This specification, depicted in Fig. 4, uses only covariates collected in the baseline survey wave. All covariates have an effect in the expected direction with the exemption of some covariates with insignificant effects. Based on these results, besides test-driving also being female, having completed tertiary education, having multiple cars, preferring to vote for green parties and higher household income seem to matter.

Lastly, following previous literature, the intention to buy a car in the future was also assessed. In the last survey wave, every respondent was asked whether they plan to buy a car in the future. When they intended to acquire a car in the future, they were asked which type of engine the car will most likely have. Selecting from all currently available energy carrier and engine combinations, this information can be used to see if BEV purchase intentions were altered by the experimental treatments. Planning to purchase a car with any other engine and energy carrier than a BEV was coded as zero, and BEVs were coded as one. In order not to overstate results, all those who were insecure which car engine type to acquire next, are coded as zero. That is, as not planning on adopting a BEV. Note the smaller number of observations, since some respondents are not interested in acquiring (or leasing) any new vehicle for their household. Table 3 shows the results. Again, an insignificant positive effect from test-drives can be observed. The effect from information (1.5 years prior to this question about the next car purchase) did not have any effect compared to the control group. Overall, due to small sample sizes as well as the very conservative nature of the coding for the dependent variable, there is no treatment effect on intentions to adopt a BEV.

6. Discussion

Using a randomised control trial field-experiment to elicit causal treatment effects of trialling (test-driving periods) and information provision on BEV adoption, might be considered as the gold standard in experimental work. However, the results presented here, might sound puzzling. It would have needed a much larger sample, or maybe a longer observational period, to

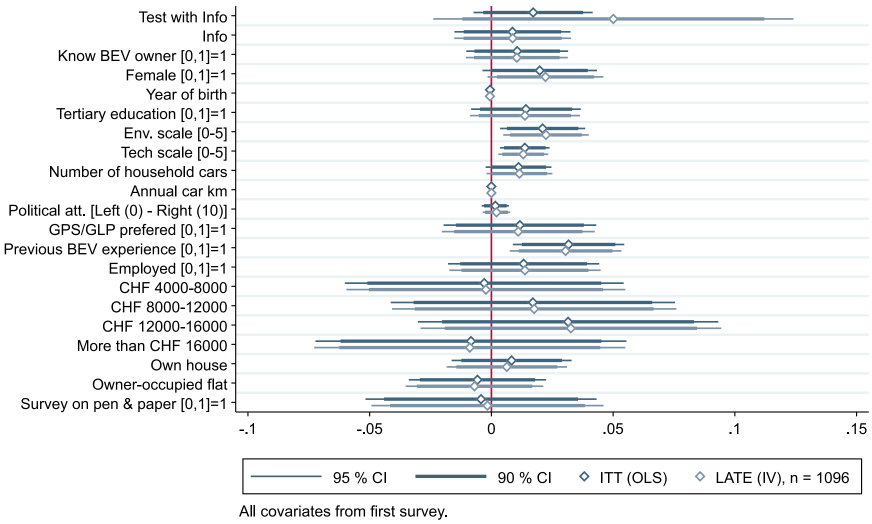

Fig. 4. Main experimental effects of both treatments on BEV adoption, with covariates. Only the instrumental variable (IV) estimates are displayed (lighter lines with diamonds), showing the Local Average Treatment Effects (LATE) estimates. The (red) vertical line at zero represents the control group. Stronger horizontal lines indicate 90%-confidence intervals (CIs) and thinner lines 95%-CIs. The results can also be found in Table SI 7.

Table 3
Dependent variable: Binary indicator if next car is BEV (linear probability models) without controls.

VARIABLES	(1) ITT (OLS)	(2) LATE (IV)
Assigned treatment = 1, Test with Info	0.00522	
	(0.0321)	
Assigned treatment = 2, Info	−0.0122	
	(0.0310)	
Realised treatment = 1, Test with Info		0.0244
		(0.0892)
Realised treatment = 2, Info		−0.0122
		(0.0312)
Constant	0.233***	0.233***
	(0.0222)	(0.0222)
Observations	1,068	1,068
R-squared	0.000	0.003

Robust standard errors in parentheses
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

detect significant results. However, some alternative explanations can be ruled out, due to the design of the experiment. Economic-wide effects, such as from a pandemic, should hit all groups alike. This is a favourable feature of this experiment. Attrition, on the other hand, might still bias the results.

Maybe the findings of basically no effects in this study on the experience with and information on BEVs are also due to the timing of the last survey wave during the SARS-COV 2 pandemic. Especially, economic downturns could have reduced the number of recorded car acquisitions. A priori, this should affect all groups alike. Maybe it turns out not equally here and it is untestable as respondents were not asked about all their car acquisitions, only BEV adoption was assessed. While the Swiss automobile stock grew in 2020 (driven by electric vehicles), and the new registrations of electric vehicles even outgrew the growth in 2019 (Federal Statistical Office, 2020), it is possible that participants from experimental treatment groups were hit harder. Along with the last survey wave, I collected data on the personally felt (subjective) impact of the crisis. This was by no means limited to financial impacts but it is likely to be included in this personal measure. A large majority 79.36% said they personally feel "not at all", "very little", or "rather little" impacted by the pandemic. Including a dummy variable for personal high pandemic impact in the main specification does not alter the results or even yield a significant coefficient, see Supplementary Information 3.5.

However all variables, also COVID related, are only assessed for those who remained in the sample. Attrition could change the results. From the baseline survey ($n = 4,149$) only 60.3% ($n = 2,501$) remained in the sample. 593 persons who took part in the second wave and not in the last survey wave. This implies 76.3% of second wave participants remained in the sample until the last wave, and only 54% of the baseline respondents remained. This attrition rates vary hardly by assigned treatment status (55.78% (test with info), 54.08% (info) and 52.16% (control), respectively). However, there is a slightly higher (42.74) drop between first and second wave in the group assigned to test-drive with information treatment. In the groups assigned to information treatment and control, only 38.02% and 42.74% left the sample. The attrition after the first wave in the group assigned to test-drive with information is significantly ($t(2805) = 2.548$, $p = .011$) different from those assigned to the information (only) treatment, and to those assigned to the control group ($t(2690) = 2.228$, $p = .026$). Further analysis could analyse this in more depth.

The findings in this paper could also be attributed due to information overload. This has previously been descriptively shown for BEVs (Cheng et al., 2020) and is negatively impacting BEV adoption. This cannot be examined using data from this study. While I did not assess information overload in this study, I assessed (in the last survey wave) if respondents gathered additional information on BEVs. I asked especially for information gathered through print media, showrooms, discussions with other people, test-drives of electric vehicles, video material, and provided the option to enter different sources of information. I carefully selected this multiple-choice questionnaire item, so that respondents get some prompts, what information gathering could have occurred. Only 1.75% of those who received the test-drive with information treatment did not seek any additional information. The comparable figures among the information treatment are 44.3% and 53.95% in the control group. This striking difference, while not directly speaking to information overload, at least does hint towards the fact, that test-drivers were more curious about electric vehicles and felt a need for more information.

Unlike previous research on mobility tool ownership (e.g., Ewing and Cervero, 2010), here the degree of urbanity is not included. As previous research (e.g., Brückmann et al., 2021) has shown that in German-speaking Swiss cantons the decision of adopting a BEV among car holders is not driven by degree of urbanity, it is not included to avoid over specification.

Lastly, the findings in this paper are specific to a population that already had a car as only car-holding households (without a BEV) were sampled. Only for these, the car registries provided the information they had not yet adopted a BEV. While sampling from the whole population would have not yielded many BEVs either, these findings are aimed to explain how information and trialling interventions alter car holders' propensities to change from CVs to BEVs. The effect for people without any cars might differ and provide additional research gaps.

7. Conclusion

This study estimated causal treatment effects of trialling and information provision on BEV adoption. This is the first study that assesses actual purchase (revealed preferences) in an experiment with test-drives. This study used two experimental treatments (test-driving with information, information alone) and a control group, effectively, establishing an RCT using a random sample of registered car holders from official sources. The RCT and the IV method allows to take into account that some respondents were unwilling (or unable) to take a test-drive at all. Therefore it overcomes the slight weakness of prior studies, that primarily provided test-drive interventions to respondents answering advertisement of this BEV test-drive studies.

Positive effects of studies with trials largely stem from the fact that those interested in electric vehicles are those most curious to take part in a trial. Here as well as in most previous studies, respondents might have guessed it is in a BEV, as already the baseline survey questionnaire had many items on electric vehicles. Therefore, positive effects of trialling can only be expected for the population group already most interested in BEV uptake.

There are many reasons why this study might have found no significant effect of the test-drive with information treatment. First of all, the author assumed a much-higher positive effect and powered the experimental treatment with test-drives in this direction. Secondly, the test-drives took mostly part in winter and in the cold season, electric vehicle battery performance is lower. Thirdly, the test-drives were offered to a random sample, and not timed to a purchase process, as are *regular test-drives* done with dealerships before car purchase. Lastly, the timing, attrition and information overload is extensively discussed in the previous Section 6. Overall, the experimental design was carefully designed to avoid potential upwards bias. It was designed not to inflate the results in the direction of positive results, potentially downward biasing the results.

Experimental effects might just take longer time to materialise than one and a half years. It is not possible to analyse this possibility of effects arising later as participants in this field-experiment cannot be surveyed again. This strongly calls for further research using much larger sample sizes and/or in an area where the adoption rates of BEVs are much higher (e.g., Netherlands) or car fleets are turned over more frequently (such as Luxembourg).

From this study, it is premature to draw clear conclusions on the overall impact of test-drives and information provision on conventional car drivers. Researchers should study revealed preferences in similar study-designs after longer times, in bigger samples before careful recommendations for trialing as a policy option for actual adoption and usage can be made with certainty. This study serves as a blueprint for similar studies, that might elicit the actual effects of these interventions, test-drive opportunities and information provision.

Declaration of competing interest

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

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.tra.2022.03.025.

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