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Challenges in credibly estimating the travel demand effects of mobility services

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ABSTRACT

Mobility services including carsharing and transportation network company (TNC) services have been growing rapidly in North America and around the world. Measuring the effects of these services on traveler behavior is challenging because the results of any such analysis are sensitive to how (1) outcomes are measured and (2) counterfactuals are constructed. The lack of good control groups or randomization of assignment leaves lingering uncertainty over the contributions of selection bias and treatment effects to reported differences in travel behavior between users and non-users of these services. This paper reports on two approaches for measuring the effects of mobility service adoption on travel rate and car ownership. We first tried a pretest-posttest randomized encouragement experiment to deal with the shortcomings of poor control groups. Then, we turned to the approach of self-reported effects based on hypothetical controls to investigate whether variations in survey question presentation could influence respondents' answers and thus lead to changes in estimated effects. The data to conduct this study came from two sources: a panel survey administered by the authors at the University of Washington (UW), and a survey by Populus Technologies, Inc. (Populus). Various statistical tests were applied to analyze the data, and the results highlight the pivotal role that the research design plays in influencing the outcomes, and manifest the fundamental challenge of establishing credible estimates of the causal effects of adopting mobility services on travel behaviors.

1. Introduction

As rapid advances in communications and computing technology have enabled a burgeoning mobility service market, the effects of these services on individual traveler choices, and the transportation system as a whole, remain uncertain. While many studies indicate that on-demand mobility services including carsharing and transportation network company (TNC) services can have significant impacts on traffic operations, land use, fuel consumption, the environment, and society (e.g. Kim, 2015; Shaheen et al., 2016; Clewlow and Mishra, 2017a; Jin et al., 2018; Henao and Marshall, 2019; Ward et al., 2019), the evidence for the effects of these services remains incomplete, and impacts may vary from location to location and person to person (e.g. Namazu et al., 2018; Namazu and Dowlatabadi, 2018). Assessing the impacts of these services is challenging because the results of any such study are sensitive to (1) how outcomes are measured, and (2) how counterfactuals are constructed.

Prior research reveals several basic approaches to measuring the effects of mobility services on outcomes such as vehicle ownership and travel rates. Some studies have surveyed users and non-users of a mobility service and compared responses from the two groups to see the differences in travel behaviors. The problem with this approach is that adopters of mobility services are a self-selected group that tends to be systematically different from non-adopters in their underlying travel needs (Namazu and Dowlatabadi, 2018). These differences in, for example, annual household income, age, employment status and education background complicate identification of the causal effects of service usage on travel behaviors. The lack of good control groups or randomization of assignment leaves lingering uncertainty over the contributions of selection bias and treatment effects to reported

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differences in travel behavior between users and non-users of these services. Randomized controlled trials (RCTs) provide a strong basis for causal inference by ensuring that assignment to a treatment (e.g. adoption of mobility services) is uncorrelated with underlying factors (e.g. workplace location) that may also affect outcomes (e.g. vehicle-miles traveled). Long the gold standard in the natural and medical sciences, RCTs have revolutionized economics and policy research in recent decades (Duflo, 2020; Banerjee, 2020). Although RCTs have occasionally been used in transportation research (Rowland et al., 2003), they are often seen as impractical in this context (Handy et al., 2005).

Given the difficulty of administering RCTs in a transportation context, some researchers have surveyed only service users, using them as their own "control group" by asking them to imagine what they would have done in a counterfactual scenario without mobility services, or how their behaviors have changed after the adoption of mobility services (e.g. Stasko et al., 2013; Martin and Shaheen, 2016; Rodier, 2018; Dill et al., 2019; Wang et al., 2019). Some studies of this type apply sophisticated weighting methods to translate individual effects into population-level estimates, providing good external validity. However, their internal validity hinges on an assumption that the respondents' answers accurately reflect the individual-level travel behavior changes resulting from use of those services. Since the respondents often know the purpose of the survey and in many cases were incentivized with carsharing or TNC credits to provide responses (Santos, 2018), they might give answers that they think surveyors would like to see (Nichols and Maner, 2008; Cohen and Shaheen, 2018). Moreover, studies have suggested that the order of asking questions and the framing of survey questions also affects respondents' answers (Zaller and Feldman, 1992; Van de Walle and Van Ryzin, 2011).

A better way to mitigate some of the challenges of an RCT, while retaining the ability to identify the causal effects of a treatment, is a randomized encouragement design (RED). In traditional RCTs, subjects are required to adhere to the assigned treatment; in an RED, a random subset of subjects receives an encouragement to participate in a treatment which may impact the outcome variables of interest, but it is up to subjects to decide whether to accept the treatment (West et al., 2008). The randomized encouragement provides an ideal instrumental variable for treatment assignment, as long as the encouragement affects the outcomes only through its effect on treatment status (Zhou and Li, 2006). This allows instrumental variables analysis to yield unbiased estimates of the causal effects of the treatment. Compared with an RCT, the chief advantage of an RED is that it is not necessary for all subjects to comply with their assigned treatments. Compared with observational approaches, the key advantage of an RED is that the randomization of the encouragement step ensures conditional independence between the treatment status and outcome variables (Keele, 2015). To the best of the authors' knowledge, REDs have not been previously applied in transportation research, and according to Santos (2018) there are no published studies focusing on offering encouragement to promote mobility services.

This paper reports on two approaches for measuring the effects of mobility service adoption on travel rate and car ownership: (1) an RED in a pretest-posttest format; and (2) a survey on self-reported changes in vehicle ownership and trip-making, employing two different question framings. While both internal and external validity are essential to establishing sound, evidence-based policy, the focus of this paper is on challenges to internal validity. The RED approach involved a sample of faculty, students, and staff from the University of Washington. The survey approach included residents of 10 major U.S. metropolitan areas recruited by an online sampling firm. Although the net impact of on-demand mobility services on the size of the vehicle fleet depends on how both passengers and drivers change their vehicle ownership (Ward et al., 2019), this study focuses only on the passengers. The results of these two approaches - the RED and the survey with two question framings - highlight the pivotal role that the research design plays in influencing the outcomes, and manifest the fundamental challenge of establishing valid estimates of the causal effects of adopting mobility services on travel behaviors.

2. Literature review

Research design has a pivotal role in shaping estimates of how mobility services affect travel behaviors. In this section, prior studies are reviewed based mainly on their research designs and are divided into two categories: static group comparison design and pretest-posttest design. The static group comparison design estimates the effect of a treatment by comparing two similar groups with each other, where only one group has received the treatment; whereas the pretest-posttest design monitors the behaviors in a same study group before and after experiencing the treatment to measure the changes consequent to the treatment (Campbell and Stanley, 1963).

2.1. Static group comparison design

The studies employing the static group comparison design often compare users and non-users of mobility services to identify the effects of adopting the service on travel behaviors. A series of papers by Cervero et al. evaluated the change in travel behaviors of San Francisco residents over three years following the launch of the City carsharing program (Cervero, 2003; Cervero and Tsai, 2004; Cervero et al., 2007). They assessed the impacts between members and non-members and found that the carsharing program induced more motorized travel for members in the first year, but members reduced their total vehicular travel in the following years. Sioui et al. (2012) compared the results of a web-based survey among carsharing members with those of the regional household travel survey in Montreal, Canada, and found that although car usage of households with no vehicle increased with an increase in the frequency of carsharing usage, it never reached the level of car usage of households with one or more vehicles. Kopp et al. (2015) compared the travel behaviors of users of a carsharing service called DriveNow with those of non-users. The results showed higher travel frequency for carsharing users and differences in mode split, as carsharing users showed significantly lower private car trips and higher bike trips compared to non-users. Alemi et al. (2019) analyzed the California Millennials dataset collected from an online survey in 2015, administered to both members and non-members of TNC services. Ordered probit models were implemented to identify factors affecting the adoption and usage frequency of TNC services. The results showed that sociodemographic variables were statistically significant predictors of service adoption, but not so much of usage frequency. Also, people who had strong preferences to use their own vehicle and those who were extremely worried about the security of TNC service were shown to be less likely to use such services.

One serious issue with comparing user and non-user groups is that travel behaviors are affected not only by which services someone has access to, but by factors such as annual household income, age, employment status and education background, which themselves affect service adoption. To avoid such problems, researchers sometimes create a hypothetical static group comparison design and ask mobility service users to imagine what they would do in the world without mobility services. For example, in a survey sent out to carsharing members who indicated sharing transportation resources with others in their household, Stasko et al. (2013) asked the respondents what they would do differently without carsharing services. Since it could be hard for people to accurately respond to how they would travel in the counterfactual situations, they also asked the respondents to provide their level of certainty for their answers, where they could select one of completely (100%), very (75%), somewhat (50%), slightly (25%) and not at all (0%) options. This was meant to mitigate overestimation of carsharing impacts, because members had to speculate, and their responses might not truly reflect the situation. In a study investigating carsharing programs in Milan and Rome, Rotaris et al. (2019) developed six hypothetical scenarios to identify how individual mobility patterns would change when the existing carsharing supply varies. They found that carsharing could mainly substitute for private vehicles, both for commute and non-commute trips. Henao (2017) conducted a regional TNC survey in the Denver metropolitan area by asking TNC passengers what they would have done if TNC services were not available. The results indicated that 34% of TNC users would have walked, biked, or taken transit trips, and 12% would not have traveled at all if they had no access to TNC services. Similarly, in a survey of 4500 US mobility service customers, approximately 14% of users indicated that if TNCs did not exist they would turn to public transit (Feigon and Murphy, 2016).

2.2. Pretest-posttest design

The pretest-posttest design investigates the effects of a treatment by studying the travel behavior changes of the same group before and after receiving the treatment. An ideal pretest-posttest design would consist of multiple phases of study where the changes are measured over time. However, due to high survey costs, most studies in this context have implemented a hypothetical pretest-posttest design by asking respondents about how their behavior has changed since receiving the treatment.

Martin et al. analyzed the results of 2008 North America carsharing members survey, a web-based before-and-after study that was conducted to find out the effects of carsharing on people's travel behaviors (Martin et al., 2010; Martin and Shaheen, 2011). Carsharing members reported a reduction in the average number of vehicles per household from 0.47 to 0.24. In another study based on the results of a self-reported two-wave travel and residential survey in seven American cities from 2014 to 2016, Clewlow and Mishra (2017b) indicated that 91% of TNC members had not made any change in their vehicle ownership since they started using TNC services and that whether TNC services complement or substitute public transit depends on the type of transit service in question. Similarly, Hampshire et al. (2017) conducted a web-based survey of Uber and Lyft members in Austin, Texas to investigate the effects of TNC service suspension (in May 2016) on car ownership. Having combined stated and revealed preference survey questions, they showed that of people who participated in the survey, 45% turned back to their own vehicles to fill in the gap, while only 3% began to use public transit. Of those switching to personal vehicles, 8.9% reported that they had bought a brand-new vehicle in response to the service suspension.

Rayle et al. (2016) conducted an intercept survey of TNC users in San Francisco during May–June 2014. The results revealed that TNC users were less likely than the general population to own a vehicle; however, 90% of users who owned one or more vehicles, indicated that they had not changed their car ownership after they started to use TNC services. Martin and Shaheen (2016) conducted a survey on approximately 7400 carsharing (car2go) members in five large North American cities (Calgary, San Diego, Seattle, Vancouver, and Washington, DC), where members were asked whether they shed a vehicle after they joined car2go, or whether they would purchase another vehicle if car2go no longer existed. In another study, based on a survey of Toronto residents, Engle-Yan and Passmore (2013) found that 55% of carsharing members abandoned or postponed purchasing a vehicle, and 29% stated that they would like to stop using or sell one private vehicle.

According to Shaheen and Rodier (2005), in the final survey questionnaire sent to CarLink II (a carsharing pilot program launched in the Palo Alto region during 2001–2002) members, participants were asked about the status of their vehicle ownership after having joined the program. The survey question presented response options including "No change in use of household personal vehicles", "Family member drives a car more frequently", "Loaned a vehicle to someone outside immediate family", "Sold or stored one or more of our personal vehicles", "Purchased or leased a personal vehicle", "Did not have a vehicle when I joined CarLink" and "Other". Respondents were asked about the conditions under which shedding a car was conceivable, whether a car had been shed in the household since they joined the carsharing service, and the reasons for shedding the car(s).

In the present study, we first try a randomized encouragement experiment to deal with the shortcomings of poor control groups. We then turn to the approach of self-reported effects based on hypothetical controls to test whether variations in question presentation lead to changes in estimated effects.

3. Data

The data to conduct this study came from two sources: a panel survey and RED administered by the authors at University of Washington (UW), and a survey by Populus Technologies, Inc., which are explained in the following subsections. The travel behavior of the UW sample is likely not generalizable beyond similar campus settings, but the work does offer some important lessons about the practicalities of REDs and challenges in establishing internal validity in transportation research.

3.1. UW randomized encouragement design

To study the effects of on-demand mobility services on vehicle ownership and travel behaviors, we designed and administered a web-based RED. Questionnaires were sent to UW faculty, staff, and students via email, and asked them about baseline travel indicators and demographic data in three categories: 1) Basic characteristics, including driver license holding, age, access to bicycle, transit pass, status on campus, mobility membership, and Email addresses; 2) Household-level information, including household vehicle ownership, home/work location, household age framework (how many people in each of the age groups of <15, 15–24, 25–44, 45–65, and >65), and household annual income; and 3) Daily trip making, including trip rates, mode share, and trip origin/destination.

The experiment was done in two waves. The first wave (pretest) was conducted in November 2015, and the second wave (posttest) in May 2017. In the second wave, the questionnaires were sent out only to those who had provided valid responses in the first wave. Between the two waves, in July 2016, mobility service credits were sent to randomly selected subsets of people who had provided valid responses in the first wave and were not already carsharing/TNC users. These credits were meant to provide encouragement for recipients to try out mobility services, with the assumption that some of such recipients would continue using the services. The credits were provided by two mobility companies: ReachNow (a carsharing company) and Lyft (a TNC). ReachNow provided 900 vouchers in three levels: $10, $25 and $40 (300 for each level), and Lyft offered 900 vouchers which allowed each new user to take three free rides. These credits constituted the encouragement in the RED. To promote survey completion, those who filled out the survey were entered in a drawing for their choice of an iPad Air 2 or an iPad mini 4 (retail prices of approximately $500 at the time the survey was completed).

The encouragements were transitory by design. In the RED, the encouragement serves as an instrument for adoption of mobility services. An essential characteristic of a good instrument is that it affects the outcome variable(s) of interest only through its effect on the treatment of interest. In other words, to infer how adoption of mobility services affects travel behavior outcomes, the encouragement should affect those outcomes only through their effect on service adoption. If ongoing mobility services subsidies or credits were provided, they could directly affect outcomes as well, by affecting not only the adoption of mobility services, but the ongoing price of mobility services. This would violate the exclusion restriction, and undermine the validity of the RED.

As noted in the introduction, a concern in survey research is that respondents' answers may be biased by the receipt of mobility service credits as incentives to fill out the survey. However, there are two key differences between the present approach and those alluded to in the introduction. For one, the carsharing/TNC credits were sent out between the first and second survey waves, and fulfilled the role of the randomly assigned encouragement in the RED. These vouchers were not linked to survey completion, and approximately 10 months passed between sending those vouchers and the second survey wave. Thus, we would not expect the vouchers to be front-of-mind when subjects were completing the survey. Second, the RED focuses on objective variables, namely the number of trips taken and number of vehicles owned. These should be less susceptible to availability bias or social desirability bias than more subjective assessments such as recollections of changes in travel or the reasons for changes.

The survey was distributed to University of Washington (UW) Seattle campus community members, including students, faculty, and staff. The UW Seattle campus, located in the northern part of Seattle, WA (approximately 5 miles from downtown Seattle) is a large public flagship research university, and educates more than 59,000 students and employs more than 31,000 faculty and staff (as of 2018–2019 Fall quarter). The UW offers students, faculty, and staff a public transportation pass that provides unlimited rides on buses, light rail, streetcars, and ferries in the region, and also provides low membership rates for Zipcar carsharing and Vanpool services by paying a fee at the beginning of the academic quarter. The UW also provides different kinds of parking permit types, including day/night permits and hourly permits. The day/night parking rates on campus are aimed at motivating carpooling: $15 for driving alone; $7.50 for cars with two people; and $5 for cars with more than two people (all for weekdays). In addition to parking on campus, there are many on/off-street parking lots within 10-minute walking distance to UW. There were several carsharing services available in Seattle at the time of the study. Car2go, owned by Daimler, was launched in Seattle in December 2012; ReachNow, initiated by BMW, entered the Seattle area in April 2016; and Zipcar has built 12 parking spaces within the 15-minute walking distance of the UW Seattle campus. The two largest TNCs, Uber and Lyft, are readily available in this region.

Originally, the survey invitation was sent to approximately 10,000 members of the UW community. In the first survey wave, 2125 responses were collected, of which 1635 were valid. In the second wave (sent only to those 1635 respondents), 646 responses were submitted and 528 of them were valid. By matching email addresses, 502 repeated respondents could be linked between the first and second waves of the survey, leading in an overall response rate of 5%. The key demographic characteristics of the respondents are reported in Table 1 and compared with the U.S. general population where applicable (U.S. Census Bureau, 2010). Table 2 provides a breakdown of the sample into groups who did or did not adopt mobility services in the first and second waves of the

Table 2
Breakdown of the UW sample into groups who did or did not adopt mobility services in the first and second waves of the survey.

Respondent Group	Count	%
(A) Identified themselves as former mobility users in the first wave	61	12.2
(B) Adopted mobility services in the first wave but abandoned them in the second wave	87	17.3
(C) Adopted mobility services in the first wave and kept using them in the second wave	113	22.5
(D) Did not adopt mobility services in the first wave but adopted in the second wave	151	30.1
(E) Did not adopt mobility services in either the first or second wave	90	17.9
Total	502	100

Table 1
Demographic characteristics of respondents in the UW sample[a] compared to the general population.

Characteristic	Categories	First Wave		Second Wave		% in U.S. General Population
		Count	%	Count	%	
Licensed driver[b]	No	89	5.4	1	0.2	12.9
	Yes	1545	94.6	527	99.8	87.1
Age	18–25	422	28.0	74	14.0	14.9
	26–45	719	47.7	285	54.0	35.1
	46–65	328	21.8	148	28.0	34.0
	>65	38	2.5	21	4.0	16.0
Gender	Female	799	61.6	323	61.6	51.5
	Male	478	36.8	191	36.5	48.5
	Prefer not to answer	20	1.6	10	1.9	N/A
Access to bicycles	No	712	47.4	211	40.3	N/A
	Yes	789	52.6	313	59.7	N/A
Transit pass	No	263	18.3	126	24.0	N/A
	Yes	1175	81.7	400	76.0	N/A
Status on campus	Faculty	139	10.7	52	9.9	N/A
	Staff	605	46.6	270	51.3	N/A
	Undergraduate	266	20.5	45	8.6	N/A
	Graduate	286	22.1	103	19.6	N/A
	Alumni	1	0.1	37	7.0	N/A
	Other	0	0	19	3.6	N/A
Household size	1	745	45.6	158	29.9	26.7
	2	446	27.3	200	37.9	33.6
	3	193	11.8	83	15.7	15.6
	4 or more	251	15.3	87	16.5	24.1
Household annual income	Less than $25,000	208	16.1	67	12.8	21.8
	$25,000 to $49,999	187	14.5	49	9.4	22.7
	$50,000 to $99,999	341	26.4	162	30.9	29.2
	$100,000 to $149,999	211	16.3	95	18.1	14.1
	$150,000 to $199,999	97	7.5	57	10.9	6.3
	$200,000 or more	97	7.5	54	10.3	5.9
	Prefer not to answer	152	11.7	40	7.6	N/A
Household vehicles	0	249	15.2	48	9.1	9.1
	1	523	32.0	222	42.0	33.8
	2	432	26.4	170	32.2	37.6
	3 or more	431	26.4	88	16.7	19.5

[a] The total number of valid responses in the first and second waves are 1635 and 528, respectively.
[b] UW sample was limited to respondents 18 and older, so the US adult population (≥18 years old) is considered for comparison.

survey.

3.2. Populus data

To understand how the estimated impacts of on-demand services are influenced by the manner in which respondents are asked about those impacts, we used data collected by Populus Technologies, Inc. (Populus, 2019). Populus is a transportation technology company specializing in data and analytics that provides independent and unbiased data and analytics to public sector agencies and private mobility operators. The data used in this study is based on a broad survey data collection effort by Populus on travel behavior decisions in major U.S. regions.

The Populus survey instrument comprised five sections, organized as follows: 1) attitudes towards travel, neighborhoods, technology, and environment; 2) household demographics; 3) current and previous residential decisions; 4) travel behaviors including use of mobility services; and 5) vehicle ownership and preferences. Its design is similar, and is partially based on regional and national travel surveys, including specifically the California Household Travel Survey and the National Household Travel Survey. The key differentiator between these surveys and the Populus survey is that the latter is deployed on an annual basis, and asks new questions about mobility services adoption, use, and associated transportation decisions.

The Populus survey was completed by a demographically balanced sample of 10,748 respondents aged 18 and older across 10 major metropolitan areas around the U.S., including Atlanta, Austin, Boston, Chicago, Denver, Los Angeles, New York City, Seattle, Washington DC, and the Bay Area, from March to April 2018. A sampling firm that specializes in online survey software for scientific and market research was utilized to facilitate online distribution of the survey instrument. The respondents were recruited via email and online ads purchased by the sampling firm with primarily cash and gift card incentives. Using data from the 2013–2017 American Community Survey (ACS) 5-year Statistics, a quota-based sampling approach was used to recruit and accept a sample of respondents whose age, income, gender, and race distributions would match the reported distributions of each metropolitan region sampled at the metropolitan statistical area (MSA) level. The sampling firm did not provide the total number of individuals invited to complete the survey.

The distribution of respondents in terms of age, gender, residential metro area and household annual income is shown in Table 3. The statistics of the U.S. general population are also included (U.S. Census Bureau, 2010) to provide comparison with the sample.

For this study, we only used a subset of the Populus survey data that includes TNC users. To do so, we referred to a question in the survey that asked respondents whether they were aware of app-based on-demand TNC services such as Uber or Lyft. Only the respondents who were familiar with and used those services were included in our dataset; we excluded those who had never heard of or never used those services, or had only ridden in them with family or friends but did not have the apps on their phone. There were 4,342 respondents (all of whom reported being TNC users) in our dataset.

Respondents were asked questions about their demographics (age, gender, and household income), vehicle ownership, and reasons for changes in vehicle ownership. The questions that were particularly related to vehicle ownership change and the effect of TNC services can be grouped into three sets:

- In Set 1, respondents were asked whether in the past 7 years they had decided not to purchase a vehicle that they originally thought they would need. If they answered "Yes", they were then asked to provide their top three reasons for that decision, from among 11 options including cost (purchasing/maintaining a vehicle), health/age, insurance, environmental impacts, available rides from others, parking, dislike for driving, using other modes (public transit, bike, or walk), easy access to another household vehicle, and having access to Uber/Lyft or other on-demand mobility services.
- In Set 2, respondents were asked if they had gotten rid of, sold, or stopped leasing a vehicle in the past 7 years. If they answered "Yes", another question was prompted asking them if they had replaced the old vehicle with a new one. They were then asked to provide up to three reasons for why they got rid of/retired that vehicle, where the available options included gas mileage, moving, a job change, and having access to Uber/Lyft or other on-demand mobility services. The ordering of the pre-defined reason choices in both Set 1 and Set 2 was randomized across different respondents.
- In the third set, people were prompted with a question including the explicit context of TNCs. Specifically, they were asked "What impact, if any, has your use of on-demand services such as Uber or Lyft, had on your vehicle ownership decisions?", and were asked to choose only one answer, where the possible responses were: got rid of my only car and didn't replace it; got rid of a second car and didn't replace it; decided not to purchase a car; delayed purchasing of a car; and no impact.

The question sets above are grouped into two categories based on their design: (a) Prompted design, comprising Set 3, where the question explicitly asked about the impact of on-demand services, and (b) Unprompted design, comprising Sets 1 and 2, which asked about car ownership changes and then gave respondents the chance to identify on-demand services as a reason for those changes. Fig. 1 shows how the responses given to the Prompted and Unprompted questions were compared.

Table 3
Demographic characteristics of respondents in the Populus sample compared to the general population.

Characteristic	Categories	% in Populus Sample	% in U.S. General Population
Age[a]	18–24	9.3	13.3
	25–44	39.9	33.7
	45–64	34.3	32.8
	>64	16.5	20.2
Gender	Female	52.6	51.0
	Male	47.4	49.0
Residential metro area[b]	Atlanta	9.1	7.9
	Austin	9.2	2.9
	Boston	9.2	6.5
	Chicago	9.1	12.7
	Denver	9.1	3.9
	Los Angeles	9.0	17.7
	New York City	8.9	25.8
	Bay Area[c]	18.2	9.0
	Seattle	9.2	5.3
	Washington, D. C.	9.0	8.3
Household annual income	Less than $25,000	13.3	20.2
	$25,000 to $49,999	18.8	21.9
	$50,000 to $99,999	31.4	30.0
	$100,000 to $149,999	19.4	14.6
	$150,000 to $199,999	8.6	6.3
	$200,000 or more	8.4	7.0

[a] Populus sample was limited to respondents 18 and older, so the US adult population (≥18 years old) is considered for comparison.
[b] The Residential Metro Area percentages in the U.S. general population column represent the ratio of each metro area's population to the combined population of the ten metro areas in the sample.
[c] In the Bay Area, Populus collected data from two areas: San Francisco-Oakland metropolitan area and the San Jose-Sunnyvale-Santa Clara metropolitan area.

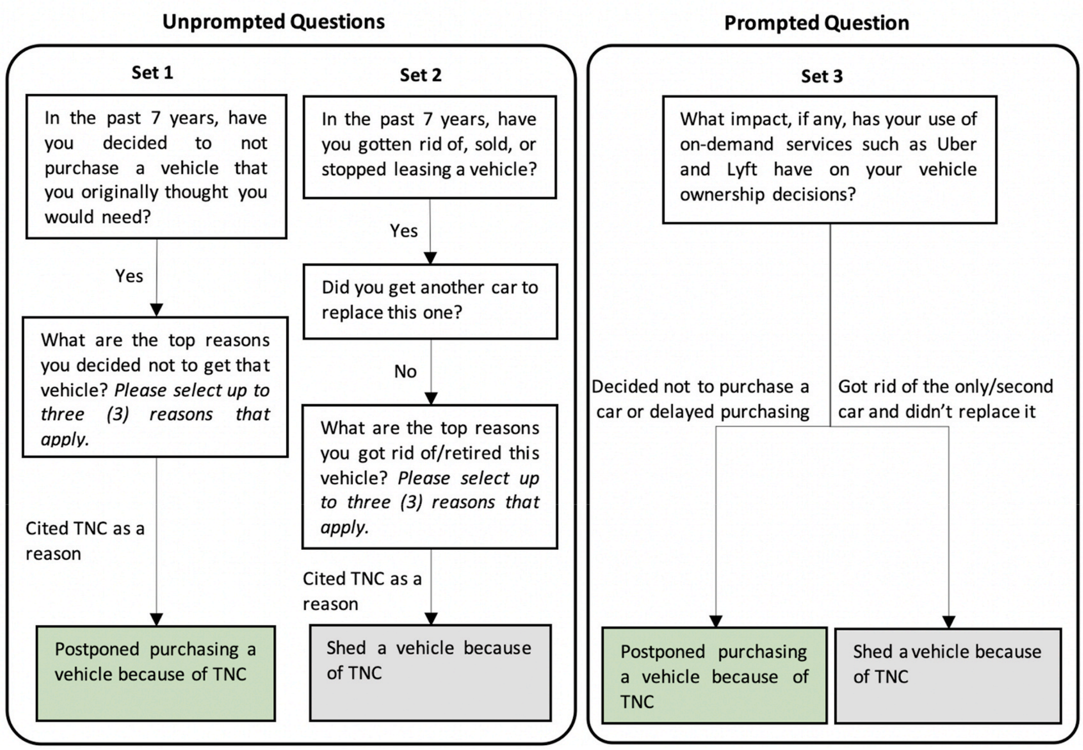


Fig. 1. Grouping of the questions in the Populus survey to study whether the question design influences the estimated impacts of TNC services on vehicle-ownership.

4. The causal effects of adopting mobility services on car ownership and trip rates

To study whether becoming a user of on-demand services (particularly carsharing and TNC services) affects users' trip rates and car ownership, two statistical analyses were conducted on the UW survey data. The trip rates in this context refer to daily trip count from a recall-based travel diary, and car ownership refers to the number of vehicles owned by each household. The statistical methods implemented are (1) instrumental variables (IV) estimation of the RED, and (2) difference-in-difference (DiD) analysis of the before-after panel data. These methods are explained in more detail in appendix.

The IV and DiD analyses were conducted on respondents who had not adopted mobility services in the first wave (Groups D and E in Table 2, equaling 241 respondents), focusing on their outcomes in the second wave of the survey. The treatment considered here is the adoption of a mobility service, the effects of which might take considerable time to manifest. This is why the second wave survey was conducted nine months after distributing the credits and one and a half years after the first wave. The non-users (based on responses in the first wave) who became carsharing/TNC users in the second wave were considered as the treatment group, while non-users who did not opt to use the services in the second wave were put in the control group.

IV estimation is a strong strategy for identifying the causal effects of endogenous variables on the outcome variable, when the outcome variable is correlated with the error term. So, we first conducted the IV analysis using the receipt of carsharing or TNC credits as the instrumental variable to identify the causal effects of adopting a mobility service (endogenous variable) on trip rates and car ownership (outcome variables). Table 4 shows the description of variables used in the IV analysis. A good instrumental variable should affect the outcome variable only by inducing changes in the endogenous variable. However, the analysis results showed that the instrumental variable (receiving credits) was weak, and therefore the IV approach was inefficient for estimating the treatment effects. The results of IV analysis (using the Wald test) to identify the causal effects of carsharing or TNC services on trip rates are shown in Table 5.

Table 4
Description of variables in the IV analysis.

Variable Category	Variable Name	Variable Description	Variable Type
Outcome Variable	Trip rate difference	Difference in trip rates reported by the respondent between the two survey waves	Interval
Endogenous Variable	Service adoption	Adopted mobility services or not	Binary
Instrumental Variable	Encouragement receipt	Received encouragement or not	Binary
Control Variables	Age	Age group of respondents	Ordinal
	Bike access	Have access to bicycles	Binary
	Transit pass	Have regular transit pass	Binary
	Gender	Male or female	Binary
	Household income	Annual income level	Ordinal
	Household age framework	<15, 15–24, 25–44, 45–65, >65	Discrete

Fig. 2 summarizes the carsharing/TNC usage of respondents. As can be seen, in the first wave, almost half of the respondents were non-carsharing/TNC users, while in the second wave the proportion of

Table 5
Results of IV estimation for treatment effects on daily trip rates.

Effects	With control variables		Without control variables	
	F-stat	p-value	F-stat	p-value
Lyft on Non-mobility users in the first wave	0.1248	0.7242	$1e-04$	0.9941
Reachnow on Non-mobility users in the first wave	1.2953	0.2757	1.5887	0.1915

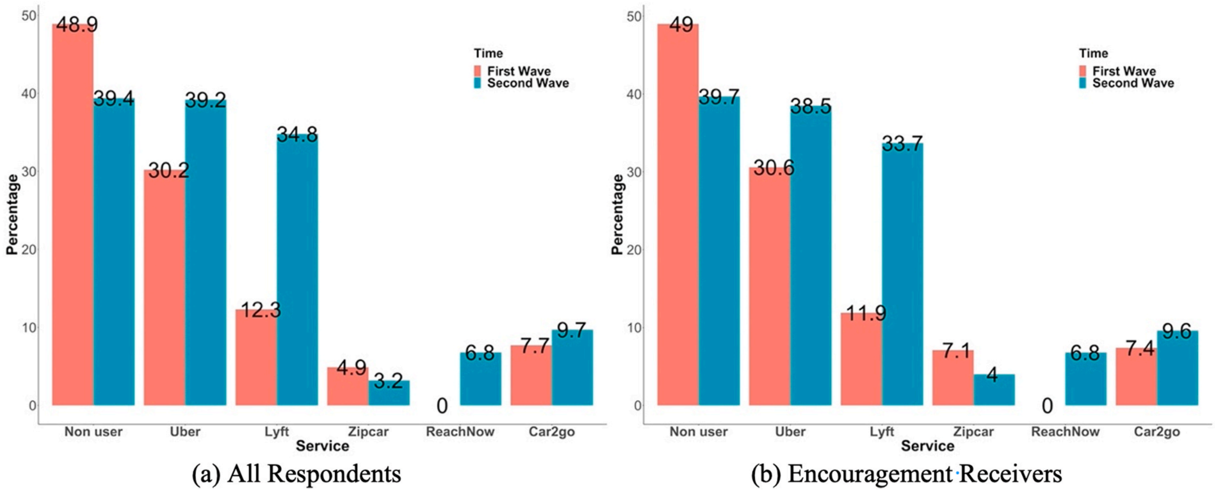


Fig. 2. Statistics of carsharing/TNC usage.

non-users decreased to 39%. Among mobility users, Uber and Lyft had the largest number of users in both waves, which implies that generally, TNC was more prevalent than carsharing among respondents. The next most popular service within the UW community was car2go, and in both waves very few people used Zipcar. This might be because Zipcar is a station-based carsharing service, which makes it less flexible and less convenient than car2go. There were no ReachNow users in the first wave, because it had not entered the Seattle market at that time, but in the second wave, a few people reported having used ReachNow.

However, as can be seen the usage trends are similar between all respondents and the subset who received the encouragements. A Wilcoxon Signed-Rank test was also conducted to investigate the effect of encouragement and to identify whether there were significant changes in mobility usage (in terms of Uber, Lyft, Zipcar, Reachnow or car2go) of encouragement receivers over time. The null hypothesis was that the rate of mobility usage among those received encouragement is the same between the first and second waves. The analysis found the p-value to be 0.8182, which means that the null hypothesis cannot be rejected and implies that sending out encouragement did not strongly induce people to use mobility services. Checking the credit usage, we found that only 32 of the 900 Lyft vouchers and 40 of 900 ReachNow vouchers were actually redeemed, which explains the above results.

Since the instrumental variable was found weak and thus the IV approach would be inefficient for estimating the treatment effects, the DiD design was then implemented to identify the causal effects of carsharing/TNC services on trip rates and car ownership. The DiD analysis estimates the causal effects of treatment on the outcome variable by measuring the changes over time between treatment and control groups. So, while DiD analysis is not as strong as the IV analysis as a causal inference strategy, it is not limited by the strength of receiving encouragement on changing behaviors. The DiD analysis was done using a mixed-effect negative binomial regression model, where the dependent variable was the trip rates or car ownership, and the treatment-posttest interaction term serves as the independent variable of interest. A random intercept term was also used to account for the repeated responses made by the same person.

Table 6 shows the results of DiD analysis for treatment effects (adopting a mobility service) on trip rates and vehicle ownership. As can be seen, in all cases, p-values are large, which means that the treatment did not have statistically significant effects on car ownership or vehicle, walk, public transit and bike trips. So, we cannot reject the null hypothesis that adopting Lyft nor ReachNow services has no effects on people's trip rates or vehicle ownership. But it is possible that mobility services can affect car ownership or trip rates in the longer term, but that the time between sending the encouragement and second wave of survey (nine months) was too short for people to shed/sell their vehicles or abandon purchasing one or to change their daily trip patterns.

5. Impacts of question design on the stated effects of mobility services

Since identifying the causal effects of mobility services on travel behaviors through a group comparison approach suffered practical challenges, we turned to self-reported data and hypotheticals to measure the changes in travel rates and car ownership. For this, we used the Populus data, explained in section 3.2.

The number of people in the Populus dataset who reported changing their vehicle ownership (shedding a vehicle or postponing a purchase) as a result of on-demand mobility services are summarized in Table 7, based on both the prompted and unprompted question sets. As outlined in Fig. 1, the numbers based on the unprompted design include people who decided not to purchase a vehicle in the past 7 years and cited TNCs as one of the top three reasons for their decision; and those who got rid of a vehicle, did not replace it, and cited TNCs as one of their top three reasons. In the prompted design, these numbers represent the number of people whose response to the impact of TNCs on their vehicle ownership decisions was that they got rid of a car and did not replace it, decided not to purchase a car, or delayed purchasing a car.

Table 6
Results of DiD analysis for causal effects of adopting mobility services.

Effects		Lyft on Non-mobility users in the first wave		ReachNow on Non-mobility users in the first wave	
		Estimated Coefficient	p-value	Estimated Coefficient	p-value
Trip Rates	Vehicle trips	−0.1480	0.6645	0.0047	0.9937
	Walk trips	−0.2703	0.784	−2.6371	0.2424
	Public transit trips	0.1048	0.762	0.8714	0.139
	Bike trips	−0.4506	0.7468	−1.1391	0.5129
Vehicle Ownership		−0.0150	0.9073	−0.3027	0.1968

Table 7
Number of respondents who did or did not say they had changed their vehicle ownership due to on-demand mobility services, under the prompted and unprompted question designs.

	Unprompted Design	Prompted Design
Changed car ownership due to on-demand services[a]	207	519
Did not change due to on-demand services	4135	3823
Pearson's Chi-Squared Test of Independence	$\chi^2 = 145.38$ p < 2.2 × 10–16	

[a] Either shedding a vehicle or postponing the purchase of one.

The number of respondents who reported changes in their vehicle ownership decisions (whether shedding a vehicle or postponing the purchase of a vehicle) as a result of TNC usage was much higher in the prompted design than in the unprompted design. This suggests that people were much more likely to attribute their decisions to TNC services when they were directly asked about the impact of those services on their vehicle ownership decisions.

To test whether the difference between responses in the two question designs is significant, we used McNemar's and Pearson's chi-squared tests (Tables 7 and 8). McNemar's test is a large sample test for matched-pair data, and the main assumption in this test is independent paired responses (McNemar, 1947). The Pearson's chi-squared test is a method to analyze group differences when dependent variable is measured nominally, and it does not require equality of variances among the groups (Pearson, 1900). Our null hypothesis in both tests was that the question structure had no effect on whether people report changing their vehicle ownership as a result of on-demand services. The results, presented in Tables 7 and 8, lead us to reject the null hypothesis. We conclude that the framing of the survey questions influences people's responses and the resulting estimates of private vehicles suppressed by on-demand services.

Fig. 3 shows the percentage of respondents who cited each listed reason for having postponed purchasing a vehicle. In the Unprompted design, there were 862 respondents who reported deciding not to buy a vehicle in the past 7 years, and they were divided into two groups based on whether or not they had cited access to TNC services as one of their top three reasons for delaying a vehicle purchase. 138 respondents cited TNC services as one of their top three reasons, while 724 did not. The other reasons for delaying a vehicle purchase were fairly similar across both groups, with two notable exceptions: Those who cited TNCs were also much more likely to cite parking and their use of other modes (public transit, bike, or walk) as reasons for delaying a vehicle purchase. This suggests that TNC services are complementary to a multimodal lifestyle, particularly in areas where parking is challenging.

Table 8
Consistency of responses by same individual to prompted and unprompted question designs.

		Prompted Design	
		Changed car ownership due to on-demand services[a]	Did not change due to on-demand services
Unprompted Design	Changed car ownership due to on-demand services[a]	54	153
	Did not change dueto on-demand services	465	3670
McNemar's Test		McNemar's $\chi^2 = 156.51$ p < 2.2 × 10–16	

[a] Either shedding a vehicle or postponing the purchase of one.

Fig. 4 shows the percentage of respondents who cited each listed reason for having shed a vehicle (got rid of, retired, or no longer leased a vehicle). A total of 392 respondents reported shedding a vehicle in the past 7 years, and they were divided into two groups based on whether or not they cited TNC services as one of their top three reasons for shedding a vehicle. There were five reasons that differed by more than 10% points between the two groups: access to carsharing, using other modes (public transit, bike, or walk), job change, moving, and wanting a new vehicle. Similar to the results above for postponing purchases, the first two suggest that TNCs complement other modes in reducing car dependence. The next two are consistent with the theory that when alternatives to car ownership are available, a disruptive life event can prompt individuals to reconsider their travel choices. The final reason (wanting a new vehicle) simply suggests that the shed vehicle was replaced with a new one.

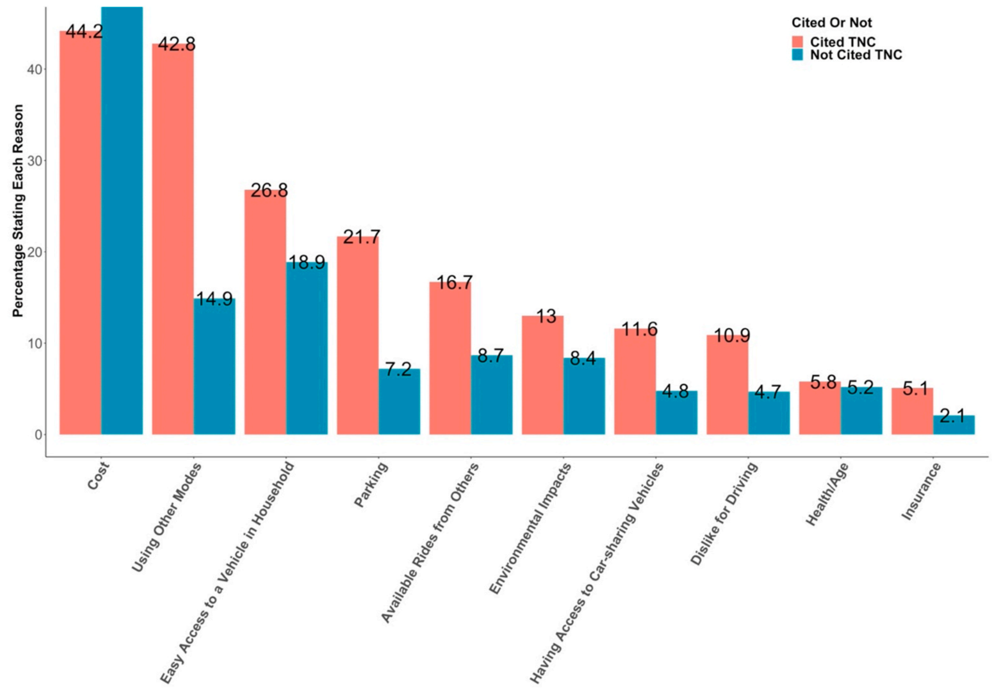
6. Discussion and conclusion

The work reported here underscores the difficulty of establishing credible estimates of the effects that mobility services have on sustainable transportation outcomes. Carsharing and TNC services were hoped by many researchers and practitioners to have environmental benefits, which are closely associated with trip rates and vehicle ownership. However, the evidence for this is confounded by potential self-selection bias among mobility service adopters, and both availability bias and social acceptability bias among survey respondents.

In studying the causal effects of on-demand services, it is confirmed that mobility services adopters own fewer cars (1.54 cars per person) than do non-adopters (1.8 cars per person), and that service adopters make fewer vehicle trips (1.51) compared to non-adopters (1.89). However, this fails to account for the possibility that adopters' underlying travel needs, and preferences are inherently different than those of non-adopters. It is possible that such different travel patterns induce adoption of mobility services, not the other way around.

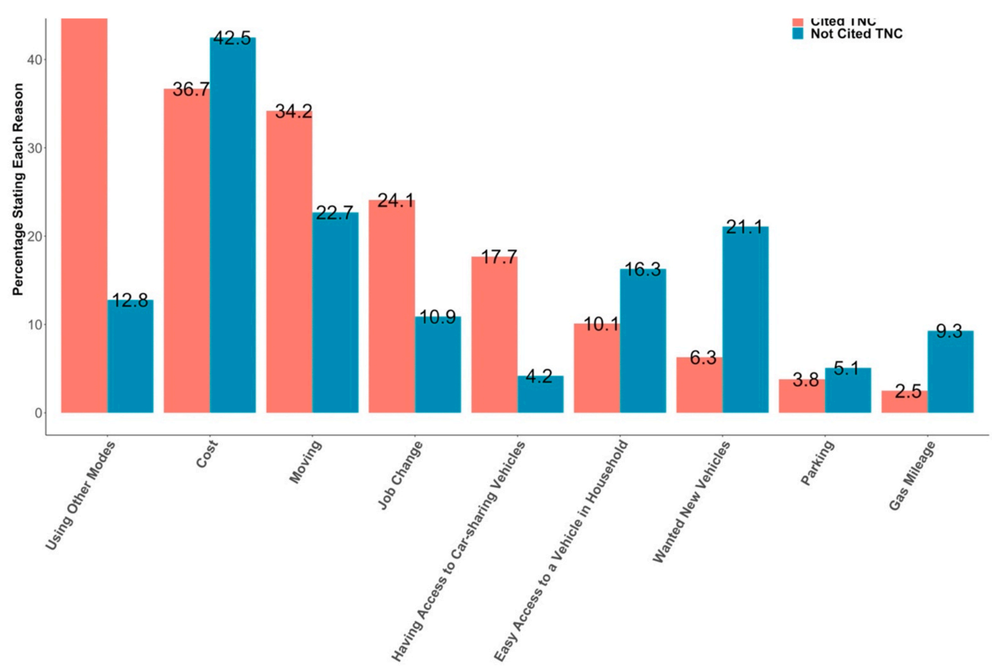
An attempted randomized encouragement design (RED) to deal with the shortcomings of poor control groups in measuring the effect of mobility services on car ownership and daily trip-making was unsuccessful due to weak instruments: the encouragement offered was too small to induce significant changes in mobility service adoption. Future REDs in this area could be improved to overcome the weak instruments problem. One way to do this is to strengthen the delivery of encouragement for mobility services adoption by offering a larger credit amount or a larger exogenous encouragement (e.g. five free Lyft rides or a $100 ReachNow voucher) for trying out the services. A testament to this proposal is that in the UW survey, 13 out of 32 people who redeemed Lyft vouchers, and 11 out of 40 who redeemed ReachNow vouchers, identified themselves as Lyft/ReachNow users in the second wave. And the adoption ratio increased as the amount of credits increased (20% for $10 vouchers; 23% for $25 vouchers; 32% for $40 vouchers). The number of available vouchers could be limited and communicating this scarcity to subjects might induce them to take the treatment sooner. The delivery of information might also be revised by emphasizing that respondents are participating in an experiment, to encourage greater compliance. Increasing the sample size might also help to get F-values close to 10.

Since the Instrumental Variable (IV) approach was inefficient, we investigated the effects of mobility services through a difference-in-difference (DiD) analysis. When time trends among non-adopters are assumed to represent what would have happened to new adopters, no significant effects of TNC or carsharing on trip rates or car ownership could be identified. Absence of proof is not proof of absence, of course, and there are several caveats to this finding. It is possible, for example, that the lack of effects is unique to the UW-Seattle university community that comprises the sample in this study. Survey respondents could be systematically different from the general population in educational background, income, or other factors that might also affect sensitivity to



*The total percentage for each category is larger than 100% because each respondent chose up to three reasons.

Fig. 3. Percentage of respondents citing each reason for having postponed buying a vehicle, among those who did and did not cite TNC access as a reason.



*The total percentage for each category is larger than 100% because each respondent chose up to three reasons.

Fig. 4. Percentage of respondents citing each reason for having shed a vehicle, among those who did and did not cite TNC access as a reason.

mobility service adoption. Regarding the car ownership results in particular, it is possible that changes could manifest over a longer time period than the 1.5 years between the two survey waves in this study. Nevertheless, these results are reasons for caution when considering reported effects of mobility services on sustainability outcomes. To assess the impacts of TNC or carsharing services as accurately as possible, studies need to employ a research design that can separate the effects of selection bias (who chooses to use such services) from treatment effects (how becoming a service user changes a person's behavior). Randomized encouragement experiments, instrumental variables, and difference-in-difference analyses have been successful in developing unbiased estimates of treatment effects in other domains, but to identify the causal effect of mobility service adoption on travel behavior, if any, they will require larger sample sizes and/or stronger incentives than those investigated in this study.

We then turned to the approach of self-reported effects based on hypothetical controls, by surveying the TNC users and asking them how the adoption of that service affected their car ownership. Our results show that the estimated impacts of mobility services are sensitive to the manner in which data are elicited from survey respondents and that minor variations in question presentation lead to sizable changes in estimated effects. In particular, when TNC users were first asked whether they had changed their vehicle ownership, and then asked whether access to on-demand services was a leading reason for that change, approximately 5% reported changing their car ownership due to mobility services. However, when the same people were asked what the impact of on-demand services had been, 12% reported changing their car ownership due to mobility services. Chi-squared and McNemar's tests indicated that these differences are highly significant, consistent with the theory that the question structure affects respondents' answers. While our analysis results show that vehicle ownership may decrease due to access to carsharing and TNC services, some studies indicate that the overall level of vehicle ownership may still increase as neighborhoods gentrify and cities develop rapidly (Boarnet and Sarmiento, 1998). As a result, the potential vehicle ownership change induced by TNC or carsharing services may be attenuated.

Which of these numbers is more accurate? It is difficult to say with certainty from the available data, but prior work suggests that the unprompted or indirect approach is likely more accurate. Compared with indirect questioning, the direct questioning such as "What impact, if any, has your use of on-demand services such as Uber or Lyft, had on your vehicle ownership decisions?", may provoke availability bias. Availability bias, or the availability heuristic, is the phenomenon by which people tend to make judgments about the likelihood of an event or the frequency of classes based on the ease with which relevant examples come to mind (Tversky and Kahneman, 1973). Furthermore, Fisher (1993) revealed that indirect questioning (as opposed to direct questioning) can lead to more credible data, because it reduces the social desirability bias, which is defined as the systematic error in self-reported measures resulting from the desire of respondents to avoid embarrassment and project a favorable image to others. For example, respondents may be more likely to say that on-demand services led them to reduce car ownership since they may believe that this is a "right" answer. So, we believe that the approach of inquiring about changes and then asking respondents to attribute those changes to various reasons is more neutral, while the approach of asking them about the effect of a specific set of services may lead to overestimation of the effects of those services. In a perfect world, we might run an experiment to resolve these discrepancies. But as our UW study shows, experiments in a travel behavior context are challenging and costly to implement, which is why there is such a heavy reliance on user surveys in the first place.

The consequences of these discrepancies are significant. State and local governments are regulating or considering regulating services including carsharing and TNC services that use public right-of-way. A significant consideration in regulating these services is the positive or negative externalities that they place on cities. Changes in car ownership and travel rates are likely to affect vehicle trips, parking demand, traffic throughput, carbon emissions and fuel consumption. Carsharing and TNC services could reduce car ownership, vehicle trips, and demand for parking. Alternatively, they could increase vehicle miles traveled (VMT), reduce transit ridership, potentially leading to cuts in public transportation service and lower job accessibility for lower-income households. Though these are critical questions, the results of this study show that the estimated number of private cars avoided due to mobility services is difficult to measure empirically, while self-reported estimates can vary by more than a factor or two depending on how the respondents are asked about this. Future experimental work on this topic should employ larger sample sizes and/or stronger incentives, while surveys should employ indirect questioning to elicit effects of service adoption on behavior.

Author statement

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Appendix. Statistical Analyses

Instrumental Variable (IV) estimation

When an exogenous variable of interest is correlated with the error term, the ordinary least squares regression method will produce biased results: the so-called endogeneity problem. In such cases, Instrumental Variable (IV) analysis, which is also called two-stage least squares (2SLS) regression, is efficient. In IV analysis, the instrumental variable is a variable that is correlated with the endogenous variable but uncorrelated with the error term. So, a good instrumental variable should affect the dependent variable only by inducing changes in the endogenous variable. This makes it possible to identify the causal effects of the endogenous variable on the outcome variable.

In the context of this study, people who do not own vehicles might be more likely to adopt carsharing/TNC services because such services provide them with access to vehicles. On the other hand, people who adopt those services, might shed or sell their vehicles because of similar reasons. Also, people who only make occasional trips might be more inclined to use mobility services instead of driving their own car, but replacing car ownership with these services could itself reduce people's trip rates. So, the current study is an example of where exogenous and endogenous variables may affect each other, and this means the traditional OLS regression models are not suitable.

Let Y be the observed outcome variable of interest, let D be the treatment variable, and let Z be the instrumental variable. The 2SLS regression model is shown in equations (1) and (2), and the assumptions of IV estimation are shown in equations (3)–(5). In these equations, u_1 and u_2 represent error terms, π_0 and α_0 are constants, π_1 is the effect of Z on D, and α_1 represents how D influences Y.

$$Y = \alpha_0 + \alpha_1 D + u_2 \tag{1}$$

$$D = \pi_0 + \pi_1 Z + u_1 \tag{2}$$

$$Cov[u_1, Z] = 0 \tag{3}$$

$$\pi_1 \neq 0 \tag{4}$$

$$Cov[u_2, Z] = 0 \tag{5}$$

The major effect which could be identified from IV analysis is the instrumental variable treatment effect; i.e., the causal influence of D on Y only occurs through the correlation between D and Z. Randomization of the encouragement ensures that equation (3) holds, while equation (5) depends on the assumption that the instrument affects the outcome only through its effect on the treatment (the so-called exclusion restriction). In the present context, it is deemed very unlikely that providing an encouragement to try out mobility services would affect car ownership or trip making, except by encouraging people to use the incentivized services.

Difference-in-Difference (DiD) analysis

Difference-in-Difference (DiD) analysis is used to estimate the causal effect of treatment on endogenous variables by measuring the changes over time between treatment and control groups. DiD requires a parallel trend assumption that the difference in the dependent variables for the treatment and control groups should be the same unless the treatment is implemented (Ge et al., 2017). Figure A.1 shows a graphical explanation for DiD estimation. The line P_1P_2 represents the treatment group while the line S_1S_2 shows the control group. The outcome variables of interest in both groups are observed before (represented by the points P_1 and S_1) and after exposure to treatment (represented by the points P_2 and S_2). DiD measures the "normal" outcome if the treatment did not exist (represented by point Q). The treatment effect is then the difference between the "normal" and observed outcome variables (P_2-Q).

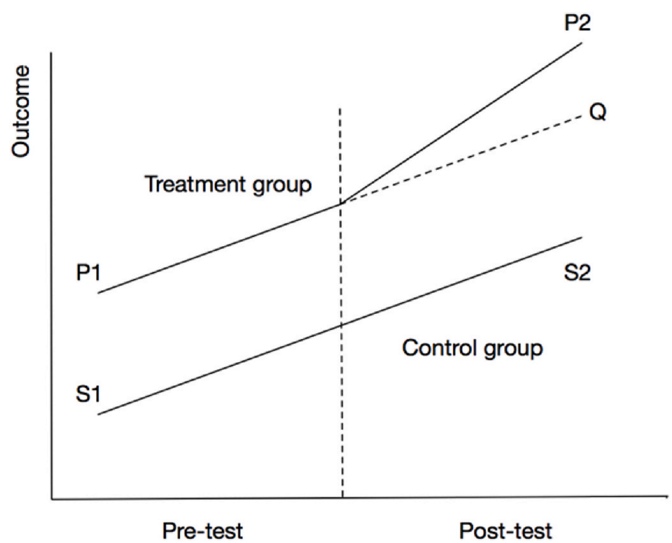

Fig. A.1. A graphical explanation for DiD estimation.

Compared with IV analysis, DiD offers a less compelling empirical strategy for identifying causal effects, since assignment to treatment and control groups is not randomized (in this case, subjects self-select whether to become users of mobility services). However, in a setting where the instrumental variable does not have a strong effect on treatment status, DiD still allows for comparison between self-selected mobility services users and non-users, by assuming that the changes between wave 1 and wave 2 in service adopters can be approximated by the changes over the same period among non-adopters. (This does not require assuming that adopters and non-adopters are the same, only that their differences are stable over time.)

Two dummy variables are created, as shown in equations (6) and (7), and the interaction term between these dummy variables is used to represent the DiD.

$$Treatment = \begin{cases} 1 & treatment\ group \\ 0 & control\ group \end{cases} \tag{6}$$

$$Time = \begin{cases} 1 & after\ treatment \\ 0 & before\ treatment \end{cases} \tag{7}$$

For continuous outcome variables, the regression modeling framework of DiD is shown in equation (8); where β_0 (represented by point S_1) is the expected value of the outcome variable of interest under the baseline condition ($Treatment = 0$, $Time = 0$); β_1 is the time trend in control group ($Treatment = 0$) ($S_2 - S_1$); β_2 is the difference between treatment and control groups before taking the treatment ($Time = 0$) ($P_1 - S_1$); β_3 is the difference in outcome variable changes over time ($P_2 - Q$), which is assumed to be the causal effect of treatment on the outcome variable of interest.

$$Y = \beta_0 + \beta_1 * Time + \beta_2 * Treatment + \beta_3 * [Time * Treatment] + \beta_4 * Covariates + \varepsilon \tag{8}$$

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