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Examining the effects of a temporary subway closure on cycling in Glasgow using bike-sharing data

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ABSTRACT

This study takes the 39-day Glasgow Subway closure in July 2016 as a natural experiment to evaluate the effect of subway closure on bike-sharing trips. We find that bike-sharing trips increased by 20.7% for incoming trips and 20.1% for outgoing trips on average for each bike station in the proximity of subway station during the subway suspension. Some of this change persisted, with 12.4% of the increased bike-sharing trips remaining after the resumption of the subway service. Our findings suggest that first, subway and bike-sharing trips are substitutes; second, this temporary service disruption was not enough to break commuters' long-term habits, and third, the diversion factors implied by our results are much lower than the recommended values for UK cities.

1. Introduction

Cycling has been gaining popularity in cities all over the world.[1] One of the driving forces for this rise in popularity has been the introduction of measures to promote cycling, such as improving cycling infrastructure and events promoting utility cycling. While measures that encourage cycling by making cycling safer and more pleasant have direct effects on the number of cycling trips, it is also of great interest to study the effects of other transport policies on cycling. For instance, how different modes of transport interact. From a transport planner's perspective, understanding how different modes interact enhances demand forecasting, investment decision making and evaluating combinations of transport policies. One approach to studying how different transport modes interact has been to make use of disruptions which force people to break their habits.

There are numerous studies on how public transport strikes or public transport service interruptions affect road traffic (Adler and van Ommeren, 2016; Anderson, 2014; Bauernschuster et al., 2017; Nguyen-Phuoc et al., 2018). Regression discontinuity design has been used in many of these studies. However, there are limited empirical studies related to cycling, partly due to data availability. Fuller et al. (2012) and Fuller et al. (2019) used interrupted time series techniques to study how the number of cycling trips changed during public transport strikes. Their studies have limitations. Firstly, they aggregated the number of trips across bike stations by day and did not make use of the variation between stations. Without the variation, the estimates are more prone to omitted variable bias. Fuller et al. (2019) used the number of bike-sharing trips in other cities without the intervention (strikes) as controls, which may not be an appropriate option for the study of some cities where suitable controls sharing a common trend may not be available. We overcome these limitations by using panel data and the variation between bike stations. We classify bike stations in the same city into treatment and control groups. Also, Fuller et al. (2019) made use of a 7-day strike, while we have a 39-day long subway closure. The 7-day period may not be sufficiently long to observe behaviour change.

These cross-mode studies can provide insights on how responsive cycling is, i.e., the values of cross elasticities (or diversion factors) involving cycling. This is an important insight because empirical studies on cross elasticities involving cycling, either as the intervention or the recipient mode, are rare. As a result, cycling has not been incorporated into many of the (national) transport models yet,[2] partly due to the lack of accurate and appropriate cycling data for model calibration. The

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[1] The top countries with highest increases in total bicycle (trip) counts between 2017 and 2018 are in Europe (Poland, Czech Republic, Luxembourg, Sweden, Norway, Finland, Germany, Austria, Switzerland, France, Ireland, Great Britain, Spain), North America (Canada, United States), South America (Chile), and Australia (Australia, New Zealand). (Source: https://www.eco-compteur.com/en/2019-worldwide-cycling-index/) [accessed 30/06/2020].

[2] For instance, the current version of Land use and Transport Integrations in Scotland (LATIS) does not include cycling. The challenges of including cycling in a Strategic Transport Model were discussed in https://www.ucl.ac.uk/transport/sites/transport/files/Gent_slides.pdf. [accessed 30/06/2020].

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recent proliferation of new data sources on bike sharing[3] has provided new opportunities to researchers to address this gap in the literature. Our study supplies a value of such a measure to represent how bike-sharing trips responded to subway closure in a medium-sized city and from a previous review on cross elasticity studies, we know that the evidence on this type of response is sparse (Dunkerley et al., 2018). This value enables us to compare our estimate with the recommended value in literature.

In this paper, we evaluate the effect of a 39-day closure of Glasgow's subway system on trips made using the city's shared bikes. This period of subway suspension makes it a suitable opportunity to study the response of cycling because this suspension can be regarded as a quasi-experiment (or natural experiment). Although the choice of bike stations in the proximity of subway stations (the treatment group) is by no means randomised, the panel data structure allows fixed effects to capture the variations between bike stations that are not time-varying.

Our paper has two main methodological strengths: first, the subway suspension is inheritably a good natural experiment because the treatment assignment is determined exogenously so potential problems of internal validity are minimised; second, the subway suspension period was over one month and the dataset contains over 300 days after the resumption of the subway service. Since it has been found that changes in cycling behaviour, especially changes in utility cycling behaviour take time (Cope et al., 2003; Goodman et al., 2013; Smith et al., 2011; Song et al., 2017, for instance), the 39-day suspension period gave enough time for travellers to adjust. Similarly, the extended period after the resumption of service allowed time for the travellers to switch back to the subway. The length of the experiment and study period enables us to evaluate both the effects during the suspension and the rebound effect after the resumption of service.

The remainder of the paper is organised as follows: Section 2 reviews the literature. Section 3 describes the data, defines the scope of the study and discusses the identification strategy. Section 4 interprets the results and concludes.

2. Literature review

We first review the previous studies on how public transport interruptions such as strikes affected road traffic (2.1). Although the focus of this section is not cycling, it is important to review these studies because the approaches and techniques used in these key papers are transferrable to studies of public transport interruptions and cycling, such as our study. Second, we discuss the common indicators (elasticities and diversion factors) representing how the demand of a certain transport mode responds to shocks (2.2). Third, we review literature on the stages of behaviour change model in transport and how cyclists respond to shocks, and the persistence of the effects. (2.3). We end this section by highlighting the contributions of our work (2.4).

2.1. How public transport affects road traffic

There is an abundance of literature which quantifies how public transport affects road traffic (Basso and Silva, 2014; Duranton and Turner, 2011; Nelson et al., 2007; Parry and Small, 2009, for instance). They either constructed analytical models and ran simulations or adopted empirical methods to evaluate how public transport affected road congestion but did not have strikes as interventions. Lo and Hall (2006) and Nguyen-Phuoc et al. (2018) had strikes as interventions and evaluated the impacts of strikes on congestion but they focused on congestion effects of motorised traffic in a network. This literature review will focus on three key papers (Adler and van Ommeren, 2016; Anderson, 2014; Bauernschuster et al., 2017) because they either produced very different results from previous literature, or made comprehensive efforts to evaluate the importance of public transport on alleviating problems associated with road traffic (e.g., congestion) using public transport strikes, in terms of their implications on transport policies such as public transport subsidies. The common ground of the literature in this subsection are as follows: The interventions involved are all public transport strikes, and car traffic was the recipient mode, with one paper (Adler and van Ommeren, 2016) also estimating the cycling response. These studies made use of data on ridership and traffic flows, and adopted either a regression discontinuity (RD) or difference-in-differences (DD) design. The time horizon and persistence of the response were not discussed.

Using a strike by Los Angeles public transport workers in 2003 and a regression discontinuity design, Anderson (2014) estimated that the strike increased average highway delays by 47 percent, showing that public transport could relieve congestion by a much larger degree than previously believed. They used the 35-day closure of public transport and implemented a regression discontinuity design with the date as the running variable. The day when the strike began is the discontinuity threshold. The dependent variable is the average delay for a location at a certain hour. Their findings provide a strong argument for subsidising public transport.

Similarly, Adler and van Ommeren (2016) studied Rotterdam, a mildly congested city, using multiple citywide strikes of one or more public transport modes. They studied how strikes affected travel time on roads and to what extent strikes induced travellers to switch to cycling. They found that the congestion relief effect was much larger for inner city roads (10 times larger) than ring roads (several times smaller than what was found in Anderson (2014)). Also, they showed that a full-day strike increased the number of bicycle users by the same magnitude as in increase in car travellers on a strike day, implying the same diversion factors for car and cycling as the recipient mode. Note that this large diversion from cars to cycling was found in a city with high cycling share (about 22% of trips in 2017[4]). Our case study city, Glasgow, has a much lower cycling share (1.6% of work trips[5]).

Bauernschuster et al. (2017) quantified the effects of public transport strikes in urban Germany. They made use of 71 public transport strikes in German cites to evaluate the effect on road traffic in terms of traffic volumes and travel times, its opportunity cost due to the increase in car hours (both traffic volume and travel time), crashes, air pollution, and health effects. They took a generalised DD approach and found that total car hours increased by 11 to 13 percent, which was a combined effect of a 2.5 to 4.3 percent increase in the number of cars and an 8.4 percent increase in travel time. The congestion costs for all 71 strikes were estimated to be €228.9 million. As a result of the increase in car hours, crashes increased by 14 percent, crash-related injuries increased by 20 percent, particle pollution increased by 14 percent and hospital admissions for respiratory diseases among young children increased by 11 percent.

These papers reported the effects of the strikes either as an absolute magnitude or as a percentage change in road traffic or cycling. Alternative representations of the effects are elasticities and diversion factors discussed in the next section. These two measures are of interest because they have been widely used by practitioners in calibration of transport models (Börjesson et al., 2017; Monchambert and Proost, 2019; Proost and Van Dender, 2000) and thus they are crucial in transport policy evaluation and cost benefit analysis. They provide a common measure for comparison among studies and enhance transferability of studies.

[3] For instance, https://bikeshare-research.org/ and https://bikesharemap.com/. [accessed 30/06/2020].

[4] https://www.kimnet.nl/mobiliteitsbeeld/mobiliteitsbeeld-2019#/rapport/1.3.2 [accessed 30/06/2020].

[5] Census 2011: https://www.understandingglasgow.com/indicators/transport/travel_to_work/trends/glasgow_trends [accessed 30/06/2020].

2.2. Cross elasticities and diversion factors

In the literature, two measures are often used to express the responsiveness across modes (Litman, 2019). We summarise these measures here as they are concepts we use throughout the rest of the paper. First, the responsiveness can be represented by own or cross demand elasticities. When the generalised price (or its components) of a trip of the intervention mode changes, the number of trips by the cross mode responds (recipient mode). This is a dimensionless number which is defined as $\varepsilon_d = \frac{\%\Delta inQ_r}{\%\Delta inP_i}$, where P_i is the generalised price of the intervention mode and Q_r is the quantity of the recipient mode.

One limitation of using cross-elasticities is that the measure is sensitive to the original quantity (market size) and the price change. This makes it less suitable for comparing results across different studies. To address this shortcoming, another way to express the same idea is the diversion factor. It is defined as follows: when there is one less trip made with the intervention mode due to the intervention, what proportion of the trip shifts to the recipient mode.

There have been numerous studies on these measures, which could be the results from empirical research on intervention and response, national travel surveys, or calculations from national or regional transport models (Dunkerley et al., 2018; Litman, 2019). For example, the elasticities used in the METS (Metropolitan Transport simulator) to simulate transport supply and demand in London came from calculations from the National Travel Survey and results from fare policy changes (Litman, 2019). Since there are so many studies associated with elasticities or diversion factors, we focus our review on meta-analyses and reviews. These analyses can provide sensible values of elasticities or diversion factors when there is no previous research available for a certain interaction in a particular area. Also, from the wide coverage of these studies, it is apparent that there are fewer values (own or cross elasticities) associated with cycling.

Wardman (2014) conducted the largest meta-analysis of travel demand which covered 167 studies in the UK, and 1633 elasticities for car, rail, bus and underground were estimated. These estimates are own price elasticities of travel demand. They made a distinction between long-run and short-run elasticities, and also the data used and how the elasticities were obtained: stated preference discrete choice analyses, or stated preference data aggregated and analysed in demand models. They also distinguished between price elasticities related to trips, kilometres and fuel consumption. Subsequently, there were more reviews (e.g., Dunkerley et al., 2014) not limited to own elasticities of road traffic. An example of studies on estimates of elasticities of cost components other than generalised price elasticities (such as time and distance elasticities) is Wardman (2012) on travel time elasticities. With 427 UK direct elasticities of travel time from 69 studies, it was the most extensive survey on direct time elasticities including studies published in academic journals, commissioned and unpublished studies conducted by commercial organisations.

However, cross elasticities between modes are of primary interest to us because of the lower availability of studies and less agreement about values among researchers and practitioners. Fearnley et al. (2018) conducted a detailed review of cross elasticities and diversion factors. Their review contains evidence from sources including Google Scholar, research databases, Bureau of Infrastructure, Transport and Regional Economics Elasticities Database Online, online journals, and unpublished materials held by authors. These sources covered the cross elasticities between public transport modes (rail, bus, light rail, metro), with most values related to fare change and in-vehicle travel time. Also, distinctions among large metropolis, metropolitan, urban conurbations, suburban and rural areas were made. The review concluded that more data are needed for urban conurbations, small towns and rural areas since most previous efforts were on metropolitan areas.

The most relevant to our study is the cross elasticities or diversion factors including cycling. Dunkerley et al. (2018) did a rapid evidence review on both academic and commissioned and unpublished literature on bus elasticities and on diversion factors involving all modes. The 934 estimates were from Europe, US, Canada, Australia, New Zealand and UK (803 estimates of the 934). But given the sparse data for cycling, more than half of the values on diversion from this mode and a quarter of values on diversion to cycling came from outside the UK. It is challenging to generalise or apply the diversion factors involving cycling because they are not symmetric and data on cycle interventions are limited. Also, it is problematic that these data are from different study designs (observed changes, reported best alternative, stated intention, transfer place/time) and distinguished by choice set, area, journey type, trip purpose or traveller type. According to this review, the recommended value of diversion factor with subway being the intervention mode and cycling being the recipient mode is 0.05 in an urban area. We will be able to draw insights and compare to this recommended value with our main regression results given appropriate assumptions.

The studies covered in these review papers are based on studies with trips which treat different modes as competitors, that is, travellers are substituting between modes. Therefore, the cross elasticities are always positive. However, it is also possible to have negative cross elasticities when the trips between two modes are combined to get from an origin to a destination (complements).

Previous studies on substitutability and complementarity of trips (Hall et al., 2018; Martin and Shaheen, 2014) found that city centre subway (or other public transport modes) and shared bike (or Uber) trips can be substitutes or complements. Using the difference-in-differences design and through the entry and growth of Uber, Hall et al. (2018) found that Uber was a complement for the average-sized public transport agency, and the effect was even stronger for smaller public transport agencies in larger cities. The main reason for this difference in magnitude is the smaller range of fixed-route and fixed-scheduled service of smaller agencies. Martin and Shaheen (2014) made use of survey data from Washington DC and Minneapolis to explore whether bike-sharing induced the shifts toward or away from bus and rail. In Washington DC, the shift was toward rail and bus in the urban periphery but away from rail and bus in the inner urban areas. The reason is bike-sharing often serves as a first-mile last-mile facilitator in less dense areas.

The story is slightly different for electric bikes. Guidon et al. (2019) used a combination of e-bike-sharing booking data and household travel survey data to give an overall picture of the competitiveness of e-bike-sharing compared with other modes in terms of distance ranges and trip times. It was shown that e-bike-sharing can potentially be a substitute of many modes including fast mode such as taxi or slow mode such as walking. While Fishman et al. (2013) summarised that low substitution of cars by bike sharing was found in various cities, and high substitution (and even complementarity) was found between walking/public transport and bike sharing, the evidence on substitution between e-bike-sharing and other modes has been sparse. Cairns et al. (2017) and de Kruijf et al. (2018) both found strong substitution of car trips by e-bike-sharing trips, but studies on substitution between e-bike-sharing and public transport are absent.

2.3. Evaluating response in stages and persistence

The following literature deals with the process and stages of behaviour changes and types of changes in transport using stated-preference surveys. By understanding the process and stages of behaviour changes, and exploring studies on the role of habit in travelling behaviour, we also have more information on the persistence of changes. In light of the recent development in policies and measures incentivising the use of e-bikes, we discuss how users reacted to these initiatives relating to the process and stages of behaviour changes literature.

2.3.1. 5-stage transtheoretical model and cycling

A large branch of literature borrowed the 5-stage transtheoretical

model first used in health promotion research on promotion of physical activity. The stages of change model consists of 5 stages: pre-contemplation, contemplation, prepared for action, action, maintenance (Prochaska and DiClemente, 1983). In the pre-contemplation stage, the traveller 'never really thinks about and not even considers cycling to a daily activity'. In the contemplation stage, the traveller 'never used a bicycle but sometimes thinks about cycling to a daily activity'. In the 'prepared for action' stage, a traveller 'rarely or sometimes cycles to a daily activity'. The action stage is characterised by an individual 'who has fairly often cycled to a daily activity' and the maintenance stage consists of travellers who 'cycle regularly to a daily activity'.

Along with Mutrie et al. (2002) and Rose and Marfurt (2007), Gatersleben and Appleton (2007) was one of the earlier efforts to apply the transtheoretical model on cycling behaviour. A survey and a trial were conducted at the University of Surrey. Members of staff and students were invited to participate in questionnaires and around 20% of them (who had never cycled to the university and were willing to try) participated in an action study where they were provided with bikes for a period of two weeks. Their perceptions of barriers (personal or external) were found to have changed at different stages. There have been more recent efforts to make policy recommendations to incentivise bike use. Nkurunziza et al. (2012) examined the motivators and barriers for commuting cycling in Dares-Salaam, Tanzania using a travel survey for daily commuters. They made the distinction between different stages of changing cycling behaviour and found that cyclists in early stages and non-cyclists in late stages reacted very differently to motivators, barriers and policy interventions of cycling. The findings on these reactions can help identify the appropriate interventions to drive individuals at different stages of behaviour change and achieve policy goals more effectively. Thigpen et al. (2015) used the campus travel survey at the UC Davis and applied the Bayesian multilevel ordinal logistic regression model based on the transtheoretical model. They found that attitude to cycling was more important than attributes of travel to push users through stages to become regular commuting cyclists. This conclusion called for 'soft' policies which aim at changing travel attitudes.

Also using the 5-stage transtheoretical model but having an intervention as a quasi-experiment, Parkes et al. (2016) studied commuter travel behaviour change before, during and after the London Olympic and Paralympic Games in 2012. A 3-wave survey was conducted between July and December 2012 which included the period right before and after the Olympic and Paralympic Games. They found that reducing and re-timing journeys were the most common changes (31% and 25% respectively), followed by re-routing (16%) and re-mode (11%). In terms of stages, re-timing was the most common type of change at each stage except for pre-contemplation. Re-mode and re-routing were more common at later stages of contemplation and maintenance, while reducing and re-routing were more prevalent in the pre-contemplation stage. In addition, they performed a two-step cluster analysis which explored the clustering of people with particular characteristics to see whether they were more likely to certain types of changes. More recently, Biehl et al. (2019) used a nested-logit model based on the transtheoretical model framework to evaluate user characteristics at different stages. They were able to translate their findings to practical policy interventions for different stages of change so that users are pushed through from intentions to behaviour.

2.3.2. Psychological process of changes in and attitude towards cycling behaviour

Other than the 5-stage transtheoretical model, there is a strand of literature in environmental psychology on psychological determinants and the process of behavioural changes initially driven by social responsibility and environmental awareness. Much of the work is about greener mode choice but not specifically associated with the public bike system (for instance, Carrus et al., 2008; Fujii, 2006; Gardner and Abraham, 2010; Vredin Johansson et al., 2006).

More specific to bike sharing, J. Kim et al. (2017) studied the psychological factors that affect user's attitudes and perceived value of public bike systems. They outlined that the process of behavioural changes starts with problem awareness, ascribing responsibility, forming personal norm, awareness of consequences of the behavioural change and finally the change in behaviour and value perception. These paths were estimated and found to be statistically significant at 5% level.

Extending the theory of planned behaviour with theories of perceived value and residual effects, Cai et al. (2019) built hypotheses around five independent variables involving intention for bicycle-sharing commuting, value and residual effects, attitude towards the bicycle-sharing commuting, subjective normal and perceived behavioural control. They found that the variables for residual effect (own/others past behaviour and experience), attitude (belief, moral obligation, perception of results), subjective norm (government policy, normative beliefs, motivation to comply), perceived behavioural control (control belief, perceived power, self-efficacy) are statistically significant in affecting intention, and in turn intention is found to be affecting behaviour.

2.3.3. Habit forming and persistence of change

After taking action to change behaviour, the formation of habit is essential to sustain persistent changes. Studies such as Larcom et al. (2017) and Gravert and Olsson Collentine (2019) explored habit forming in public transport: the former showed that a change in travel routes caused by a strike on the London underground was persistent (people sticking to the change) even after the strike period, while the latter utilised different measures (behavioural interventions) such as free public transport access, social norm messages and two different length of time of providing the free access and found that the increase in length of the free access helped with habit forming. These studies identified measures enhancing habit forming in public transport usage, similar literature of habit forming in cycling has been scarce.

Although the results from applying the stages of change model can provide insights on the persistence of effects when discussing the movement between stages, the above literature do not directly make inferences on persistence. Rose and Marfurt (2007) used a monthly panel survey of the 'Ride to Work Day' participants and a follow-up survey five months after the event to measure the stages in the event-based behaviour change process in Victoria, Australia. They found that 17% of participants were first-time riders to work and 27% of the first-time riders were still riding to work five months later. Out of the 83% of prior riders, 67% of them still rode to work five months later. Their results show a certain degree of persistence of the behaviour changes brought by 'Ride to Work Day'. Also, Fuller et al. (2012) estimated the changes in the number of shared bike trips during and after public transport strikes using the interrupted time series (ITS) techniques. Later on, they furthered their study using controlled interrupted time series techniques (Fuller et al., 2019) and found an increase of between 86 and 92 trips per 100,000 population for Philadelphia's bike-sharing trips during public transport strikes but a decrease of 80 trips was found after the strikes so the effect brought by strikes was not sustained. We have a 39-day suspension period compared to two 1-day strikes in Fuller et al. (2012) and a 7-day period in Fuller et al. (2019). This longer suspension period allows more time to better capture cycling behaviour change.

2.3.4. Recent e-bike incentives

Not directly applying the 5-stage transtheoretical model, the effects of e-bike incentive programmes were reviewed extensively in Cairns et al. (2017). In these reviewed schemes, participants were either given free access to e-bikes, rental of e-bikes or purchase subsidies. The effects of the schemes on travel behaviours during the intervention (schemes) were reported and questions on future plans to use e-bikes were asked. Although in many of the reviewed studies a substantial proportion of respondents (30%-70%) said they planned to continue using e-bikes after the scheme, their claims could not be verified as no further surveys

Table 1
Bike stations and start dates.

Phase	Service start date	Number of stations
1	24 June 2014	31
2	4 May 2015	10
3	18 April 2016	2
4	19 September 2017	10
5	5 April 2018	9

were conducted (after giving time for respondents to access or purchase e-bikes). In the Brighton study conducted by the same authors, follow-up surveys were conducted one year after the initial scheme, 5 respondents out of 80 actually owned an e-bike and reported increased propensity to cycle. Also related to the re-mode decision, de Kruijf et al. (2018) evaluated the effects of an e-bike incentive scheme in the Netherlands and found that on top of factors such as age, gender physical condition, car ownership and household composition, the shift to e-bike is affected by participants' openness to different modes and their dissatisfaction with the current travel mode.

2.4. Our study

Our study is able to enrich the scarce literature on evaluating the response involving cycling, especially for diversion factors obtained from observed changes[6] and in urban conurbations (but not metropolitan areas or large metropolis), and bridge the gap in the lack of discussion on the relationship between public transport modes and bike-sharing (whether they are substitutes or complements). Also the previous studies on persistence of effects suggest that there have been fewer studies on persistence of changes outside surveys. Fuller et al. (2019) is the closest to our study because we have a similar intervention (public transport strikes vs service suspension) and similar datasets of shared bike trips and control variables. However, we do not adopt the interrupted time series techniques because we have panel data of all bike stations and we are able to make fuller use of the panel data structure of different shared bike stations over time. The variation between bike stations helps with pinning down the time-fixed effects and improving our estimates. Also, while in their study they used cities of similar level of shared bike users out of the whole population as controls, but the station-level data allows us to divide the bike stations into treatment and control stations within the city. This helps us identify the change in number of trips in bike stations near subway stations comparing with the change in number of trips in bike stations not within the proximity of subway stations. In addition, we have longer subway closure period than their studies to allow us to examine the process of cycling behaviour changes.

3. Methodology

3.1. The case study city

We take Glasgow, Scotland as our case study. The city is the largest in Scotland with over 633,000 people living within the city council's boundaries.[7] Cycling mode share has traditionally been low in the city. For example, in the 2011 census only 1.6% of people made their journey to work by bicycle. This compares to 4.3% in Edinburgh. Significant efforts have been made to encourage more people to cycle, including the provision of shared bikes.

Glasgow's subway system is the third oldest subway system in the world, having been in existence for over 120 years. Strathclyde Partnership for Transport (SPT) and Glasgow City Council announced the planned suspension in April 2016 on their websites and through their social media channels. The subway closure dates and arrangements were also featured in a local newspaper, the Glasgow Times.[8] The service was suspended from the 2nd of July 2016 "to allow for essential renewal works to take place on the 'ramps and turnout' section of the Subway tunnels"[9]. This was part of a £288 million modernisation plan of the subway which included the upgrade and replacement of trains, signalling, platforms and stations. The closure was planned for four weeks and a replacement bus service which followed the subway route was provided at a lower fare than the usual subway fare. The subway service resumed after a delay of about one week on the 10th of August 2016.

3.2. Data

Nextbike works with local stakeholders to develop and operate bikeshare systems in cities such as Glasgow, Stirling and Cardiff. In Glasgow, Nextbike owns 62 docking stations (as of August 2019). To use the bikes, the users must register through the app, the website or a hotline, then enter the number of the available bike to release the lock. To return the bikes, the user parks the bike at the frame at an official station. In terms of pricing, the users have the options 'Pay as you ride', monthly and annual memberships. A bike rental is free for the first 30 min with monthly or annual membership and addition charges are incurred every 30 min. Students and staff members of the University of Glasgow have been offered free memberships since the 15th October 2015. There is also a maximum charge for every 24 h and a fine will be incurred if it is returned outside an official station (increased fine if repeated offense). When a user returns a bike incorrectly more than three times, the account will be shut down.

The bike-sharing dataset (Nextbike) we obtained from Glasgow City Council contains information about the trips (origin, destination, start and end time, duration of a trip) within the city of Glasgow. The trips were recorded from the launch of Nextbike rental in Glasgow on the 24th of June 2014 to the 29th of August 2019 and the date of the subway suspension was from the 2nd of July 2016 to the 9th of August 2016 (the intervention).[10]

Based on the evidence of significant weather effects on cycling activities from literature, we also include data on weather as control variables. Maximum temperature, rainfall and windspeed during the study period are available through the MET Office website.[11]

3.2.1. Trip data description and cleaning data

As of August 2019, there are 62 Nextbike stations in the city of Glasgow. However, not all of these bike stations started to operate when Nexbike launched in the city. Table 1 shows the date on which the first trip was recorded at the bike stations. We categorise the launch of bike stations into five phases according to their service start date in Table 1.

The bike stations that started to operate before the subway suspension period are categorised as the first three phases. Due to the operations of new bike stations in phase 4 and phase 5, a sharp increase in trip numbers is observed when we graph the number of trips by day. The increase was caused by the direct increase in trips to and from the new stations, and the indirect increase in trips caused by the growth in number of trips and users when there were more bike stations in the city. To avoid these trips skewing our estimates, we only include stations in

[6] Only 26% of diversion factors reviewed in Dunkerley et al. (2018) were obtained from observed changes.

[7] https://www.nrscotland.gov.uk/files/statistics/council-area-data-sheets/glasgow-city-council-profile.html [accessed 12/06/2020].

[8] https://www.glasgowtimes.co.uk/news/14449096.subway-closure-dates-confirmed-how-will-you-travel/ [accessed 11/04/2021].

[9] SPT News: http://www.spt.co.uk/2016/07/21688/ [accessed 30/06/2020].

[10] BBC News: https://www.bbc.co.uk/news/uk-scotland-glasgow-west-37025050 [accessed 30/06/2020].

[11] Met Office: https://www.metoffice.gov.uk/ [accessed 30/06/2020].

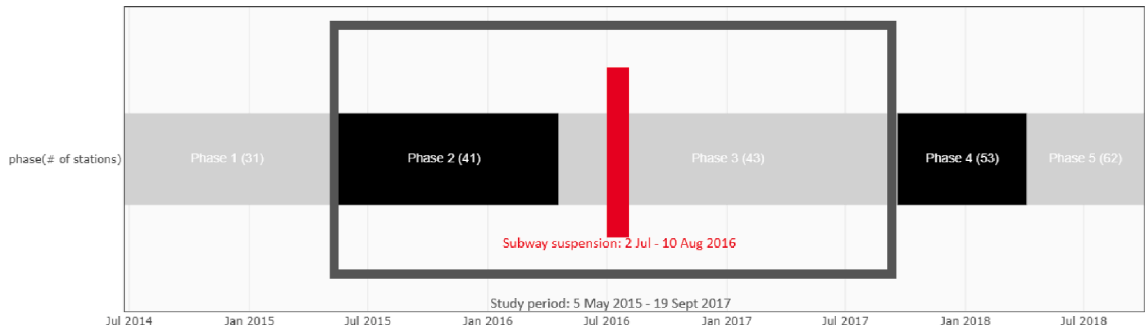

Fig. 1. Timeline. Timeline created by authors using the vistime package (Raabe, 2021) based on data from Mcpherson (2014) and news articles.

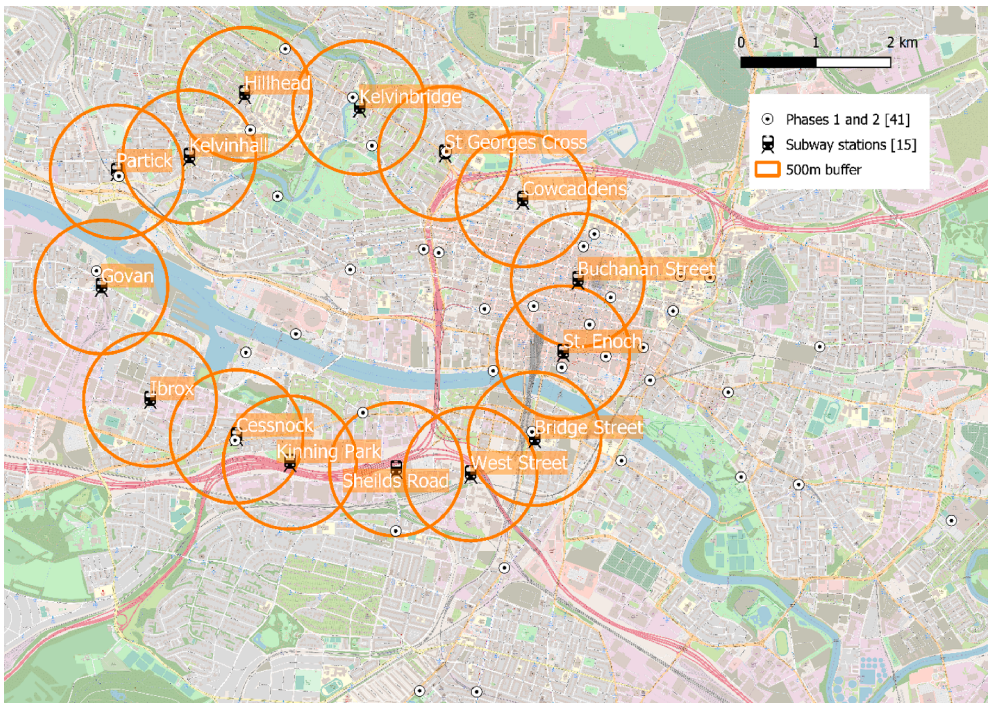

Fig. 2. Glasgow City subway stations, catchment and bike stations. (Background map: Open Street Map; number of features in squared brackets.)

phase 1 and phase 2. We ignore the two stations in phase 3 due to the small number. Also, we expect the introduction of phase 3 stations to have little effect on the existing stations.

For the study period, we choose to drop the period before the introduction of phase 2 stations (5 May 2015, day 326) and the period after the introduction of phase 4 stations (19 September 2017, day 1184). This can ensure that all the phase 1 and phase 2 stations were in service during the entire study period and minimise the effect of the new entrant stations have on the number of trips in these existing stations in phase 1 and 2. The length of the study period is appropriate for the analysis, with 415 days before the suspension period (39 days) and 406 days after the suspension period. Fig. 1 shows the timeline and thus justifies the choice of the stations and study period.

Since our objective is to quantify how bike-sharing trips were affected by subway suspension, in other words, whether bike-sharing trips were replacing some of the previous subway trips, we focus on the bike stations in the proximity of subway stations. A catchment area of 500 m radius[12] is created for each subway station and any bike stations within at least one catchment area are in the treatment group (18 stations). The remaining stations are in the control group (23 stations). Fig. 2 shows the location of subway stations, their catchment areas and bike stations in phase 1 and phase 2.[13]

To prepare for the main analysis, we tabulate the number of trips by day associated with each station (meaning that the trips either have the stations as origin or destination). Our main goal of cleaning the data is to exclude errors and recreational trips that are not associated with subway trips. The bike-sharing trip dataset includes some records with possible errors, for example, entries with invalid start or end stations. We

[12] The main regressions were also run using a 200m-radius and a 800m-radius to see the effects of different catchment sizes. The results are presented in [Appendix A. Different sizes of catchment areas].

[13] The treatment group and the control group are not randomly assigned. On top of examining the common trends, the comparison of the two groups using the difference-in-differences method can be justified by testing for common trends of the two groups before the treatment. We follow the test in Autor (2003) by including leads and lags of the treatment effect. For the difference-in-differences setup to be justified, the coefficients of leads should be zero while the coefficients of lags may not be identical. We found that the lead coefficients centred around zero and were different from zero during the treatment and after the treatment.

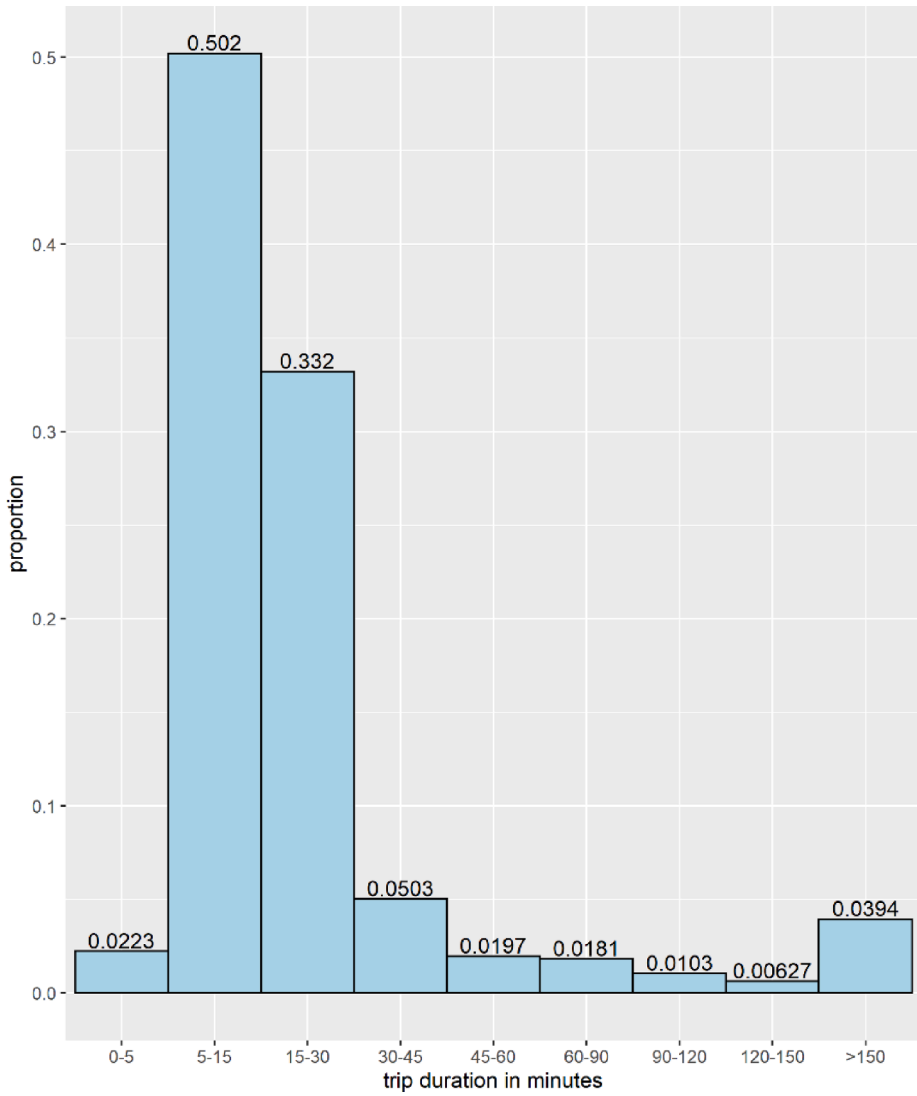

Fig. 3. Proportion of trips and duration.

matched the bike station list of Glasgow[14] with the origin and destination of all entries and excluded records which did not match. This reduced the data set from 857,402 entries to 802,068 entries. Another possible error is that the users unlocked the bikes by mistake. Trips with the same start and end stations are excluded, which leaves us with 669,303 entries. In this way we also exclude some recreational cycling trips that are not associated with subway trips. Then we exclude the trips that were longer than 150 min because it was very likely that these were recreational bike trips that are not associated with subway trips, or simply mistakes. The 150-minute upper limit is meant to exclude recreational trips that are not associated with subway trips because the longest bike trip between two phase 1 and phase 2 stations (Queen Margaret Drive station and Emirates Arena station) is 9.6 km. This takes 36 min assuming an average speed of 16 km per hour, which is slow for a commuter but fast for occasional cyclists. The 150-minute cut-off is possibly too long for this reason. To understand how this choice of cut-off could affect our results potentially, Fig. 3 shows the number of trips with certain durations in the form of a histogram. 26,361 trips (3.94% of total) are dropped as a result of the 150-minute upper limit.[15]

Fig. 4 plots the daily average number of trips of all stations, treatment stations, and control stations. The suspension is marked by the two vertical lines. The upper panel shows the average incoming trips and the lower panel shows the average outgoing trips. Looking at the smoothened lines (Loess regression, 10% smoothing span), we notice the seasonal fluctuations and sharp rises around the suspension. Note that the largest differences among the all stations average, treatment stations average and control stations average are observed during the suspension. This provides a strong argument for the difference-in-differences design. Note that the average number of trips for each bike station is rather low (around 10 trips); this can potentially be a limitation to the study.[16]

[14] Available at Nextbike API: https://api.nextbike.net/maps/gbfs/v1/nextbike_gg/gbfs.json. [accessed 30/06/2020].

[15] We have also test for the sensitivity of this cut off rule by using an alternative cut off rule: assuming a minimum speed of cycling and computing different thresholds for different origins-destinations. The results are shown in [Appendix B. Dropping trips by a different rule].

[16] Despite the lower number of trips, we have consistent results under different settings and the results are shown to be robust (see Section 4.2).

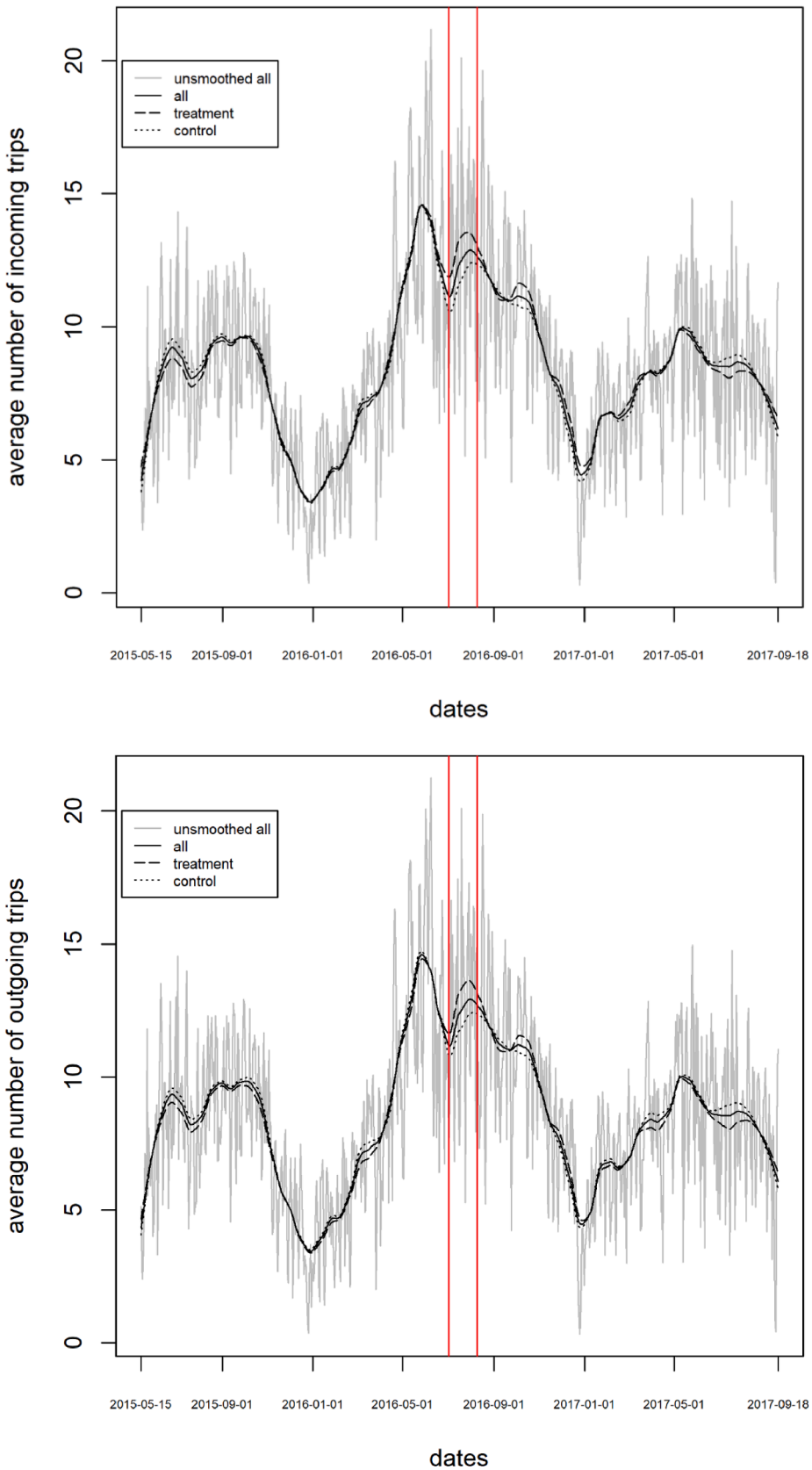

Fig. 4. Daily number of bike-sharing trips. (upper panel: incoming trips; lower panel: outgoing trips).

Table 2
DD setup.

Time period	Treatment	Control
Pre-suspension	θ	0
Suspension	$\delta_1 + \theta + \mu_1$	δ_1
Post-suspension	$\delta_2 + \theta + \mu_2$	δ_2

Table 3
Regression results (Incoming/Outgoing trips).

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
weekend	−2.251*** (0.098)	−2.267*** (0.095)
publichol	−2.325*** (0.181)	−2.356*** (0.176)
max_air_temp	0.314*** (0.011)	0.315*** (0.010)
rain	−0.130*** (0.004)	−0.130*** (0.004)
wind	−0.151*** (0.009)	−0.153*** (0.009)
mon	0.167* (0.098)	0.145 (0.096)
tue	0.684*** (0.098)	0.688*** (0.095)
wed	0.761*** (0.098)	0.780*** (0.095)
thu	0.500*** (0.098)	0.503*** (0.095)
sat	0.170* (0.097)	0.176* (0.095)
jan	0.790*** (0.170)	0.851*** (0.165)
feb	1.673*** (0.174)	1.726*** (0.169)
mar	2.464*** (0.169)	2.534*** (0.164)
apr	3.014*** (0.169)	3.089*** (0.164)
may	3.539*** (0.158)	3.599*** (0.153)
jun	2.542*** (0.160)	2.590*** (0.155)
jul	1.213*** (0.167)	1.269*** (0.162)
aug	1.576*** (0.162)	1.627*** (0.158)
sep	1.629*** (0.152)	1.675*** (0.147)
oct	2.989*** (0.148)	3.055*** (0.143)
nov	1.929*** (0.140)	1.936*** (0.136)
2015	−0.089 (0.184)	0.043 (0.178)
2016	1.266*** (0.111)	1.286*** (0.108)
suspend	1.041*** (0.202)	1.052*** (0.196)
post	−0.376*** (0.136)	−0.322** (0.132)
subwayD:suspend	1.692*** (0.258)	1.649*** (0.250)
subwayD:post	0.208* (0.108)	0.152 (0.104)
Fixed effects?	Yes	Yes
Observations	35,137	35,137
R^2	0.254	0.266
Adjusted R^2	0.252	0.265
F Statistic (df = 27; 35069)	441.551***	471.568***

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors(non-robust) in parentheses.

3.2.2. Other covariates

We have also made use of other data to control for factors affecting bike-sharing trips such as maximum daily temperature, daily rainfall, and average daily wind speed, since cycling activities are known to be affected by weather conditions (Hong et al., 2020; Kim, 2018; Liu et al., 2015). In addition, year, month, day of week dummies are included to capture the trend and seasonal fluctuations. A public holiday dummy is also added. These covariates are important because by handling the suspension as a natural experiment, it is essential that we justify whether the choice of the subway suspension period is in itself "randomised". Although this is not the case, the choice is transparent enough to be incorporated in the model. One of the reasons for choosing July as the suspension month is that subway ridership is usually lower and the inconvenience for travellers can be minimised. For instance, students are not attending the city's three universities and many workers will have taken a holiday. We take into account the seasonality and weather to make sure that the estimates are consistent.

3.3. Identification strategy and hypothesis

Natural experiment 'is often implicitly defined as a law change that affects outcomes for identifiable individuals who are otherwise indistinguishable from those not directly affected by the law change.'(Meyer, 1995). While the term "natural experiment" seems to suggest that the events are experiments and they are spontaneous, this is usually not the case. The spatial and temporal variations of the event can be endogenous (Besley and Case, 2000) and we need to interpret the results with caution in the presence of the control variables especially in the time dimension to minimise the potential biases. We bear this in mind in presenting the identification strategy.

Among studies that have similar interventions (public transport strikes or closures), regression discontinuity (RD) design (used in Adler

Table 4
Regression results: 3 pooled periods.

Main regression in 3 pooled periods		
	Dependent variable: trips	
	(1) Incoming	(2) Outgoing
max_air_temp	0.321*** (0.006)	0.325*** (0.008)
rain	−0.158*** (0.004)	−0.159*** (0.006)
wind	−0.178*** (0.009)	−0.180*** (0.012)
weekend	−2.581*** (0.060)	−2.595*** (0.080)
publichol	−2.721*** (0.179)	−2.774*** (0.241)
subwayD		−0.224** (0.105)
suspend	0.956*** (0.182)	0.889*** (0.245)
post	−0.632*** (0.074)	−0.659*** (0.100)
subwayD:suspend	1.692*** (0.265)	1.649*** (0.357)
subwayD:post	0.208* (0.110)	0.152 (0.149)
Constant		7.865*** (0.137)
Observations	35,137	35,137
R^2	0.212	0.130
Adjusted R^2	0.211	0.130
F Statistic	1,051.943*** (df = 9; 35087)	526.531*** (df = 10; 35126)

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors in parentheses.

Table 5
Driscoll and Kraay standard errors.

Driscoll and Kraay standard errors		
	Dependent variable: trips	
	(1) Incoming	(2) Outgoing
weekend	−2.251*** (0.216)	−2.267*** (0.215)
publichol	−2.325*** (0.492)	−2.356*** (0.486)
max_air_temp	0.314*** (0.035)	0.315*** (0.035)
rain	−0.130*** (0.025)	−0.130*** (0.025)
wind	−0.151*** (0.035)	−0.153*** (0.035)
mon	0.167 (0.213)	0.145 (0.212)
tue	0.684*** (0.202)	0.688*** (0.204)
wed	0.761*** (0.193)	0.780*** (0.193)
thu	0.500*** (0.184)	0.503*** (0.185)
sat	0.170 (0.212)	0.176 (0.212)
jan	0.790 (0.802)	0.851 (0.806)
feb	1.673** (0.790)	1.726** (0.795)
mar	2.464*** (0.806)	2.534*** (0.810)
apr	3.014*** (0.773)	3.089*** (0.780)
may	3.539*** (0.899)	3.599*** (0.907)
jun	2.542*** (0.692)	2.590*** (0.699)
jul	1.213* (0.673)	1.269* (0.679)
aug	1.576** (0.662)	1.627** (0.667)
sep	1.629** (0.707)	1.675** (0.712)
oct	2.989*** (0.581)	3.055*** (0.585)
nov	1.929*** (0.564)	1.936*** (0.568)
2015	−0.089 (1.035)	0.043 (1.042)
2016	1.266** (0.518)	1.286** (0.522)
suspend	1.041 (0.646)	1.052 (0.641)
post	−0.376 (0.699)	−0.322 (0.700)
subwayD:suspend	1.692*** (0.194)	1.649*** (0.200)
subwayD:post	0.208** (0.103)	0.152 (0.097)

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors (Driscoll and Kraay) in parentheses.

and van Ommeren, 2016; Anderson, 2014), interrupted time series (ITS) design (used in Fuller et al., 2012, 2019) and difference-in-differences (DD) design (used in Bauernschuster et al., 2017) are adopted. To provide good estimates, RD relies on the fact that the observations around the intervention or cut-off are as good as randomly assigned, and we need to be confident about the functional form of the fitted function or have many observations around the cut-off (Lee and Lemieux, 2010). ITS can be treated as a special case of RD because ITS can be classified as RD which has the time variable determining the cut-off. In our study, we have a long time span but not a lot of observations around the cut-off. As a result, the flexibility of RD can be misleading in functional form specification. The ITS design takes advantage of the long time series and the possible use of controls (controlled interrupted time series) but the time series model specification lacks economic theoretical foundations. Therefore, we adopt the difference-in-differences (DD) design to take advantage of the presence of a control group. Also, this approach enables us to exploit the non-time-varying intrinsic differences between the bike stations in terms of locations and usage patterns, and further explores the potential differences of the response to subway suspension among different bike stations.

We make use of the panel (41 bike stations, daily number of trips from the 5th of May 2015 to the 19th of September 2017) to evaluate the change in bike-sharing trips during the subway suspension period and after the service resumption. Previously, difference-in-differences (DD) design has often been used in event studies, especially for economic policies as outlined in Meyer (1995). For instance, the seminal work of Card and Krueger (1994) examined the effects of minimum wage state law in New Jersey on employment using Pennsylvania as an unrelated comparable group. The main goals of this research design are identifying the exogenous variation in the key explanatory variables and finding comparable control groups (Meyer, 1995). With regard to the second goal, the DD design is appropriate when there exists a group that does not receive the treatment but experiences some or all of the other influences that affect the treatment group. We adopt this design because it is possible that there were other measures implemented in the same period, such as an improvement in cycling infrastructure, ongoing Nextbike advertising campaigns, and the subway suspension. The simultaneous occurrence of these measures makes the effects of any particular measure difficult to identify. Since the subway suspension only affected the bike sharing trips that might substitute or complement the subway trips, it is possible to use the bike stations within the catchment of subway stations as the treatment stations and the rest as the control stations. One slight difference in the method we adopted is that we extend the analysis to the post-intervention period: we set the period after the subway suspension as a second 'intervention' and the outcome of treatment stations is again compared to the outcome of control stations. A set up like this enables us to investigate the short-term and long-term effects, that is, whether the effect of subway suspension on shared bike trips was persistent, or more precisely, persistent and strong enough to outweigh the effect of the resumption of subway service.

The dependent variable in the regression is the daily number of shared bike trips associated with each shared bike station. The regression equation is

$$trip_{it} = FE_i + FE_t + \beta X_{it} + \delta_1 D_{sus} + \theta D_m + \mu_1 D_{sus} D_m + \delta_2 D_{post} + \mu_2 D_{post} D_m + \varepsilon_{it}, \quad (1)$$

where the subscripts i and t represent the bike stations ($i = 1, \cdots, 41$) and the count of days since the start of the study period ($t = 1, \cdots, 1184$). FE_i and FE_t are the bike station fixed effects and the time fixed effects (year, month, day of week), X_{it} represents the vector of controls (maximum air temperature in degrees Celsius, precipitation in millimetres, average wind speed in knots, public holiday dummy). Lastly, we have three dummy variables which make up our variables of interest. The dummy variables D_{sus}, D_{post} and D_m denote the dummy for the subway suspension period (which takes the value of 1 during the suspension, and 0 otherwise), the dummy for after the subway suspension period (which takes the value of 1 after the suspension, and 0 otherwise) and the dummy for the proximity of subway stations (which takes the value of 1 if the bike station is within the 500-metre radius of any subway station, and 0 otherwise).

Table 2 lists that with a difference-in-differences set up like this, our coefficients of interest μ can be obtained with the difference between the pre-post difference of the treatment group and the same difference of the control group. If μ is negative, the subway suspension decreased the number of shared bike trips. In other words, subway trips complement shared bike trips, and this can be explained by shared bike being the first-mile or last-mile of a commuting trip with the users cycle to and from the subway station for a subway ride. If μ is positive, the subway suspension increased the number of shared bike trips, meaning that subway trips and shared bike trips are substitutes. Travellers substituted the subway trips with shared bike trips during the suspension of subway service.

Previous studies on substitutability and complementarity of trips (e.g., Martin and Shaheen, 2014) found that city centre subway and shared bike trips are substitutes (while suburb train trips and shared bike trips are likely to be complements) because bike-sharing serves as a first-mile last-mile facilitator in less dense areas. We will test this hypothesis: *Since the Glasgow subway serves a dense city area, city centre subway trips and bike sharing trips are expected to be substitutes. In other words, the coefficient μ is expected to be positive.*

This hypothesis describes the expected changes during the subway suspension. However, it is unclear whether the effects will continue even after the resumption of the subway service. While we expect some subway riders who switched to bike-sharing during subway suspension would switch back to ride the subway after the service resumption, some shared bike users would continue to use shared bikes. Therefore we expect *the coefficient μ to be positive, but of a smaller magnitude.*

This modelling framework has a number of limitations. For instance, we implicitly assume that the covariates have the same effects on different bike stations. This is a strong assumption and it might be oversimplifying because some bike stations dominated by some nature of trips could be more affected by some covariates. For example, some bike stations close to office locations could be dominated by work related trips and these trips would be more affected by the covariate representing public holidays. Another major setback is the *sus* and *post* dummies, which imply that the effect of subway suspension was constant during the whole period of suspension and after suspension. This setup does not allow the effects to change over time within the periods.[17]

3.4. Implementation

The data have been analysed using *R* programming language. The dataset was cleaned and arranged using the *dplyr* package (Wickman and Francois, 2016) and the *plm* package (Croissant and Millo, 2008) was used for running the regressions. The tests for serial correlation, spatial correlation and unit root were implemented with the *plm* and *tseries* packages (Trapletti and Hornik, 2016). Lastly, the visualisation of the figure was aided by the *tseries* package and the *stargazer* package (Hlavac, 2015) generated the regression tables.

[17] A referee suggested that the effects may not be constant, especially after suspension, because travellers might slowly switch back to the subway after the resumption of service. We incorporated this potential change in effects by including a variable *postday*, which is the number of days since the resumption of subway service. The results are shown in the appendix [Appendix C. Allowing for varying effect after subway resumption].

4. Results

4.1. Main results

Table 3 shows two regressions of incoming trips and outgoing trips in columns (1) and (2) respectively. In both columns, there are a dummy to mark the suspension period (*suspend*), a weekend dummy (*weekend*), a public holiday dummy (*publichol*), the weather variables, the dummy of subway proximity (treatment group) during the suspension period ($subwayD : suspend$), and the day of week, month and year dummies. This regression is represented by Eq. (1).

We can see that most of the coefficients are significant and align with our expectations. The coefficient of *max_air_temp* is positive and the coefficients of rain and wind are negative. These are statistically significant at the 0.05 level of significance and are in the reasonable range and directions. From the time variables, it is apparent that there are more weekday shared bike trips than weekend trips, and there are slightly more Wednesday trips than the rest of the week. This is consistent with the existing findings of cycling behaviours (Raux et al., 2016, for instance), and the dominance of weekday trips shows that the trips we include in the analysis are more likely to be utility trips than recreational trips. Besides, the year dummies also depict a weak growing trend of shared bike trips from 2014 to 2016 probably due to the overall promotion and thus growth of the use of shared bikes in the city.

The coefficient for the suspension period dummy (*suspend*) is positive and significant, but the coefficient for the post-suspension period dummy (*post*) is negative but of smaller magnitude. The two coefficients of interest ($\delta_1$and $\delta_2$of $subwayD : suspend, subwayD : post$) for incoming and outgoing trips are significant at the 0.05 level of significance, and claim that subway suspension brought 1.69 more incoming bike-sharing trips and 1.65 more outgoing bike-sharing trips per day than before the suspension for bikeshare stations (treatment group) close to the subway stations. Note that these changes in number of trips might seem small and negligible but if we consider that the average number of trips per bike station rarely exceeded 20 trips per day, the increases are non-negligible. These trip increases are equivalent to a 20.7% increase in incoming trips and 20.1% of increase in outgoing trips when compared to the average number of incoming trips per day in July in the study period.

But after the subway service resumed, the increase in average shared bike trips by day was only 0.21 incoming trips higher than before the suspension, and this is not statistically significant at the 0.05 level of significance. The increase of 0.15 outgoing trips after the resumption is not statistically significant. The extra shared bike trips brought by the subway suspension did not bring a persistent effect, with only 12.4% (=0.21/1.69) of bikeshare users kept using the service after the resumption of service. One reason could be due to more travellers switching back to the improved subway service. Another reason could be related to trip purposes. Since travellers do not usually change their cycling behaviour without first trying out with recreational trips (Song et al., 2017), it is reasonable that after the resumption of subway service, we found an opposite effect which captured the travellers who switched back to taking the subway. Some of the initial switches to bike-sharing during the suspension were the recreational trips made by those who were trying out the cycling routes (in prepared for action or action stages of the stages of change model (Prochaska and DiClemente, 1983)), but did not maintain (failed to move to the later maintenance stage).

These key results are consistent with our hypothesis (that the Glasgow subway serves a dense city area, city centre subway trips and bike sharing trips are expected to be substitutes.) and our expectation that some effects did not persist.

4.2. Robustness checks

Bertrand et al. (2004) found that the standard errors in DD analysis are often underestimated due to the presence of serial correlation. As a result, the significance of results is overstated. Therefore, we test for serial correlation using the Breusche-Godfrey test for serial correlation in panel models (Breusch, 1978; Godfrey, 1978) and found that serial correlation in idiosyncratic errors is present. The result of the augmented Dickey-Fuller test (Cheung and La, 1995) shows that the series has no unit root (stationarity). In addition, the result of the Breusch-Pagan test (Breusch and Pagan, 1979; a test for heteroskedastic disturbances in linear regression) and the Breusch-Pagan LM test (Baltagi et al., 2012; a test for cross-sectional dependence in panel data models) show the presence of heteroskedasticity and cross-sectional dependence. The latter is a common problem for long time series that the residuals are correlated across entities.

These test results suggest that the estimates are possibly biased, and the standard errors are underestimated. To correct for the understated standard errors, we follow a method proposed in Bertrand et al. (2004): we run the main regression collapsing the time series details into the before-, during- and after-suspension groups. This gives a panel with three periods. Ignoring the time series information in the regression results in higher standard errors. In Table 4, the first column shows the results for incoming trips and the second column shows the results for outgoing trips. Despite the increase in some of the standard errors, many of the coefficients are still significant. An exception is the effect of subway proximity after the resumption of subway service, which becomes insignificant for both incoming trips and outgoing trips when the time series details are discarded. The standard errors for the coefficients of interest ($subwayD : suspend, subwayD : post$) are 0.265 and 0.110 for incoming trips and 0.257 and 0.107 for outgoing trips, which are slightly higher than the standard errors in the main regression in Table 3.

To correct the inconsistent standard errors caused by cross-sectional dependence or spatial dependence, we compute the standard errors using the Driscoll and Kraay robust covariance matrix of parameters that is consistent with cross sectional and serial correlation and heteroskedasticity (Driscoll and Kraay, 1998). We observe the increases in some of the standard errors in Table 5 that coefficients of $suspend, post, mon, sat, jan$, 2015 become insignificant. But the coefficients of interest ($subwayD : suspend, subwayD : post$) remain significant.

5. Discussion and conclusion

This paper uses the 39-day subway suspension period in the city of Glasgow to study its short-term and long-term effects on bike-sharing trips. We find an increase in bike-sharing trips during the suspension 20.7% (1.69 trips) increase for average daily incoming trips per station, 20.1% (1.65 trips) increase for outgoing trips), confirming that city centre subway and bike-sharing were indeed substitutes, as found in existing literature. 12.4% of the increase persisted after the subway resumption. Fuller et al. (2012) found that the two London underground strikes brought a 26% increase and a 77% increase in bike-sharing trips, while Fuller et al. (2019) reported a 57% increase in bike-sharing trips from a Philadelphia public transport strike. One reason for these larger increases than what we have found for the Glasgow subway closure is the ridership of the strike/closure modes (London Underground or Philadelphia public transport vs. the much smaller Glasgow subway system).

For the sake of comparison, we look into diversion factors. Since the city of Glasgow has the same set of possible alternatives (bus, car, rail cycle, walk, taxi, no travel) as in the table of recommended values for urban conurbations, we can use the value in the table directly. The value recommended in the UK for use is that out of a 1,000 passenger decrease in city subway/light rail trips, 50 users switched to cycling (Dunkerley et al., 2018). From our regression results we know that there was an increase of<2 trips per station per day. From the tap-in/tap-out numbers supplied by the SPT, the average ridership of subway from 2nd July to 9th August in 2015 and 2017 was 1,153,846 trips so this was the number of trips lost during subway closure. During the 39-day closure, an increase of 1,954 trips in total [=(1.65 + 1.69)x15 stations x39 days] was

estimated. This is equivalent to a diversion ratio of 0.002, meaning that out of 1000 decrease in subway trips, 2 users switched to cycling using shared bikes. This value we estimated from the Glasgow subway closure for bike-sharing is much lower than the recommended value for cycling generally. Some users likely had access to their own bicycles so the bike-sharing data only captured a small proportion of the switch. However, this may still not account for the large difference between the recommended value and our estimate.

Future work would be to further investigate the response of other modes such as other public transport modes and the subway to this intervention. It might also be of interest to consider how the disruption is announced, how far in advance it is announced and how/what alternatives are promoted in the study. Another interesting extension will be partitioning the bike stations and analysing the changes in bike usage patterns during subway and after subway closure. For this purpose, we have to look into the detailed cycling routes and the subway travel time improvement at station level. Besides, we can combine this study with analysing other cycling data such as from exercise apps and cycling counts. This can potentially explain the low diversion factor from subway to cycling implied by our results.

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7. Availability of data and material

Not applicable.

8. Code availability

Not applicable.

CRediT authorship contribution statement

Chau Man Fung: Conceptualization, Formal analysis, Writing - original draft. **David Philip McArthur:** Conceptualization, Writing - review & editing. **Jinhyun Hong:** Conceptualization, Writing - review & editing.

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.

Appendix A:. Different sizes of catchment areas

In this appendix, the main regression results for incoming and outgoing trips are run with different sizes of catchment areas. While a 500 m-radius

Table 6
Regression results (Incoming/Outgoing trips, 200 m radius).

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
weekend	1.471*** (0.177)	1.475*** (0.172)
publichol	−0.256** (0.130)	−0.186 (0.126)
max_air_temp	−2.251*** (0.098)	−2.267*** (0.095)
rain	−2.325*** (0.181)	−2.356*** (0.176)
wind	0.314*** (0.011)	0.315*** (0.010)
mon	−0.130*** (0.004)	−0.130*** (0.004)
tue	−0.151*** (0.009)	−0.153*** (0.009)
wed	0.167* (0.098)	0.145 (0.096)
thu	0.684*** (0.098)	0.688*** (0.095)
sat	0.761*** (0.098)	0.780*** (0.095)
jan	0.500*** (0.098)	0.503*** (0.095)
feb	0.170* (0.097)	0.176* (0.095)
mar	0.790*** (0.170)	0.851*** (0.165)
apr	1.673*** (0.174)	1.726*** (0.169)
may	2.464*** (0.169)	2.534*** (0.164)
jun	3.014*** (0.169)	3.089*** (0.164)
jul	3.539*** (0.158)	3.599*** (0.153)
aug	2.542*** (0.160)	2.590*** (0.155)
sep	1.213*** (0.167)	1.269*** (0.162)
oct	1.576*** (0.162)	1.627*** (0.158)
nov	1.629*** (0.152)	1.675*** (0.147)
2015	2.989*** (0.148)	3.055*** (0.143)
2016	1.929*** (0.140)	1.936*** (0.136)
suspend	−0.089 (0.184)	0.043 (0.178)
post	1.266*** (0.111)	1.286*** (0.108)
subwayD:suspend	1.838*** (0.340)	1.760*** (0.330)
subwayD:post	−0.168 (0.142)	−0.406*** (0.138)
Fixed effects?	Yes	Yes
Observations	35,137	35,137
R^2	0.254	0.266
Adjusted R^2	0.252	0.265
F Statistic (df = 27; 35069)	441.120*** (df = 27; 35069)	471.675*** (df = 27; 35069)

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors(non-robust) in parentheses.

is picked for the main analysis, the results of the same regressions (Table 3) for a 200 m-radius (Table 6) and 800 m-radius (Table 7) are shown respectively. The number of bike stations in the treatment group is 7 (200 m radius), 18 (500 m radius), and 30 (800 m radius). We could see that the *subwayD* : *suspend* coefficients are slightly larger than those in our default catchment (500 m radius). The coefficients of *subway* : *post* are of opposite signs for 200 m radius but as in the main results (500 m radius), these coefficients are not always statistically significant. Therefore, we keep the 500 m radius for the catchment areas as there is no strong evidence suggesting that the size of the catchment areas will is driving the results.

Table 7
Regression results (Incoming/Outgoing trips, 800 m radius).

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
weekend	0.066 (0.269)	0.223 (0.261)
publichol	−0.481*** (0.155)	−0.382** (0.151)
max_air_temp	−2.251*** (0.098)	−2.267*** (0.095)
rain	−2.325*** (0.181)	−2.356*** (0.176)
wind	0.314*** (0.011)	0.315*** (0.010)
mon	−0.130*** (0.004)	−0.130*** (0.004)
tue	−0.151*** (0.009)	−0.153*** (0.009)
wed	0.167* (0.098)	0.145 (0.096)
thu	0.684*** (0.098)	0.688*** (0.095)
sat	0.761*** (0.098)	0.780*** (0.095)
jan	0.500*** (0.098)	0.503*** (0.095)
feb	0.170* (0.097)	0.176* (0.095)
mar	0.790*** (0.169)	0.851*** (0.165)
apr	1.673*** (0.174)	1.726*** (0.169)
may	2.464*** (0.169)	2.534*** (0.164)
jun	3.014*** (0.169)	3.089*** (0.164)
jul	3.539*** (0.158)	3.599*** (0.153)
aug	2.542*** (0.160)	2.590*** (0.155)
sep	1.213*** (0.167)	1.269*** (0.162)
oct	1.576*** (0.162)	1.627*** (0.158)
nov	1.629*** (0.152)	1.675*** (0.147)
2015	2.989*** (0.148)	3.055*** (0.143)
2016	1.929*** (0.140)	1.936*** (0.136)
suspend	−0.089 (0.184)	0.043 (0.178)
post	1.266*** (0.111)	1.286*** (0.108)
subwayD:suspend	2.349*** (0.288)	2.122*** (0.280)
subwayD:post	0.268** (0.120)	0.174 (0.117)
Fixed effects?	Yes	Yes
Observations	35,137	35,137
R^2	0.254	0.266
Adjusted R^2	0.253	0.265
F Statistic (df = 27; 35069)	442.697*** (df = 27; 35069)	471.675*** (df = 27; 35069)

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors(non-robust) in parentheses.

Appendix B:. Dropping trips by a different rule

In this appendix, the main regression results for incoming and outgoing trips are run with applying a different rule of dropping trips that are believed to be unrelated to subway trips. First, the distance matrix of the available bike stations is computed. Second, a minimum speed of 5 km/h is assumed and we derive a maximum trip duration for each origin–destination pair. Third, we exclude trips with a duration that exceeds this maximum duration of the corresponding O-D pair. Table 8 shows the main regression results of incoming and outgoing trips, respectively (cf. Table 3). We could see that the coefficients are similar to the main results dropping all trips exceeding 150 min. The effects we focus on (*subway* : *suspend* and *subway* : *post*) are smaller due to the higher number of trips being dropped under this new rule (531,510 trips out of 669,303 remain, 79%) but the direction of the effects is the same.

Table 8
Regression results (Incoming/outgoing trips, minimum speed = 5 km/h).

Incoming/Outgoing trips		
	Dependent variable: trips (1) incoming	(2) outgoing
weekend	0.739*** (0.134)	0.737*** (0.128)
publichol	0.140 (0.091)	0.154* (0.087)
max_air_temp	−2.498*** (0.065)	−2.494*** (0.062)
rain	−2.203*** (0.120)	−2.203*** (0.115)

(*continued on next page*)

Table 8 (*continued*)

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
wind	0.156*** (0.007)	0.156*** (0.007)
mon	−0.078*** (0.003)	−0.078*** (0.003)
tue	−0.074*** (0.006)	−0.074*** (0.006)
wed	0.040 (0.065)	0.042 (0.063)
thu	0.562*** (0.065)	0.560*** (0.062)
sat	0.642*** (0.065)	0.643*** (0.062)
jan	0.350*** (0.065)	0.352*** (0.062)
feb	0.261*** (0.065)	0.263*** (0.062)
mar	0.515*** (0.113)	0.526*** (0.108)
apr	1.088*** (0.115)	1.095*** (0.111)
may	1.537*** (0.112)	1.539*** (0.107)
jun	1.790*** (0.112)	1.796*** (0.107)
jul	2.006*** (0.105)	2.006*** (0.100)
aug	1.460*** (0.106)	1.463*** (0.102)
sep	0.659*** (0.111)	0.659*** (0.106)
oct	0.968*** (0.108)	0.977*** (0.103)
nov	1.167*** (0.101)	1.168*** (0.096)
2015	2.364*** (0.098)	2.371*** (0.094)
2016	1.400*** (0.093)	1.397*** (0.089)
suspend	0.016 (0.122)	0.030 (0.117)
post	0.815*** (0.074)	0.824*** (0.071)
subwayD:suspend	1.238*** (0.171)	1.209*** (0.164)
subwayD:post	0.182** (0.071)	0.168** (0.068)
Fixed effects?	Yes	Yes
Observations	35,137	35,137
R^2	0.261	0.278
Adjusted R^2	0.260	0.276
F Statistic (df = 27; 35069)	459.003*** (df = 27; 35069)	499.415*** (df = 27; 35069)

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors(non-robust) in parentheses.

Appendix C:. Allowing for varying effect after subway resumption

In this appendix, the main regression results for incoming and outgoing trips are run with a variable *postday* instead of the *post* dummy representing the period after the resumption of subway service (see Table 9). This variable is the number of days since the resumption of subway service. We could see that the coefficients *subwayD* : *postday* for the regressions of both incoming and outgoing trips are statistically insignificant, suggesting that nothing conclusive can be said about how subway riders might have switched back from shared bikes to subway slowly after the resumption of subway service.

Table 9
Regression results (Incoming/outgoing trips, days since resumption of subway service (postday)).

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
weekend	−2.248*** (0.096)	−2.264*** (0.093)
publichol	−2.259*** (0.178)	−2.291*** (0.173)
max_air_temp	0.265*** (0.011)	0.267*** (0.010)
rain	−0.132*** (0.004)	−0.133*** (0.004)
wind	−0.129*** (0.009)	−0.130*** (0.009)
mon	0.181* (0.097)	0.159* (0.094)
tue	0.698*** (0.097)	0.703*** (0.094)
wed	0.758*** (0.097)	0.777*** (0.094)
thu	0.493*** (0.097)	0.495*** (0.094)
sat	0.194** (0.096)	0.200** (0.093)
jan	−3.231*** (0.202)	−3.217*** (0.196)
feb	−1.964*** (0.197)	−1.956*** (0.191)
mar	−0.694*** (0.182)	−0.666*** (0.177)
apr	0.329* (0.173)	0.364** (0.167)
may	1.815*** (0.150)	1.846*** (0.146)
jun	1.407*** (0.148)	1.429*** (0.144)
jul	1.029*** (0.157)	1.071*** (0.152)
aug	1.315*** (0.157)	1.355*** (0.152)
sep	1.294*** (0.149)	1.332*** (0.144)
oct	2.383*** (0.147)	2.444*** (0.143)
nov	1.547*** (0.139)	1.552*** (0.134)
2015	−8.733*** (0.308)	−8.702*** (0.298)
2016	−5.529*** (0.239)	−5.578*** (0.232)

(*continued on next page*)

Table 9 (*continued*)

Incoming/Outgoing trips		
	Dependent variable: trips	
	(1) incoming	(2) outgoing
suspend	−0.738*** (0.199)	−0.763*** (0.193)
postday	−0.027*** (0.001)	−0.027*** (0.001)
subwayD:suspend	1.590*** (0.252)	1.562*** (0.245)
subwayD:postday	0.00001 (0.0004)	−0.0001 (0.0004)
Fixed effects?	Yes	Yes
Observations	35,137	35,137
R^2	0.273	0.286
Adjusted R^2	0.271	0.285
F Statistic (df = 27; 35069)	486.965***	521.474***

Note: (1) *p < 0.1; **p < 0.05; ***p < 0.01.
(2) Standard errors(non-robust) in parentheses.

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