VancUber: The Long-Run Effect of Ride-hailing on Public Transportation, Congestion, and Traffic Fatalities*

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Abstract

We investigate the long-run effect of ride-hailing on public transit ridership, traffic congestion, and traffic fatalities. We estimate the long-run effect of ride-hailing by exploiting British Columbia's use of a pre-existing regulation in 2013 to ban ride-hailing from Vancouver and using the synthetic control method to construct a counterfactual Vancouver. We do not find a statistically significant effect of ride-hailing on our outcomes. Our results for fatalities are imprecise, but our confidence intervals for the effect on transit ridership and congestion provide bounds on the likely effect that are smaller in magnitude than many existing estimates.

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1 Introduction

New transportation technologies have repeatedly reshaped cities. The invention of steam railways allowed cities to start expanding beyond walking distance (Heblich et al., 2020), the invention of the automobile changed where rich and poor lived within cities (LeRoy and Sonstelie, 1983), and the construction of limited-access highways caused cities to spread out further (Baum-Snow, 2007). Ride-hailing, as exemplified by firms such as Uber, Lyft, and Didi, is the latest new transportation technology to affect cities. The rapid onset of ride-hailing has inspired significant policy debates, and lead to a wave of academic research seeking to measure its impact on cities. Because ride-hailing is so new, most studies have only measured the short-run impact. However, designing effective policy requires understanding the long-run impacts.

We investigate the long-run effect of ride-hailing on three transportation-related outcomes of particular interest to policymakers: (1) public transit ridership, (2) traffic congestion, and (3) traffic fatalities. Each of these outcomes has attracted significant interest; however, there remains great uncertainty about even the short-run impact, as existing estimates vary in sign and magnitude.

A natural challenge in estimating the long-term effect of ride-hailing on cities is that it is difficult to know what the counterfactual outcome would have been in its absence. Ride-hailing firms spread quickly, with their most popular service (i.e., UberX or Lyft) entering 46 of the 50 largest metropolitan areas in the US and Canada within 22 months, and so it is difficult to find a credible comparison group, especially for estimating long-run effects. For example, in a standard difference-in-differences design, effects estimated in 2015 require comparing cities such as New York City, Los Angeles, and Chicago to cities such as Buffalo, New York; Iowa City, Iowa; and Springfield, Missouri. It is more difficult to accept the parallel trends assumption for cities that are so different. Estimating effects in later years requires even more challenging comparisons.

In this paper, we overcome this challenge by taking advantage of British Columbia's ban on ride-hailing. In 2012, British Columbia used pre-existing regulations to block Uber from entering Vancouver and then successfully excluded all ride-hailing services until January 2020. Vancouver's unique regulation of ride-hailing provides two important benefits for studying the long-run effects of ride-hailing services. First, the ban lasted a long time in a city comparable to early-entry cities. Second, the long-standing character of British Columbia's taxi regulations gives a strong case for the availability of ride-hailing being exogenous to our measured outcomes.

To estimate what would have happened in Vancouver had Uber or Lyft entered, we build a synthetic Vancouver following the synthetic control methodology of Abadie and Gardeazabal (2003) and Abadie et al. (2010). For each of our three outcomes—public transit ridership, traffic congestion, and traffic fatalities—we create a synthetic Vancouver by finding the weighted average of cities in the US and Canada that best approximates Vancouver's time series before the entry of ride-hailing. We estimate ride-hailing's effect for each post-treatment year by comparing the time path of this synthetic Vancouver with the city's actual outcomes.

We find a statistically-insignificant, positive effect of ride-hailing on public transit ridership, traffic congestion, and traffic fatalities. While our estimates are statistically insignificant, Abadie (2020) argues against "the usual practice of conferring point null rejections a higher level of scientific significance than non-rejections" since nonsignificant results are informative, and in some cases, are more informative than statistically significant results. Our confidence intervals, constructed following Firpo and Possebom (2018), are $[-1.3\%, 4.1\%]$, $[-6.1\%, 20.7\%]$, and $[-30.6\%, 198.6\%]$ for transit ridership, congestion, and fatalities, respectively. While the interval for fatalities is not informative, the confidence intervals for transit ridership and congestion imply the larger estimates in the literature do not apply to Vancouver, with 33%–50% of existing point estimates lying outside our confidence intervals.

These results are informative about the impact of ride-hailing in cities other than Vancouver. Our results apply most directly to other mid-sized cities in the US and Canada, and imply the effects of ride-hailing on these cities are smaller than current evidence suggests.[1] Additionally, given that our findings are so different than those from outside of the US and Canada, this implies that local context is important for the impact of ride-hailing on cities.

This paper builds on a quickly growing literature seeking to quantify the effects of ride-hailing on public transportation, traffic congestion, and traffic fatalities. This literature uses diverse methods and spans several fields, including economics, geography, transportation engineering, and information systems.[2] Table 1 summarizes some of the key papers estimating the effect of ride-hailing on our outcomes of interest, showing that estimates vary in sign and magnitude.[3] Estimates of ride-hailing's effect on public transit ridership range from -16% to +5%, estimates of the effect on traffic congestion range from -14% to +40%, and estimates of the effect on traffic fatalities range from -40% to +16%. Some heterogeneity is to be expected; there are a variety of mechanisms by which ride-hailing can affect each outcome, and in different contexts, different mechanisms matter more. For example, in cities with small transit agencies, ride-hail might solve last-mile problems and therefore increase ridership, but in cities with large transit agencies ride-hail might not increase the reach of transit, and so just be a competitor.[4] However, the magnitude of the heterogeneity suggests profound disagreement on the effects of ride-hailing.

More importantly, the literature has struggled with estimating long-run effects. Those that do so rely on two approaches. First, some papers

[1]More formally, if the scientific community has a prior, based on the existing literature, on how ride-hailing is affecting these outcomes, our paper shifts the center of the posterior distribution towards zero.

[2]See Chapter 2 of Thomas et al. (2021) for a detailed literature review.

[3]Other papers looking at the effect of ride-hailing on public transit and safety, but that do not measure the overall effect, include Rayle et al. (2016), Greenwood and Wattal (2017), Teltser et al. (2021), Breuer et al. (2020), and Young et al. (2020).

[4]See Hall et al. (2018) for a longer discussion of the mechanisms by which ride-hailing could affect public transit ridership, and see Krishnamurthy and Ngo (2021) and Barrios et al. (2020) for discussions of how ride-hailing could affect congestion and traffic safety, respectively.

assume that cities with and without ride-hailing follow parallel trends in their outcomes. This assumption is reasonable for the bulk of their sample period, but in the long run this is more difficult to accept. Second, a few papers use models, such as city travel demand models, to predict what would have happened in the absence of ride-hailing (e.g., Erhardt et al., 2019). This approach assumes there were no shocks or structural breaks not included in the model.

This paper's contribution is to estimate the long-run effect of ride-hailing on these outcomes using a compelling identification strategy with a credible comparison group. We find that the long-run impact of ride-hailing on public transit and traffic congestion in Vancouver, and likely also for other US and Canadian cities, are smaller than current evidence would predict.

2 History of ride-hailing in Vancouver

To identify the long-term effects of ride-hailing on cities, we rely on Vancouver's strict regulation of ride-hailing services. Despite strong latent demand for ride-hailing, the taxi industry successfully blocked these services from entering the Vancouver market. Thus, Vancouver provides a good case study of what a city looks like without ride-hailing services in the long-run.

Uber's early attempt to enter Vancouver in 2012 was nearly successful, but it was ultimately thwarted by British Columbia's Passenger Transportation Board (PTB). Throughout its early years, Uber entered markets following a well-established pattern: offer the service without permission and deal with regulatory concerns later (see Hall et al. (2018) for additional detail on patterns in Uber's entry decisions). Its entrance into Vancouver was supposed to be the same. During the Summer of 2012, Uber provided limited service in Vancouver under its "Secret Uber" program.[5] After exploring the market, Uber planned to launch full service in November 2012. Ahead of the launch, the PTB informed Uber that it would be classified as a

[5]https://web.archive.org/web/20131023023017/http://blog.uber.com/2012/11/22/helpubervan/

limousine company and would therefore have to charge a $75 minimum fee, regardless of trip distance or duration.[6] Uber CEO Travis Kalanick claimed that the company knew of the rule but also that their research found few comparable services were following it; that many limo companies offered rides for less than $75 and that airport limousines had an exemption. Nevertheless, Uber and other ride-hailing services were prevented from entering the market.

Importantly, for this paper, the city's resistance did not come from concerns about public transit, traffic safety, or congestion. The PTB stopped Uber because of concerns about the taxi industry. While taxis opposed Uber in every city it entered, the opposition succeeded to a much greater degree in Vancouver. Its inordinate success preceded Uber. At the beginning of 2012, Vancouver had an abnormally low supply of taxis: 9.4 per 10,000 compared to Montreal's 27 and Toronto's 18.[7] Although the city restricted licenses, it distributed them for a small fee of 522 Canadian dollars (CAD) per license. License owners could then lease the licenses on a secondary market where access to a full license would sell for 800,000 CAD. Although Vancouver officials acknowledged the severe shortage of taxis, they also recognized the industry's political power and stake in the status quo. Uber recognized their main barrier was the taxi industry. When Vancouver first stopped Uber in November 2012, the company responded with a call to Vancouver residents to contact their representatives and the PTB with the message, "ABOLISH TAXI-PROTECTIONISM - LET UBER CHARGE LESS THAN $75 FOR A RIDE!"[8] Thus preventing Uber's entry protected the pre-existing taxi rents and was unrelated to concerns over the outcomes we are measuring.

Another important feature of Vancouver is that even after the initial

[6]https://www.straight.com/news/uber-town-car-service-shut-down-vancouver-bc-passenger-transportation-board

[7]https://web.archive.org/web/20120707063224/http://thedependent.ca/featured/taxiland/

[8]https://web.archive.org/web/20131023023017/http://blog.uber.com/2012/11/22/helpubervan/

failure, Uber wanted to enter the market and there was clearly latent demand for the services. In the Summer of 2014, Uber tried to build more goodwill and brand awareness by delivering ice cream.[9] Then, in October of that year, despite no operations in British Columbia, Uber held a hiring fair in a Vancouver hotel to recruit drivers.[10] While Uber focused on obeying the rules and softening public opinion, other services entered through underground channels. For instance, some Chinese-language companies began offering ride-hailing services illegally.[11] Over the years, political pressure mounted to allow ride-hailing platforms to enter the market, and by 2017 the three major political parties announced intent to open British Columbia.[12] In January 2020, Uber entered Vancouver, its last major metropolitan market in North America.[13] The PTB approved Lyft the same week and approved other ride-hailing services in the following months.

Vancouver's regulation provides a unique setting to identify the long-run effect of ride-hailing on cities. Since Uber wanted to enter Vancouver at the same time as other large cities but was rebuffed, it serves as a better comparison city than markets where Uber delayed entry because they were too small.[14] We set Uber's intended entry into Vancouver as 2013. We do this for two reasons. First, the treatment of interest is when cities get UberX, the service that most people associate with Uber, not UberBlack, Uber's first service, which was a high-end limousine service. Most cities did not get UberX until 2013, and we assume that with its early intent to enter the Vancouver market, Uber would have also launched this service in 2013. Second, while Uber's official launch was scheduled for late November 2012, we are using annual data, so it makes more sense to attribute the first

[9] https://www.cbc.ca/news/canada/british-columbia/taxi-app-tries-for-a-comeback-in-vancouver-1.2711847

[10] https://www.cbc.ca/news/canada/british-columbia/uber-vancouver-hiring-fair-goes-on-despite-moratorium-1.2798663

[11] https://nationalpost.com/news/canada/underground-ride-sharing-services-in-b-c-appear-to-be-thriving-amid-crackdown

[12] https://www.cbc.ca/news/canada/british-columbia/ndp-stalls-on-election-promise-to-bring-ride-hailing-to-b-c-by-end-of-year-1.4357347

[13] https://www.uber.com/en-CA/newsroom/vancouver-uber-is-here/

[14] Per Hall et al. (2018), Uber entered markets with larger populations first.

year of treatment to 2013. Setting treatment as 2013 means the donor group includes cities like Atlanta, Seattle, and Washington DC. We also conduct a robustness test where we set treatment to 2014 because that is when UberX came to other major Canadian cities like Toronto and Montreal.

3 Data

We gather annual data on public transit ridership, traffic fatalities, and traffic congestion; Uber and Lyft entry dates; and economic indicators for metropolitan areas and cities in the US and Canada.[15] One problem is that the data are reported at different levels of geographical detail: public transit ridership is reported for transit agencies, traffic congestion is reported for metropolitan areas, and traffic fatalities are reported for cities. For expositional ease, in the analysis we will refer to the unit of observation for all outcomes as a city.[16]

3.1 Public transit ridership

We obtain annual public transit ridership data for 2006 through 2017 from publicly-available reports by the American Public Transit Association (APTA) and TransLink, the primary transit agency for Greater Vancouver. The APTA data feature several Canadian cities, including Toronto, Montreal,

[15] In the US, we use Core Based Statistical Areas (CBSA) and in Canada, we use Census Metropolitan Areas (CMA).

[16] We use annual data for three reasons. Most importantly, constructing a synthetic treated unit necessitates choosing the counterfactual time period when Uber would have entered Vancouver in the absence of the pre-existing regulation. This time period then determines the set of potential donor cities used to form the synthetic control. Using annual data, instead of quarterly or monthly data, allows us to have a large donor pool. Moreover, we prefer to use a consistent frequency across specifications, and our congestion data are only available at annual frequency. Finally, using annual data mitigates seasonality concerns. As we are interested in the long-run impacts of ride-hailing, the use of annual data is appropriate in our context. We acknowledge that using annual data means we are measuring the average annual impact, and that we cannot detect potential heterogeneous effects at smaller time scales. For example, a decrease in off-peak congestion that is offset by an increase in peak congestion, as found in Krishnamurthy and Ngo (2021).

and Vancouver, and are agency-mode specific. For each metropolitan area, we use data for the primary transit agency which serves the central city and any commuter rail agencies.

One feature of the APTA data is that each year's report contains data from both the current and previous year. The likely reason for this is that data for some agencies may be unavailable at publication time. Thus the following year's report updates ridership data for the same quarter in the previous year. This revision may also include corrections to misreports in the previous year, for example, because of typographical errors. As a default, we use the later-year report to extract data for the previous year; thus, the 2007 through 2018 reports give us ridership data for 2006 through 2017. Since the universe of reporting agencies is not constant over time, an agency may report in one year and not in the next. We fill any gaps in the data using current-year reports.

The APTA data present a problem with respect to TransLink. TransLink is absent from APTA's Q4 2015 report, and data for TransLink are partial for 2016 and 2017. Even filling the 2014 data using the current-year report leaves a two-year gap for some modes, and total ridership, in the post-period for Vancouver. However, mode-specific ridership data are available from TransLink's annual reports from 2006 through 2017. Accordingly, we use TransLink's data for Vancouver and continue to use APTA data for all other metropolitan areas. Figure A1 compares ridership for TransLink using the two data sources. Correlations between each series are over 90% for all modes except "other," at 76%.

3.2 Traffic congestion

We obtain congestion data for 102 metropolitan areas for 2010 through 2019. This data comes from two sources, TomTom and Inrix. Both services use data from various sources, including smartphones, navigation devices, and roadway sensors to measure travel times. TomTom data from 2012 through 2019 are publicly available. The TomTom congestion data present a challenge, as, given our preference for 2013 as the treatment year, they give only one pre-period year in which to generate synthetic control weights. For this reason, we supplement the TomTom data using Inrix, which has publicly-available data from 2010 through 2013.

The TomTom and Inrix congestion indices measure how much longer an average trip takes relative to uncongested conditions. That is, an index level of 25 means the average trip takes 25% longer than it would when there is no congestion. We use indices that take the average delay for all daily trips, as measures focusing on specific times of day (such as the morning peak) are not available over our entire sample period.

The Tomton and Inrix measures are highly correlated across metros within overlapping years (the correlation is greater than 80% in 2012 and 2013). However, the correlation between changes in the TomTom and Inrix indices across CBSAs within overlapping years is low. For example, the across-city correlation of the change in the respective congestion measures from 2012 to 2013 is only about 0.1. However, since we treat all cities similarly when combining data sources, the synthetic control methodology should generate weights that produce an unbiased estimate of treatment effects. We combine the TomTom and Inrix data by taking the average of the two measures in the two overlapping years.

3.3 Traffic fatalities

We obtain traffic fatality data for the primary city of each metropolitan area for 2004 through 2017 for 120 cities.[17] Our US fatalities data come from the Federal Accident Reporting System (FARS). We aggregate this data to the city level. Canadian fatalities data were collected from publicly-accessible websites maintained by provincial and municipal governments.

[17] We include both Minneapolis and St. Paul as well as Dallas and Ft. Worth.

3.4 Uber and Lyft entry

We extend the ride-hailing entry data from Hall et al. (2018) by extending our Uber entry data through 2020 and adding Lyft entry dates. We use official press releases where possible, but also use newspaper articles, blog posts, and social media posts. We corroborated our Lyft entry dates with data collected independently by Keith Teltser. For Uber, we consider only UberX (not, e.g., UberBlack). Where several municipalities within a single metropolitan area received ride-hailing in different years, we take the principal city as providing the metropolitan entry date. We also exclude any metros where ride-hailing entered, only to subsequently leave either temporarily or permanently.

3.5 Population, gas prices and unemployment

We use data on metropolitan populations to normalize public transit ridership and fatalities. We collect data on population for US and Canadian metropolitan areas from the US Census Bureau and Statistics Canada. Additionally, in a robustness check, we add gas prices and metropolitan unemployment rates to the list of covariates we use to construct a synthetic "Vancouver." For the US, these data come from the US Energy Administration and the Bureau of Labor Statistics, respectively. Canadian data for both measures are from Statistics Canada.

4 Motivating analysis

We start with looking at the trends for our three outcomes in Vancouver and three natural comparison cities: Seattle, Toronto, and Portland. The three cities are natural comparisons because of their similarities to Vancouver with size and public transit systems, but they are also convenient because Uber entered the three cities in different years. The trends are plotted in Figure 1, with the year of Uber entry marked with a vertical line (Seattle, 2013; Toronto, 2014; Portland, 2015).

With the three comparison cities, one could try to estimate the effect of ride-hailing on each outcome using a simplified difference-in-differences exercise, but the results are sensitive to the reference city. Public transit ridership provides a good example. Comparing the changes in the growth rate of per capita transit ridership in Vancouver to those in Seattle, Toronto, and Portland before and after the entry of ride-hailing yields simple difference-in-differences estimates of the effect of ride-hailing on ridership, reported in Table A1, of 4.1%, 1.8%, and -3.8%, respectively. The results for congestion and fatalities are similarly sensitive to the city chosen.

The fact that the estimates for public transit ridership and congestion vary around zero suggests that the true effect of ride-hailing on these outcomes could be small, while the estimates for the effect on traffic fatalities are all positive, suggesting ride-hailing may be increasing traffic fatalities. To more carefully test these possibilities, we use synthetic control to carefully construct comparison groups for Vancouver.

5 Methodology

5.1 Synthetic treatment

To estimate the long-term effect of ride-hailing on cities, we use a modified synthetic control analysis. The synthetic control method of Abadie et al. (2010) formalizes the selection of comparison groups in comparative case studies. Specifically, in studies with panel data and a small number of treated units, the methodology constructs a synthetic comparison group as a weighted average of untreated units. The treatment effect in period t is then the difference in the outcome variable between the treated unit and its synthetic counterpart. See Appendix A for a formal description of the synthetic control method and additional details on inference, and see Abadie (2021) for a longer review.

Synthetic control has a key advantage over difference-in-differences analysis, whether estimating the average post-treatment effect or dynamic effects via an event study. Crucially, synthetic control relaxes the parallel trends assumption (Abadie, 2021), which may be problematic in our context. Moreover, Goodman-Bacon (2021) shows that two-way-fixed-effect estimation may be problematic in contexts where treatment effects are dynamic, as seems reasonable here. Our aim is to estimate the dynamic effects of ride-hailing entry over the long term for Vancouver, an objective for which synthetic control is well-suited.

We adapt synthetic control to create a synthetic *treated* Vancouver. Given a treatment date, our donor group is the set of all cities where Uber or Lyft entered in that year. As explained in Section 2, we choose 2013 as our baseline date. The donor group is then weighted to construct a synthetic treated Vancouver that best matches the pre-treatment level of the variables we are matching on. The number of observables on which to match is potentially large, and there is no consensus on how best to choose them (see the discussion in Abadie (2021)). We match on the pre-treatment average of the outcome variable of interest in the pre-treatment period, following Abadie and Gardeazabal (2003) and Abadie et al. (2010), plus the level of the outcome variable in even years before 2012. In our baseline specifications we exclude 2008 and 2009 — the Great Recession — from both the average pre-treatment outcome and the set of individual years used to match. We include these in a robustness check. In a further robustness check, we add average pre-treatment unemployment and gas prices to the list of variables we match on, as changes in gas prices and unemployment may predict changes in our outcomes. This changes the set of donors and their weights for both ridership and fatalities, although not for congestion.

It is important to note how synthetic control works across different outcomes. Given a set of donor cities, the objective of synthetic control is to select weights that minimize the difference between the synthetic outcome and the treated outcome. Since the data generating process is different for each outcome, the donor weights will likewise be different. For example, putting a non-zero weight on Seattle may minimize the mean squared prediction error for public transit, but when analyzing congestion it may be optimal to give Seattle a weight of zero. Thus, as is seen in the Appendix, while the donor groups for each outcome are roughly the same size, the number of cities that receive non-zero weights and their identities differ for each analysis.

We transform our outcome variables in four steps. First, we normalize public transit ridership and traffic fatalities by population. Second, we take logs of all outcomes. Third, we subtract the log of the outcome in 2012, the last pre-treatment year in our main specification. The second and third steps transform our analysis to being of growth rates in the outcomes (rather than levels).[18] Thus, our synthetic treatment procedure finds the weighted average of cities whose growth relative to 2012 best matches Vancouver, and uses this weighted average to predict how Vancouver would have grown relative to 2012 had ride-hailing entered in 2013.

5.2 Inference

Abadie et al. (2010) and Abadie et al. (2015) propose permutation-based inference, in which statistical significance is inferred by comparing the estimated treatment effect for the unit that was treated, with similar placebo effects for untreated units constructed by running synthetic control on those units. We follow Abadie et al. (2010) and Abadie et al. (2015) in performing a test of overall significance by comparing the post-treatment root mean squared prediction error (RMSPE) for Vancouver with the RMSPE for placebo cities. If the null hypothesis of no treatment effect holds, we expect the post-treatment RMSPE for the placebo cities to be larger than Vancouver's. p-values are "standardized" by scaling post-treatment RMSPEs by pre-treatment RMSPEs. This ensures that we do not identify a statistically significant overall effect in Vancouver just because its pre-treatment fit tends

[18]Our rationale for subtracting the log 2012 level of the outcome variable is twofold. First, Vancouver has the highest per-capita transit ridership of any of our potential donors, and a synthetic Vancouver using this measure could not be attained as a convex combination of those cities. Second, our donor cities are very heterogeneous. Because we are interested in changes in our outcomes, using levels of cities' outcome variables to weight the donor group would be inappropriate.

to be poor relative to placebo cities.

Given the wide variety of estimates in the literature, we consider it important to be able to rule out certain effects with a relatively high degree of confidence. Firpo and Possebom (2018) propose inverting permutation tests to obtain confidence intervals.[19] We also implement this procedure. Note that we invert overall p-values that account for the quality of pre-treatment fit, ensuring a consistent interpretation of p-values and confidence intervals.

6 Results

6.1 Public transit ridership

Figure 2, panel (a) shows the estimated treatment effects for Vancouver and all placebos. Table 2 reports the point estimates, standardized p-values, and the confidence interval for the average effect. Our point estimates for the effect of ride-hailing on public transit ridership are positive for the first three years and negative for the last two.[20] None of these point estimates are statistically significant. Our p-value for the overall RMSPE likewise indicates that we cannot reject that the observed effects occurred simply by chance.

The pattern in the point estimates further suggests ride-hailing is not

[19] Note that in Firpo and Possebom (2018)'s procedure, confidence is expressed as a fraction, where the numerator must be an integer and the denominator is given by the sum of treated and placebo units. We choose confidence levels as close as possible to 95%.

[20] Part of the large effect in 2017 is likely due to Vancouver extending its Sky Train by 11-kilometers in December of 2016. It is difficult to separate out the effect of the extension on total ridership, as the extension likely increased ridership on other lines and modes by increasing the reach of the transit system, but also replaced a popular bus route, which accounts for a third of the weekday ridership on the extension in 2017 (https://globalnews.ca/news/3280798/evergreen-line-ridership-reaches-30000-trips-a-day-in-january/). However, if these effects cancel out, then we can approximate the change in transit ridership in the absence of the extension by what happened on all other lines and modes. Doing so implies that total transit ridership in the absence of the extension would have increased by 3.7% (rather than 5.7%). This suggests that had Vancouver not extended the Sky Train we would have found a treatment effect in 2017 of -0.063.

having a large effect on transit ridership. Were ride-hailing impacting transit, then, since usage of ride-hailing has been growing with time, we would expect to see the magnitude of the effect grow. [21] However, we find an effect that is sometimes positive and sometimes negative.

Referring to Table 1, our point estimates are smaller in magnitude than others in the literature. Furthermore, Table 2 reports a confidence interval for the effect of ride-hailing on log transit ridership of $[-0.013, 0.040]$, which translates to $[-1.3\%, 4.1\%]$.[22] These results rule out large effects in Vancouver, and suggest that the impact of ride-hailing on public transit on US and Canadian cities is smaller than implied by much of the existing literature.

6.2 Traffic Congestion

Figure 2, panel (b) and Table 2 report estimated treatment effects for Vancouver, using the combined Inrix and Tomtom dataset described above. Our point estimates are all positive, and range from 0.2% to 12.5%. Point estimates in all post-treatment years are insignificant, and our p-value for the overall effect indicates that we cannot reject that the observed effects happened by chance in any post-treatment year. The 95% confidence interval for the average effect is $[-6.1\%, 20.7\%]$. Again, the confidence interval provides bounds smaller than many existing estimates.

As with public transit ridership, the pattern in the point estimates displayed in Figure 2 provides further evidence that the effect is small, as the effect of ride-hailing on congestion fails to grow with time, even though ride-hailing ridership grows with time.

[21] For evidence that ride-hailing ridership has grown with time (pre-pandemic), see Hall et al. (2018) or https://web.archive.org/web/20211013213709/https://backlinko.com/uber-users#uber-trips.

[22] This conversion is made by exponentiating the ends of the confidence interval and subtracting one.

6.3 Traffic Fatalities

Figure 2, panel (c) and Table 2 show the estimated treatment effect for city-level traffic fatalities, normalized by CBSA population. We again observe statistically insignificant effects in all post-treatment years. However, our point estimates grow throughout post-treatment to a high of 92% in 2017. Given that Uber and Lyft accounted for 5.6% of vehicle miles traveled in the core counties of six large US cities in September, 2018, (Balding et al., 2019), this requires that ride-hailing has more than fifteen times the crash risk per mile as other vehicle travel, which seems implausible. Our confidence interval for the average effect is very large — between -30.6% and +198.6%. One possible reason that we observe effects of this magnitude is that, relative to our other outcomes, pre-treatment fit for fatalities is generally poor. On average across cities, the pre-treatment RMSPE is 0.161 for fatalities, relative to 0.028 for ridership and 0.049 for congestion. Our confidence interval is so large as to be uninformative about the effect of ride-hailing on traffic fatalities.

7 Robustness

We perform four robustness checks with the goal of understanding the sensitivity of our results to changes in the set of predictors, potential donors, and donor weights. These are reported in Table 3, where the first column reports our baseline results for comparison. In designing our robustness checks, we follow recommendations by Abadie (2021). Our first check is to add average pre-period CBSA gas prices and unemployment rates to the set of variables we match on to create a weighted average of donor cities. Gas prices and unemployment rates are known predictors of whether to travel and which travel mode to use (Taylor and Fink, 2013). Additionally, British Columbia introduced a carbon tax in 2008 which steady increased until 2012; suggesting gas prices may be a particularly important predictor. As can be seen in Column (2), adding these additional predictors has no effect on the statistical significance of the estimated effects of ride-hailing on public transit ridership, traffic congestion, and traffic fatalities. For transit ridership, adding average gas prices and unemployment increases the point estimate slightly, but the estimate remains statistically insignificant. For congestion, adding gas prices and unemployment to the set of predictors has, surprisingly, no effect on the allocation of weights to donors, and therefore no effect on the results. For traffic fatalities, we find a positive point estimate of similar magnitude to our baseline. The estimated effect remains insignificant.

Our second robustness check revises our baseline to include the Great Recession years of 2008 and 2009 in the set of pre-treatment matching variables, plus the additional predictors described above. As our data for congestion start in 2010, we perform this robustness check for ridership and fatalities only. As can be seen in Column (3), adding the Great Recession years while retaining alternate matching has no effect on the statistical significant of overall estimates for either ridership or traffic fatalities. The ridership estimate is revised slightly downward, and is slightly more precisely estimated than Column (2). The fatalities estimate is revised upward, but remains highly imprecise.

Our third robustness check is a leave-one-out analysis. For each outcome, we re-run the baseline analysis alternately omitting each of the donors that were assigned positive weight in the baseline synthetic control analysis. We rerun the synthetic control analysis for each donor, generating new point estimates and confidence intervals. This checks the robustness of our results to changes in the set of potential (and actual) donors. In Column (4), we report the average point estimate and confidence intervals over all leave-one-out synthetic control analyses. On average, we find results that are comparable to our baseline. For ridership and congestion, the average point estimates are very close to our baseline, as are average confidence intervals. For fatalities, we find that the average leave-one-out point estimate is slightly larger than our baseline and the average confidence interval is somewhat larger.

Our fourth robustness check is in the spirit of Abadie et al. (2015), who backdate German reunification as an "in-time placebo" test. Unique to our analysis is that the time of "treatment" — non-entry of Uber into Vancouver — is not precisely defined. While our choice of 2013 is reasonable given that Uber attempted to enter Vancouver with UberBlack in November 2012 (and, conditional on success, would have likely launched UberX in Vancouver in 2013), it is also true that ride-hailing did not enter other Canadian cities until 2014 (perhaps in response to Uber's failed attempt to enter Vancouver). We therefore perform robustness with respect to our treatment date by changing the year of hypothetical ride-hailing non-entry to 2014. We then take as potential donors the set of cities where ride-hailing actually entered in that year. Choosing 2014 as an alternate treatment date changes the donor pool entirely, and includes other Canadian cities such as Toronto. In keeping with our baseline, we continue to normalize outcomes by their 2012 level, and use exactly the same set of predictors as in the baseline (average outcome up to and including 2012, and alternating years before, excluding Great Recession years). Column (5) reports the results. In terms of overall effects, with 2014 "treatment" we estimate a negative effect on transit ridership. This in contrast to our baseline. The signs on estimated effects for traffic congestion and fatalities, however, continue to have the same sign as previously. More importantly, all estimated effects continue to be insignificant, and we estimate confidence intervals for all three outcomes that are larger than in our baseline. Moreover, in Figure A3 we plot the time path of estimated dynamic effects with 2014 treatment. These are broadly similar to our baseline. Overall, the results of this robustness check reinforce the message that the effect of ride-hailing on transit ridership, traffic congestion, and traffic fatalities is likely to be small.

In addition to our robustness checks, we test whether the effect of ride-hailing on public transit ridership differs by mode. As Figure A4 and Table A2 show, we do not find statistically significant effects on either bus or rail.[23]

[23]There are transit modes that are not bus or rail, notably ferry in Vancouver and

8 Conclusion

A better understanding of the long-run effects of ride-hailing services, such as Uber, is necessary for policymakers to optimally respond to their entry. Unfortunately, analyzing the long-term effects has been difficult because ride-hailing spread across cities quickly, making it difficult to find a credible comparison group. We address this challenge by using British Columbia's ban on ride-hailing services, which prohibited such services from entering Vancouver until February 2020.

Using a synthetic control analysis, we find little evidence that ride-hailing would have had large effects on public transit ridership or traffic congestion in Vancouver, even after six years. Although the results are not statistically different from zero, the point estimates are positive for both transit ridership and congestion. Thus, the results lend some support to studies finding that ride-hailing services complement public transit (Hall et al., 2018) and increase congestion (cf., Barrios et al., 2020). Our estimates for the effect of ride-hailing on traffic fatalities are too imprecise to be informative.

Our estimates for Vancouver are informative for the long-run impact of ride-hailing on transit ridership and congestion for other mid-sized US and Canadian cities, suggesting that the impact of ride-hailing for these cities are likely smaller in magnitude than the literature suggests. As Table 1 shows, existing estimates range widely, with 33%–50% of existing point estimates lying outside our confidence intervals for transit and congestion. Given that our findings are so different than those from outside of the US and Canada, this implies that the local context is important for ride-hailing's impact on cities.

It is also possible that the long-run effects of ride-hailing have yet to manifest themselves. As ride-hailing grows in popularity and as it begins affecting long-term choices such as vehicle ownership and land use (e.g.

paratransit services everywhere. Given the varied nature of services in this category, it is not reasonable to compare them.

Gorback, 2020), its effect on cities may change.

Yet another possibility is that we do not find a longer-term effect because ride-hailing helps or hurts these outcomes through different mechanisms, and these effects are canceling out. For example, ride-hailing can help public transit by making it easier to get to and from train stations but hurt transit by being an alternative. Likewise, ride-hailing may worsen congestion in specific areas or times while helping in others. Policies that address specific mechanisms by which ride-hailing causes socially harmful effects will be helpful, such as subsidizing ride-hail trips that connect with transit (Agrawal and Zhao, 2020) or congestion pricing (e.g., Hall, 2021, Herzog, 2021).

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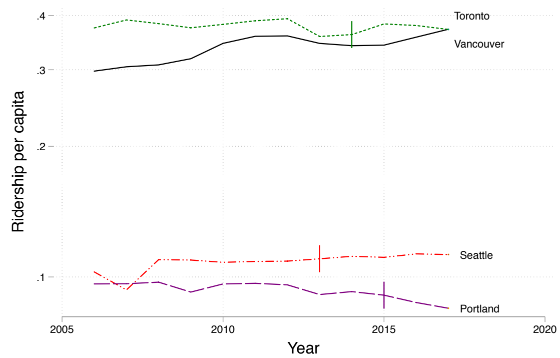
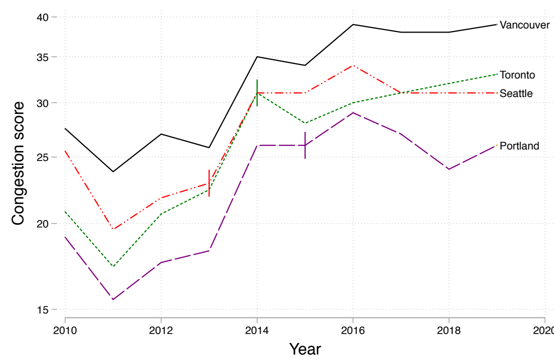
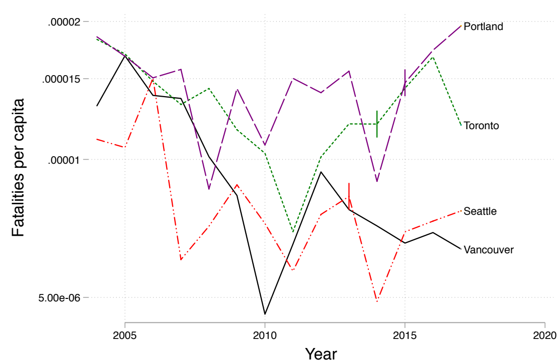
Figure 1: Outcome variables over time for Vancouver and comparable cities



(a) Transit ridership (per capita)



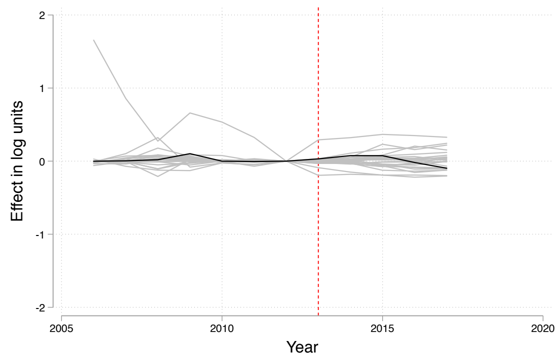
(b) Congestion (combined measure)



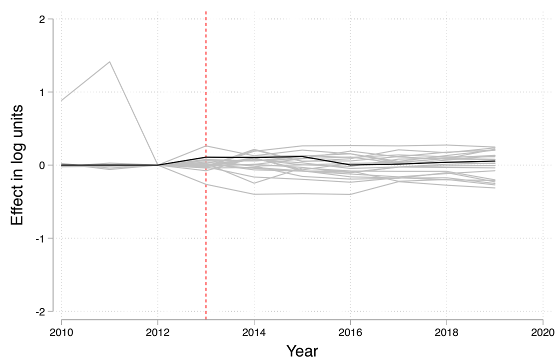
(c) Traffic fatalities (per capita)

Notes: Each panel shows outcome variables for Vancouver, BC, Toronto, ON, Seattle, WA, and Portland, OR. Panel (a) shows ridership, (b) shows traffic congestion, and (c) shows traffic fatalities. Uber entered Portland for a few weeks in 2014, but neither Uber or Lyft established a permanent presence until 2015.

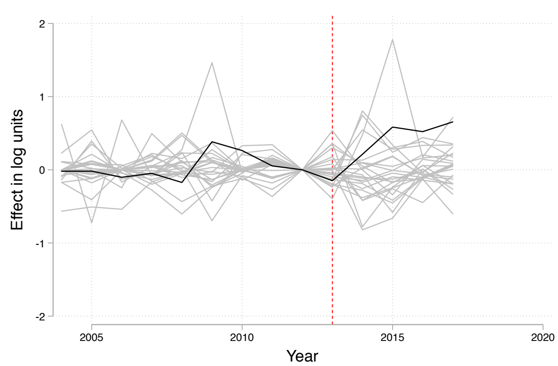
Figure 2: Estimated treatment effects



(a) Ridership



(b) Traffic congestion



(c) Traffic fatalities

Notes: Each panel shows treatment effects for Vancouver (black line) and placebo treatment effects for donors (light gray). Panel (a) shows ridership, (b) shows traffic congestion, and (c) shows traffic fatalities. The donor group is cities where a ride-hailing company began offering services in 2013. For the list of donor cities and their weights, see Appendix Tables A2-A4.

Table 1: Summary of the effects of ride-hailing on cities

Paper	Outcome	Effect	Sample
		Panel A: Transit ridership	
Hall et al. (2018)	Transit ridership	+2.6% on average, +5% after two years	US
Nelson and Sadowsky (2018)	Transit ridership	Increases with one ride-hailing firm, no effect or decreases with two	US
Graehler et al. (2019)	Transit ridership	-8.5% for bus after five years, -6.45% for heavy rail after five years	22 US cities
Babar and Burtch (2020)	Transit ridership	-1.3% for bus, +3.0% for commuter rail, no effect on subway or light rail	US
Erhardt et al. (2021)	Transit ridership	-10% for bus after five years, no effect on light rail	San Francisco
Diao et al. (2021)	Transit ridership	-8.9% on average, -16.28% after four years	US
		Panel B: Traffic congestion	
Mangrum and Molnar (2020)	Speed	-11%	Midtown Manhattan
Qian et al. (2020)	Speed	-22.5% on weekdays	Manhattan
Tarduno (2021)	Speed	-2.3%	Austin, TX
Hall et al. (2018)	Travel time	+0.6% to +1.3% in MSAs with high population or low transit ridership	US
Agarwal et al. (2019)	Travel time	+10.1% to +14.8%	Mumbai, New Delhi, and Bangalore
Diao et al. (2021)	Travel time	0.89% on average	US
Erhardt et al. (2019)	Vehicle hours of delay	+40%	San Francisco
Barrios et al. (2020)	Vehicle hours of delay	+1.6%	US
Krishnamurthy and Ngo (2021)	Vehicle hours of delay	-14%	California
		Panel C: Traffic fatalities	
Brazil and Kirk (2016)	Traffic fatalities	No effect	US
Dills and Mulholland (2018)	Traffic fatalities	-4.0% on average, -17% to -40% after four or more years	US
Barreto et al. (2021)	Traffic fatalities	-10% on average, -16% after five quarters	Brazil
Barrios et al. (2020)	Traffic fatalities	+3% on average, +16% after four years	US
Anderson and Davis (2021)	Traffic fatalities	-4% in 2019	US

Table 2: Effect of ride-hailing on ridership, congestion, and fatalities

Year	Ridership (1)	Congestion (2)	Fatalities (3)
2013	0.031	0.109	−0.150
	(0.292)	(0.333)	(0.500)
2014	0.076	0.103	0.212
	(0.208)	(0.524)	(0.625)
2015	0.075	0.118	0.583
	(0.500)	(0.524)	(0.167)
2016	−0.019	0.002	0.521
	(0.792)	(0.714)	(0.042)
2017	−0.097	0.013	0.654
	(0.625)	(0.714)	(0.042)
2018		0.039	
		(0.667)	
2019		0.055	
		(0.667)	
Average	0.013	0.063	0.365
(RMSPE std. p)	(0.667)	(0.667)	(0.167)
Overall C.I.	[-0.013,0.040]	[-0.063,0.188]	[-0.365,1.094]

Notes: Standardized p-values are in parentheses and confidence intervals are in brackets. Confidence levels are 96% for ridership, 95% for congestion, and 96% for fatalities. See Footnote 19 for an explanation of how confidence levels are expressed following Firpo and Possebom (2018).

Table 3: Robustness checks

	2013 synth. (1)	Alt. matching (2)	Recess. + alt. match. (3)	Leave-one-out (4)	2014 synth. (5)
Transit ridership	0.013	0.059	0.033	0.008	-0.059
	$[-0.013, 0.040]$	$[-0.059, 0.178]$	$[-0.033, 0.099]$	$[-0.028, 0.044]$	$[-0.176, 0.059]$
Congestion	0.063	0.063	—	0.062	0.150
	$[-0.063, 0.188]$	$[-0.063, 0.188]$	—	$[-0.062, 0.186]$	$[-0.150, 0.451]$
Traffic fatalities	0.365	0.367	0.435	0.416	0.509
	$[-0.365, 1.094]$	$[-0.367, 1.101]$	$[-0.435, 1.304]$	$[-0.416, 1.247]$	$[-0.509, 1.528]$

Notes: The first column reproduces our baseline results for comparison. Confidence levels for the first four columns are 96% for ridership, 95% for congestion, and 96% for fatalities. For 2014 "treatment," (Column 5), confidence levels are 94%, 96%, and 95%, respectively. See Footnote 19 for an explanation of how confidence levels are expressed following Firpo and Possebom (2018). Column (4) reports the averages of point estimates and confidence interval bounds over all leave-one-out synthetic controls.

A Additional figures

Figure A1: Reported ridership by APTA and Translink, by mode and total

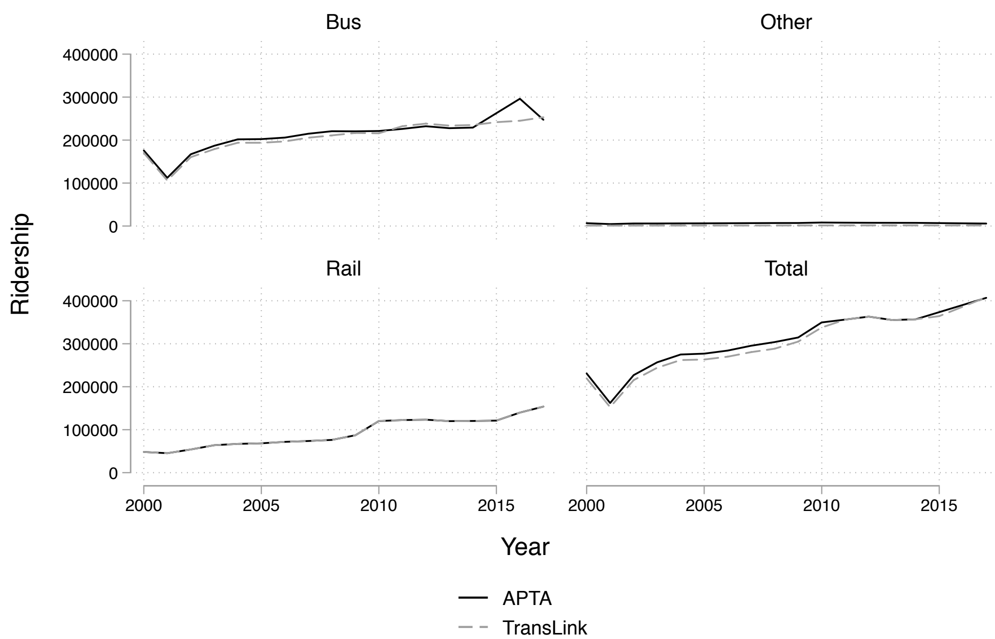
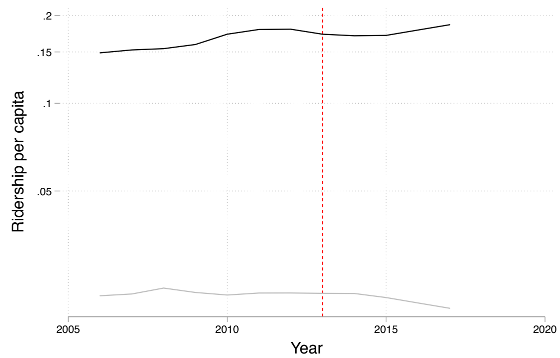
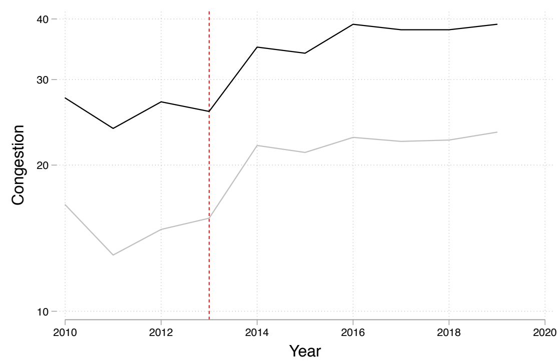


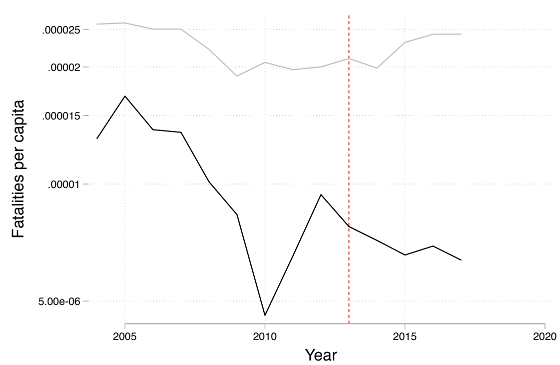
Figure A2: Outcome variables over time for Vancouver and potential donors



(a) Transit ridership (per capita)



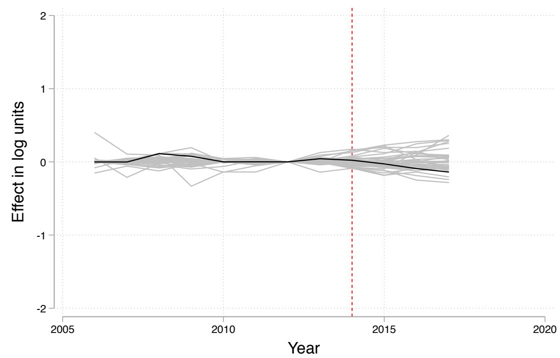
(b) Congestion (combined measure; raw score)



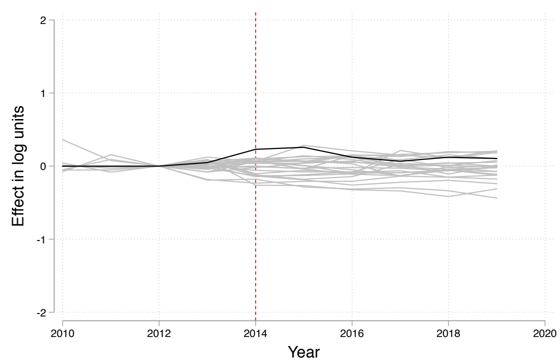
(c) Traffic fatalities (per capita)

Notes: Each panel shows outcome variables for Vancouver (black line) and donors (light gray). Panel (a) shows ridership, (b) shows traffic congestion, and (c) shows traffic fatalities. The potential donor group is cities where a ride-hailing company began offering services in 2013.

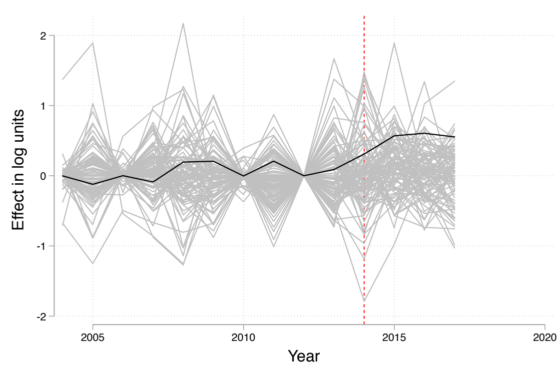
Figure A3: Estimated treatment effects with 2014 treatment



(a) Ridership



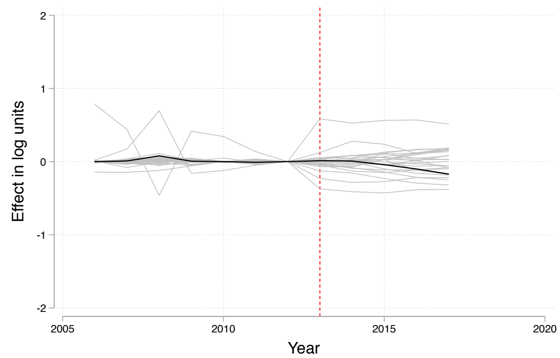
(b) Congestion



(c) Fatalities

Notes: Each panel shows treatment effects for Vancouver (black line) and placebo treatment effects for donors (light gray). Panel (a) shows ridership, (b) shows traffic congestion, and (c) shows traffic fatalities. The donor group is cities where a ride-hailing company began offering services in 2014.

Figure A4: Ridership treatment effects by mode



(a) Bus

(b) Rail

Notes: Each panel shows ridership for Vancouver (black line) and donors (light gray). Panel (a) shows ridership by bus, and (b) shows rail. The potential donor group is cities with that mode where a ride-hailing company began offering services in 2013.

B Additional tables

Table A1: Simple difference-in-difference estimates

	Seattle	Vancouver	Toronto	Vancouver	Portland	Vancouver
Panel A: Transit ridership						
Before	-0.2%	3.9%	-0.6%	1.7%	-0.6%	1.4%
After	0.7%	0.7%	0.9%	1.9%	-2.9%	2.9%
Difference	0.9%	-3.2%	1.6%	-0.2%	-2.3%	1.5%
Difference-in-differences	4.1%		1.8%		-3.8%	
Panel B: Traffic congestion						
Before	-7.5%	-0.9%	2.5%	-2.1%	8.0%	6.2%
After	5.2%	5.4%	6.7%	7.1%	0.0%	2.2%
Difference	12.7%	6.3%	4.2%	9.2%	-8.0%	-4.0%
Difference-in-differences	6.4%		-5.0%		-4.0%	
Panel C: Traffic fatalities						
Before	-4.6%	-4.1%	-7.0%	-5.85%	-7.0%	-5.9%
After	0.4%	-7.5%	-0.1%	-4.8%	29.7%	-3.8%
Difference	5.0%	-3.4%	4.5%	0.8%	36.7%	2.1%
Difference-in-differences	8.4%		3.7%		34.6%	

Notes: This table reports difference-in-difference results comparing average growth rates before and after ride-hailing was available in Seattle, Toronto, and Portland to growth rates in Vancouver over the same time period. Average growth rates are calculated using the geometric mean. For Seattle's transit ridership, we take the mean growth rate starting in 2009, rather than 2005, as its growth rate was remarkably consistent from 2009–2012 and so this seems the reasonable comparison.

Table A2: Ridership results by mode

Year	Bus (1)	Rail (2)
2013	0.012	0.048
	(0.792)	(0.190)
2014	0.007	0.221
	(0.958)	(0.238)
2015	−0.042	0.350
	(0.750)	(0.190)
2016	−0.100	0.303
	(0.583)	(0.286)
2017	−0.170	0.263
	(0.375)	(0.333)
Average	−0.059	0.237
(RMSPE std. p)	(0.583)	(0.286)
Overall C.I.	$[-0.176, 0.059]$	$[-0.237, 0.712]$

Notes: Standardized p-values are in parentheses and confidence intervals are in brackets. Confidence levels are 96% for Bus ridership and 95% for Rail ridership. See Footnote 19 for an explanation of how confidence levels are expressed following Firpo and Possebom (2018).

C Synthetic treatment

Consider $J + 1$ units indexed j, where the first unit is untreated, and units $j = 2, ..., J + 1$ are treated at time $t > T_0$, T_0 being the last pre-intervention period. Denote potential outcomes under treatment and non-treatment for unit j at time t as Y_{jt}^T and Y_{jt}^N, respectively. The estimand is $\alpha_{1t} = Y_{1t}^T - Y_{1t}^N$, for all t in which the donor group is treated.

Following Abadie (2021), we observe a vector of k outcome predictors for each unit, $X_{1j}, ..., X_{kj}$, where $\mathbf{X}_1, ..., \mathbf{X}_{J+1}$ collects the $k \times 1$ vectors of predictors for all j. Let $\mathbf{X_0}$ collect these vectors for units $j = 2, ..., J + 1$. Finally, let $\mathbf{W}$ denote the $J \times 1$ vector of weights w_j for all treated units. The estimator of the potential outcome under treatment for the untreated unit is then

$$\hat{Y}_{1t}^T = \sum_{j=2}^{J+1} w_j Y_{jt}^T \tag{1}$$

and the treatment effect estimator is

$$\hat{\alpha}_{1t} = \hat{Y}_{1t}^T - Y_{1t}^N. \tag{2}$$

Taking as our predictor of interest the average value of the key outcome variable in the pre-treatment period, we choose $w_2, ..., w_{J+1}$ to minimize $\left| \bar{Y}_{1t}^N - \sum_{j=2}^{J+1} w_j \bar{Y}_{jt}^N \right|$. More generally, Abadie and Gardeazabal (2003) and Abadie et al. (2010) propose choosing $\mathbf{W}^*$ to minimize

$$(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W}) \tag{3}$$

or the pre-treatment root mean squared prediction error (RMSPE), where $\mathbf{V}$ weights the relative importance of various predictors of the outcome in post-treatment. See Abadie (2021) for a deeper discussion of how to choose $\mathbf{V}$ in cases where more than one predictor is used.

Given estimated treatment effect $\hat{\alpha}_{1t}$, we follow Abadie et al. (2015) by constructing p-values using the sample analogue to

$$p = Pr\left(\left|\hat{\alpha}_{1t}^{PL}\right| \geq \left|\hat{\alpha}_{1t}\right|\right) \tag{4}$$

where the superscript PL indicates that we have run the synthetic treatment procedure on all potential donors and constructed an effect for each. The p-value will be large if, after running the synthetic treatment on every potential donor, we observe an effect that is frequently as large or larger for placebo units than for the untreated unit. We can also adjust p-values for pre-treatment fit by scaling the estimated treatment effect by the respective unit's pre-treatment RMSPE. Note that this inferential procedure can also be performed using the post-treatment RMSPE for the unit of interest and placebos, as a test of overall significance.

D Donor weights – 2013 "treatment"

Table A3: Ridership

Agency	CBSA	Baseline	Alt. matching	Great Recession
Metrop. Atlanta Rapid Transit Auth.	Atlanta-Sandy Springs-Marietta, GA	0.013	0	0
Maryland Transit Admin.	Baltimore-Towson, MD	0.023	0	0
Massachusetts Bay Transport. Auth.	Boston-Cambridge-Quincy, MA-NH	0.021	0	0
Charlotte Area Transit System	Charlotte-Gastonia-Rock Hill, NC-SC	0.026	0	0
Chicago Transit Auth.	Chicago-Joliet-Naperville, IL-IN-WI	0.022	0	0
Metra Rail	Chicago-Joliet-Naperville, IL-IN-WI	0.015	0	0
Northern Indiana Commuter Transport. Dist.	Chicago-Joliet-Naperville, IL-IN-WI	0.013	0	0
Trinity Railway Express	Dallas-Fort Worth-Arlington, TX	0.006	0	0
Denver Regional Transport. Dist.	Denver-Aurora-Broomfield, CO	0.012	0.498	0.187
Detroit Transport. Corp.	Detroit-Warren-Livonia, MI	0.019	0	0.324
Metro	Los Angeles-Long Beach-Santa Ana, CA	0.017	0	0
Metrolink	Los Angeles-Long Beach-Santa Ana, CA	0.017	0	0
Metro Transit	Minneapolis-St. Paul-Bloomington, MN-WI	0.016	0	0
Metrop. Transit Auth.	Nashville-Davidson–Murfreesboro–Franklin, TN	0.015	0	0
Regional Transport. Auth.	Nashville-Davidson–Murfreesboro–Franklin, TN	0.017	0.090	0.010
Central Oklahoma Transport. and Parking Auth.	Oklahoma City, OK	0.204	0.412	0
City of Phoenix (Valley Metro)	Phoenix-Mesa-Glendale, AZ	0.015	0	0
San Diego Metrop. Transit System	San Diego-Carlsbad-San Marcos, CA	0.430	0	0.295
Santa Clara Valley Transport. Auth.	San Jose-Sunnyvale-Santa Clara, CA	0.014	0	0
Santa Barbara Metrop. Transit Dist.	Santa Barbara-Santa Maria-Goleta, CA	0.014	0	0
King County Dept. of Transport.	Seattle-Tacoma-Bellevue, WA	0.015	0	0
City of Tucson	Tucson, AZ	0.024	0	0
Virginia Railway Express	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.021	0	0.183
Metro	Washington-Arlington-Alexandria, DC-VA-MD-WV	0.013	0	0

Table A4: Traffic congestion

CBSA	Baseline	Alt. matching
Atlanta-Sandy Springs-Marietta, GA	0.006	0.006
Baltimore-Towson, MD	0.005	0.005
Boston-Cambridge-Quincy, MA-NH	0.363	0.363
Charlotte-Gastonia-Rock Hill, NC-SC	0.006	0.006
Chicago-Joliet-Naperville, IL-IN-WI	0.005	0.005
Dallas-Fort Worth-Arlington, TX	0.011	0.011
Denver-Aurora-Broomfield, CO	0.540	0.540
Detroit-Warren-Livonia, MI	0.003	0.003
Indianapolis-Carmel, IN	0.005	0.005
Los Angeles-Long Beach-Santa Ana, CA	0.007	0.007
Minneapolis-St. Paul-Bloomington, MN-WI	0.004	0.004
Nashville-Davidson–Murfreesboro–Franklin, TN	0.006	0.006
Oklahoma City, OK	0	0
Phoenix-Mesa-Glendale, AZ	0.003	0.003
Providence-New Bedford-Fall River, RI-MA	0.011	0.011
Sacramento–Arden-Arcade–Roseville, CA	0.003	0.003
San Diego-Carlsbad-San Marcos, CA	0.005	0.005
San Jose-Sunnyvale-Santa Clara, CA	0.006	0.006
Seattle-Tacoma-Bellevue, WA	0.004	0.004
Tucson, AZ	0	0
Washington-Arlington-Alexandria, DC-VA-MD-WV	0.007	0.007

Table A5: Traffic fatalities

City	Baseline	Alt. matching	Great Recession
Atlanta, GA	0	0	0
Baltimore, MD	0	0	0
Boston, MA	0.164	0.157	0
Charlotte, NC	0	0	0.239
Chicago, IL	0	0	0
Dallas, TX	0	0	0
Denver, CO	0	0	0
Detroit, MI	0	0	0
Fort Worth, TX	0	0	0
Indianapolis, IN	0	0	0
Los Angeles, CA	0	0	0
Minneapolis, MN	0	0	0
Nashville, TN	0	0	0
Oklahoma City, OK	0	0	0
Phoenix, AZ	0	0	0
Providence, RI	0	0	0
Sacramento, CA	0	0	0
San Diego, CA	0	0	0
San Jose, CA	0	0	0
Santa Barbara, CA	0	0	0
Seattle, WA	0	0	0
St. Paul, MN	0.836	0.843	0.761
Tucson, AZ	0	0	0
Washington, DC	0	0	0

E Donor weights – 2014 “treatment”

Table A6: Ridership

Agency	CBSA	Weight
New Mexico Dept. of Transport.	Albuquerque, NM	0.090
Golden Empire Transit Dist.	Bakersfield-Delano, CA	0.007
Stark Area RTA	Canton-Massillon, OH	0.003
Lee County Transit	Cape Coral-Fort Myers, FL	0.002
Chattanooga Area RTA	Chattanooga, TN-GA	0.006
Southwest Ohio RTA	Cincinnati-Middletown, OH-KY-IN	0.003
Greater Cleveland RTA	Cleveland-Elyria-Mentor, OH	0
Mountain Metrop. Transit	Colorado Springs, CO	0.002
Central Ohio Transit Auth.	Columbus, OH	0.009
Chapel Hill Transit	Durham-Chapel Hill, NC	0.212
Mass Transp. Auth.	Flint, MI	0.002
Fresno Area Express	Fresno, CA	0
Greensboro Transit Auth.	Greensboro-High Point, NC	0.008
CTTRANSIT (Hartford)	Hartford-West Hartford-East Hartford, CT	0.007
Harris County MTA, Texas	Houston-Sugar Land-Baytown, TX	0.003
Jacksonville Transp. Auth.	Jacksonville, FL	0.005
Capital Area Transp. Auth.	Lansing-East Lansing, MI	0.008
River City Transit Auth.	Louisville/Jefferson County, KY-IN	0.004
Memphis Area Transit Auth.	Memphis, TN-MS-AR	0.007
Miami-Dade Transit	Miami-Fort Lauderdale-Pompano Beach, FL	0.004
South Florida Regional Transp. Auth.	Miami-Fort Lauderdale-Pompano Beach, FL	0.008
Milwaukee County Transit System	Milwaukee-Waukesha-West Allis, WI	0.004
OC Transpo	Ottawa, ON	0.006
Delaware Transit Corp.	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.011
Southeastern Pennsylvania Transp. Auth.	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.008
Port Auth. of Allegheny County	Pittsburgh, PA	0.005
Northern New England Passenger Rail Auth.	Portland-South Portland-Biddeford, ME	0.155
Riverside Transit Agency	Riverside-San Bernardino-Ontario, CA	0.006
Salem-Keizer Transit	Salem, OR	0.003
Utah Transit Auth.	Salt Lake City, UT	0.369
Spokane Transit Auth.	Spokane, WA	0.012
Hillsborough Area Regional Transit Auth.	Tampa-St. Petersburg-Clearwater, FL	0.008
Toronto Transit Commission	Toronto, ON	0.007
Hampton Roads Transit	Virginia Beach-Norfolk-Newport News, VA-NC	0.003
Visalia City Coach	Visalia-Porterville, CA	0.007

Table A7: Traffic congestion

CBSA	Weight
Cincinnati-Middletown, OH-KY-IN	0.001
Cleveland-Elyria-Mentor, OH	0
Columbus, OH	0.001
Hartford-West Hartford-East Hartford, CT	0.562
Honolulu, HI	0
Houston-Sugar Land-Baytown, TX	0.001
Jacksonville, FL	0.001
Kansas City, MO-KS	0.001
Louisville/Jefferson County, KY-IN	0.416
Memphis, TN-MS-AR	0.001
Miami-Fort Lauderdale-Pompano Beach, FL	0.002
Milwaukee-Waukesha-West Allis, WI	0.004
Montreal, QC	0
Orlando-Kissimmee-Sanford, FL	0.001
Ottawa, ON	0.001
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	0.001
Pittsburgh, PA	0.001
Raleigh-Cary, NC	0.001
Richmond, VA	0.001
Riverside-San Bernardino-Ontario, CA	0.001
Salt Lake City, UT	0
Tampa-St. Petersburg-Clearwater, FL	0.003
Toronto, ON	0.002
Virginia Beach-Norfolk-Newport News, VA-NC	0.001

Table A8: Traffic fatalities

City	Weight	City	Weight
Akron, OH	0.006	Lexington-Fayette, KY	0.005
Albuquerque, NM	0.005	Lincoln, NE	0.007
Amarillo, TX	0.004	Little Rock, AR	0.004
Asheville, NC	0.003	Louisville, KY	0.004
Athens-Clarke, GA	0.002	Lubbock, TX	0.003
Bakersfield, CA	0.004	Madison, WI	0.004
Baton Rouge, LA	0.003	Manchester, NH	0.002
Bloomington, IN	0.002	Memphis, TN	0.004
Boise City, ID	0.003	Miami, FL	0.005
Cape Coral, FL	0.005	Milwaukee, WI	0.003
Cedar Rapids, IA	0.003	Modesto, CA	0.005
Charleston, SC	0.006	Myrtle Beach, SC	0.004
Chattanooga, TN	0.004	New Haven, CT	0.003
Cincinnati, OH	0.008	North Port, FL	0.003
Cleveland, OH	0.004	Ocala, FL	0.004
College Station, TX	0.052	Omaha, NE	0.004
Colorado Springs, CO	0.004	Orlando, FL	0.006
Columbia, MO	0.250	Ottawa, ON	0.003
Columbia, SC	0.002	Palm Bay, FL	0

Columbus, OH	0.004	Pensacola, FL	0.173
Dayton, OH	0.004	Philadelphia, PA	0.004
Deltona, FL	0.068	Pittsburgh, PA	0.003
Des Moines, IA	0.003	Port St. Lucie, FL	0.002
Durham, NC	0.003	Raleigh, NC	0.004
El Paso, TX	0.004	Richmond, VA	0.005
Fayetteville, AR	0.020	Riverside, CA	0.005
Fayetteville, NC	0.003	Roanoke, VA	0.005
Flagstaff, AZ	0.006	Salem, OR	0.003
Flint, MI	0.004	Salt Lake City, UT	0.005
Fort Collins, CO	0.003	Santa Fe, NM	0.004
Fresno, CA	0.004	Santa Rosa, CA	0.104
Gainesville, FL	0.005	South Bend, IN	0.002
Grand Rapids, MI	0.003	Spokane, WA	0.005
Green Bay, WI	0.003	Tacoma, WA	0.003
Greensboro, NC	0.003	Tallahassee, FL	0.003
Greenville, SC	0.003	Tampa, FL	0.004
Hartford, CT	0.003	Toledo, OH	0.003
Houston, TX	0.004	Toronto, ON	0.004
Jackson, MS	0.005	Tulsa, OK	0.004
Jacksonville, FL	0.004	Urban Honolulu, HI	0.004
Kahului, HI	0.003	Vancouver, WA	0.002
Kalamazoo, MI	0.003	Virginia Beach, VA	0.002
Kansas City, MO	0.004	Visalia, CA	0.004
Knoxville, TN	0.004	Waco, TX	0.005
Lafayette, IN	0.002	Wilmington, NC	0.007
Lakeland, FL	0.003	Winston-Salem, NC	0.004
Lansing, MI	0.004	Worcester, MA	0.003