Estimating effects of Uber ride-sharing service on road traffic-related deaths in South Africa: a quasi-experimental study

Jonathan Yinhao Huang,[1,2,5] Farhan Majid,[3] Mark Daku[4]

► Additional material is published online only. To view please visit the journal online (http://dx.doi.org/10.1136/jech-2018-211006).

[1]Institute for Health and Social Policy, McGill University, Montreal, Quebec, Canada
[2]Singapore Institute for Clinical Sciences, Agency for Science, Technology and Research, Singapore, Singapore
[3]Rice University's Baker Institute for Public Policy, Houston, Texas, USA
[4]Department of Political Science, Texas Christian University AddRan College of Liberal Arts, Fort Worth, Texas, USA
[5]Department of Epidemiology, Biostatistics and Occupational Health, McGill University, Montreal, Quebec, Canada

Correspondence to
Dr Jonathan Yinhao Huang; jon.huang@mail.mcgill.ca; jonathan_huang@sics.a-star.edu.sg

Received 10 May 2018
Revised 22 October 2018
Accepted 9 December 2018

To cite: Huang JY, Majid F, Daku M. *J Epidemiol Community Health* Epub ahead of print: [*please include* Day Month Year]. doi:10.1136/jech-2018-211006

ABSTRACT

Background Road traffic deaths are a substantial barrier to population health improvement in low-income and middle-income countries (LMICs). In South Africa, the road-traffic injury mortality (RTM) rate of 27 per 100 000 population is twice the global average, over 60% of which are alcohol-related. Recent US studies suggest the Uber ride-sharing service may reduce alcohol-related RTM, however RTM burden in the USA is relatively low and transport behaviours differ from LMICs.

Methods Using certification data from all deaths occurring in South Africa in the years 2010–2014 (n=2 498 216), we investigated the relative change in weekly road traffic-related death counts between provinces which received Uber services (beginning in 2013) against those that did not using a difference-in-differences approach.

Results Weekly road traffic-related deaths in provinces with Uber were lower following Uber introduction than in comparison provinces without Uber. The effect size was larger in the province which had Uber the longest (Gauteng) and among young adult males (aged 17–39 years). However, the absolute effects were very small (<2 deaths per year) and may coincide with seasonal variation.

Conclusions Overall, findings did not support either an increase or large decrease in province-level road traffic-related deaths associated with Uber introduction to South Africa. More localised investigations in South Africa and other LMICs are needed.

Over 1.25 million people die on the world's roads annually.[1] Globally, road traffic injuries are the number one killer of people aged 15–29 years, and over 90% of fatal crashes occur in low-income and middle-income countries (LMIC). Alcohol use is a leading risk factor in low-income countries, where alcohol has been detected in 33%–69% of fatally injured drivers.[2 3] Technological solutions which can reduce the incidence of drunk driving have the potential to reduce drunk driving deaths. Ride sharing services such as Uber, which now spans over 630 cities in more than 80 countries globally,[4] show potential to reduce driving under the influence of alcohol, road traffic collisions and road-traffic injury mortality (RTM) in the USA.[5] Such benefits may arise through increased availability, reduced costs, improved safety and improved accountability relative to prevailing taxi services. However, formal empirical investigations of its effects in US cities have produced mixed findings. A California study found Uber's introduction to be related to a 3.6%–5.6% reduction in alcohol-related motor vehicle homicides.[6] A New York City study found Uber to be related to a 25%–35% reduction in alcohol related collisions.[7] Brazil and Kirk found no effect of Uber's introduction into the 100 most populous US metropolitan areas[8] while Morrison *et al*[9] found a 62% reduction in alcohol-related traffic collisions in Portland, Oregon, but not in three other cities.

The effects of Uber outside the US may be radically different and have been largely unexplored. In South Africa, where Uber has been available since late 2013, Uber's introduction may have a substantial influence on road traffic collisions due in part to two major differences from the US context. First, RTM in South Africa is 27 deaths per 100 000 population, well above the African Region and over twice the global average.[3 10 11] RTM remains a leading cause of death within South Africa and the fourth largest contributor to years of life lost.[11 12] In 2012, road traffic injuries accounted for US$10.5 billion in health services expenditures, approximately 3% of South Africa's gross domestic product.[3] Second, poor perceptions of safety for taxis[13] and public transportation[10] may increase dependence on private automobiles or walking, including after the consumption of alcohol. Recent qualitative interviewing by a co-author in South African cities served by Uber suggests that potential users may be driven to Uber due to perceived safety-related features of the service such as: the vetting of driver credentials, a user-driven driver rating system, transparency of driver routes and cashless transactions. Indeed, Uber reported over 1 million trips in 2014 in its three South African markets (up to 2 million in the first half of 2015) far outstripping growth at launch in San Francisco, London or Paris.[14]

Using publicly available mortality records from 2010 to 2014 and a difference-in-differences (DiD) approach, we investigate whether the introduction of Uber to three major South African urban areas in late 2013–early 2014 changed trends in weekly road traffic-related deaths (RTD) relative to areas where the service was unavailable. We hypothesise that the introduction of Uber may have had a measurable effect on reducing RTD in a setting where alcohol-related traffic mortality is high and where Uber may strongly compete with alternative forms of transportation.

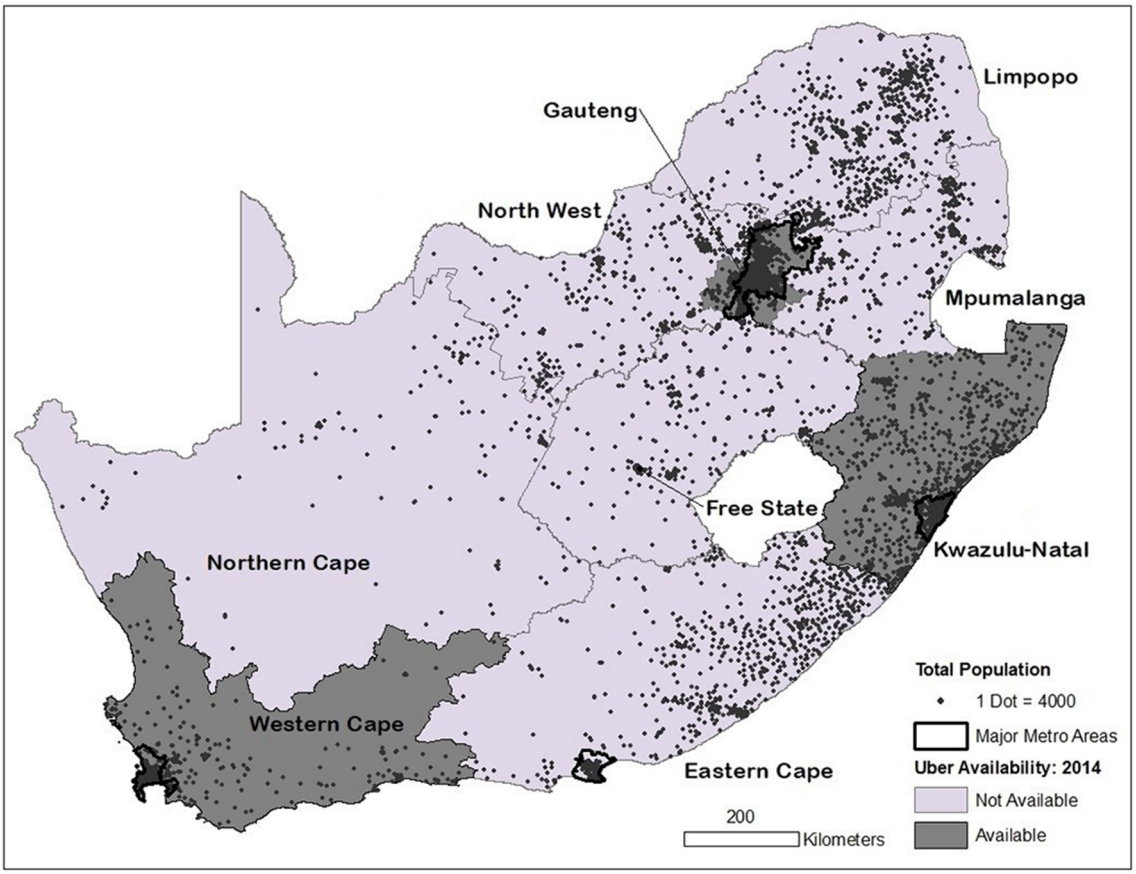

Figure 1 Population Density Map of South Africa. Provinces with Uber service available in 2014 (treated provinces) in a darker shade. The bold lines demarcate large metropolitan areas. Each dot represents 4,000 people. Data drawn from 2011 South African Census and the Municipal Demarcation Board (www.demarcation.org.za).

METHODS

Data sources

Uber service availability (primary exposure): Based on company press releases, Uber was launched first on 11 September 2013 in Johannesburg, Gauteng Province, on 11 October 2013 in Cape Town, Western Cape, and on 12 February 2014 in Durban, KwaZulu-Natal.[15] The other six provinces in South Africa did not have Uber service during the study period. The three treated cities represent the major urban centres in each of their respective provinces (figure 1).[16] A systematic search though popular media for changes in taxi or ride-sharing services, new road traffic-related laws or policies, and public or private road safety campaigns found two limited taxi booking applications,[17] but no other relevant changes during our study period (additional details in online supplementary materials).

RTDs (primary outcome): Vital statistics on all registered deaths in South Africa between 1 January 2010 and 31 December 2014 were obtained from DataFirst, an online service of the University of Cape Town.[18] The data include causes of death classified by WHO International Classification of Diseases, 10th Revision (ICD-10) codes, province in which the death occurred, death date and selected demographic characteristics reported on the death register.[19] We included all deaths reported to vital registration excluding the 0.7% of records for which a death province was not reported (n=2 514 561 total death records–16 345 missing location=2 498 216 deaths included for analyses). For our main analyses, we classified a death as road-traffic related if any (primary or underlying) cause of death was assigned an ICD-10 code indicating a 'transport accident'[20] (n=27 497 road-traffic deaths, table 1). No ethics approval was required for the use of these data.

Pseudo-panel design: RTDs were aggregated into 2340 province-weeks (nine provinces×5 years×52 weeks). The week is the smallest interval of aggregation that prevents empty cells as well as stabilises known differences in week versus weekend event rates. We implemented a pseudo-panel design used in economics to analyse repeated cross-sections.[21–24] To this end, we further aggregated RTDs into cohorts defined by birth province, birth year and sex. The median number of cohorts was 252 (mean=272.5), as not all ages and provinces are represented in each unit (figure 2). Consequently, our final data set included 637 657 observations (2340 province-weeks×272.5 cohorts per province-week) of weekly RTD counts (figure 2).

Analytical strategy

We investigated the extent to which average weekly RTDs within provinces changed after Uber became available (first difference) varied by whether or not Uber was available in that province (second difference). Specifically, this was done by fitting DiD models[25 26] of following form:

$$Y_{wjtps} = \beta(\underline{Treated}_j * Post_w) + \gamma Treated_j + \alpha_w + \alpha_t + \alpha_p + \alpha_s + \alpha_{wjtps} \quad (1)$$

Where Y_{wjtps} is the number of RTDs in week w and province j for a cohort defined by birth year t, birth province p and sex s. The binary indicator variable $Treated_j$ takes the value of 1 for cohorts who died in a treated province j (Gauteng, Western Cape

Table 1 Total reported deaths in official South Africa mortality reports 2010–2014, by cause (road traffic-related deaths or cancer) and province

Province	Cause	2010	2011	2012	2013	2014	All
Western Cape	RTD	573	485	504	554	526	2642
	Cancer	6074	6368	6374	6576	6605	31 997
	All	47 227	47 207	47 609	47 562	46 581	236 186
	*Population (in millions)**	5.22	5.29	5.82	6.02	6.12	–
Eastern Cape	RTD	862	836	656	802	932	4088
	Cancer	5360	5061	4503	4382	4808	24 114
	All	81 399	73 720	66 200	64 662	67 902	353 883
	*Population (in millions)**	6.74	6.83	6.56	6.62	6.79	–
Northern Cape	RTD	116	64	213	375	475	1243
	Cancer	1190	1105	1165	1106	1221	5787
	All	15 591	15 087	14 379	14 107	14 594	73 758
	*Population (in millions)**	1.10	1.10	1.15	1.16	1.17	–
Free State	RTD	537	519	558	589	583	2786
	Cancer	2758	2435	2166	2314	2331	12 004
	All	46 485	41 973	36 219	34 514	34 305	193 496
	*Population (in millions)**	2.82	2.76	2.75	2.75	2.79	–
KwaZulu-Natal	RTD	939	936	988	944	1016	4823
	Cancer	5325	5537	5514	4772	4654	25 802
	All	119 527	108 075	100 185	87 969	80 705	496 461
	*Population (in millions)**	10.65	10.82	10.27	10.46	10.69	–
North West	RTD	281	319	350	421	411	1782
	Cancer	2237	2421	2386	2495	2455	11 994
	All	41 156	38 599	37 030	36 746	36 354	189 885
	*Population (in millions)**	3.20	3.25	3.51	3.60	3.68	–
Gauteng	RTD	562	288	327	374	363	1914
	Cancer	7231	7361	7334	7587	7588	37 101
	All	108 897	103 853	102 011	100 864	99 725	515 350
	*Population (in millions)**	11.19	11.33	12.27	12.73	12.91	–
Mpumalanga	RTD	372	410	341	435	427	1985
	Cancer	2090	2103	2115	2103	2088	10 499
	All	42 811	39 065	37 401	35 805	35 926	191 008
	*Population (in millions)**	3.62	3.66	4.04	4.13	4.23	–
Limpopo	RTD	1376	1211	1286	1180	1181	6234
	Cancer	2224	2251	2337	2386	2453	11 651
	All	51 101	48 424	50 397	48 919	49 348	248 189
	*Population (in millions)**	5.44	5.55	5.40	5.52	5.63	–
All Provinces	*RTD*	*5618*	*5068*	*5223*	*5674*	*5914*	*27 497*
	Cancer	*34 489*	*34 642*	*33 894*	*33 721*	*34 203*	*170 949*
	All	*554 194*	*516 003*	*491 431*	*471 148*	*465 440*	*2 498 216*
	*Population (in millions)**	*49.991*	*50.587*	*51.771*	*52.982*	*54.002*	–

*Populations taken from Statistics South Africa mid-year population estimates (Release P0302) except for 2012, for which the counts from the census taken in 9–31 October 2011 are used.
RTD, road traffic-related death.

or KwaZulu-Natal) and 0 for others. The indicator variable $Post_w$ is equal to one for deaths during the post-treatment period and 0 otherwise. Following the pseudo-panel design, we estimated within-cohort effects by including fixed effects terms for birth year (α_t), birth province (α_p) and sex (α_s).[21 23] Moreover, we included a fixed effect term for week of death (α_w) to further relax parallel trends assumptions.[26] Consequently, the DiD coefficient gives the average change in weekly road deaths within treated cohorts after Uber's introduction relative to corresponding changes within untreated cohorts, accounting for unobserved heterogeneity within cohorts and week-specific effects.

Three main analyses were conducted using equation (1): first, an overall DiD for all provinces was estimated by including a single and for all treated provinces combined; second, a treated province-specific model was fit using separate and for each of the three treated provinces. Finally, each treated province was compared with each of the untreated provinces in separate models. To reduce the potential influence of differential changes in underlying populations, the reciprocal of the total number of deaths (a proxy for population size) was used as a probability weight for each observation in all analyses.

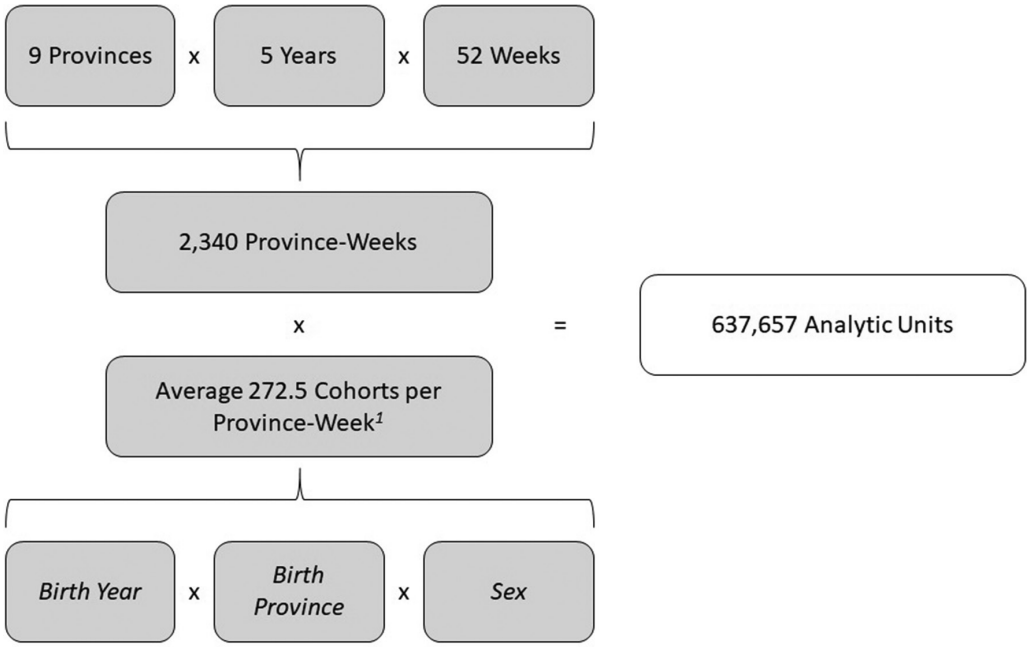

Figure 2 Data Structure. All deaths were aggregated into 2340 province-weeks (nine provinces×260 weeks). Within each province week, deaths were further aggregated by cohorts defined on the basis of common birth year, birth province and sex. While 1854 (103 years×9 provinces×2 sexes) was the maximum number of possible cohorts, most province-weeks were represented by far fewer due to the large overlap between birth and death province and the limited number of birth years represented in each province-week. [1]The median number of cohorts per province-week was 252 (range: 111–640).

We hypothesised that effects may be strongest among young adult males, as that demographic represents the largest population of alcohol related traffic mortality and also most likely to adopt Uber service. Consequently, we ran models stratified by sex and age group (based on mean within-cohort age). Age cut-offs were chosen to be 16 (17 is the legal age for licensed operation of motor vehicles) and 40, leaving an intermediate young adult category (17–39 years old) representing about half of all traffic deaths and 45% of the underlying population.

Sensitivity analyses

Violations of parallel trends in population composition

Given mortality record data, we were unable to directly account for changes in underlying population composition (eg, no denominator data). To test whether differential between-province changes in population composition or reporting may bias results, we estimated our DiD model using weekly cancer deaths as an outcome. Since Uber should have no effect on cancer deaths, there should be no difference in trends over the treatment period. Additionally, we estimated our main model using the difference between cancer and road deaths as an outcome. Assuming reporting standards (over or under road traffic deaths vs cancer deaths) within province are consistent, observed differences in this outcome should be only a function of changes in road deaths, with a negative change in the difference (weekly RTDs–cancer deaths) indicating reduced road traffic deaths.

Uncaptured seasonal or stochastic effects

An important threat to effect identification in this setting is the fact that road traffic deaths exhibit seasonal trends[27] and may be sensitive to short-term or stochastic effects. To test whether such seasonal interprovincial differences may explain our findings, we estimated our models using placebo treatment dates coinciding with the same week 1 year prior to official launch (ie, 11 September 2012 for Gauteng) which would produce a similar imbalance in treated weeks, but where Uber service was not available. Additionally, we explored whether we could model, and thereby correct for, pretreatment time trends using Fourier transformation and automated autoregressive integrated moving average (ARIMA) models.

Pre-existing trends and lagged effects

We further investigated whether stochastic differences or a mis-specified treatment period may explain our findings by considering alternate exposure models. We tested the sensitivity of the analyses to the specification of treatment date by re-estimating the DiD using 1 week lags and leads for −60 up to +60 weeks (for Western Cape and Kwazulu-Natal only 56 and 46 weeks post-treatment were available). This was accomplished by estimating equation (1) adding □ coefficients for each of the 60 weeks before and after the true treatment date, resulting in week-specific DiDs. Coefficients and 95% CIs were then plotted as an event study graph. In addition to evaluating the possibility of lagged and/or changing effect sizes, this approach allows us to test the assumption of no effects in the pretreatment period (ie, an extension of our single placebo test).

Model specification

Finally, we also evaluated whether alternate model specification changed estimates through the use of negative binomial models, random effects estimators, SEs clustered by province of death, adding fixed effects for month, year and their interactions (province × year or province × month), and estimating the main DiD model and the week-specific model (event study) without grouping into cohorts.

Analyses were conducted in Stata V.14.1/15.0 SE and RStudio V.1.1.453 (Boston, Massachusetts, USA)/R V.3.5.0 (Vienna, Austria).

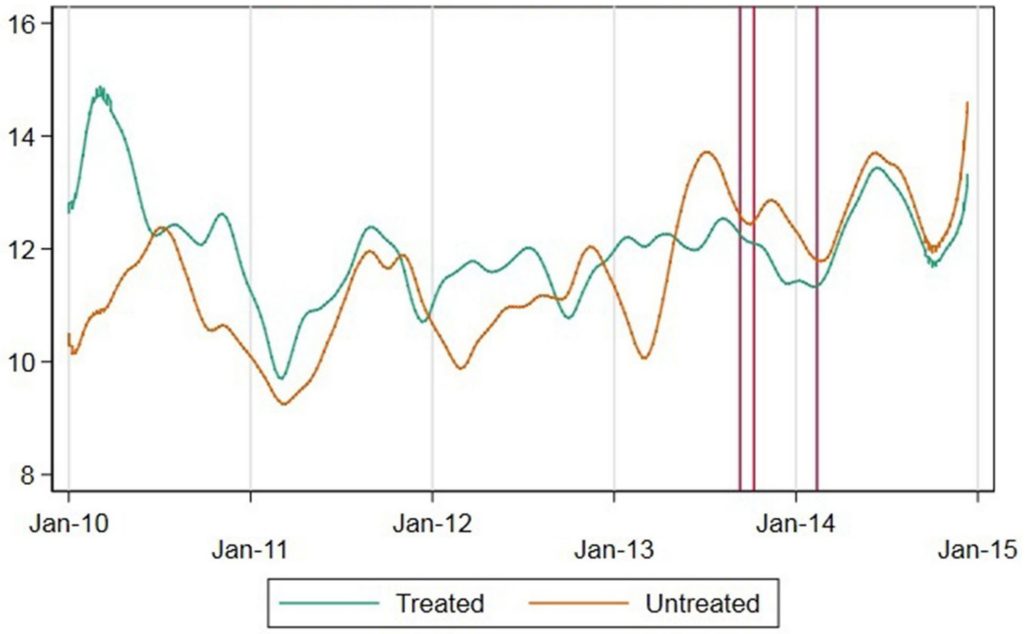

Figure 3 Weekly road traffic-related deaths in South Africa, by treatment status, 2010–2014. Treated provinces show slightly lower weekly road traffic-related deaths following Uber introduction in September 2013 (Gauteng and Western Cape) and February 2014 (KwaZulu-Natal).

RESULTS

Between 2010 and 2014, Western Cape, Northern Cape, North West, Gauteng and Mpumalanga showed population growth while the other four had stable or fluctuating populations (table 1). RTDs comprised a similar proportion of all deaths between 2010 and 2014 across seven provinces (~1%), but were less common in Gauteng (0.4%), an urban province with Uber access, and more common in Limpopo (2.5%; table 1). From LOWESS smoothed curves, we observed an apparent reduction in combined treated (Gauteng, Western Cape, Kwazulu-Natal) versus untreated provinces (figure 3). Substantial within-year variation in rates previously described[27] were observed but not easily attributable to seasonal trends using Fourier or automated ARIMA models (online supplementary figure 1-3). When plotting observed weekly road death counts against those predicted from observed pre-treatment province, month, year, age and sex fixed effects (figure 4), it appears that observed death rates were lower in treated provinces than expected. This was observed even when accounting for seasonality (online supplementary figure 4-6).

From DiD models, it appeared that treated provinces experienced a small reduction in road traffic deaths after Uber

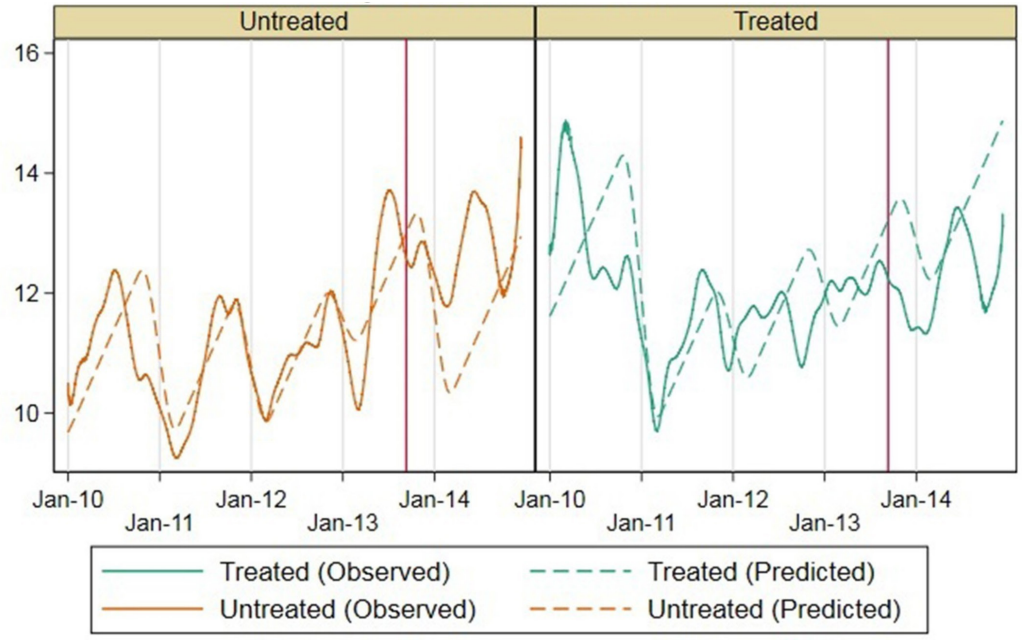

Figure 4 Observed versus predicted weekly road traffic-related deaths, stratified by treatment status (Uber availability). Untreated provinces (left) show a slightly higher observed death count following Uber entry in September 2013 than expected by modelled (province, year and month) trends whereas treated provinces (right) show a slightly lower death count than expected.

Table 2 Reduction in weekly RTDs, by treated province

Contrast	RTDs per week	95% CI	P value
Treated vs untreated	−0.008	−0.010 to 0.006	<0.001
Western Cape vs untreated	−0.005	−0.008 to 0.001	0.005
Gauteng vs untreated	−0.008	−0.010 to 0.007	<0.001
KZN vs untreated	0.003	−0.001 to 0.007	0.157

Birth year, birth province, male sex and death week (dummy variables) included as fixed effects; observations were weighted by inverse probability of selection (no of deaths in a week).
KZN, KwaZulu-Natal; RTD, road traffic-related death.

introduction relative to untreated provinces (table 2). In province-specific models, Gauteng and Western Cape, but not KwaZulu-Natal, appear to have slightly reduced traffic deaths against all untreated provinces following Uber entry (average <1 death per year). In models comparing individual provinces, weekly road-traffic deaths in Western Cape appeared to decrease or remain unchanged (against North West, Mpumalanga and Limpopo) relative to other provinces except for an increase against Gauteng (table 3). Gauteng appeared to improve or remain unchanged (against North West and Limpopo) relative to all other provinces (table 3). In regressions stratified by age and sex (table 4), we observed the largest effects for males and young adults between 17 and 39 years of age. The largest magnitude of difference was observed among young adult males in Gauteng and the Western Cape (0.03 reduction in traffic deaths per week; ~1.5 deaths per year). The proportion of young adult males were similar across provinces (between 20% and 23%) and remained roughly stable during the study period, however the proportion of all deaths among young adult males due to RTDs ranged from 1.2% in Gauteng to 10.6% in Limpopo (online supplementary table 1).

Sensitivity analyses

We found no consistent evidence of effects of Uber entry on cancer deaths (online supplementary table 2). Effects on the (road traffic death) – (cancer death) difference were consistent with reduction in road traffic deaths (eg, for Gauteng: −0.006 (95% CI: −0.011 to −0.001); online supplementary table 3). Using placebo treatment dates, effect estimates for both weekly road traffic deaths (online supplementary table 4) and the (road traffic death) – (cancer death) difference (online supplementary table 3) were similar to those found by using the official launch dates. For example, the estimated effects of Uber on weekly road deaths for Gauteng were −0.009 (95% CI: −0.011 to −0.008) overall and −0.03 (95% CI: −0.038 to −0.023) for young adult males using the placebo date versus −0.008 (95% CI: −0.010 to −0.007) and −0.028 (95% CI: −0.037 to −0.019), respectively, using the official launch date. When modelled as week-specific effects with leads and lags, we did not find strong evidence for an effect on road traffic (figure 5) or cancer deaths (online supplementary figure 7).

Models implementing a negative binomial model (online supplementary table 6), SEs by province of death (online supplementary table 6), random effects estimators, month and year fixed effects, or analysed without grouping into cohorts (online supplementary figure 8) produced qualitatively similar findings to the main models.

Table 3 Province-specific differences in road traffic-related deaths (RTDs) following Uber introduction, by treated province (Gauteng or Western Cape)

		Versus Western Cape		Versus Gauteng
Province	RTDs per week	95% CI	RTI deaths per week	95% CI
Western Cape	–	–	0.003	0.0002 to 0.007
Eastern Cape	0.011	0.006 to 0.016	0.014	0.010 to 0.018
Northern Cape	0.033	0.027 to 0.040	0.037	0.031 to 0.042
Free State	0.004	−0.002 to 0.009	0.007	0.003 to 0.011
KwaZulu-Natal	0.006	0.001 to 0.011	0.010	0.006 to 0.014
North West	−0.003	−0.007 to 0.001	0.001	−0.002 0.003
Gauteng	−0.004	−0.007 to 0.001	–	–
Mpumalanga	−0.001	−0.005 to 0.003	0.003	0.0003 to 0.006
Limpopo	−0.003	−0.008 to 0.003	0.001	−0.004 to 0.006

Birth year, birth province, male sex and death week (dummy variables) included as fixed effects; observations were weighted by inverse probability of selection (no of deaths in a week).

DISCUSSION

To the best of our knowledge, our study is the first to investigate the effect of Uber on RTM outside of the US context. Particularly in South Africa, any potential to reduce RTM is of substantial public health interest. Using publicly available mortality records, we found that the timing of Uber's entry into South Africa coincided with minor reductions in RTDs, particularly in Gauteng Province where Uber was first launched in the country on 11 September 2013, and among young adult males as expected. However, not only was the absolute magnitude of potential reduction small (<2 deaths per province per year), placebo and week-specific analyses suggested the difference may be wholly explained by aggregation and interprovincial variation in seasonal trends.

Previous literature

While previous studies on Uber's effects on road traffic accidents in the US context may not be particularly applicable to the South African context, certain features are worth highlighting. In a California study, Greenwood and Wattal[6] found reductions in alcohol-related RTM to reach a peak at about 15 months (five quarters) after Uber's entry potentially due to scale-up. This may explain the absence of an observed effect in KwaZulu-Natal, for which we only observed 10 months on treatment. Using an interrupted time series (ITS) approach, Morrison *et al*[9] found immediate and sustained effects on alcohol-related collisions, but not on total road traffic injuries. However, we did not have data on alcohol involvement, which would have improved outcome classification. Like Morrison *et al*, we investigated differences in outcomes at the week level. However, we opted against the ITS approach for two reasons: first, our preliminary time series plots and models suggested that we would be unable to well model pretreatment trends and thus result in biased effect estimates; second, as this is a novel, understudied exposure, the appropriate induction period and shape of effect (impact model) is unclear, potentially leading to model misspecification and chance findings.[28]

Instead, we opted for the DiD approach to indirectly account for trends through control groups coupled with sensitivity analyses for exposure timing. In doing so, we found that differences in RTM based on Uber treatment dates may have been artefactual, justifying our concern regarding uncaptured trends and chance findings. In fact, in follow-up analyses applying placebo treatment dates to province-specific time series, we find evidence of reduced road traffic deaths even after correcting for time trends (online supplementary figure 9-11), suggesting that ITS would have also falsely detected an effect.

Table 4 Reduction in RTDs, stratified by age and sex

	RTDs per week (95% CI)		
	Young (≤16 years)	Young adult (17–39 years)	Older adult (≥40 years)
Western Cape vs untreated	−0.005 (−0.027 to 0.018)	−0.016 (−0.025 to 0.007)	0.001 (−0.002 to 0.004)
Gauteng vs untreated	−0.014 (−0.023 to 0.006)	−0.019 (−0.024 to 0.014)	−0.003 (−0.005 to −0.002)
KZN vs untreated	0.007 (−0.009 to 0.024)	0.002 (−0.010 to 0.014)	0.001 (−0.002 to 0.005)
Western Cape vs untreated	−0.008 (−0.013 to 0.003)	−0.002 (−0.006 to 0.002)	−0.031 (−0.044 to −0.017)
	All males	All females	Adult males (17–39 years)
Gauteng vs untreated	−0.012 (−0.015 to 0.009)	−0.005 (−0.007 to 0.003)	−0.028 (−0.037 to 0.019)
KZN vs untreated	0.005 (−0.002 to 0.012)	0.001 (−0.003 to 0.006)	0.003 (−0.017 to 0.023)

Birth year, birth province, male sex and death week (dummy variables) included as fixed effects; observations were weighted by inverse probability of selection (no of deaths in a week).
KZN, KwaZulu-Natal; RTD, road traffic-related deaths.

Strengths and limitations

A major strength of our investigation was the negative control exposure (placebo treatment dates) and outcome (cancer deaths) sensitivity analyses. In RTM studies where there is substantial seasonal or stochastic variability, control of trends by covariates or trend modelling may be insufficient. This was particularly true in our study which lacked rich covariate data. Alternatively, the use of negative controls, common in economics,[7] are increasingly applied in epidemiology[29 30] to detect the presence and magnitude of residual biases. In our study, we were particularly concerned the exposure period may coincide with a period of relatively lower road traffic deaths by chance, for example, if treated provinces happened to experience less of an increase in end-of-year mortality than untreated provinces. An estimation of the effect at a corresponding date in a previous year can give us an estimate of the bias due to unmodelled heterogeneity (eg, imbalance in seasons). Finding an effect of a similar direction and magnitude suggested the observed effect may in fact be non-causal with respect to Uber. In fact, when we estimated week-specific effects in the event study (figure 5), thus completely removing the potential for seasonal effects, we found no effects of treatment. Similarly, data limitations prevented us from directly accounting for interprovincial differences in population changes (eg, size of at-risk population) or death reporting. Finding no effects on cancer deaths, which remained stable throughout the study period, we provided evidence against these alternative hypotheses in spite of data limitations. More detailed discussion on negative controls can be found in online supplementary material.

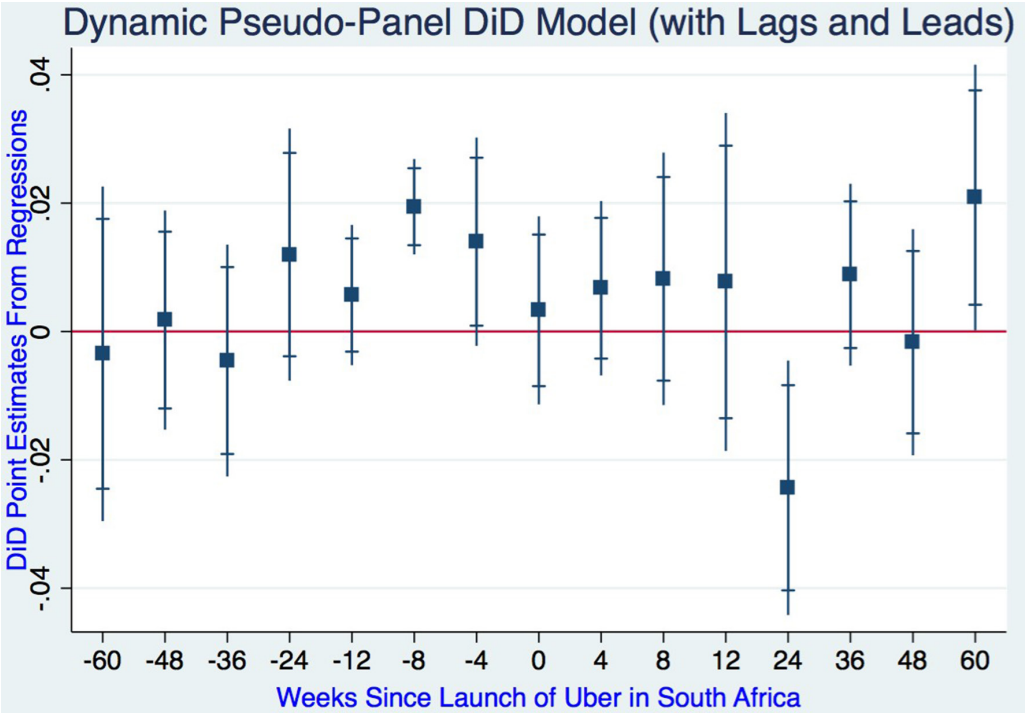

Figure 5 Estimate of week-specific difference-in-differences (DiD) in road traffic deaths at select lags and leads relative to official launch dates. Each point estimate represents the difference in the road traffic death trend post-treatment in treated versus untreated provinces for that specific week relative to the actual treatment date (ie, one estimate was produced for each week; only select weeks are presented). Error bars represent 95% CIs (with a cross-hatch at the 90% CI). Estimates at 48 weeks and 60 weeks post-treatment include only Gauteng and Western Cape due to the limited time on treatment for KwaZulu-Natal in the data.

On this note, our study faces several important limitations concerning data quality and availability. The Road Traffic Management Corporation (RTMC), South Africa's official state agency tasked with investigating and verifying deaths related to motor vehicles, recently released a backlog of annual reports (2011–2016) suggesting that routine administrative death records greatly under-report RTDs.[31] For example, the RTMC reported 11 844 RTDs in 2013 (vs 5674 in mortality records) and 12 702 in 2014 (vs 5914). Unfortunately, more granular data are not currently available. Nonetheless, our control outcome analyses did not suggest any difference in mortality reporting between provinces. Our study was also limited by high variability in weekly death counts and a limited intervention period. Consequently, we chose an initial model with no lag and a single post-treatment effect parameter. This assumption of an acute and consistent effects may have led to model misspecification and vulnerability to a coincidental stochastic or seasonal variation. The one past study of weekly collision counts[9] found a large (62%), immediate and consistent reduction. However, the study leveraged Uber resumption of services (after temporary bans) rather than initiation. Also, both Greenwood and Wattal,[6] and Peck[7] observed non-lagged effects on mean monthly and quarterly events, respectively, when aggregated over a comparable post-treatment period (eg, 1 year). Furthermore, our event study graph suggested there was no consistent week-specific effects through 60 weeks post-treatment. However, is possible that an effect may be observed over a longer time window, as Greenwood and Wattal, and Peck found greater effects after 1 year (or four quarters).[6 7] This should be a direction for future investigation, though important competing exposures, such as legislative changes and competitors such as Taxify (a service launched in 2016) will need to be taken into account.

Perhaps most importantly, we lacked ability to detect effects in relevant subgroups. Notably, while we were able to stratify by age and sex, we did not have detailed information on socioeconomic status or race. Additionally, given the use of provincial mortality records, we were only able to disaggregate whether individuals had access to Uber services at the provincial level. Segments of each treated province would in fact not have had access to the Uber service. The metropolitan areas in which Uber was available consisted of greatly varying proportions of the provincial population: While the three urbanised municipalities centred around Johannesburg (City of Johannesburg, Ekurhuleni and City of Tshwane) account for 86% of the population of Gauteng, eThekwini municipality within which Durban is located, contains only 33% of the population of KwaZulu-Natal.[16] Data that allow identification of populations that may most benefit, for example, wealthier South Africans living in specific Uber service areas, will enable more sensitive analysis.

CONCLUSIONS

The UN Decade of Action and, more recently, the Sustainable Development Goals, have highlighted road traffic deaths as a major barrier to improving population health and well-being, particularly in LMICs.[1 2] Reduction of road traffic deaths and context-specific interventions to improve safe and reliable transportation access to the most vulnerable has been a particular focus. Past studies have examined the potential for Uber to affect RTM in the USA. However, it is in LMICs such as South Africa—countries which have unique, urban public transport challenges and high burdens of RTM—where the greatest promise of potential benefit likely exist. Under data limits, we have demonstrated, in contrast to findings in the USA, Uber did not appear to affect weekly RTM at the provincial level within the first year of launch. Moreover, we demonstrated through exhaustive sensitivity analyses that perceived effects may be artefactual and due to fluctuations in RTM. Nonetheless, future analyses that more specifically target populations that stand to benefit at the local and municipal level will be able to better quantify if there are any benefits to alternative taxi services in South Africa or other LMICs where such services are becoming increasingly prevalent.

What is already known on this subject

- Reducing road traffic injury mortality (RTM) is a global priority and mobile-based ride sharing technologies such as Uber promise to be a novel solution.
- As of 4 December 2017, only four studies were identified that investigate the effect of Uber on road traffic collisions, injury or mortality; all were conducted in the USA, a country with low RTM, and found inconsistent effects between and within metropolitan areas.
- Estimates ranged widely from 3% to 62% reduction in alcohol-related RTM.
- No studies have been conducted in low-income or middle-income countries where Uber have recently become available but have much higher burdens of RTM.

What this study adds

- This study is the first to investigate the effect of Uber on road traffic mortality in a setting (South Africa) where the potential of RTM reductions is high, by the nature of prevailing mortality burden and transport behaviours.
- We found some evidence for a very small reduction in road traffic-related deaths in provinces where Uber became available versus those that did not, particularly among young adult males. However, these findings may be explained by uncaptured seasonal variations.
- Replications in this and other low-income and middle-income settings are critically needed: in South Africa, RTM represents the fourth largest contributor to lost life years, and costs associated with road traffic injury account for about 3% of gross domestic product.

Acknowledgements The authors wish to than Sean Crotty, Assistant Professor of Geography, Texas Christian University for producing the provincial map, and anonymous reviewers whose comments greatly improved the quality of the work. FM would like to thank Vivian Ho for comments on a earlier version of the manuscript.

Contributors All authors contributed to the design and hypotheses of the study. JYH and FM acquired, analysed and interpreted data. JYH wrote the main manuscript. All authors contributed to substantive revisions and final approval of the manuscript.

Funding JYH and MD received salary support from the Canadian Institutes of Health Research Operating Grant #115214, "Ethics, Social Determinants of Health, and Health Equity: Integrating Theory and Practice," though no direct funding was received or set aside for the writing of this paper and funders had no input in the conduct of this study.

Competing interests None declared.

Patient consent Not required.

Provenance and peer review Not commissioned; externally peer reviewed.

Data sharing statement Data were obtained from DataFirst, a service of the University of Cape Town. Details on access can be found at their website: https://www.datafirst.uct.ac.za/

REFERENCES

1 World Health Organization. *Global status report on road safety 2015*. Geneva: World Health Organization, 2015.
2. World Health Organization. In: Peden M, Scurfield R, Sleet D, *World report on road traffic injury prevention*. Geneva: World Health Organization, 2004.
3 Wesson HK, Boikhutso N, Hyder AA, *et al*. Informing road traffic intervention choices in south africa: the role of economic evaluations. *Glob Health Action* 2016;9:30728.
4 *Uber.com*. Uber cities across the globe. 2017 https://www.uber.com/cities (cited 5 Dec 2017).
5 Uber. More options. shifting mindsets. driving better choices. 2015 http://blog.uber.com/wp-content/uploads/2015/01/UberMADD-Report.pdf (Cited 5 Dec 2017).
6 Greenwood BN, Wattal S. Temple University. Show me the way to go home: an empirical investigation of ride-sharing and alcohol related motor vehicle fatalities. *MIS Quarterly* 2017;41:163–87.
7 Peck JL. New York City drunk driving after Uber=. CUNY Academic Works: Economics Working Papers. 2017 https://ideas.repec.org/p/cgc/wpaper/013.html (Cited 5 Dec 2017).
8 Brazil N, Kirk DS. Uber and metropolitan traffic fatalities in the United States. *Am J Epidemiol* 2016;184:192–8.
9 Morrison CN, Jacoby SF, Dong B, *et al*. Ridesharing and motor vehicle crashes in 4 us cities: an interrupted time-series analysis. *Am J Epidemiol* 2018;187:224–32.
10 van Schoor O, van Niekerk JL, Grobbelaar B. Mechanical failures as a contributing cause to motor vehicle accidents South Africa. *Accid Anal Prev* 2001;33:713–21.
11 Seedat M, Van Niekerk A, Jewkes R, *et al*. Violence and injuries in South Africa: prioritising an agenda for prevention. *Lancet* 2009;374:1011–22.
12 Norman R, Matzopoulos R, Groenewald P, *et al*. The high burden of injuries in South Africa. *Bull World Health Organ* 2007;85:695–702.
13 MacDonald IL, Lerer LB. A time-series analysis of trends in firearm-related homicide and suicide. *Int J Epidemiol* 1994;23:66–72.
14 McLeod D. Uber users log over 2 million trips in 2015's first half Moneyweb.co.za. https://www.moneyweb.co.za/news/tech/sa-uber-users-log-over-2-million-trips-in-2015s-first-half/ (Cited 18 Feb 2017).
15 Uber. Uber Newsroom Uber.com. https://www.uber.com/en-ZA/newsroom/uber-has-launched-in-johannesburg/ https://www.uber.com/en-ZA/newsroom/connecting-cape-town-uber-has-officially-launched/ https://www.uber.com/en-ZA/newsroom/uber-durban-has-officially-launched/ (Cited 5 December 2017).
16 Statistics South Africa. Statistics by Place. Pretoria: statistics South Africa. https://www.statssa.gov.za/publications/P03014/P030142011.pdf (Cited 5 Dec 2017).
17 Erasmus L. The apps changing South Africa's transport services. *BBC.com. [* https://www.bbc.com/news/business-24656027 (Cited 5 Dec 2017).
18 University of Cape Town. Data First. Data portal. https://www.datafirst.uct.ac.za/dataportal/index.php/catalog/central/about
19 Africa SS. *Mortality and causes of death in South Africa, 2014: findings from death notification. statistical release P0309.3*. Pretoria: Statistics South Africa, 2015.
20 World Health Organization. *International statistical classification of diseases and related health problems 10th revision (ICD-10) - WHO Version for 2016: Chapter XX - External causes of morbidity and mortality [Internet*: World Health Organization, 2016.
21 Deaton A. Panel data from time series of cross-sections. *J Econom* 1985;30:109–26.
22 Agüero JM, Majid MF. War and the stock of human capital. university of connecticut, department of economics working papers. 2016 https://ideas.repec.org/p/uct/uconnp/2016-24.html (Cited 5 Dec 2017).
23 Verbeek M. Pseudo-Panels and Repeated Cross-Sections. *The econometrics of panel data*. Berlin: Springer-Verlag Berlin Heidelberg, 2008.
24 Guillerm M. Pseudo–panel methods and an example of application to Household Wealth data. *Economie et Statistique* 2017:109–30.
25 Imbens GW, Wooldridge JM. Recent developments in the econometrics of program evaluation. *J Econ Lit* 2009;47:5–86.
26 Angrist JD, Pischke JS. *Mostly harmless econometrics: an empiricist's companion*: Princeton university press, 2008.
27 Sukhai A, Jones AP, Love BS, *et al*. Temporal variations in road traffic fatalities in South Africa. *Accid Anal Prev* 2011;43:421–8.
28 Bernal JL, Cummins S, Gasparrini A. Interrupted time series regression for the evaluation of public health interventions: a tutorial. *Int J Epidemiol* 2017;46:348–55.
29 Flanders WD, Klein M, Darrow LA, *et al*. A method for detection of residual confounding in time-series and other observational studies. *Epidemiology* 2011;22:59–67.
30 Dusetzina SB, Brookhart MA, Maciejewski ML. Control outcomes and exposures for improving internal validity of nonrandomized studies. *Health Serv Res* 2015;50:1432–51.
31 Road Traffic Management Corporation. Road Traffic Management Corporation. Road traffic calendar report, 2014. 2017 http://www.rtmc.co.za/images/docs/Calender/Calendar2014/Calendar2014.pdf (Cited 9 Nov 2017).