LAST CALL: THE EFFECT OF CAR SHARING APPS ON TRAFFIC ACCIDENTS
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Dissertação apresentada à Escola de Economia de São Paulo da Fundação Getulio Vargas como requisito para obtenção do título de Mestre em Economia de Empresas

Campo de Conhecimento: Microeconomia Aplicada

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Abstract

By taking advantage of the timing in the expansion of car sharing platforms in Brazil, we investigated their impact on road safety, measured as user-reported car accidents on the popular navigation platform Waze. We found no conclusive effect on the overall number of accidents. However, we found substantial heterogeneity depending on the weekday and a significant reduction on the number of accident on Sundays. Dividing the days into periods showed further heterogeneity, with a significant reduction on the number of accidents both on Saturday and Sunday nights. Contrary to our expectations we found a stronger effect on the short run (first two months) rather than in the long run (more then two months). This findings suggest that car sharing apps have a relevant impact in reducing the number of car accidents, specially at weekend nights, which corroborates to the popular hypotheses that such platforms help curb drunk driving. The results also indicate that there are more channels in play leading to the reduction of car accidents other than restricting drunk driving.

Key-words: Road safety, car sharing apps, Uber, impact evaluation.
Resumo

Ao tirarmos proveito das diferentes datas de lançamento de plataformas de compartilhamento de veículos particulares, pudemos investigar o efeitos destas na segurança do trânsito, mensurada pelo número de acidentes reportados na popular plataforma de navegação Waze. Não encontramos efeito conclusivo no número total de acidentes. No entanto, encontramos um heterogeneidade substancial em função do dia da semana analisado, com uma redução significativa no número de acidentes aos domingos. Dividindo os dias em períodos, encontramos efeitos ainda mais heterogêneos, com reduções significativas tanto aos sábados como aos domingos pela noite. Ao contrário de nossa expectativa, encontramos efeitos mais fortes no curto prazo (primeiros dois meses) do que no longo prazo (mais de dois meses). Estes resultados sugerem que aplicativos de compartilhamento de veículos particulares têm um impacto relevante na redução do número de acidentes de trânsito, especificamente aos finais de semana a noite, o que corrobora com a popular hipótese de que tais plataformas reduzem o número de motoristas alcoolizados. Os resultados também indicam que há outros canais (além da diminuição do número de motoristas alcoolizados) pelos quais os aplicativos levam a uma redução no número de acidentes.

Palavras-chaves: Segurança no trânsito, aplicativos de compartilhamento de veículos, Uber, avaliação de impacto
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1 Introduction

More than 1.2 million people die every year and 50 million more are injured in road traffic crashes around the world according to the World Health Organization (2017). They are the ninth leading cause of death across all age groups and the main one among young people aged between 15 and 29 years. The WHO estimates that low and middle income countries loose approximately 3% of GDP as a result of road traffic death and injuries. This is a global challenge and the United Nations has set a goal to reduce global road traffic deaths and injuries by 50% by 2020. They established a set of best practice policies to help countries work toward this goal, many of which involve stricter enforcement and implementation of regulations that limit behaviors that are considered hazardous.

The economic theory of crime, however (first introduced by Garry Becker in he’s seminal paper Becker (1968)), tells us that the decision to commit a crime (e.g. speeding or drink-driving) is the result of a rational choice and therefore, increasing the incentives for desired behavior can be as effective as increasing the punishment for undesired ones. Car sharing platforms may provide such initiatives for desired behavior and this hypothesis has been stated repeatedly. The reasoning is simple: car sharing apps allow for a greater supply of cars for individual transportation (adding to the limited number of taxi cabs), which make it easier for people that go out to drink to hail a car and get a safe ride home.

This argument is present in many newspapers and magazine articles such as Badger (2014) and Mathis (2014) and is used by companies to promote their image. Uber released a report with Mothers Against Drunk Driving (MADD) in 2015 claiming that the company was responsible for a 10% decrease in DUI arrests in Seattle (UBER... , 2015). Such evidences, however, are far from careful evaluations on the subject and more often than not mix correlation with causation. Nevertheless, they show us the public’s interest on the question and highlights the role it has on the public’s perception as well as the political leverage for a service that, to this day, remains controversial in some countries and was banned from others (such as Norway). Those decisions, however, are being made by policymakers without the full understanding of the effects that such platforms can have on road safety. The literature on the subject is still incipient and focuses, to the extent of our knowledge, only on alcohol-related deaths, accidents and arrests, leaving aside any potential effects on more superficial car crashes.

The goal of this paper is to expand on the existing literature of the social impact of car sharing apps. We used traffic data from the navigation app Waze in Brazil and a natural experiment - the different timing in the availability of car sharing apps on different municipalities - to evaluate the impact of car sharing apps on the total number
of car accidents (as reported by Waze users). Using a difference-in-difference methodology, we found no statistical significant effect of car sharing platforms on the overall number of accidents. Thanks to the detailed level of data from our dataset, we were able to evaluate heterogeneous effects on different weekdays and periods within the same day. We found a significant reduction on the number of accidents on Sundays and, when dividing weekends into day periods, we found significant reductions both on Saturday and Sunday nights. We argue that those results, differently from most of the literature, are free from the confounding effect of car haling smartphone technology, due to the existence of such platforms for traditional taxi cabs in Brazil when the first car sharing app started to operate. We believe that this is the relevant policy measure, since we failed to find any article proposing the ban of platforms for all kinds of services, which is being proposed and enacted in many countries for car sharing apps.

The remainder of the paper is as follows. Section 2 outlines the related literature about the impact of car sharing apps on transportation variables as well as other studies that help us understand the mechanisms in which driver's decisions are made. Section 3 and 4 present the dataset and empirical strategy, respectively. Section 5 comments on the results and section 6 concludes.
2 Related literature

The effects of the so called gig economy are still being studied, but the literature on the topic is growing fast (as is the technology and the number of companies that fit in the category). According to Parker e Alstyne (2005), by increasing the amount of publicly available knowledge regarding prices and products, platforms are able to expedite the exchange of goods and services while creating a surplus of welfare for both the buyer and the seller. Even narrowing down our analysis to car sharing apps, the range of potential effects is vast. Hall et al. (2017) evaluates the impact of such platforms on public transport while Park, Kim e Lee (2016) examines their effect on sexual assault incidents, only to name two.

There is a growing literature on the effects of car sharing apps on road safety but, likely due the lack of data availability, most of the research focuses on traffic deaths, drunk driving arrests or other criminal activities, for which data can be found in official government databases. Peck (2017) analyses the impact of Uber’s car service in New York City’s alcohol related collisions using the New York State Department of Motor Vehicle’s database. Using difference-in-difference estimation she found a 25–35% decrease in in alcohol related collision rate for boroughs that experienced significant Uber service coverage (Manhattan, the Bronx, Brooklyn, and Queens). When allowing for heterogeneous effects between boroughs, she found much stronger effects for Manhattan and smaller ones for Queens, which was interpreted as Uber serving as an addition mode to access the established urban center, rather then filling in areas with less access to public-transit. Taking time heterogeneity into account showed that the effect was smaller in the first year and rose in the second and third, which might be explained by slow salience immediately after the launch, low consumer trust for non-medallion taxi service or the dynamics of Uber’s fleet size through driver acquisition. Greenwood e Wattal (2015) also found significant and negative effects of Uber’s entry in the number of alcohol related motor vehicle homicide in the state of California (USA) using data from the California Highway Patrol. Their results were smaller in comparison to Peck’s (3.6-5.6%), but nonetheless significant when looking at the lower priced Uber option and robust to different specifications. They reinforce the importance of price in generating such effects, providing evidence that there is no effect for the higher priced Uber option or when surge pricing is likely in effect (during weekends and drinking holidays).

Dills e Mulholland (2016) used U.S. county level data from the National Highway Traffic Safety Administration aggregated monthly and a difference-in-difference specification to evaluate the impact of Uber on four different measures of fatal traffic accidents per capita (total, alcohol-related, night time and number of fatalities) as well as other cri-
mes. They found negative effect in all the measures for the unweighted specification, but significant only in fatal crashes per capita, with a reduction of 0.28, a 7 percent decline at the mean. Allowing for the effect to differ as time passes, the results remain negative for all measures, but significant for both fatal crashes (-0.5% for each additional month) and night-time fatal crashes (-0.9% for each additional month). They conclude that the entry of ride-sharing apps tends to decrease fatal vehicular crashes. They also observed declines in arrests for assault and DUIs, which are somewhat offset by increases in arrests for motor vehicle thefts. Brazil e Kirk (2016) uses monthly county-level data from the same source as Dills e Mulholland (2016), and use negative binomial and Poisson regression models, limiting the dataset to the 100 most populated metropolitan areas in the United States. They found that Uber "had no association with the number of subsequent traffic fatalities, whether measured in aggregate or specific to drunk driving-related fatalities or fatalities that occurred on weekends and holidays".

As previously mentioned, the literature on the topic focuses on alcohol related deaths, accidents and arrests. To the extent of our knowledge, there is no literature on the effect of car sharing apps on a more broad measures of road safety, such as car crashes. We believe the reason for that is lack of data, since it is very hard to find trustworthy sources for superficial car crashes. Ex-ante the result of analyzing this type of data is ambiguous. On the one hand, most of the mentioned literature shows a reduction in the number of alcohol related accidents, which might lead the overall effect to be negative. On the other hand, there are strong arguments to support the hypothesis that the overall effect could be positive, even more if we analyze superficial car crashes. Both in Brazil and in the US, Uber drivers were arrested for drunk driving and there are reports of accidents caused by drivers that were too tired to drive after spending all night working. Also, car sharing apps can take the drivers attention of the wheel, since they have only a few seconds after receiving a passenger request to: see where it is coming from, make the decision if he wants to drive to that part of town and finally accept or refuse the drive. Failing to do so fairly quickly will lead to immediate loss (since the request will be passed to another driver) and, if recurrent, could lead to a penalty from the app company. As pointed out by Richtel (2014), "You need not be a neuroscientist or safety advocate to see the potential for danger when the phone, a potential source of driver distraction, becomes an essential means of transaction".

Other variables that influence the number of accidents are even more ambiguous when thinking about the direction of a possible effect. One might argue that professional drivers will be more careful than a self driving person because if there is an accident, the potential income cost associated with the time spent fixing the car will be much higher. He might also be more careful because the driver will not have an incentive to speed and risk getting a ticket if the passenger is late or even because (differently from cab drives), car sharing drivers are more likely to own the vehicles they work with and therefore are more
concerned about the residual value of the car, making them more aware on the safety and appearance of the vehicle. As stated by Dills e Mulholland (2016), "Being an owner-driver reduces many of the principal-agent problems in the vehicle-for-hire market, leading to safer drivers and vehicles". On the other hand, the presence of such drivers could increase the likelihood of an accident, since the more miles they drive, the more money they will make, which is an incentive for car sharing app drivers not to speed up occasionally (as individuals drivers might occasionally do), but all the time. Studying income differences by gender using Uber’s database, Cook et al. (2018) found that men earn roughly 7% more per hour worked than women, which is partially explained by male preferences for driving faster. They estimate that a 1% increase in speed increases earnings by 27% or even 46% when controlling for geohash and hour of week (thus removing the fact that congestion both lowers speeds and increases earnings due to Uber’s surge price) which shows that drives have a big incentive to speed up. Another ambiguous variable is traffic. The more miles driven per capita in a municipality on a given day, the more likely it is for an accident to occur. If the main clients of car sharing apps are drivers that used to drive alone and demand private rides, the number of miles driven per capita will remain approximately the same (ignoring moments when drivers are looking for clients). On the other hand, if they share the ride with more passengers, the number of miles driven per capita is expected to drop and so is the number of accidents. Finally, if passengers migrated from public transportation, both the number of miles driven per capita and the number of accidents are expected to rise. This means that the effect of car sharing apps might differ depending on the availability (and use) of public transportation, income level, and other variables that determine the use of such apps.

Economically speaking, the entry of a car sharing app in a municipality is a positive supply shock to the transportation market. Therefore, the basic expected effect would be a lowering of the price and an increase in the quantity. There is no inherent reason why a similar shock would generate different effects and, the more similar the shock, (i.e. the more it targets the same customer base) the more it is expected that the effects will be similar, not only to price and quantity, but also to changes in incentives and behaviors that might spillover to other markets. When focusing on the effect of car sharing apps on drunk driving, any supply shock in the transportation market that affects potential drinkers is expected to have similar consequences. This is precisely the case of the train schedule changes in Washington DC, studied by Jackson e Owens (2011). In their paper, they present “a simple model that links alcohol consumption and intoxicated driving to public transportation, provide some intuition for the possible moral hazard created by Metro’s expanded late night service” that translates directly to the goal of this paper. The main idea of the model is that the extension of Metro’s late night service leads to a drop in transportation costs at night, which leads to a higher demand for nights out and drinking. Since both nights out and drinking are typically social activities, they are
strategic complements between individuals and, therefore, an individual increase in the
demand for those goods will induce other individuals to raise their demands for those
goods. This means that in equilibrium there is an increase in aggregate nights out and an
increase in aggregate drinking for both drivers and non drivers. This leads to the main
prediction regarding the connection between this two papers: that "In equilibrium, the
effect of aggregate intoxicated driving is ambiguous. Because the number of individuals
who go out drinking will increase, if the fraction of drinkers who drive home is not large
enough, there may be a net increase in total intoxicated driving. Alternatively, as more
bar patrons use the metro, the amount of alcohol consumed by any given bar patron’s
peers, including drivers, will rise”.

Using a triple difference estimation strategy and data from Washington DC’s Me-
tropolitan Police Department, they find that there was little effect of expanding public
transit services on DUI arrests, alcohol related fatal traffic accidents and alcohol related
arrests. However, according to the authors, those effects mask considerable heterogeneity
across geographic areas. They found that areas where bars are within walking distance
to transit stations experience increases in alcohol related arrests and decrease in DUI
arrests. All this evidence supports that there are many possible channels through which
car sharing apps can make a relevant impact on society and that, even narrowing down
our interest to car accidents, there are still many complex processes to consider that in-
fluence individual decision making, being the decision to drink and drive, to drive faster
and many other decisions, as well as other variables that can influence the number of car
crashes and are specific to time, geography and social-economic characteristics.
3 Data

3.1 Waze Live Map

Our main database was collected by a web crawler of the Waze Live Map. Waze is a GPS navigation software that works on smartphones and tablets with GPS support that provides turn-by-turn navigation information. As an additional feature, the Waze app allows its users to set alerts in order to help the community while getting points to upgrade their avatar on the platform. The Live Map is the web version of the app. Using this platform, a user can see the fastest route to a destination at different times as well as the alerts made by other users at the present time just by hovering over an area with the mouse. The web crawler for this website was developed to save at an hourly rate the precise location (five digit latitude and longitude) of every alert in Brazil active during the moment the crawler did the query. This data was collected continuously from December 2015 onward. For the purpose of this paper, we gathered the data until November 2017.

After the data was collected we used Brazilian Geography and Statistics Bureau’s (IBGE) database to convert the location of the alert into a code for the municipality it belonged to. We then proceeded to aggregate the data first by day, municipality and type of alert and later adding the hour of the reported alert. We chose to aggregate the data in the municipality level for two reasons. First, this is the level for which car sharing apps expand their operations and, therefore, the level at which the treatment was conducted. Secondly, since March 2018, Brazilian municipalities are the public level of administration that has the legal competency to sanction and regulate those services and therefore, this is the level at which we should give some insight on the costs and benefits of such platforms, in order to aid the decision process to ban or allow such services. We do not defend that this is the only relevant variable, but we do believe that such an information should be taken into account in the regulation process.

Unfortunately the web crawler had some technical operational issues, which means that we don’t have a complete dataset. This happened on two different scales. On the first one, some hours were missing when aggregating the data. We ignored this and aggregated it without taking any measures to counterfeit this problem as we understand that this wouldn’t bias our results. On the the second scale, hole days were left out of the dataset. This led to a smaller sample size both in the temporal and spacial dimensions, since some smaller municipalities - which don’t have alerts every day - had a smaller window of opportunity to show up.

We argue that none of those two scales should cause any concern. A missing hour
from the crawler would create an homogeneous effect across municipalities and have a similar effect on both the treatment and control groups. The same is true for a day missing. If anything, this works against the detection of a statistically significant effect since missing data might hide a widely different number of accidents in the two groups and, therefore, make it harder to detect a significant treatment effect. If any, the most likely effect of such issues is a desirable selection bias toward bigger municipalities. Car sharing companies tend to launch their products first in bigger cities (that have both higher driver supply and passenger demand) and therefore municipalities that failed to appear in our dataset are likely to be poor counterfactuals for our treatment group, since they are either too small or have too few car accidents and so are unlikely to share the same characteristics as the treatment group.

The final dataset is composed of 58,027,789 alerts of which 725,798 are accident alerts. They were issued over 701 days between 12/12/2015 and 19/11/2017 and 3,020 municipalities. This database has only strictly positive data, since a pair day-municipality only shows up when a Waze user reports an alert. This is a problem since, at first, we were not able to distinguish an hour when there were no car accidents in a municipality from an hour when the crawler crashed. Since any issues to the crawler would cause it to crash for the whole country, we could identify the moments where it was functioning properly by looking at the days and hours where we had an alerts of some kind (not restricted to accidents). That way we were able to balance our panel adding zero car accidents to those days/hour-municipalities that didn’t show up in the database but that had some alerts at the same time, somewhere in the country. We then summed the number of reported accidents by month, reducing the number of parameters to be estimated and the number of data points restricted at the zero lower bound. Some municipalities only appeared in the dataset due to other types of alerts and were a sequence of zeros in the variable of interest (car accidents). Since they added a lot of noise without adding much value, as they were unlikely to be good counterfactuals to our treatment municipalities, they were dropped from the dataset.

3.2 App entry date

Our treatment group is defined as the municipalities that have had at least a full month of one or more operational car sharing apps available to use. Therefore, we needed the date of entry of all the competing companies on each municipality in order to establish when each municipality leaves the control group to join the treatment group. The main companies in Brazil are Uber, 99 and Cabify. We contacted all three companies and both 99 and Cabify kindly provided us with the entry date for each municipality. Uber replied only after the analysis had already been conducted. Fortunately, they have a blog (UBER..., ) in which they post all launches in new cities, where we were able see
when and which municipalities had the availability of their service.

![Car sharing apps growth](image)

**Figura 1 – Car sharing apps growth**

Source: Own elaboration

Other companies such as Easy (formerly Easy Taxi) entered the car sharing market later on and only in cities where other companies were already active and thus didn’t change any municipalities from the control to the treatment group. Easy was sold to Cabify on June 2017 and its car sharing product - Easy GO - was discontinued shortly after. The company is still operating but focusing only on taxis. Other worldwide famous companies, such as Lyft, don’t operate in the Brazilian market. With the data provided by the companies and their online posts we were able to build a dataset with the date that the first car sharing app was available at each municipality and the ones that, to this day, still don’t have such services available. Figure 1 shows the cumulative number of cities that have at least one car sharing app in Brazil.

### Traffic legislation

The 1988 Brazilian Constitution redesigned traffic attributions between the federal, state and municipal authorities. According to the national traffic department (DE-NATRAN), municipalities had their attributions largely widened. The Brazilian Traffic Code (CTB) specifies 21 activities for which municipalities are responsible. This activities can be classified into 4 responsibilities: traffic engineering, traffic supervision, traffic education, and data collection and statistical analysis as well as establish a board to analyze traffic penalties appeals.
In order to exert this attributions, municipalities must integrate the National Traffic System (SNT). To do so they must pass laws creating the appropriate bureaus to manage the new responsibilities. Only then they can handle the income generated by traffic tickets (obeying the allocation restrictions determined by law). Most municipalities still depend on the states for traffic engineering and enforce traffic laws by partnerships with military police, which may draw resources from street patrolling. To this date, only 1581 out of the 5570 municipalities have joined the SNT.
When regarding traffic safety, the municipalities that integrate the SNT are unlikely to be comparable to the ones that do not integrate the system. Firstly, they have shown political will and support to pass all the required legislation and secondly, they have bureaus that manage both the traffic accounting for their specific needs and the funds from traffic tickets to improve road safety, while the other municipalities have to compete for resources with the other municipalities in the same state. According to DENATRAN, the CTB increased municipality responsibility because the citizen lives, works and moves in the municipalities and therefore they should do a better job managing their on particularities in road safety. It is therefore reasonable to expect that the municipalities that comply to this legislation perform differently. Due to these distinctions, we restricted our sample to municipalities that integrate the SNT, improving the likelihood that municipalities that do not have car sharing apps are a good control group for those that do. Figure 3 shows all municipalities that integrate the SNT. Those in Blue have at least one car sharing app operational in November 2017 and those in green don’t.

3.4 Database reliability

As previously mentioned, most of the literature on road safety uses official government data sets, since this is the data source that is usually available to social scientists. Our dataset differs from this pattern. Although the use of tools such as web crawlers brings new and exciting possibilities to researches, reliability can be an issue. In order to evaluate our dataset we compare it with a well known and reliable official database. Infosiga is a database managed by the São Paulo state government and gathers information from several sources such as the civil police, military police and federal highway patrol.

![Proportion of accidents by day](image)

**Figura 4 – Proportion of accidents by day**

*Source: Own elaboration, Infosiga, Waze*

Although not directly comparable, this official dataset allows us to see if our da-
Dataset is markedly different from the usual sources, which might raise some confidentiality issues. We say the data is not directly comparable because it has information only on the state of São Paulo and of traffic deaths, while our dataset is based on user-reported car accidents, which can include minor accidents without any injuries. We then use only a subset of the Waze dataset that corresponds to municipalities in São Paulo state, are part of the SNT and appear in both datasets (Waze and Infosiga) in order to get a ballpark comparison. It is worth noting that São Paulo is the most populous state with over 21% of the country’s population. On the one hand this means that we don’t lose as much data as we would if we were using data from a smaller state. On the other hand, São Paulo is a very particular state with both the highest GDP and vehicle fleet in the country. We analyze only percentage ratios as the number of deaths is going to be a fraction of the true number of car crashes, regardless of the reliability of the datasets.

Figure 4 shows on the left the percentage of deaths by transport accident by day of the week (according to infosiga) and on the right the number of car accidents reported on Waze by day of the week. The charts are practically identical which shows it is unlikely that the Waze dataset is biased in this dimension of analysis. Figure 5 shows on the left the percentage of deaths according to Infosiga by day of the week and period of the day and on the right the number of reported accidents on Waze divided the same way. By comparing the bar charts, the first thing that we notice is that the proportion of deaths in the Infosiga database in the small hours period (midnight to 6 am) is much bigger than the same proportion of of accidents in the Waze database. This result is expected once it is unlikely that a car driver will report an accident at such hours either because he is eager to get to his destination or because there is no traffic and it is not practical to stop and use your phone to do so. The corresponding proportion of accidents seems to be taken almost entirely by the morning period (6am to noon) and some by the afternoon period (noon to 6 pm). It is interesting to note that the proportion of accidents and deaths at
night (6 pm to midnight) is very similar. This indicates that we have to be careful with the results that we get in this paper (as we always should) since the dataset we use have some biases, but such biases tend to be larger for the small hour period and smaller for the night period.
4 Empirical Strategy

Taking advantage of geographic and temporal heterogeneity in the expansion of car sharing apps, we conducted a difference-in-difference (DID) estimation strategy to evaluate the impact that car sharing apps have on the number of traffic accidents. It is worth noting that, differently from what happened in other countries, taxi platforms were operational in Brazil before the launch of the first car sharing app. Manechini (2013) talks about the competition between four different taxi cab apps a year before Uber started its operation in the country. This shows that, differently from other papers on the subject, there is little confounding factor on our estimates between the platform effect and the car sharing effect, an issue raised by Greenwood e Wattal (2015). This is the first paper that we know of to be able to evaluate car sharing apps without this important confounding effect, which is the relevant policy question once the regulation of car sharing apps are a largely debated topic, but we haven’t seen anyone raising the possibility of banning car haling apps altogether (including traditional taxi cabs) as we do with car sharing ones (as they have been banned in some counties such as Norway).

The key identifying assumption in DID regressions is that trends in the number of car accident reports would be the same in both treated and control groups in the absence of treatment (in this case the availability of car sharing apps). As pointed out by Angrist e Pischke (2008), "if there is a common trend in logs, there will not be one in levels and vice versa". It is important therefore, to run different specifications in order to evaluate if the results obtained are caused by the treatment or by a violation of this fundamental assumption. In our DID estimation, we use three different specifications: log-linear, linear probability and standardized.

Instead of running a traditional level model, we chose to standardize the data by each municipality across the hole time period. We chose to do so in order to take into account that different municipalities could have a heterogeneous treatment effect due to some factors (such as population size), which would be taken into account on average by the fixed effect parameter, but not in the variance, which could affect our inference. We decided not to standardize the log-linear model since a transformation had already been made to the series (adding one to the independent variable and taking the natural logarithm) and the effects would be smaller since taking the log already reduces the series variance. We also estimated a linear probability model where outcome is equal to one if there were any reported accidents in a given day and municipality. We did so following Jackson e Owens (2011), that use both a linear probability model and a log-linear model, claiming that the former is functionally equivalent to negative binomial count model and more appropriate when the values of the outcome variable are high while the
latter is most appropriate when the values of the outcome variable are small (as is the case for the number of reported car accidents for many municipalities).

The data was first aggregated daily so we could first estimate the average overall effect of car sharing apps on the number of car crashes in Brazilian municipalities reported on the Waze platform. To do so, we used the traditional DID estimation equation, represented by equation 4.1, where $y_{i,t}$ represents the number of car crashes on municipality $i$ on day $t$, $\alpha$ is the intercept, $\lambda_t$ is a fixed effect for day $t$, $\theta_i$ is the municipality fixed effect, $D_{i,t}$ is the treatment dummy that takes the value of one if municipality $i$ has a car sharing app at date $t$ and zero otherwise and $\epsilon_{i,t}$ is the error term. Bertrand, Duflo e Mullainathan (2004) shows that conventional DID standard errors are not consistent due to serial correlation and suggest several ways that can outperform this method. We follow their recommendation for a large number of groups (the largest they use is 50 and we have 87 in our dataset) and use arbitrary variance-covariance matrix, which is easily implemented by clustering on municipality.

$$y_{i,t} = \alpha + \lambda_t + \theta_i + \rho D_{i,t} + \epsilon_{i,t} \quad (4.1)$$

Despite the previously mentioned multitude of possible channels from which car sharing apps could impact the number of car accidents, most of the literature and public debates are around drunk driving. If this channel is stronger than others, we would expect a stronger effect on weekends. To test this we separated the data by day of the week and ran seven regressions, one for each day. Furthermore, we ran an extra set of regressions for the weekends in order to explore this important claim. We split Saturday and Sunday into four periods of six hours each. The periods are defined as follows: Small Hours from midnight (excluded) to 6 a.m. (included), Morning from 6 a.m. (excluded) to noon (included), Afternoon from noon (excluded) to 6 p.m. (included) and Night from 6 p.m. (excluded) to midnight (included). If drinking is such an important part of the previously estimated effect, one would expect a higher effect on Saturday nights or Sunday small hours.

$$y_{i,t} = \alpha + \lambda_t + \theta_i + \gamma Pre_{i,t} + \delta ST_{i,t} + \beta LR_{i,t} + \epsilon_{i,t} \quad (4.2)$$

Car sharing platforms might take some time to be fully implemented in a city. The more drivers are added to the platform, the more reliable the service is and the more customers will be willing to use them. Also, users themselves might take some time to know about the availability of the service in their city and get used to the platform (which might be a barrier to initial use). In order to allow for more flexibility, we run another set of regressions represented by equation 4.2, where the treatment dummy $D_{i,t}$ was replaced by three dummies: $Pre_{i,t}$, which equals one if municipality $i$ will have a car sharing app in less then 60 days from $t$ and zero otherwise; $ST_{i,t}$ equals one if municipality $i$ has a
car sharing platform and $t$ is within 60 days of the launch of a car sharing app and zero otherwise; $LR_{i,t}$ equals one if municipality $i$ has had a car sharing app for more then 60 days at day $t$.

Since this equation takes into account how long the city has had a car sharing app, we dropped any datapoints of municipalities that had less then a full month of an operating car sharing app (which in the previous regressions were treated as part of the control group given our definition of treatment stated at the beginning of section 3.2). In this setting, $\gamma$ is a measure of parallel trends between municipalities in the two months preceding the launch of a car sharing platform, $\delta$ is a measure of the short term effect of such platforms and $\beta$ is a measure of the long run effect.
5 Results

Table 1 presents the average effect of car sharing apps on the number of car crashes (as reported by Waze users) estimated as in equation 4.1. Each row uses a different specification. The log-linear model coefficient is negative but not significant while the standardized model coefficient is positive but not significant. The linear probability model, however, is negative and significant at 1%. The results are therefore ambiguous. The fact that the result of each specification is so different from one another raises doubts on any particular possible interpretation of an average effect.

**Tabla 1 – Average effect**

<table>
<thead>
<tr>
<th>Specification</th>
<th>Coefficient</th>
<th>Standard Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log linear model</td>
<td>-0.1489</td>
<td>(0.091)</td>
</tr>
<tr>
<td>Linear probability model</td>
<td>-0.0443***</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Standardized model</td>
<td>0.0244</td>
<td>(0.107)</td>
</tr>
</tbody>
</table>

N 27,650
Number of treated Municipalities 87
Number of Municipalities 1,106

Source: own elaboration
Clustered on municipality standard errors in parenthesis.
*p<0.1, **p<0.05, ***p<0.01

In order to gain further insight, we use the same equation, but restrict the data to one day of the week at a time so we can evaluate heterogeneous effects between them. Table 2 shows us the results. Overall we find the same ambiguous effect on weekdays. However on the weekend the coefficients are negative across all specifications, specially on Sunday, when all coefficients are statistically significant. The results for bot the log linear and the linear probability specification are all negative, with the exception of Thursday’s coefficient in the log linear specification, while the standardized specification is positive for weekdays, but negative on the weekend. The Linear probability model has significant coefficient for all days but Wednesday and Thursday while the other specifications have significant coefficients on Sunday only, which raises the concern of the linear probability specification over rejecting the null.

This result is very interesting once we observe a different pattern between weekdays and weekends. Since most of the literature studies the effect of car sharing apps on alcohol related accidents and finds a negative correlation, one possible hypotheses to interpret our findings is that alcohol consumption is higher on the weekend and therefore car sharing apps are able to significantly diminish the number of accidents on those days only. If
correct, this hypothesis narrows down to alcohol consumption as the main channel through which car sharing apps affect the number of car accidents. Still, if that is indeed the main channel, one would expect coefficients on Saturdays to be also significant and more negative than on Sundays, since it is reasonable to assume that alcohol consumption is even higher on those days.

A possible explanation for this puzzle, one that does not go against the alcohol hypothesis, is that heavy drinkers on Saturday (the ones more likely to cause an accident) drive their cars after midnight and therefore the accidents that were caused by drinking on Saturday are being prevented by the platforms from happening only on Sunday. To test this idea we split the duration of the days into four 6-hour-length periods and, taking advantage of the detail of our dataset, aggregated the number of reported car crashes by those periods. Table 3 shows the result for Saturday on Panel A and on Sunday on Panel B. Each column shows the coefficients for a time period. Small Hours aggregated accidents from midnight to 6 am, Morning aggregates accidents from 6 am to noon, Afternoon has the result for accidents between noon and 6 pm and Night for the period between 6 pm and midnight.

The effect of car sharing apps on Saturdays are negative across all specification and all time periods, with the exception of the log linear specification on both periods preceding noon. Only 3 coefficients are statistically significant at 10%. Afternoon in the linear probability specification and Night for both the linear probability and the standardized specification. Sunday’s results were against our expectations. All coefficients are negative but they are statistically significant at night only, which indicates that the statistically significant negative correlation between car accidents and car sharing app availability on Sundays is not due to the drinking spillover from Saturday, but from behaviors specific to Sunday. Even more surprising is that across all the specifications, the effect on Sunday night is significant at 1%, a very strong result.

This is not an intuitive result and unfortunately we are not able to pinpoint the exact cause of such a peculiar effect. Alcohol might still be an explanation if people turn out to drink and drive more on Sundays than on Saturdays or if potential drunk drivers are more likely to get a ride on Sundays then on Saturdays. Perhaps there is a non-alcohol related channel that builds on top of DUIs on Sunday or even overshadows them. In the previous sections we argued that there were many channels through which car sharing apps might have an impact on the number of car crashes, but none of them seems to properly explain such strong effect on Sunday night.

As previously mentioned, the origin of the dataset is both an asset and a liability. On the one hand it allows us to take a more holistic approach. On the other hand, the data is generated by Waze users and might not be as reliable as government official data. If there is a correlation between those reports and the treatment, the results would be
biased. Although we think it is unlikely for consistent report error to be correlated with both the location and timing of the car sharing apps’ operation expansion, there is at least one possibility for such an event. Car sharing drivers may disproportionately use Waze (since they drive across town with no particular training or knowledge of the city grid) and therefore disproportionately report car crashes. We argue firstly that such an effect would be small due to the widespread use of Waze among car drivers in Brazil and secondly that such an impact would work against the detection of significant negative effects like the one we found so far. Such a relationship would cause the estimator to have a positive bias since there would be a positive correlation between car crashes in our dataset and the number of car sharing platform drivers, not because of their impact, but because accidents are more accurately reported.

Finally, we looked even closer to Saturday and Sunday nights and estimated equation 4.2, allowing for a pre-treatment effect as well as a shot term effect (after less than two months of the launch of a car sharing app) and a long run effect (more than two months after the launch). It is worth noting that a statistically significant result in the pre–treatment coefficients would weaken our result since it indicates we do not have parallel trends between our treatment and control groups before treatment, a fundamental

\[\text{Tabela 2 – Effect by weekday}\]

<table>
<thead>
<tr>
<th></th>
<th>Monday</th>
<th>Tuesday</th>
<th>Wednesday</th>
<th>Thursday</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log linear</td>
<td>-0.0751</td>
<td>-0.0650</td>
<td>-0.0277</td>
<td>0.0196</td>
</tr>
<tr>
<td>model</td>
<td>(0.077)</td>
<td>(0.066)</td>
<td>(0.066)</td>
<td>(0.062)</td>
</tr>
<tr>
<td>Linear probability</td>
<td>-0.0453*</td>
<td>-0.0580**</td>
<td>-0.0237</td>
<td>-0.0200</td>
</tr>
<tr>
<td>model</td>
<td>(0.027)</td>
<td>(0.024)</td>
<td>(0.025)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Standardized</td>
<td>0.0392</td>
<td>0.0309</td>
<td>0.1108</td>
<td>0.0902</td>
</tr>
<tr>
<td>model</td>
<td>(0.093)</td>
<td>(0.082)</td>
<td>(0.080)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>N</td>
<td>21,975</td>
<td>21,525</td>
<td>21,125</td>
<td>21,950</td>
</tr>
<tr>
<td>Treated Municipalities</td>
<td>83</td>
<td>83</td>
<td>83</td>
<td>84</td>
</tr>
<tr>
<td>Total Municipalities</td>
<td>879</td>
<td>861</td>
<td>845</td>
<td>878</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Friday</th>
<th>Saturday</th>
<th>Sunday</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log linear</td>
<td>-0.0457</td>
<td>-0.0999</td>
<td>-0.1421*</td>
</tr>
<tr>
<td>model</td>
<td>(0.080)</td>
<td>(0.078)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Linear probability</td>
<td>-0.0574**</td>
<td>-0.0523**</td>
<td>-0.0674***</td>
</tr>
<tr>
<td>model</td>
<td>(0.027)</td>
<td>(0.026)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Standardized</td>
<td>0.0846</td>
<td>-0.0918</td>
<td>-0.2368**</td>
</tr>
<tr>
<td>model</td>
<td>(0.093)</td>
<td>(0.094)</td>
<td>(0.099)</td>
</tr>
<tr>
<td>N</td>
<td>22,225</td>
<td>22,275</td>
<td>22,825</td>
</tr>
<tr>
<td>Treated Municipalities</td>
<td>83</td>
<td>83</td>
<td>83</td>
</tr>
<tr>
<td>Total Municipalities</td>
<td>889</td>
<td>891</td>
<td>913</td>
</tr>
</tbody>
</table>

Source: own elaboration
Clustered on municipality standard errors in parenthesis
*p<0.1, **p<0.05, ***p<0.01
Tabela 3 – Saturday and Sunday effect by day period

<table>
<thead>
<tr>
<th></th>
<th>Small Hours</th>
<th>Morning</th>
<th>Afternoon</th>
<th>Night</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Saturday</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log linear model</td>
<td>0.0113</td>
<td>0.0103</td>
<td>-0.0676</td>
<td>-0.0802</td>
</tr>
<tr>
<td>(0.033)</td>
<td>(0.060)</td>
<td>(0.062)</td>
<td>(0.050)</td>
<td></td>
</tr>
<tr>
<td>Linear probability model</td>
<td>-0.0171</td>
<td>-0.0315</td>
<td>-0.0528*</td>
<td>-0.0546*</td>
</tr>
<tr>
<td>(0.028)</td>
<td>(0.029)</td>
<td>(0.029)</td>
<td>(0.031)</td>
<td></td>
</tr>
<tr>
<td>Standardized model</td>
<td>-0.0705</td>
<td>0.0724</td>
<td>-0.1155</td>
<td>-0.1267*</td>
</tr>
<tr>
<td>(0.067)</td>
<td>(0.086)</td>
<td>(0.081)</td>
<td>(0.074)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>11,900</td>
<td>17,725</td>
<td>18,425</td>
<td>18,875</td>
</tr>
<tr>
<td>Treated Municipalities</td>
<td>72</td>
<td>82</td>
<td>83</td>
<td>81</td>
</tr>
<tr>
<td>Total Municipalities</td>
<td>476</td>
<td>709</td>
<td>737</td>
<td>675</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Saturday</th>
<th>Sunday</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Log linear model</td>
<td>-0.0192</td>
<td>-0.0290</td>
<td>-0.0456</td>
<td>-0.1422***</td>
</tr>
<tr>
<td>(0.036)</td>
<td>(0.055)</td>
<td>(0.065)</td>
<td>(0.053)</td>
<td></td>
</tr>
<tr>
<td>Linear probability model</td>
<td>-0.0223</td>
<td>-0.0411</td>
<td>-0.0358</td>
<td>-0.0735***</td>
</tr>
<tr>
<td>(0.027)</td>
<td>(0.034)</td>
<td>(0.032)</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>Standardized model</td>
<td>-0.0127</td>
<td>-0.0986</td>
<td>-0.0342</td>
<td>-0.2207***</td>
</tr>
<tr>
<td>(0.067)</td>
<td>(0.080)</td>
<td>(0.083)</td>
<td>(0.067)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>11,075</td>
<td>17,900</td>
<td>19,425</td>
<td>17,800</td>
</tr>
<tr>
<td>Treated Municipalities</td>
<td>68</td>
<td>82</td>
<td>81</td>
<td>82</td>
</tr>
<tr>
<td>Total Municipalities</td>
<td>443</td>
<td>716</td>
<td>777</td>
<td>712</td>
</tr>
</tbody>
</table>

Source: own elaboration
Clustered on municipality standard errors in parenthesis
*p<0.1, **p<0.05, ***p<0.01

premise for our methodology. This could happen either because cities without car sharing apps are not a good control group for those that do have this platforms available or because the specifications we used are not appropriate. The results are show in table 4. Panel A shows the results for Saturday while Panel B show the results for Sunday.

Saturday’s pre–treatment coefficients are negative but not statistically significant. The short term coefficient is negative across all specifications and significant at 1% for the log linear model and at 5% for both the linear probability and the standardized model. The long run coefficients are smaller and significant at 10% for the linear probability specification only, which is not the result we expected since one would imagine that the time that a platform has been operational in a city would be positively correlated with client familiarity and the number of drivers, both of which would yield stronger coefficients. However, thes findings could be explained by a surge in advertising both paid and word-of-mouth (due to the novelty of it) or promotions that lasted only for the first months after the launch of an app in a particular city (as it was the case when Uber launched its platform in São Paulo).
### Table 4 – Saturday and Sunday Night Test

<table>
<thead>
<tr>
<th></th>
<th>Pre-treatment</th>
<th>Short-term effect</th>
<th>Long-run effect</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Saturday</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log linear model</td>
<td>-0.0640</td>
<td>-0.1484***</td>
<td>-0.0888</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.055)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>Linear probability</td>
<td>-0.0157</td>
<td>-0.0783*</td>
<td>-0.0599*</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.042)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Standardized model</td>
<td>-0.0642</td>
<td>-0.1795*</td>
<td>-0.1292</td>
</tr>
<tr>
<td></td>
<td>(0.092)</td>
<td>(0.097)</td>
<td>(0.091)</td>
</tr>
<tr>
<td><strong>Panel B: Sunday</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log linear model</td>
<td>-0.0963*</td>
<td>-0.2013***</td>
<td>-0.1681**</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.064)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>Linear probability</td>
<td>0.0440</td>
<td>-0.0915*</td>
<td>-0.0668*</td>
</tr>
<tr>
<td></td>
<td>(0.037)</td>
<td>(0.039)</td>
<td>(0.033)</td>
</tr>
<tr>
<td>Standardized model</td>
<td>-0.1001</td>
<td>-0.3348***</td>
<td>-0.2368***</td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.096)</td>
<td>(0.086)</td>
</tr>
</tbody>
</table>

Source: own elaboration  
Clustered on municipality standard errors in parenthesis  
*\(p<0.1\), **\(p<0.05\), ***\(p<0.01\)

Sunday’s results are once again very interesting. Although the pre-treatment coefficient is negative and significant at 10% in the log linear specification, the effect is much stronger in the following coefficients, reaching 1% significance in short term and 5% significance in the long term effect coefficient. The linear probability specification makes a strong case in favor of a true detected effect since its pre treatment coefficient is positive and not significant while both the short term and long term effect coefficients are negative and significant at 10%. Lastly, the standardized specification also makes a strong argument since its pre treatment effect is not statistically different than zero, but both short term and long term effect coefficients are significant at 1%.
6 Conclusion

Differently than most of the literature on the subject, we use car crashes rather than the number of deaths on traffic or police reported alcohol related accidents to evaluate the impact of car sharing platforms on road safety. Using a difference-in-difference strategy with three different specifications (log linear, linear probability and standardized) we found inconclusive results on the overall number of car crashes. Such aggregation, however, masks the heterogeneous effects that such platforms have on different weekdays. Analyzing each weekday separately, we found a significant negative effect in the number of car crashes on Sundays across all specifications.

When splitting Saturday and Sunday into four 6-hour-length periods, we found a significant reduction in the number of car crashes both on Saturdays and Sundays nights, with a much stronger and consistent effect on Sundays. These results corroborate with the popular hypothesis that car sharing apps reduce the number of drunk drivers, but it also indicates that this is not the only channel through which those platforms affect road safety as it doesn't intuitively explain why the effect is larger on Sundays then on Saturdays. Although it is possible that alcohol causes more crashes on Sundays then Saturdays (or drunk drivers are more likely to use car sharing services on Sundays), it is likely that other channels are also in play leading to those results, such as changes in incentives for speeding or principal-agent problems. More research is needed in order to identify the true channels from which the detected effect manifests itself.

Lastly, we found a consistent significant negative effect on Saturdays only in the short term (first two months after the launch of an app), while the effect for Sundays were negative and significant both on the short and long term across all specification, although they were stronger in the short term. One possible explanation for this effect is a surge in advertising or promotions when a platform is launched in a city. It would be interesting to see further research evaluating if both Saturday - Sunday and short term - long term heterogeneous effect could be explained by price sensitivity and therefore related to younger or lower income individuals and their preference towards alcohol consumption and transportation method.

The main premise in the used methodology are parallel trends between treated and control groups in the pre treatment period. We (imperfectly) evaluated this premise on Saturday and Sunday nights by defining our treatment as occurring two months prior to the actual treatment and found both positive and negative coefficients with no statistically significant effect on most specifications. For the one specification with a 10% significant negative effect we found much stronger outcomes on the short and long run with 1% and
5% significance, respectively. Despite this encouraging results, the used dataset is prone to reporting errors and there seems to be under-reporting, specially in the first hours of the day, although this shouldn’t bias our results.

It is worth mentioning that this research has mainly three characteristics that distinguishes itself from previous inquiries. First is the use of a developing country database. Previous research has shown that the effect of car sharing platforms depends highly on the price charged, therefore one could expect that the effects in developing countries might be different than those in developed ones. Second is the use of a very detailed database. Most papers focus on deaths on traffic or alcohol related crashes. We take a more holistic approach allowed by our unusual database and look at all accidents as well as analyze heterogeneous effects for different weekdays and periods within the same day. Finally, car sharing apps were only implemented in Brazil after other companies were well established with cellphone platforms that matched passengers to traditional taxi cabs. This allows us to identify only the effect of the car sharing apps and not be confounded by the platform effect, which we argue is the relevant policy variable.
Referências


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Blog, UBER Blog. Available at: <https://www.uber.com/blog/>.