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# Highway concessions and road safety:Evidence from Brazil[☆]

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A B S T R A C T

Reducing road fatalities is a key policy goal in several countries and there is a vast literature on what factors affect road safety performance. Nonetheless, there is limited evidence on whether highway concessions and Public Private Partnerships (PPP) can bring road safety benefits, despite the growing number of countries adopting this type of policy to finance and manage road infrastructure. In this paper, we use a difference-in-differences approach to examine the causal effect of highway concessions on road safety outcomes using daily data from Brazilian Federal highways over 11 years between 2007 and 2017. We exploit the transition from public to private management in some but not all Brazilian states to provide both within- and between-states comparison. We find that concessions promote a small but significant reduction in the number and fatality of road crashes as well as and the number of people and vehicles involved in crashes. Between 2007 and 2017, procured roads had on average 16 fewer deaths then publicly managed highways for every 1000 crashes each year. These road safety benefits only become statistically significant a few years after a concession implementation, but they are marginally larger for every additional year of concession. Finally, our results suggest that including safety-based incentives in concession contracts can substantially improve road safety performance. The findings of the paper have important implications for the social and economic evaluation of road concessions and for road infrastructure policy more broadly.

## 1. Introduction

Road traffic crashes represent the eighth leading cause of death globally, summing 1.35 mil-lion deaths and 50 million injured people each year (WHO, 2018). Various studies investigate how road safety is influenced by factors such as weather conditions (Theofilatos and Yannis, 2014; Brijs et al., 2008), speed limits and road design (Pauw et al., 2014; Wang et al., 2013), law enforcement and awareness campaigns (Lewis et al., 2007), as well as individuals attitudes and skills (Shinar, 2017; Anstey et al., 2012). Nonetheless, there is still little understanding about whether the implementation of road concessions can help improve road safety performance, particularly in low- and middle-income countries.

Since the 1990s, it has become increasingly common for governments to use road concessions via Public Private Partnerships (PPP) as an alternative means to finance and manage roads (Bel & Foote, 2009; Galilea & Medda, 2010; Albalate & Bel-Pinãna, 2019). Between 1990 and 2015, several countries worldwide have awarded over 950 PPP road projects totaling an investment of 267,039 million dollars (Albalate & Bel-Pinãna, 2019). A common motivation behind road procurement is the expectation that private sector operators can more efficiently upgrade and maintain road quality, bringing about both economic as well as road safety outcomes (Grimsey & Lewis, 2007). Nonetheless,private operators can often face potential conflicts of interest due to trade-offs between profits and the quality and safety of services (Hart, 2003), raising questions about whether road concessions could effectively bring any road safety benefits.

This paper examines the impact of road concessions on road safety outcomes using daily data aggregated by month and year on traffic crashes in all of Brazilian federal highways over a 11-year period between 2007 and 2017. Using a difference-in-differences approach, we test whether the introduction of road concessions significantly reduces the number and severity of road crashes in treated roads after concession. As exogenous variation in the road safety, we exploit the transition

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from public to private management in some but not all Brazilian states to provide both within- and between-states comparisons over time. We test whether these concessions, which are expected to promote more efficient road improvements and maintenance, can lead to road safety benefits by reducing number of road crashes and deaths. The analysis looks at various road safety outcomes including number of crashes per kilometer, number of deaths per crashes, number of vehicles involved in crashes, among others. A small number of studies that previously have addressed this question suggest that the road concessions through PPP could reduce the number of traffic crashes (Rangel et al., 2012; Rangel & Vassallo, 2015; Baumgarten & Middelkamp, 2015; Albalate & Bel-Pinãna, 2019). Nonetheless, these studies are largely focused on developed countries, even though most global road traffic deaths occur in the Global South (Abubakar et al., 2015). Moreover, the methods used in previous studies are largely based on negative binomial or panel regression models that do not capture causal effects (see section 2). This paper advances this literature by showing how a quasi-experimental research design can help estimate more robust evidence of the causal effect of road concessions on road safety outcomes. It also shows the importance of capturing how the effect of road concessions on road safety varies over time. Finally, this study brings new evidence on the relationship between road procurement and road safety using detailed data from a middle-income country with one of the largest road networks under private concessions in the world (Brochado & Vassallo, 2014; Neto et al., 2018).

The remainder of this paper is as follows. Section 2 reviews the literature on road safety and road concessions. Section 3 presents a brief context about road concessions in Brazil. Data and methods are presented in Section 4 and results presented in Section 5 Finally, Section 6 discusses the main conclusions about the case study of Brazil and some lessons that can be drawn for road safety performance more broadly.

## 2. Literature review

Every year, traffic crashes cost approximately 130 billion euros to countries in the European Union (equivalent to 2% of Europe's GDP) (European Commission, 2010), and around 1.8 trillion dollars globally (approximately 3% of the world's GDP) (iRAP, 2016). In Brazil, road fatalities is the 10th leading cause of death and the second most common external cause of death, with over 33 thousand deaths a year and more than a million of potential years of life lost by road traffic injuries (and de Mello-Jorge,2016de Araujo Andrade ).

Reducing road fatalities has become a key policy concern in several countries (WHO, 2018). This issue has also received substantial attention from researchers who investigate how different factors affect road safety outcomes. Multiple studies analyze the extent to which the incidence and severity of road crashes are influenced by speed limits and the geometric and traffic characteristics of highways (Milton & Mannering, 1998; Wang et al., 2013; Pauw et al., 2014) or weather conditions (Theofilatos and Yannis, 2014; Brijs et al., 2008). Others have investigated the role played by law enforcement and awareness campaigns (Lewis et al., 2007), as well as individuals attitudes and skills (Shinar, 2017; Anstey et al., 2012).

The work of Hermans et al. (2009), for example, uses a data envelopment analysis (DEA) to compare road safety performance across 21 European countries. The authors find that some of the key factors shaping road safety include the use of alcohol and drugs by drivers, speeding, vehicle types, infrastructure and trauma management. In a detailed study in Brazil, Lima et al. (2008) investigated the determinants of road crashes on Brazilian highways between 2004 and 2005 based on inspection and field research in sections of highways BR-116 and BR-324. The authors found that poor road signs close to urban areas, inadequate traffic conditions, behavior of pedestrians and drivers and unfavorable weather conditions are the main causes of crashes.

Many of the key factors discussed in the literature - such as road maintenance and signs, speed controls, law enforcement and trauma management teams - are directly affected when governments outsource road management through road concessions. Nonetheless, despite the growing number of countries using road concessions and Public Private Partnerships (PPP) as a policy strategy to finance and manage road infrastructure (Bel & Foote, 2009; Galilea & Medda, 2010; Albalate & Bel-Pinãna, 2019), there are still very few studies that investigate whether this type of policy bring any road safety benefits.

In some of the earlier studies on this topic, Rangel and colleagues (Rangel et al., 2012; Rangel et al., 2013; Rangel & Vassallo, 2015) evaluated whether safety-based incentives incorporated in highway concession contracts in Spain helped reduce road crashes between 2007 and 2009. These economic incentives include for example bonuses and penalties for contractors who do not meet certain safety performance level. Using a negative binomial regression model, the authors find that the implementation of these incentives is associated with a reduction of 0.252 in the expected number of crashes compared with public highways, suggesting that tolled highways with safety performance incentives are safer than conventionally procured roads. A similar result has been found by Albalate and Bel-Pinãna (2019), who studied the effects of PPPs on road safety outcomes between 2008 and 2012 in Spain, using a panel-data fixed-effects method. The authors used a poisson regression and found that roads managed under PPP have, on average, 0.41 fewer number of crashes with victims compared to regular roads each year.

Looking at the case of Mexico, Geddes et al. (2015) used fixed-effect multiple regression models to estimate the association between road concessions and the number of crashes and fatal collisions in federal and state highways between 1997 and 2009. After aggregating annual data at the municipality level, the authors found that 100 km more on private roads had an average reduction of 1980 in number of crashes on federal roads. However, results were not statistically significant for fatal crashes and no significant effects were found after including fixed effects.

Finally, the paper by Pereira, Pereira, & dos Santos (2021) also aggregated road crash data at the municipal level to investigate the effects of the introduction of tolls on road safety in Portugal's highways between 2008 and 2014. Exploiting the fact that decisions to implement road tolls are taken at national policy level without the direct involvement of local governments, the authors used a differences-in-differences regression to overcome potential endogeneity problems in the data. The treatment group consisted of 59 municipalities that contained some segment of a tolled highway while the control group consisted of the remaining 219 municipalities in Portugal. The study shows that the introduction of tolls significantly improved road safety in highways, with a reduction between 21.1% and 16.4% in total number of crashes and a between 27.2% and 22.4% in total number of victims. The authors warn, though, that these results were followed by an increase between 3% and 9% in the number of crashes and victims with minor injuries in other types of roads. This result is consistent with known traffic diversion effects (Albalate, 2011; Albalate & Germa Bel, 2011), where the implementation of tolls induces some traffic shift to non-tolled adjacent secondary roads. A similar result was found in Germany by Baumgarten and Middelkamp (2015), who found that the introduction of tolls for heavy vehicles on highways between 2000 and 2010 increased the total number of crashes in adjacent roads by 3.7%.

In summary, the accumulated evidence in the literature suggests significant association between tolled road concessions and better road safety performance. Nonetheless, most of these studies are focused on developed countries and based on statistical analyses that do not capture causal effects, with few exceptions (Baumgarten & Middelkamp, 2015; Pereira, Pereira, & dos Santos, 2021). There is particularly little rigorous evidence on the effectiveness of road safety programs in the Latin American context (Martinez, Sanchez and Yañez-Pagans, 2019). In the next sections we advance this literature by using a quasi-experimental research design to examine the causal effect of toll road concessions on road safety outcomes in Brazil, a middle-income country with one of the largest road networks under private concessions in the world (Brochado & Vassallo, 2014; Neto et al., 2018).

Compared to previous studies, we use detailed daily data on traffic crashes aggregated for each month at the road segment level. We use monthly variation in our analysis because daily data might be noisy, particularly for death measures. Furthermore, the method deployed in this paper uses fixed controls and effects on multiple robustness checks and road safety indicators, what allows us to measure how the effect of road concessions on road safety outcomes change over time.

## 3. Context of road concessions in Brazil

The Brazilian Federal government launched first phase of the Federal Highway Concession Program in 1995, in a context of severe fiscal constraint and having recently overcome a period of hyperinflation. The first phase included the procurement of six sections of highways with terms ranging from 20 to 27 years. The second phase started in 2008, the third one in 2013 and the last phased started between 2018 and 2019 (Table 1). Up until 2017, approximately 10,215.4 km (12.50%) of all federal highways were managed by private contractors under concession contracts (ANTT, 2018).

**Table 1**
Extension of federal highways Brazil (Km).

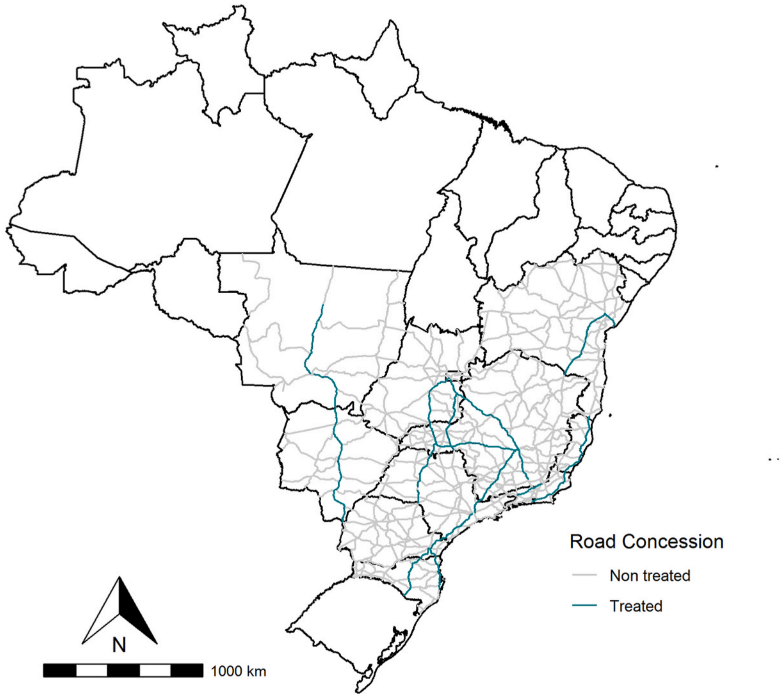
| **Public Concession** | Extension | Number of Roads |
|---|---|---|
| 1° Phase | 1602.9 | 7 |
| 2° Phase | 3755.4 | 12 |
| 3° Phase | 4056.6 | 14 |
| 4° Phase | 800.5 | 5 |
| **Public** | 117,471.9 | 347 |
| **Total** | 127,687.3 | 385 |

*Note:* National Department of Transport Infrastructure (DNIT) and Brazilian Land Transportation Agency (ANTT).

There were two consecutive road segments procured by two different companies during same period in the second phase. For computational purposes, we considered them as a single procured road segment.

Companies running road concessions are obliged by contract to perform regular road maintenance and improvements, including the expansion of two-lane roads into four-lane divided roads, construction of crossings and installation of speed cameras and traffic signs. All concession contracts require companies to provide road assistance including tow trucks, medical assistance, traffic inspection and control centers. Moreover, Since the second phase of road concessions, companies were only allowed to start charging tolls after the completion of initial recovery works and services on road segments considered most urgent. Furthermore, for those roads procured in the third phase, the collection of tolls was only approved if companies would meet road recovery goals as well as if they managed to improve road safety outcomes compared to other concessions. This difference between concession contracts of phase 2 and 3 is explored in our data analysis.

In this paper we only consider road concessions from the second and third phases. This is because there is no detailed data on road crashes before the year 2000, and because there is yet not enough information on road safety performance of concessions in the fourth phase since its implementation in 2019. We also narrowed our baseline comparison group to only include roads in the states where at least one road segment was procured. Fig. 1 shows the road segments (treated and non-treated) of the federal highways considered in our analysis. The TableA3 in Appendix also shows the amount of resources expected to be invested in road improvements defined in each road concession contract.



Note: The figure only shows the highways of states where at least one federal road has been procured.

**Fig. 1.** Federal highways in our sample.
Note: The figure only shows the highways of states where at least one federal road has been procured.

**Table 2**
Investments on federal highways (R$ millions).

| Year | Highways under concession | Highways under public management | Total |
|---|---|---|---|
| 2010 | 6115.51 | 17862.17 | **23977.68** |
| 2011 | 6201.26 | 18320.37 | **24521.63** |
| 2012 | 7147.50 | 14435.53 | **21583.02** |
| 2013 | 10071.61 | 12187.61 | **22259.22** |
| 2014 | 9552.71 | 12396.19 | **21948.91** |
| 2015 | 8244.84 | 7364.84 | **15609.68** |
| 2016 | 7854.46 | 10024.19 | **17878.65** |
| 2017 | 7626.19 | 9021.60 | **16647.79** |
| 2018 | 6624.25 | 8155.58 | **14779.83** |
| 2019 | 5472.80 | 6904.36 | **12377.16** |

*Note*: Data were collected in National Transport Confederation.

Investments in Brazilian federal highways have more than halved between 2010 and 2019 (see Table 2). This was particularly due to fiscal contraction with a substantial reduction of government investments on public highways, which dropped by (61%) in the last 10 years. Meanwhile, private investments in procured highways only reduced by (10%) in the same period. By 2019, private investments accounted for over (44%) of all investments on federal highways, even though procured roads under concession only represented (8%) of the national highway network.

## 4. Methods

We use a difference-in-difference regression model to capture whether the implementation of road concessions have affected historical trends in the daily number of crashes comparing pro-cured and publicly managed highways. The following subsections present more details about the data and model specifications used as well as some of methodological of its limitations.[1]

### 4.1. Data

We use data on daily road crashes between 2007 and 2017 from the national information system on Federal highway crashes, organized by the Brazilian Federal Highway Police Department (PRF). These data set brings detailed information on all road crashes on federal highways, including the location and characteristics of crashes such as severity, number of deaths, number of people injured, and number and types of vehicles involved.

Spatial data on the Brazilian national highways come from the National Department of Transport Infrastructure (DNIT). For management purposes, DNIT divides national high-ways into road segments with similar characteristics, including number of lanes, pavement deflection etc (DNIT, 2011). These segments vary in extension from 6 Km up to 1361 Km (median of 305 Km).[2] We were able to locate each crash observation onto a road segment, and thus aggregate crash data by segment. This allowed us to run our analysis at the road segment level to examine whether the implementation of a road concession has influenced safety levels while controlling for the characteristics of road segments.

To allow for a more rigorous analysis, we only compare treated highway segments (managed by private operators under concession) and control group (highway segments managed by the federal government) within the same state. We analyze all the road crashes on federal highways in states that have at least one road segment under concession in a period of eleven years between January 2007 and December 2017, which covers the second and third phases of the Brazilian road's concession program. We aggregate the daily cases by road segment, treatment group, state and month of the year in our analyses. This way, the unit of analysis of our model is the comparison between publicly managed road segments in each state × procured road segments in the same state, which is the equivalent of a treatment group is our analysis. The final data set contains on average more than 95,000 crashes a year spread over 26 treated road segments and 110 non-treat road segments in 11 states.

One caveat of these data set is that it does not contain information on traffic volume. This information is only available for the year 2020 with vehicle count data aggregated by highways, making it impossible to use this information as a control variable in our model. To circumvent this issue of omitted variable bias, we carried out several robustness tests by changing our main regression specification to include control variables that indirectly capture traffic volume levels (see sections 4.3 and 4.4). Moreover, although each concession contract includes a detailed list of road improvements that must be done by concessionaire companies, there is no available data on which measures were and were not effectively implemented in each road segment.

### 4.2. Road safety outcomes

A common practice in the literature is to look at the association between the implementation of road concessions and the absolute number of road crashes and fatalities while controlling for traffic volumes. A limitation to this approach is that the number of crashes or fatalities might decrease simply because tolled concessions reduce traffic levels, not necessarily because they improve road safety. Another important limitation is that this approach overlooks potential endogeneity in the data because of how the number of crashes is affected by traffic levels. On one hand, traffic levels before a concession is implemented might influence the selection of roads where concessions are implemented. Roads with higher traffic volumes are more commercially attractive to private sector operators. Hence, using traffic volume information as a control variable might generate endogeneity issues since there is a selection bias where public roads with heavier traffic are more likely to be procured. On the other hand, the implementation of a concession itself can influence traffic levels, either because improved infrastructure could attract more drivers or because the additional toll costs would reduce demand levels.

To overcome this issue, we run the analysis considering multiple measures of road safety performance relative to road lengths and numbers of crashes. One advantage of this approach is that the road safety outcome measure is less sensible to traffic levels, what allows us to isolate the road safety effects. For the sake of brevity we only present the full results for number of crashes per Km, number of deaths per road crash and number of injured people per road crash in each road segment per month. The results for all other road safety outcomes are presented in table A1 in the Appendix.

**Table 3**
Descriptive statistics of road safety measures in Brazilian federal highways under concession, 2007 and 2017.

| Variables | 2007 | | 2017 | |
|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. |
| Deaths per road crash | 0.07 | 0.14 | 0.09 | 0.17 |
| Deaths per people involved | 0.03 | 0.06 | 0.04 | 0.06 |
| Crashes per kilometer | 0.22 | 0.39 | 0.16 | 0.27 |
| People involved per kilometer | 0.46 | 0.85 | 0.36 | 0.62 |
| Death per kilometer | 0.01 | 0.02 | 0.01 | 0.01 |
| Injured per kilometer | 0.13 | 0.22 | 0.15 | 0.27 |
| Vehicles per kilometer | 0.38 | 0.73 | 0.26 | 0.46 |
| Lightly injured per kilometer | 0.10 | 0.17 | 0.12 | 0.23 |
| Seriously injured per kilometer | 0.04 | 0.06 | 0.03 | 0.05 |

*Note*: This table provides summary statistics on all road crashes in federal highways that had at least one road segment procured during the second and third rounds of the Brazilian concession program.

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[1] The data sets used in this paper are publicly available and can be downloaded along with the code to reproduce this paper from this github repository: https://github.com/ipeaGIT/Concessions-and-Road-safety.

[2] More information can be found at http://www1.dnit.gov.br/anexo/Projetos/Projetos_edital0034_14-14_2.pdf.

Table 3 presents the descriptive statistics of various road safety measures in 2007 and 2017 for the federal highways in our sample. Although the number of crashes per kilometer has dropped in this period, there was an increase in the number of deaths per crash and per number of people involved in crashes, a worrying result that suggests an increase in the fatality of road crashes between 2007 and 2017.

### 4.3. Empirical strategy

The aim of the analysis is to examine whether the implementation of road concessions has had any impact on road safety performance. The ideal scenario would be to compare the fatality rates observed in each treated segment of highway with its counterfactual. Therefore, we would like to observe what would have happened to the same segment of highway if the concession had never been implemented. However, since we are unable to observe such counterfactual, we approach this problem by using a quasi-experimental differences-in-differences strategy. We use this empirical strategy to estimate the effect of a treatment (in our case, the period of a concession operation) on a road safety outcome variable by comparing the average change over time in the outcome variable for the treatment group (procured roads) and the average variation over time in the control group (non-procured roads) (Angrist and Pischke, 2009).

Two different regression specifications were used to estimate the causal effects (equations (1) and (2)), while two additional specifications (equations (3) and (4)) were used to check the robustness of the first two models. In summary, the model presented in equation (1) was used to estimate the average effect of road concessions on road safety for the whole period of analysis. The model of equation (2) was used to capture how the magnitude of this effect might vary over time. Meanwhile, the model specification of equation (3) was conducted to test the robustness of our estimates to endogeneity problems, and test whether fatality rates had not already started to fall before the implementation of road concessions. Finally, we use the model specification of equation (4) to control for issues spatial autocorrelation in the data and to check whether for spatial spillover effects among neighboring road segments.

We now present each specification in more detail. Our basic specification is given by the following equation (1):

$$Y_{imt} = \beta PPP_{it} + X^{'}_{imt}\Theta + \mu_i + \omega_m + \lambda_t + \varepsilon_{imt} \tag{1}$$

where $Y_{imt}$ is our variable of interest represented by one of the safety outcome measures (as detailed in subsection 4.2) observed for road segment $i$, occurred in month $m$ and year $t$. $X_{imt}$ represents the set of covariates described in subsection 4.4 to control for the characteristics of highway segments. The id fixed effect $\mu_i$ accounts for unobserved time-invariant determinants of crashes outcomes occurred in the same road segment (such as road geometric characteristics), while the inclusion of month and year fixed effects, $\omega_m$ and $\lambda_t$, adjusts for shocks that are common to all road segment at a specific moment in time (such as variations in traffic levels over time). Finally, $\varepsilon_{imt}$ is a random error term clustered at the road segment level to make estimations robust to serial correlation and heteroskedasticity (see Bertrand et al., 2004). *PPP* is a dummy variable that equals one for crashes in procured roads under concession period and zero for crashes in publicly managed roads. Thus, the key parameter of interest is then $\beta$, which measures whether drivers exposed to roads during the concession period have a lower probability of being involved in a fatal road crash.

Our empirical approach requires a parallel trends assumption, according to which the fatality rates for procured roads would have followed the same trend as in the and non-procured roads if concessions had not been implemented. We assume that conditional on time and road segment fixed effects characteristics, the variation in a concession status is exogenous. Under the validity of this assumption, we can interpret β in Equation1as the causal effect of road concession on fatality rate.

The model presented in equation (1) allows us to estimate only one β that tell us the average effect of road concessions on road safety for the whole period of analysis. However, the effect of a concessions may not be instantaneous, for example, because a company may take time to implement road improvements. In order to capture how this effect might vary over time, we also use a second specification, represented by:

$$Y_{imt} = \sum_{j=1}^{10} \beta_j PPP_{jit} + X^{'}_{imt}\Theta + \mu_i + \omega_m + \lambda_t + \varepsilon_{imt} \tag{2}$$

where $PPP_{jit}$ are dummies indicating whether the road segment $i$ in the year $t$ and month $m$ has benefited from PPP for $j$ years. In other words, in equation (2) we define our parameters of interest (treatment variables) as dummy variables indicating the number of years a given road segment has managed under a procurement contract. Hence, we are able to analyze how the average effect of road concessions on road safety performance varies each year since the introduction of a road concession.

Afterwards, we use the model specified in Equation (3) to test whether fatality rates had not already started to fall before the implementation of road concessions. For that, we consider first estimating the model with additional dummies indicating years before concession. We check therefore whether causes happen before consequences, by allowing the model to have heterogeneous anticipatory effects (leads), in addition to the heterogeneous post-treatment effects (lags) already included in the model (Equation (3)).

$$y_{it} = \sum_{k=1}^{6} \beta_{-k} PPP_{-kit} \sum_{j=1}^{10} \beta_j PPP_{jit} + X^{'}_{imt}\Theta + \mu_i + \omega_m + \lambda_t + \varepsilon_{imt} \tag{3}$$

We set the coefficient on $\beta_0$ equal to zero to use the year immediately prior to the concessions start as a reference. If the model we estimate in equation (2) incorrectly attributes pre-existing trends in fatality rates to our treatment effect, then dummies indicating years before adoption should matter in equation (3) and anticipatory effects captured in $\beta_{-k}$, should show up as significant.

The identifying assumption is that the time trend in the probability of road crashes in treated highway segments would have a similar trend as the one observed in similar non-treated highway segments in the absence of the policy intervention. A crucial methodological concern that could undermine the causal validity of results relates to the endogenous nature of concessions. There is the possibility that the implementation of road concessions in certain highway segments is statistically associated to unobserved roads segment characteristics that also affect traffic crashes, preventing us from obtaining unbiased estimates. If this unobserved component changes between road segments but is fixed across time, the road segment fixed-effect included in the model should be sufficient to allow for a causal interpretation of the estimated effects. If, however, this endogeneity is based on dynamic shocks to roads crashes, then we might face problems in identifying the pure effect of policy intervention.

We try to address this potential endogeneity problem in different ways. First, we attempt to "clean the path" between the road concessions and road crashes/fatality by including a substantial set of controls in our specifications. These include annual precipitation, GDP per capita, formal sector workers, population. We also include a dummy variable for indicating the beginning of toll collection period, and a variable of the predominant agricultural harvest month in each road segment to control seasonal fluctuations in number of trucks. Any changes that might have occurred in road safety legislation or in vehicle safety parameters are meant to be equally present on both types of roads (treated and untreated group). These changes are also captured by our regression in the road segment fixed effects and therefore do not affect the results of the analysis.

We also show how robust are the estimates when road segment with large fatality rates are allowed to converge to the average fatality rate observed in the data. If those segments with large fatality rates are naturally catching up with those with average fatality rates, then estimates should converge towards zero when accounting for this behavior. As another test for pre-existing time trends, we run models that include linear and state linear trends. Finally, we test the robustness of our results by using month × year of crash fixed effects to control for time varying characteristics common to all road segments and for the effects of seasonality on road safety outcomes.

Finally, we used the model in Equation (4) to check for spillover effects among neighboring road segments. This allows us to examine whether the implementation of a concession in a given road may affect crashes on non-tolled adjacent roads due to traffic diversion effects. This model is a spatial extension of the difference-in-differences estimator recently proposed by Delgado and Florax (2015). This spatial specification is commonly known as the SLX model to capture possible spillover effects which are captured as indirect effects, for example, due to traffic flow changes (Vega & Paul Elhorst, 2015). This strategy allows us to explicitly consider the local spatial dependence of the treatment variable, so that the outcome of road segment i depends not only on their own treatment, but also on the treatment status of close neighbors. For that, we use a binary contiguity matrix build based on the inverse distance that is row-normalized to ensure row sums equal to 1, under the criterion that the minimum distance is sufficient to ensure that all highways have at least one neighbor. This equation (4) includes a spatial lag of the treatment dummy as well as spatial lags for explanatory variables. The indirect spillover effect at this scenario can be interpreted as a substitution effect. In other words, we verify whether the eventual diversion of traffic from procured roads to their neighboring road segments could explain our results.

$$Y_{imt} = \beta PPP_{it} + \rho \sum_{j=1}^{N} w_{ij} PPP_{it} + X'_{imt}\Theta + \sum_{j=1}^{N} w_{ij} X'_{imt}\Phi + \mu_i + \omega_m + \lambda_t + \varepsilon_{imt} \tag{4}$$

### 4.4. Covariates

Since traffic volume data was not available, we have included in our estimations some co-variates which are known in literature to correlate with road crashes because of how they indirectly capture traffic levels. Some authors have shown road crashes to be positively associated with population size (Pereira, Pereira, & dos Santos, 2021), unemployment rates (Ruhm, 2015) and intensity of urbanization (Ossenbruggen et al., 2001; Kmet & Macarthur, 2006). Other studies also suggest that road crashes can be affected by weather conditions (Theofilatos and Yannis, 2014; Brijs et al., 2008). In this study, the included control variables can be divided in three groups. The first group includes annual demographic and economic indicators of the municipalities that intersect with each highway segment: GDP per capita, total number of formal sector workers, population size and a dummy for the month of the year with the largest harvest of any agricultural product as a proxy for heavy duty vehicles traffic. These data come from the Brazilian Institute of Geography and Statistics (IBGE). The second group consists of weather variables at municipal level: average precipitation and average temperature by month were constructed by using the Terrestrial Air Temperature and Terrestrial Precipitation: 1900–2017 Gridded Monthly Time Series, version 5.01 (Matsuura & J Willmott, 2018, p. 19716).[3] Finally, the third group consists of geographic variables: distance from the road segment to nearest large city, defined by IBGE as cities above 750 thousand inhabitants. A list of variables and data sources used in the analysis is presented in the Appendix TableA4. All covariates were used in log form to address possible nonlinearities.

We use the traffic volume data available for the year 2020 to test whether these covariates can be used as reasonable proxies for traffic levels on highways. We find that highway traffic volume was highly correlated with population size (0.58), number of workers in the formal sector (0.58), GDP per capita (0.28), precipitation (0.49) and distance to large city (−0.24). All correlations were found to be statistically significant at 1%, confirming the literature that these variables are able to indirectly capture the effect of traffic volume levels missing from our model.

Covariates at the municipal level, such as GDP per capita and average precipitation, were assigned to road segments when there is an intersection between a road segment and a municipal boundary. In case the same road segment crosses the boundaries of more than one municipality, we considered the average values weighted by the proportion of the segment length in each municipality. Finally, to keep a consistent comparison throughout the whole 11 years of analysis, we drop the roads that were procured in the first phase because there is not crash data available for them before they were procured.

## 5. Results

### 5.1. Basic specification

Panel A of Table 4 reports regression results of the average effect of concessions over the whole period (2007–2017) on the three main outcomes considered in our analysis: number of crashes per Km, number of deaths per crash and number of injured people per crash. Column one presents the results for the difference-in-differences (DiD) specification considering fixed effects only, without controlling for other variables, while column (2) presents the DiD results controlling for covariates. Meanwhile, Panel B reports how these effects vary over time.

A first look at these results (Panel A of Table 4) suggests that road concessions did not reduce the number of crashes per Km, although they did reduce the severity of crashes. Our estimates indicate that the implementation of highway concessions reduced crash fatalities by 1.6%, with 16 fewer deaths for every one thousand crashes compared to publicly managed highways.

Nonetheless, Panel B of Table4shows that the safety benefits of road concessions are not immediate and that the magnitude and significance of these effects vary substantially over time. The reduction in the number of crashes and fatality rates resulting from road concessions only becomes statistically significant 8 and 5 years after a concession is implemented. Moreover, the magnitude of such effects becomes marginally larger for every additional year of a concession. In the tenth year after implementation, road concessions reduced road fatality rates by 2.6% and road crashes by 41%. Back-of-the-envelope calculations suggest that had all treated highways been under concession for ten years, the highway concession program in Brazil could have avoided approximately 484 deaths and 332.2 thousand crashes between 2007 and 2017.

However, the results of Table 4 also show that the reductions in crash fatality rates were followed by a 3.8% increase in the number of injured people per crashes in procured roads. This could partly result from the fact that improved roads reduce the severity of crashes, hence making them less fatal while leaving injured victims alive. Morever, we cannot rule out the possibility that improvements in procured roads might have increased the level of risk taken by drivers, generating an offsetting behavior known as the Peltzman effect (Peltzman, 1975; Noland, 2013). If this were the case, the observed road safety benefits could have been even larger had it not been for this compensating effect.

When we look at other road safety outcomes weighted by road length (Appendix, Table A1), the effects only become statistically significant 8 years after the concessions are implemented. Looking at the marginal effect on the 10th year after implementation, procured roads led to an average of 790 fewer people and 754 fewer vehicles involved in crashes for every one thousand kilometers of procured roads.

Combined, these findings show that the introduction of highway concessions reduced road fatality rates and improved road safety outcomes, though such improvements only become significant a few years after concession is implemented (Fig. 2). The magnitude of these effect

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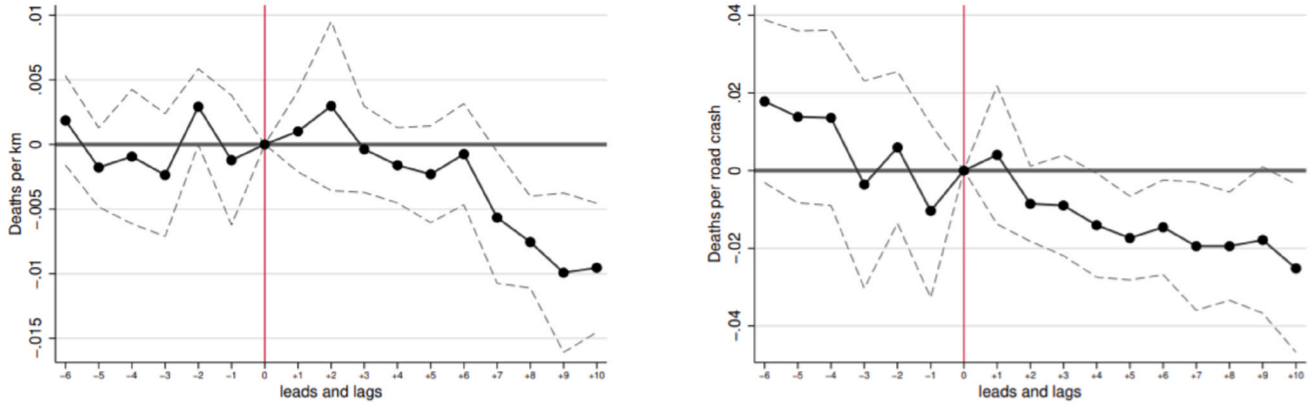
[3] These data sets provide worldwide monthly temperature and precipitation estimates at the 0.5° × 0.5° level (0.5° corresponds to roughly 56 km).

**Table 4**
Main results.

| | Craches per km | | Deaths per km | | Injured per km | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (1) | (2) | (1) | (2) |
| Panel A | | | | | | |
| Treat | −0.014 | −0.007 | −0.013*** | −0.016*** | 0.031 | 0.038* |
| | (0.060) | (0.061) | (0.005) | (0.006) | (0.024) | (0.023) |
| Observations | 17,952 | 17,952 | 14,967 | 14,967 | 14,967 | 14,967 |
| Panel B | | | | | | |
| Treat 1yr | −0.046 | −0.045 | 0.004 | 0.001 | 0.031 | 0.040* |
| | (0.046) | (0.046) | (0.013) | (0.014) | (0.022) | (0.022) |
| Treat 2yr | 0.011 | 0.016 | −0.008 | −0.011 | 0.021 | 0.030 |
| | (0.071) | (0.072) | (0.006) | (0.007) | (0.025) | (0.026) |
| Treat 3yr | 0.002 | 0.004 | −0.008 | −0.011 | 0.056 | 0.071* |
| | (0.079) | (0.079) | (0.010) | (0.010) | (0.037) | (0.038) |
| Treat 4yr | −0.005 | 0.001 | −0.013 | −0.016 | 0.028 | 0.035 |
| | (0.075) | (0.076) | (0.010) | (0.011) | (0.031) | (0.028) |
| Treat 5yr | 0.028 | 0.035 | −0.015** | −0.017** | 0.037 | 0.038 |
| | (0.068) | (0.066) | (0.007) | (0.008) | (0.040) | (0.037) |
| Treat 6yr | 0.060 | 0.070 | −0.013 | −0.016 | 0.012 | 0.015 |
| | (0.073) | (0.074) | (0.009) | (0.009) | (0.032) | (0.030) |
| Treat 7yr | −0.115 | −0.103 | −0.019 | −0.021* | 0.000 | −0.012 |
| | (0.078) | (0.077) | (0.012) | (0.012) | (0.036) | (0.034) |
| Treat 8yr | −0.309*** | −0.295*** | −0.019** | −0.021** | 0.057 | 0.042 |
| | (0.087) | (0.087) | (0.009) | (0.010) | (0.044) | (0.043) |
| Treat 9yr | −0.432*** | −0.410*** | −0.016 | −0.019 | 0.084 | 0.055 |
| | (0.099) | (0.099) | (0.011) | (0.012) | (0.057) | (0.055) |
| Treat 10yr | −0.435*** | −0.409*** | −0.024* | −0.026* | −0.003 | −0.039 |
| | (0.096) | (0.096) | (0.013) | (0.014) | (0.058) | (0.057) |
| Observations | 17,952 | 17,952 | 14,967 | 14,967 | 14,967 | 14,967 |
| ID FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Control variables | No | Yes | No | Yes | No | Yes |
| Crash month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Crash year FE | Yes | Yes | Yes | Yes | Yes | Yes |

Note: This table shows the results of estimating Equation (1) in Panel A and Equation (2) in Panel B. Standard errors clustered at the road-segment level. Column (1) results for DiD with fixed effects only, while column (2) results for DiD with fixed effects controlling for covariates. Control variables are: GDP per capita, formal sector workers, population, average precipitation, average temperature.
***p < 0.01, **p < 0.05, *p < 0.1.

can vary over time due toa combination of factors. First, it takes a few years for contractor companies to finish more substantial road investments and effectively improve road safety. Second, publicly managed highways are more likely to face poor maintenance and accumulate over time the negative effects of road degradation on road safety. For the next analysis and robustness checks presented in the remainder of the paper, we consider the DiD full specification with covariates as benchmark.



**(a)** Deaths per kilometer **(b)** Deaths per crash

*Note*: This figure plots the coefficients of Equation (3). Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature. The year immediately prior to the concession is used as the reference period. Dashed lines show 95 percent confidence intervals.

**Fig. 2.** Event-study results
*Note*: This figure plots the coefficients of Equation (3). Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature. The year immediately prior to the concession is used as the reference period. Dashed lines show 95 percent confidence intervals.

### 5.2. Heterogeneous response

We now explore a few other aspects that could influence the possible causal channels between road concessions and road safety. For this purpose, we analyze whether concession contracts with road safety performance incentives had larger road safety benefits. We also conduct separate analyses to check whether road safety effects could be particularly concentrated in urban or rural areas or in day/night crashes. These specifications also serve to show how robust our main results are, since there are only small changes in the magnitude of the effects captured with different specifications.

A key element could be the difference in road safety incentives in concession contracts. As previously discussed in section3, companies in the third phase of road concessions in Brazil had stronger incentives to improve road quality because they were only allowed to start charging tolls after they met certain road improvements and safety performance goals. When we look at columns 1–2 of Table 5, we verify that concessions that had safety performance incentives in their contracts had marginally larger reductions in the number of deaths per crash on average. The results of Table 5 also show that road concessions were particularly effective in reducing fatality rates of crashes that occur in rural areas and at night. This indicates that improvements in road maintenance and highway signage have had only a small, if any, effect in reducing fatalities during the day and in urban contexts. Finally, as shown in TableA2 we find that the implementation of concessions reduced fatality rates for car and motorcycle crashes but not for truck.

### 5.3. Robustness checks

It is possible that the results presented so far do not capture spatial effects that might occur for example due to traffic diversion from tolled highways to other nearby highways. To address this, we show in Table6the results of the spatial DiD accounting for the possible existence of spatial spillovers. Table 6 shows the results for a spatial specification similar to those estimated in our main results but accounting for geographical interactions (Column 1) using a nearest neighborhood contiguity matrix, (Column 2) including spatial lags of covariates, and (Column 3), limiting spatial interactions to the two closest neighbors.

These results indicate the estimated indirect spillover effect is not significant. This suggests there is no evidence of traffic diversion to other road federal highways, although we cannot entirely rule out the possibility of traffic diversion to other types of roads, such as state highways and local roads. Moreover, it is noteworthy that the magnitude of the direct impact of road concessions on road safety is slightly larger when using a spatially explicit regression model that accounts for spatial dependence in the data. This result suggests that regression models commonly used in the literature might generate biased and underestimated effects.

In order to support a causal interpretation of the results presented thus far, we perform several robustness exercises. Table7reports the following modifications in Equations (1) and (2): In column (1), we substitute the year of crash fixed-effect and month of crash fixed-effect for an interaction term month × year of crash fixed-effect to control for time varying characteristics common to all road segments and for the

**Table 5**
Heterogeneity over day period, land use and phase period of concession.

| Death per road crash | | | | | | |
|---|---|---|---|---|---|---|
| | (1) Without road safety incentive | (2) With road safety incentive | (3) Urban area | (4) Rural area | (5) Day | (6) Night |
| | Panel A | | | | | |
| Treat | −0.018*** | −0.020** | −0.003 | −0.023*** | −0.007 | −0.026*** |
| | (0.007) | (0.010) | (0.004) | (0.007) | (0.005) | (0.008) |
| Observations | 9383 | 9158 | 13,400 | 14,156 | 14,607 | 14,265 |
| | Panel B | | | | | |
| Treat 1yr | −0.001 | 0.004 | −0.004 | 0.006 | −0.004 | −0.007 |
| | (0.009) | (0.024) | (0.005) | (0.020) | (0.006) | (0.018) |
| Treat 2yr | −0.003 | −0.020* | 0.007 | −0.017* | −0.008 | −0.015 |
| | (0.009) | (0.011) | (0.007) | (0.009) | (0.005) | (0.012) |
| Treat 3yr | −0.013 | −0.011 | −0.008 | −0.019 | −0.013** | −0.011 |
| | (0.008) | (0.016) | (0.006) | (0.012) | (0.005) | (0.018) |
| Treat 4yr | −0.015* | −0.020 | −0.005 | −0.024* | −0.004 | −0.033*** |
| | (0.008) | (0.019) | (0.005) | (0.013) | (0.011) | (0.012) |
| Treat 5yr | −0.020** | −0.021 | −0.001 | −0.027*** | −0.004 | −0.039*** |
| | (0.009) | (0.019) | (0.007) | (0.010) | (0.007) | (0.011) |
| Treat 6yr | −0.017** | | −0.013 | −0.020* | −0.013 | −0.023** |
| | (0.008) | | (0.009) | (0.011) | (0.008) | (0.011) |
| Treat 7yr | −0.029** | | −0.006 | −0.024 | −0.010 | −0.039*** |
| | (0.011) | | (0.009) | (0.015) | (0.009) | (0.014) |
| Treat 8yr | −0.023** | | −0.005 | −0.027** | −0.013 | −0.032** |
| | (0.009) | | (0.008) | (0.013) | (0.008) | (0.013) |
| Treat 9yr | −0.026** | | −0.015 | −0.019 | −0.011 | −0.032* |
| | (0.012) | | (0.010) | (0.014) | (0.008) | (0.017) |
| Treat 10yr | −0.035*** | | 0.004 | −0.041** | −0.014 | −0.045** |
| | (0.013) | | (0.012) | (0.017) | (0.011) | (0.018) |
| Observations | 9383 | 9158 | 13,400 | 14,156 | 14,607 | 14,265 |
| ID FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Crash month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Crash year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* This table shows the results of estimating Equation (1) in Panel A and Equation (2) in Panel B. Standard errors clustered at the road-segment level. Presented results account for fixed effects controlling for covariates. Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

**Table 6**
Robustness check: Including spatial spillovers, 2007–2017.

| | Deaths per crash | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Treat (Direct effect) | −0.018** | −0.018** | −0.025*** |
| | (0.007) | (0.007) | (0.008) |
| Treat*W (Indirect effect) | 0.003 | 0.002 | 0.011 |
| | (0.006) | (0.006) | (0.008) |
| Observations | 14,967 | 14,967 | 14,967 |
| ID FE | Yes | Yes | Yes |
| Crash month FE | Yes | Yes | Yes |
| Crash year FE | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| Spatial Controls | No | Yes | Yes |
| Weight matrix | Binary | Binary | 2-Nearest |

*Note:* This table shows the results of estimating Equation (4). Standard errors clustered at the road-segment level. Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

effects of seasonality on fatality outcomes. In column (2), we add linear trend within states, while in columns (3) we add linear trend within road segments in order to capture potential diverging trends across different states and road segments, respectively. The inclusion of id specific linear time trends enables an assessment of the parallel trends assumption underlying this differences-in-differences approach (Angrist and Pischke, 2009). Next, to control for seasonal fluctuations in the flow of trucks and others vehicles, we add an indicator variable for the predominant agricultural harvest month in each road segment in column (4), a dummy variable indicating the month/year when toll collections started in column (5), and the distance to nearest large city interacted with linear trends, in column (6). In column (7), we show that the negative impact of the policy on fatality rate remain after trimming the tails of our sample (bottom/top 10% in total of road crashes per road segment). Finally, in column (8), we show that our estimates are robust to different definitions of the comparison group, by using all federal road segments in the country.

Even after accounting for multiple robustness checks in Table 7, including divergent trends across road segments and for the potential influence of other variables omitted from the general specification adopted, we find that point estimates are strikingly similar across a number of alternative specifications. This set of results support the hypothesis that the estimated safety benefits from road concessions is not a mere statistical coincidence and it increases the reliability of our results. In summary, several regression models and specifications gives us robust evidence to claim that the road concession program in Brazil was able to shift the trend of fatality rates in federal highways and effectively improve road safety performance.

**Table 7**
Robustness check: Additional Controls and Fixed Effects.

| | Deaths per Crash | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Panel A | | | | | | | |
| Treat | −0.017*** | −0.014*** | −0.010* | −0.019*** | −0.012** | −0.016*** | −0.018** | −0.016*** |
| | (0.006) | (0.005) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.005) |
| Observations | 14,967 | 14,967 | 14,967 | 14,798 | 14,967 | 14,967 | 13,185 | 26,438 |
| | Panel B | | | | | | | |
| Treat 1yr | 0.001 | 0.004 | 0.007 | −0.001 | 0.001 | 0.002 | 0.001 | 0.001 |
| | (0.014) | (0.013) | (0.011) | (0.011) | (0.014) | (0.013) | (0.018) | (0.013) |
| Treat 2yr | −0.011 | −0.008 | −0.004 | −0.014** | −0.012 | −0.010 | −0.014* | −0.010 |
| | (0.007) | (0.006) | (0.006) | (0.006) | (0.009) | (0.007) | (0.009) | (0.007) |
| Treat 3yr | −0.011 | −0.008 | −0.005 | −0.013* | −0.013 | −0.010 | −0.009 | −0.009 |
| | (0.010) | (0.009) | (0.009) | (0.007) | (0.012) | (0.010) | (0.013) | (0.010) |
| Treat 4yr | −0.016 | −0.013 | −0.010 | −0.019** | −0.018 | −0.015 | −0.019 | −0.019* |
| | (0.011) | (0.010) | (0.011) | (0.008) | (0.012) | (0.011) | (0.014) | (0.010) |
| Treat 5yr | −0.018** | −0.020** | −0.023 | −0.024** | −0.019* | −0.020** | −0.017** | −0.016*** |
| | (0.008) | (0.008) | (0.017) | (0.010) | (0.010) | (0.008) | (0.009) | (0.006) |
| Treat 6yr | −0.016 | −0.018* | −0.019 | −0.017* | −0.017 | −0.017* | −0.018 | −0.014* |
| | (0.010) | (0.010) | (0.017) | (0.009) | (0.011) | (0.009) | (0.011) | (0.008) |
| Treat 7yr | −0.021* | −0.023* | −0.025 | −0.023** | −0.022* | −0.022* | −0.021 | −0.018* |
| | (0.012) | (0.013) | (0.020) | (0.012) | (0.013) | (0.012) | (0.013) | (0.010) |
| Treat 8yr | −0.021** | −0.024** | −0.027 | −0.023** | −0.022* | −0.023** | −0.016 | −0.020** |
| | (0.010) | (0.010) | (0.026) | (0.009) | (0.012) | (0.009) | (0.010) | (0.009) |
| Treat 9yr | −0.019 | −0.023** | −0.026 | −0.021** | −0.020 | −0.021* | −0.013 | −0.020* |
| | (0.012) | (0.011) | (0.029) | (0.010) | (0.013) | (0.011) | (0.014) | (0.011) |
| Treat 10yr | −0.027* | −0.026** | −0.035 | −0.026** | −0.028* | −0.028** | −0.024 | −0.033*** |
| | (0.014) | (0.013) | (0.034) | (0.013) | (0.014) | (0.013) | (0.016) | (0.012) |
| Observations | 14,967 | 14,967 | 14,967 | 14,798 | 14,967 | 14,967 | 13,185 | 26,438 |
| ID FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Month*year of Crash FE | Yes | | | | | | | |
| Control variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Crash month FE | | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Crash year FE | | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Distance to nearest large city | | | | | | Yes | | |
| Including a toll start dummy | | | | | Yes | | | |
| Harvest month | | | | Yes | | | | |
| ID linear trend | | | Yes | | | | | |
| State linear trend | | Yes | | | | | | |
| Triming the tails of the sample | | | | | | | Yes | |
| Concerning all states | | | | | | | | Yes |

*Note*: This table shows the results of estimating Equation (1) in Panel A and Equation (2) in Panel B. Standard errors clustered at the road-segment level. Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

## 6. Conclusion

In this paper we analyzed the impact of highway concessions on road safety performance using detailed daily data aggregated by month and year on road crashes on Brazilian federal highways between 2007 and 2017. Using a difference-in-differences approach, we provide causal evidence that the implementation of highway concessions effectively improved multiple road safety measures, including crash fatality rates, number of people and vehicles involved in crashes.

Our results show a small but significant road safety benefit from the road concessions in Brazil. For every one thousand crashes, procured roads had on average 16 fewer deaths then publicly managed highways each year. These results consider fixed-effects controlled for several covariates known in the literature to influence traffic volumes and road crashes. The results are also consistent after considering multiple robustness checks and after looking at different heterogeneity effects by period of the day, whether road segments are located in rural or urban areas, and the time of the year when crashes occurred.

This paper advances previous studies by presenting how a difference-in-differences approach can be used to robustly test the causal impact of highway concessions on road safety performance. It also shows that concession contracts with safety-based incentives can substantially improve road safety outcomes of procured highways. Moreover, the analysis presented in this paper allowed us to determine for the first time the temporal heterogeneity in the road safety benefits of highway concessions. In the case of procured federal highways in Brazil, our findings indicate that such benefits only start to show a few years after concessions are implemented depending on the outcome analyzed, and that these effects become marginally larger for every additional year of a concession.

One limitation of this study is that it did not consider concessions implemented before the year 2000 and after 2019 due to the lack of sufficient data. Moreover, there is no historical data on traffic volume, so our analysis had to control for traffic levels indirectly using other covariates that are correlated with traffic volume. Finally, these results cannot be directly generalized to other countries. Road concession policies can vary between countries and the effects of concessions on road safety are dependent on the terms of concession contracts. This raises the need for future studies to investigate whether and how concessions can affect the quality and safety of highways in other contexts.

While we have not found an increase in crashes in other federal highways (traffic diversion effects), we cannot rule out the possibility of indirect spillover effects to local roads due to the lack data. Finally, our results indicate a 3.8% increase in the number of injured people per crashes in procured roads, but we cannot determine whether this is a result of offsetting behavior (Peltzman effect). If the this were the case, it is possible that the road safety benefits of road concessions would have been even larger had it not been for this compensating effect. A few broad lessons can be drawn from this study. From a methodological standpoint, this study illustrates how difference-in-differences models could provide a robust method for future studies to overcome endogeneity issues when evaluating the causal impact of road concessions on road safety. Moreover, the results of this paper show that regression models commonly used in the literature might underestimate the road safety benefits of road concessions by overlooking spatial dependence in the data.

From a policy perspective, this study provides strong evidence that the social and eco-nomic evaluation of road concessions should consider how this type of policy can effectively improve road safety performance. Moreover, our findings suggest that governments should not expect to reap such safety benefits immediately after a concession is implemented. We show that these benefits tend to increase over time and that the introduction of safety-based incentives in concession contracts can substantially improve road safety outcomes of pro-cured roads. Finally, the results of this paper could help calibrate contract parameters used in future bidding processes of the road concession program in Brazil. Further research is needed to examine the extent to which the magnitude of the road safety benefits of road concessions varies in other countries, particularly in low- and middle-income contexts where there is still little evidence on the subject.

## CRediT authorship contribution statement

**Pedro Jorge Alves:** Conceptualization, Methodology, Formal Analysis, Investigation, Data Curation, Writing – Original Draft, Writing – Review & Editing, Visualization. **Lucas Emanuel:** Conceptualization, Methodology, Formal Analysis, Investigation, Data Curation, Writing – Original Draft, Writing – Review & Editing, Visualization, **Rafael H.M. Pereira:** Writing – original draft, conceived the research questions and designed the study, interpreted the results and wrote the manuscript, read and revised the final manuscript.

## Appendix

**Table A1**
Other outcomes - road crash dataset

| | People per km | Deaths per km | Vehicles per km | Soft injured per km | Serious injured per km |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Panel A | | | | |
| Treat | −0.055 | −0.001 | −0.041 | 0.030* | 0.009 |
| | (0.151) | (0.002) | (0.139) | (0.016) | (0.011) |
| Observations | 17,952 | 17,952 | 17,952 | 14,967 | 14,967 |
| | Panel B | | | | |
| Treat 1yr | −0.108 | 0.001 | −0.095 | 0.017 | 0.023** |
| | (0.108) | (0.002) | (0.100) | (0.017) | (0.011) |
| Treat 2yr | −0.021 | 0.003 | −0.014 | 0.015 | 0.015 |
| | (0.174) | (0.003) | (0.157) | (0.019) | (0.013) |
| Treat 3yr | −0.058 | 0.000 | −0.047 | 0.053* | 0.017 |
| | (0.196) | (0.002) | (0.183) | (0.030) | (0.013) |
| Treat 4yr | −0.037 | −0.001 | −0.025 | 0.043** | −0.007 |
| | (0.185) | (0.002) | (0.169) | (0.019) | (0.015) |
| Treat 5yr | 0.037 | −0.002 | 0.071 | 0.025 | 0.013 |
| | (0.152) | (0.002) | (0.143) | (0.027) | (0.014) |
| Treat 6yr | 0.146 | −0.001 | 0.158 | 0.005 | 0.010 |

(*continued on next page*)

**Table A1** (*continued*)

| | People per km | Deaths per km | Vehicles per km | Soft injured per km | Serious injured per km |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Panel A | | | | |
| | (0.172) | (0.002) | (0.159) | (0.021) | (0.013) |
| Treat 7yr | −0.177 | −0.005** | −0.142 | −0.019 | 0.007 |
| | (0.171) | (0.003) | (0.156) | (0.028) | (0.015) |
| Treat 8yr | −0.574*** | −0.007*** | −0.526*** | 0.038 | 0.004 |
| | (0.186) | (0.002) | (0.174) | (0.032) | (0.018) |
| Treat 9yr | −0.816*** | −0.010*** | −0.754*** | 0.062 | −0.007 |
| | (0.210) | (0.003) | (0.197) | (0.043) | (0.020) |
| Treat 10yr | −0.790*** | −0.009*** | −0.754*** | −0.010 | −0.029 |
| | (0.201) | (0.003) | (0.189) | (0.045) | (0.024) |
| Observations | 17,952 | 17,952 | 17,952 | 14,967 | 14,967 |
| ID FE | Yes | Yes | Yes | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes | Yes |
| Crash month FE | Yes | Yes | Yes | Yes | Yes |
| Crash year FE | Yes | Yes | Yes | Yes | Yes |

Note: This table shows the results of estimating Equation (1) in Panel A and Equation (2) in Panel B. Standard errors clustered at the road-segment level. Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

**Table A2**
Other outcomes: fatality by vehicle type

| | Car death by crash | Motorcycle death by crash | Truck or bus death by crash |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Panel A | | |
| Treat | −0.002** | −0.003*** | −0.002 |
| | (0.001) | (0.001) | (0.001) |
| | Panel B | | |
| Treat 1yr | −0.0004 | −0.001 | 0.0001 |
| | (0.001) | (0.0004) | (0.001) |
| Treat 2yr | −0.001 | −0.001 | −0.0002 |
| | (0.001) | (0.0005) | (0.001) |
| Treat 3yr | −0.003*** | −0.005*** | −0.004** |
| | (0.001) | (0.001) | (0.002) |
| Treat 4yr | −0.003** | −0.004*** | −0.005** |
| | (0.001) | (0.001) | (0.002) |
| Treat 5yr | −0.002 | −0.002* | 0.001 |
| | (0.001) | (0.001) | (0.003) |
| Treat 6yr | −0.001 | −0.001 | −0.0003 |
| | (0.001) | (0.001) | (0.001) |
| Treat 7yr | −0.001 | −0.001 | −0.0003 |
| | (0.001) | (0.001) | (0.001) |
| Treat 8yr | −0.002 | −0.001 | −0.0003 |
| | (0.001) | (0.001) | (0.002) |
| Treat 9yr | −0.003 | −0.008*** | −0.007** |
| | (0.002) | (0.002) | (0.003) |
| Treat 10yr | −0.010*** | −0.008*** | −0.010*** |
| | (0.003) | (0.002) | (0.003) |
| Observations | 14,967 | 14,967 | 14,967 |
| ID FE | Yes | Yes | Yes |
| Crash month FE | Yes | Yes | Yes |
| Crash year FE | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |

Note: This table shows the results of estimating Equation (1) in Panel A and Equation (2) in Panel B. Standard errors clustered at the road-segment level. Control variables are: GDP per capita, formal sector workers, population, average precipitation and average temperature.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

**Table A3**
Investment level recorded in concession contracts

| Highway stretch concessions | Concession phase | Investment (R$ Millions) | | | | |
|---|---|---|---|---|---|---|
| | | Initial Capital | Environmental studies | Duplication | Traffic safety | Technological |
| BR- 116/SP/PR | 2° phase | 42.00 | – | – | – | 0.84 |
| BR-116/PR/SC | 2° phase | 22.00 | – | – | – | – |
| BR-381/MG/SP | 2° phase | 39.00 | – | – | – | 0.79 |

(*continued on next page*)

**Table A3** (*continued*)

| Highway stretch concessions | Concession phase | Investment (R$ Millions) | | | | |
|---|---|---|---|---|---|---|
| | | Initial Capital | Environmental studies | Duplication | Traffic safety | Technological |
| BR-393/RJ | 2° phase | 21.00 | – | – | – | – |
| BR-116/376/PR and 101/SC | 2° phase | 34.00 | – | – | – | 0.64 |
| BR-101/RJ | 2° phase | 20.00 | – | – | – | – |
| BR-153/SP | 2° phase | 30.00 | – | – | – | – |
| BR-163/MT | 3° phase | 2400.00 | 11.81 | 425.00 | 1.58 | 1.04 |
| BR-101/RJ | 3° phase | 120.00 | – | – | 0.54 | 0.42 |
| BR-153/TO/GO | 3° phase | 221.00 | 8.67 | 425.00 | 1.16 | 1.03 |
| BR-101/ES/BA | 3° phase | 150.00 | – | – | 0.89 | 0.62 |
| BR-163/MS | 3° phase | 307.00 | 11.75 | 500.00 | 1.58 | 1.31 |
| BR-116/BA and BR-324/BA | 2° phase | 102.00 | – | – | 0.69 | – |
| BR-050/GO/MG | 3° phase | 162.00 | 6.06 | 500.00 | 0.81 | 0.68 |
| BR-040/DF/MG | 3° phase | 395.00 | 13.00 | 650.00 | 1.33 | 1.84 |
| BR-60, BR-153 and BR-262-DF/GO/MG | 3° phase | 380.00 | 16.32 | 600.00 | 2.19 | 1.61 |

*Note*: SeeConcession contractsfor more information. Fill with “-” represents missing information from the concession reports.

**Table A4**
Description of the data

| Variable | Description | Source |
|---|---|---|
| ***Socioeconomic variables*** | | |
| GDP per capita | Yearly municipality gross national product at current prices (R$) per population, aggregated at the level of municipalities that intersect with each highway segment in our sample. | IBGE |
| Population | Yearly municipality population aggregated at the level of municipalities that intersect with each highway segment in our sample. | IBGE |
| Agricultural Seasonality | Month (most commonly occurring value) that has the largest collection of any agricultural product based on the municipalities that inter- sect with each highway segment in our sample | IBGE |
| Formal sector workers | Yearly number of formal employees per munic- ipality, aggregated at the level of municipali- ties that intersect with each highway segment in our sample. | RAIS |
| Distance to nearest large city | Euclidean distance from the road segment cen- troid to the closest municipality with more than 750 thousand inhabitants | IBGE |
| ***Temperature variables*** | | |
| Average level of rain | Monthly average of rain level per segment | Matsuura and J Willmott (2018) |
| Average temperature level | Monthly average of temperature in region | Matsuura and J Willmott (2018) |
| ***Accidents Characteristics*** | | |
| Crashes per km | Monthly number of crashes per Kilometer at the highway segment level in our sample | PRF |
| Deaths per road crash | Monthly number of deaths per road crash in each road segment in our sample | PRF |
| Injured per road crash | Monthly number of injured people per road crash in each road segment in our sample | PRF |

*Source:* Own elaboration. Matsuura and J Willmott (2018) means Terrestrial Air Temperature and Terrestrial Precipitation: 1900–2017 Gridded Monthly Time Series, version 5.01. IBGE means Brazilian Institute of Geography and Statistics, and PRF means Federal Highway Police Department. RAIS means Annual Report of Social Information.

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