Should Congested Cities Reduce their Speed Limits? Evidence from São Paulo, Brazil

Amanda Ang Peter Christensen Renato Vieira*

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Abstract

Cities are experimenting with more stringent speed limits in an effort to reduce road accidents, which are the leading cause of unnatural deaths worldwide. The effectiveness of these policies is of particular interest in developing countries, where a disproportionate share of accident damages occur but also where congestion creates heightened concern about speed regulations. We evaluate a speed limit reduction program in São Paulo, Brazil using a dynamic event study design and measurements of 125 thousand traffic accidents, 38 million traffic tickets issued by monitoring cameras, and 1.4 million repeat observations of real-time trip durations before and after a regulatory change. We estimate that the program reduced accidents by 21.7% on treated roads and resulted in 1,889 averted accidents within the first 18 months, with larger effects on roads with camera-based enforcement. The program also affected travel times on treated roads (5.5%), though the social benefits from reduced accidents are at least 1.77 times larger than the social costs of longer trip times. The benefits of accident reductions accrue largely to lower income pedestrians and motorcyclists, indicating that speed limit reductions may have important impacts on low income residents in developing country cities.

Key words: Speed Limit Changes, Road Accidents, Transportation, Congestion
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*Ang: Department of Economics, University of Southern California, 300 Kaprielian Hall, 3260 Vermont Avenue, Los Angeles, California 90089 (email: qang@usc.edu); Christensen: Department of Agricultural and Consumer Economics, University of Illinois, 431 Mumford Hall, 1301 W. Gregory, Urbana, Illinois 61801 (email: pchrist@illinois.edu); Vieira: Department of Agricultural and Consumer Economics, University of Illinois, 431 Mumford Hall, 1301 W. Gregory, Urbana, Illinois 61801 (email: renato.sv.1988@gmail.com). We are grateful for helpful comments from Andre Chagas, Sandy Dall'erba, Edward Glaeser, Eduardo Haddad, Marieke Kleemans, Gabriel Kreindler, Mary Arends-Kuenning, Adam Osman, Julian Reif, Nick Tsivanidis, and Matthew Turner. We thank participants at the Federal University of São Carlos Economics Seminar, the University of São Paulo Economics Seminar, the University of Illinois pERE Seminar, the Urban Economics Association Annual Meeting (2018), and the Cities and Development Workshop (SAIS) for helpful comments. We thank computer scientists in the UIUC Big Data in Environmental Economics and Policy Research Team for excellent programming work. In particular, we acknowledge Stephen Sullivan, Elena Lee, Nate McCord, Surya Tadigadapa, and Abhishek Banerjee. This work was made possible by generous support from the Lemann Institute for Brazilian Studies and the National Center for Supercomputing Applications. All data and replication files will be made available in the following data repository: https://github.com/uiuc-bdeep/speed_change (DOI: 10.5281/zenodo.2000257). All errors are our own.

Road injuries are the leading cause of unnatural deaths worldwide, representing 27.1% of all human unnatural deaths and approximately one third of the external costs related to private transportation (WHO, 2015, Parry and Small, 2005, Parry et al., 2007). Road safety is particularly problematic in the developing world. While low- and middle-income countries account for only half of world's vehicles, 90% of road fatalities occur in those countries. In an effort to stem worldwide growth in road accidents, the UN World Health Organization (WHO) has declared 2010-2020 the "Decade of Road Safety" and has promulgated the ambitious goal of averting 5 million road fatalities during this period. Cities around the world have responded by reducing speed limits and improving speed limit enforcement on urban roads.[1] There is evidence that well-enforced speed limits tend to reduce vehicle speeds and damages from road accidents (Archer et al., 2008, Wilmot and Khanal, 1999, Musicant et al., 2016). However, reducing speed flows may increase travel costs as commuters face longer journeys. The empirical literature is less clear on the magnitude and significance of these effects (Archer et al., 2008). As a result, the net benefit of increasing the stringency of speed regulations is still largely an open question, particularly in developing country cities where they could have the greatest impact on both accident reductions and increased drive times. Not surprisingly, many highly congested cities have been reluctant to adopt reduced speed limits.

This paper brings together a number of novel data sources to provide estimates of the effects of a major speed limit reductions program in one of the most congested cities of the developing world.[2] In 2015, the government of São Paulo, Brazil, enacted a policy program that successively reduced speed limits on the city's highways (Marginais) and arterial roads. However, the policy became highly contentious and in early 2017 the speed limits on urban highways were reverted to pre-reduction levels. We exploit exogenous variation in the timing of speed limit reductions adopted on each road in São Paulo and a subsequent reversal (speed limit increase to pre-policy levels) as a natural experiment, identifying effects using a semi-dynamic event study design (Abraham and Sun, 2018, Borusyak and Jaravel, 2016). We utilize detailed data from the universe of traffic accidents

[1] OECD/ITF (2018) presents a comprehensive review of speed limit policy changes in recent years.
[2] São Paulo is ranked the 5th most congested city in the world by INRIX: http://inrix.com/scorecard/

during the study period and trip durations queried in real-time from Google's transit API as the policy was reversed.[3] Speed limit reductions could alter the travel and routing decisions of residents, potentially reducing travel on treated roads or displacing accidents from treated roads to alternate routes. We examine changes in the volume of individual traffic violations that are unrelated to speeding to test for substitution or route switching in response to the policy. We also analyze changes in individual speeding tickets from on-road traffic cameras to learn about how drivers adjust to the regulatory change.

Our results indicate that the 2015 speed limit program reduced road accidents by 21.7% on treated road segments, resulting in approximately 1,889 averted road accidents and 104 averted fatalities within the first 18 months of adoption. The speed limit reduction appears to have had a larger impact on fatalities than gun control policies aimed at reducing violent crime, which is the other primary contributor to unnatural deaths in the city.[4] São Paulo's system of camera-based speed enforcement may have been an important factor in achieving reductions – we find that roads where speed limit reductions occur alongside contemporaneous onset of camera-based enforcement experienced an additional 11.5-11.8 percentage point reduction in accidents. We find that the 2017 speed limit increase also had a non-negligible impact on travel speeds. We estimate a 5.5% effect on travel speeds on treated highways during the reversal, which is consistent with estimates from studies in developed countries and implies an econometrically as well as economically significant social cost (Musicant et al., 2016, van Benthem, 2015).[5] We compare the social cost of increased trip duration from reduced speeds with the benefits of reduced accidents using standard parameters for the value of time, the value of statistical life, and non-fatal costs of accidents.[6] We estimate that the benefits associated with

[3]API stands for application program interface.

[4]Recent papers have identified an 11-13% reduction in fatalities as a result of a disarmament and conceal carry policies on fatalities in Brazilian cities over the past 15 years (Schneider, 2018, Cabral, 2016, dos Santos and Kassouf, 2013). See Figure 1 for a comparison of unnatural death rates attributed to road accidents versus violent crime in São Paulo.

[5]In this paper, we use the terms commute times, travel times, drive times, and trip times to refer to the duration of private, motorized trips taken throughout the city of São Paulo. Our results are representative of trips taken in São Paulo on a week day, taken from an origin-destination travel survey (Table A.3). They are not particular to work trips.

[6]We utilize accident-specific data and costs to vehicles and to non-fatal victims from IPEA (2016). We value fatalities using the Value of Statistical Life (VSL) of USD$ 1.695 Million[7] estimated for Brazil by Viscusi and Masterman (2017). We use 50% of individual after-tax hourly wages for VOT Wolff (2014)

reduced accident damages are at least 1.77 times larger than the costs of longer drive times.

The current paper builds upon prior studies that have evaluated the positive and negative economic impacts of speed limit changes. Two notable examples are van Benthem (2015) and Ashenfelter and Greenstone (2004), which study speed limit increases on American highways in the 1980's and 1990's. Our paper extends this literature in a number of ways. First, while van Benthem (2015) finds that the social costs of increasing speed limits interstate highways in the United States are 2-7 times larger than the benefits. However, it is not clear that the available findings will generalize to a developing country city such as São Paulo, where exacerbating urban congestion is a concern when considering more stringent speed limits. The current paper provides estimates of the impacts of speed limit changes on the key transportation corridors of one of the most congested cities in the world. Our findings indicate that speed limit reductions are, in fact, benefit-enhancing in São Paulo and provide evidence on enforcement and distributional concerns that may be common in many developing country cities. As is true in the case of most road safety policies, urban highways and arterial roads were not randomly selected for speed limit reductions in the São Paulo program. We estimate cohort-specific average treatment effects (CATT) to support the internal validity of our event study results (Abraham and Sun, 2018). However, we express a note of caution regarding the extrapolation of specific estimates from São Paulo roadways to those in other cities, where impacts may depend on local conditions.

This study also builds on recent work that uses real-time web routing services to study the impact of urban transportation policies and congestion in developing country cities (Hanna et al., 2017, Akbar et al., 2018, Akbar and Duranton, 2017).[8] With respect to analyzing the impact of changing the speed limit on travel time, our study is novel in combining a city-wide sample of representative trips from an origin-destination travel survey with repeated observations of real-time trip durations that capture the effect of

as well as trip-specific VOT VTPI (2016).

[8]Hanna et al. (2017) evaluates the reduced-form effects of a policy change and is more similar to this study. Akbar and Duranton (2017) and Akbar et al. (2018) study congestion and mobility using structural methods.

changing traffic conditions. By simulating this set of representative trips before and after the speed limit change, we are able to attribute policy impacts to individuals who vary in the types of trips that they are taking, in their value of time, and in their accident risk. Our results indicate that this is an important methodological choice that can have first-order impacts on estimates of commute costs. In our setting, we find that failing to account for (otherwise unobservable) heterogeneity in the population of travelers who utilize treated roads would attenuate our cost estimates by more than 35%.

This paper makes an additional contribution by examining the extent to which speed limit reductions have progressive/regressive impacts, which is an important and unresolved issue in the transportation literature (van Benthem, 2015). By pairing the travel survey with real-time congestion data, we find that the costs associated with increased travel time (slower trips) accrue disproportionately to wealthier individuals that have higher rates of private vehicle ownership and tend to commute on treated roads. This is expected. However, we also find evidence of a striking difference in the distribution of the benefits from accident reductions across income groups: 86% of the benefits from reduced accident damages accrue to low-income residents, who bear a disproportionate share of the accident risk as pedestrians and motorcyclists. Our analysis reveals that speed limit reductions likely have strongly progressive impacts in São Paulo and could have important effects on reducing unnatural deaths in other urbanizing regions where low-income residents rely upon motorcycle or other risky modes of transport.

The remainder of the paper is structured as follows: Section 1 describes the speed limit changes investigated in our study, section 2 details the data sets used for our analysis, and section 3 presents our main empirical strategies and reduced-form estimates. In Section 4, we compare the costs and benefits of the policies, including our distributional analysis. Section 5 concludes.

1 Speed Limit Reductions of 2015 and 2017 Reversal

With more than 20 million residents, São Paulo is one of the largest and most heavily congested metropolitan areas in the world. Of the 30 million motorized trips made on an average weekday in São Paulo, approximately half are made by public transportation and the other half by private modes (METRO, 2013). While the distance of the average motorized trip is 8 km, the average duration is 51 minutes. Travel-related injuries are also a major problem in the city. During the 2005-2014 period, there were on average 12.5 deaths per 100,000 residents in the Metropolitan Area of São Paulo, which is 56.2% higher than the average among OECD countries (DATASUS, 2018).

Figure 1 illustrates differences in magnitude and composition of road fatalities in São Paulo relative to New York City, which are the largest cities in Brazil and the USA. Between 2012 and 2016, road fatalities per resident were almost three times larger in São Paulo than in New York. The composition of road fatalities also differs, with a larger share of motorcycle driver deaths in the Brazilian city. Road accidents and physical violence are the two primary causes of unnatural deaths in both cities. Figure 1 shows that road fatalities represent the larger contributor to fatality risk in São Paulo, whereas physical violence is the more important factor in New York. Despite that fact, physical violence has received a disproportionate share of policy attention in São Paulo over the past decade (Schneider, 2018, Cabral, 2016, dos Santos and Kassouf, 2013).

As a result of growing awareness of the road safety problem, the City of São Paulo joined the World Health Organization's "Decade of Action for Road Safety" by setting a target for reducing road mortality in the city to 6 deaths per 100,000 inhabitants by 2020 (CET, 2016). In 2015, the city implemented a set of speed limit reductions on major urban roads throughout the city. The rationale behind the focus on speed limit regulations in São Paulo is the growing body of evidence of a strong correlation between vehicle speeds and the probability and severity of road accidents (Vadeby and Forsman, 2017, Musicant et al., 2016, Li and Graham, 2016, Sayed and Sacchi, 2016, Pauw et al., 2014, Elvik, 2013, Archer et al., 2008, OECD/ECMT, 2006, Woolley, 2005).

The speed limit program had two major phases: (1) On July 20, 2015, the speed limits on the main urban highways of the city (Marginais) were reduced from 90 km/h to 70 km/h. (2) Over the following six months, speed limits on arterial roads were successively reduced from 60 km/h to 50 km/h (CET, 2016). This culminated in speed limit reductions on approximately 570 km of roadways, which constitute 29% of private vehicle VMT and about 40% of the city's accidents.[9] In the year prior to the speed limit reductions (2014), there were 9,401 accidents and 453 road fatalities on treated segments, representing 39.9% and 38.4% of the city's total, respectively. During the year after the speed limit reductions but before the reversal (2016), the number of accidents and fatalities decreased to 5,425 and 311, or 33.7% and 36.3% of the city's total. The speed limit reversal was only implemented on the Marginais Highways, which had represented 3.17% of São Paulo's accidents in 2016. That fraction increased to 3.47% in the year following the reversal.

Another major component of the road safety program involved the concomitant expansion of speed control cameras, which is the dominant method of speed limit enforcement in Brazil.[10] Automated traffic cameras register vehicle speeds and issue speeding tickets to vehicles that are traveling above the speed limit (CET, 2016).[11] Figure 2 plots the growth in the number of speed control cameras installed in São Paulo between 2014 and 2016. The total number of cameras grew from 397 in the beginning of 2015 to 733 by the end of that year, an increase of 83%. A large fraction of the new cameras were installed on arterial roads and highways where speed limits reductions were also implemented.[12] While 55.6% of the cameras installed before July 20, 2015 were located on road segments that would undergo speed limit reductions by the new program, that share increased to

[9]Figure A.2 in Appendix A maps the roads where speed limits were reduced, indicating the final speed limit in each treated road segment after the 2015 changes. Figure A.3 plots the cumulative length of roads which were treated during the study period. The total length of treated segments increased at a near constant rate, suggesting that the implementation of speed limit reductions was relatively evenly distributed throughout the second half of 2015.

[10]Human policing accounts for fewer than 8% of the 2-3 million tickets issued for failure to comply with the São Paulo driving restriction and none of the 5-6 million speeding tickets issued per year in the City.

[11]The fees for speed limit violations are defined by the National Traffic Code (Código de Trânsito Brasileiro). In 2015, the penalties for speeding were: a) R$85.13 for speeding up to 20% above the speed limit, b) R$127.69 if 20%-50% above the limit, c) R$574.63 if speeding above 50% the limit. In November 2016, these amounts were updated to respectively R$130.16, R$195.23 and R$884.41

[12]Figure A.5 in Appendix A compares the location of cameras installed before and after the first speed limit reduction implemented on July 20, 2015, and Figure A.6 maps the location of the new cameras installed after that same date.

69.1% as additional cameras were installed. The design of the program resulted in two different treatment conditions under the new speed limit regime: (1) road segments where the speed limit reduction occurred in the absence of camera-based enforcement and (2) road segments where the speed limit reduction occurred in the presence of camera-based enforcement.[13]

In January 2017, the speed limits on the Marginais Highways were returned to pre-2015 levels (raised back from 70 km/h to 90 km/h). This measure was a major campaign promise of the winning mayoral candidate during the election of 2016 and was adopted within the first month of his election.[14] Contrary to the speed limit reduction of 2015, the reversal was restricted to the Marginais Highways and the speed limits of the arterial roads remained unchanged from their post-reduction levels.

2 Data

This section describes the data that we utilize to evaluate the impacts of speed limit changes on accidents, commuting time, traffic volume and traffic violations.[15]

Road Segments with Speed Limit Changes

During the period when speed limit reductions were implemented in 2015, the traffic agency of São Paulo (CET) posted a series of announcements on its website to provide details about each upcoming speed limit change. These announcements described the exact road segments which would have their speed limit reduced and the date on which the change would be implemented. These announcements also described the existing speed limit of these segments and the new limits that would be adopted.

We collect all such reports from the agency website and, using their exact geocoded location and date, identify the roads and timing of speed limit changes. In total, the 37

[13] In other ongoing work, we examine the specific effect of camera-based enforcement on driver behavior.

[14] The debate about urban speed limits was a contentious topic during the political campaign. Examples of the debate during the electoral campaign can be found at goo.gl/Ju38zV, goo.gl/LRFWsA and goo.gl/bXW82N.

[15] Figure A.9 in Appendix A summarizes these datasets and their temporal coverage.

reports used in our study describe the speed limit changes implemented on 202 different roads in São Paulo, including approximately 570 km of treated roads.[16] We divide treated and non-treated roads into segments of 400 meters in order to capture the effects of camera-based enforcement and other heterogeneous conditions along roadways. The resulting panel consists of a segment-specific count of accidents per month.

Traffic Accidents

We construct a panel dataset of accidents by road segment-month using data from the São Paulo Traffic Agency's yearly reports of road accidents from 2012 to 2017. These reports contain information for all road accidents registered within the city of São Paulo by the police departments, traffic agents, hospitals, and morgues (CET, 2017).[17] For each accident, we observe: the exact location, time, number of victims, vehicles involved, the severity of injuries for each victim (unharmed, injured, dead), the alcohol level of drivers, victim's age, gender and educational attainment, and the types of vehicles involved in the accident (car, van, motorcycle, etc.).

Panel A of Table 1 reports descriptive statistics for the accidents included in our dataset. Between 2012 and 2017, 125,769 accidents were recorded in São Paulo, involving 146,991 injured victims and 5,997 fatalities. The total number of accidents per year declined throughout São Paulo during this period, a secular trend that is observed before the implementation of speed limit reductions. In the following section, we show that secular declines are highly similar for roads where the speed limit reductions occurred and a large set of never treated roads in São Paulo, which allows for the addition of never-treated segments as controls to our base event study design (Pre-trends are shown in Panel A of Figure 4.). A variety of factors have been proposed to explain the overall downward trend, including improved safety features in vehicles and stringent drinking and driving legislation implemented in 2008 (*"Lei Seca"*) (Campos et al., 2013).[18]

[16] Table A.1 in Appendix A describes the 37 announcements used in our study.

[17] We requested access to the individual accidents data from these reports using the Brazilian Law of Access to Information (Lei de Acesso à Informação). The requests for our project were registered as LAI request 21,151 and opened on March 31, 2017. LAI request 25,968 opened on November 1, 2017, and LAI request 30,818 opened on May 23, 2018.

[18] *"Lei Seca"* was updated in 2012 with more stringent rules.

Travelers and Trips in São Paulo

In order to measure the effects of the speed limit changes on the duration of trip times, we simulate a set of representative motorized journeys using the Google Directions API. When a driver enables location tracking on their mobile phone while using Google Maps, anonymized data regarding vehicle speed is collected by Google. Google aggregates data collected from all the mobile phones in a given area and uses vehicle speed data to generate an estimate of the speed of traffic on a given road segment. These traffic speed estimates are used to provide trip duration estimates to Google Map users that reflect an optimal route for a pair of origin and destination coordinates given real-time traffic conditions (Google, 2009). As is true for many developing country cities, travel time and vehicle speed databases with high temporal and spatial granularity are not readily available for São Paulo, increasing the value of data from Google and similar providers.

We note two advantages of using the Google Directions API relative to more conventional observations of vehicle speeds on highways in policy evaluation. First, the outcome measure that we are generally interested in when studying transport demand is a change in the duration of a potential trip for a given sample of travelers (given their value of time). The conversion of traffic counts and flows into trip duration requires restrictive structural and functional form assumptions regarding optimal routing behavior.

Second, it is often difficult to measure how drivers adapt their routing behavior following a policy shift, which is important in an event study of this kind given that drivers are likely optimizing in real time. The Google Directions API provides the outcome measure directly and adjusts in real-time based on evolving traffic conditions. We obtained a set of origin and destination coordinates from a travel survey conducted by the São Paulo Company of Traffic Engineers in 2012. The survey collected detailed information about 46,861 trips taken by 8,115 households and was designed to be representative of commuting patterns in the Metropolitan Region of São Paulo on a regular weekday. Trips in the survey are summarized in Table A.3. Our sample of representative trips is restricted to the subset of 15,055 trips taken by car in the survey, which are designed to be representative of 12.49 million motorized trips on a weekday in São Paulo. We queried the

OpenStreetMap (OSM) API using the survey coordinates to get a set of optimal trip routes for each origin-destination pair. We intersect this set of optimal trip routes with a shapefile of São Paulo roads to determine to what extent each survey trip intersects with treated road segments. Each surveyed trip is assigned a value for the proportion of the trip that intersects with treated road segments. We assign a trip to the treatment group if the trip utilizes more than 400 m of treated road segments.

From July 2016 to September 2017, we queried Google's Directions API using these origin-destination pairs at 20 minute intervals within 2 hours of the time that the surveyed trip was taken in order to obtain real-time estimates for trip duration.[19] For each surveyed trip, we obtain approximately 75 observations of real-time trip durations collected over the course of a week. We note that treatment intensity is assigned based on the optimal route given to the survey origin and destination coordinates, which reflects the treatment intensity of the traveler's baseline route. We then observe repeat observations of the duration of each trip before and after treatment and our model estimates the average effect of a speed limit change on trip duration. Since Google Maps may provide a new optimal route as a result of changing real-time traffic conditions after speed limits change, our estimates reflect the costs facing drivers who optimally adjust to conditions imposed by the new speed regime. The assumption is that rather than continue to utilize routes that might become inefficient in the wake of the policy, drivers are able to adapt their routing behavior as conditions change.

Panel B of Table 1 describes the characteristics of the simulated trips from our study. In total, we simulated 1.47 million trips between July 2016 and September 2017.[20] 243,700 of these trips were assigned into the treatment group. The table compares the mean characteristics of all queried trips to the ones within the treatment group. A third of all observations were queried after the speed limit reversal and about 40% of the queries were made during peak hours. The overall average estimated travel time was 20 minutes, with

[19]In our main policy analysis, we calculate the social costs of speed limit reductions based on changes in the estimated travel time of individuals who travel by private modes. Very few bus routes circulate on the Marginais highways and only 1.7% of public transit trips take place on bus routes that run on the Marginais highways. We discuss possible effects of the policy on public transit trips in Appendix A.

[20]The crawler was inactive during the period between March 15, 2017 to May 21, 2017.

an average length of 8.31 km. Trips using the Marginais Highways were considerably longer in length and tended to have a higher average speed. According to our OSM measure of trip length along treated roads, trips that utilize the Marginais Highways spend about 22% of their total trip duration on the treated segments.

Electronic Traffic Cameras and Tickets

In order to more fully understand how the speed limit program affected driver behavior, we make use of comprehensive data on traffic tickets issued by monitoring cameras in São Paulo during the study period. The cameras automatically identify traffic violations and tickets are mailed to the driver's residence. Speeding tickets received by mail are the main form of speed limit enforcement in the city. Each observation in the tickets dataset contains information about: (1) the type of traffic violation that was registered, (2) the date and hour of its occurrence, and (3) its location. Table A.4 summarizes the sample of traffic tickets, which includes all tickets issued by cameras in São Paulo between 2014 and 2017.[21] In total, more than 35 million traffic tickets were issued during that period, with an average of 9.46 million tickets per year. Camera-based enforcement became increasingly important during the study period. In 2015, the total number of tickets issued increased by more than 53%, and then by another 26% in 2016. However, in 2017, the number decreased by almost 16%. Across the entire study period, speeding tickets make up approximately half of all traffic violations. Approximately one quarter of all traffic tickets were issued by cameras located on the Marginais Highways.

We use observed traffic violations and camera locations to gain traction on three different questions related to the speed limit program. First, we identify the presence of speed monitoring cameras on all segments included in our analysis, identifying the exact timing that a camera was installed or discontinued. We use this variable to estimate the interaction between the effect of speed limit reductions and the effect of contemporaneous introduction of camera-based enforcement on the same segment.

[21] Traffic tickets can be issued manually by the police and by traffic agents. Speeding tickets are only issued by the automated cameras (there are no tickets issued by police in our data), though about 7% of driving restriction violations are issued through manual policing.

Second, we estimate the effect of the speed limit reduction on speeding tickets issued on nearby non-treated roads immediately following a speed limit reduction. Since most trips utilize a combination of treated and non-treated road segments and drivers may not be cognizant of the exact geographic boundaries of the policy, a speed limit reduction could affect driver behavior on nearby non-treated roads and cause a reduction in accident risk there as well. We test for changes in the number of speeding tickets issued on nearby non-treated roads to examine whether drivers reduce their speeds beyond the strict boundaries of the treatment, which we interpret as evidence of behavioral spillovers.

Third, we estimate the effect of the speed limit reduction on the volume of cars on non-treated roads. We assume that the number of *non-speeding* violations issued per traffic camera is a function of the number of cars circulating on monitored road segments and use number of non-speeding tickets issued as a proxy for volume of cars on a given road segment. We use this proxy to test for evidence of substitution away from treated roads after the speed limit reductions.

3 Did São Paulo's Program Reduce Accidents?

We use exogeneous variation in the timing of speed limit reductions as a natural experiment and identify the effects of speed limit changes on road accidents using a semi-dynamic event study design. Our empirical setting includes road segments that were treated at different points in time during 2015 and a dataset of monthly accidents between 2012 and ending before the reversal of the policy in January of 2017.[22] The semi-dynamic model flexibly captures effects that can grow or decline in the periods following initial treatment, which avoids a key under-identification problem that arises in static event study designs and allows us to learn about the effects of treatment over time (Borusyak and Jaravel, 2016). We separately estimate a version of the dynamic event study model proposed by Abraham and Sun (2018) for estimating cohort-specific average treatment effects (CATT) in settings characterized by non-random selection in the sequence of a

[22]In Appendix B, we examine the effect of the 2017 reversal on accidents although these estimates are limited by a shorter panel of data and a more limited treatment group.

treatment, which was likely true of the roll-out of the speed limit program. This estimator generates cohort-specific effects using an interacted model saturated in relative time and cohort indicators and then weights effects in every period using the share of each cohort in the sample. The primary identification assumption in these event study models is that changes in road accidents observed just after the policy change on a given treated segment follow a path parallel to what would be observed on the same segment in the absence of the policy.

We use the following Poisson event study model[23] of accident counts to estimate of the impact of changing the speed limit:

$$log\left(E\left(y_{it}\right)\right) = \alpha_i + \beta X_t + \left(\sum_{q=1}^{6} \gamma_q D_{it}^{q}\right) + \zeta C_{it} + \eta C_{it} SLR_{it} \tag{1}$$

where y_{it} is the number of accidents on segment i during month t, α_i is a segment fixed effect that captures the time-invariant component of accidents on each segment. X_t is a vector of time-varying controls that are measured at the city level. It includes controls for secular changes in driver behavior across the time series with a linear time trend and two covariates that capture aggregate changes in driving behavior during the period: (1) the log of fuel sales in the State of São Paulo and (2) the log of the total number of speed monitoring cameras in São Paulo. The variable D_{it}^{q} is an indicator for the number of q quarters relative to i's initial treatment ($q = 1$ is the quarter of initial treatment). We cap observations on all segments at the sixth relative quarter, which ensures that all segments have the same time in treatment. The sixth quarter is our longest-term estimate and reflects our best estimate of the longer-run effect of the policy.

The primary coefficients of interest are the γ_q terms, which measure changes in the number of accidents on a treated segment in each of the quarters following the treatment of segment (i).[24] C_{it} is an indicator for the presence of a speed monitoring camera on

[23]Estimates from the Poisson model are converted to relative incidence ratios. We report estimates from equivalent negative binomial and linear specifications in Appendix B.

[24]For instance, γ_1 indicates the average relative change in accidents on treated segments during the first three months after speed limit reduction. Effects are therefore measured in terms of the relative time-distance (in quarters) to treatment. We aggregate the effects in terms of relative quarters for two reasons: 1) by using a longer time interval, coefficients are more precisely estimated; 2) aggregation by quarter allows us to present time-varying effects occurring across 6 quarters rather than 18 months (=

segment i during month t, such that ζ estimates the change in accidents on road segments where camera-based enforcement is initiated during the study period.[25] The interaction term $C_{it} \cdot SLR_{it}$ is an indicator for whether the speed limit reduction policy occurred on a segment that also received camera-based enforcement, such that the coefficient η measures the interaction between the speed limit reduction and the onset of camera-based speed enforcement. We restrict our sample to December 2016 to isolate the effects of the speed limit reduction, though we estimate and discuss an extended version of this model that includes the post-reversal period in Appendix B.

In addition to the base event study specifications, we consider 3 alternate dynamic models using samples of never-treated segments as controls. A never-treated sample provides an alternate, non-parametric solution to the under-identification problem in static event study models (Borusyak and Jaravel, 2016). This set of specifications involves a stronger identification assumption than the base model (never-treated segments must be valid controls), but avoids parametric assumptions regarding the functional form of city-wide accident trends and provides a valuable alternative for comparison with the base model. Two particular concerns arise regarding the use of never-treated control samples in the São Paulo setting: (a) since the selection of treated segments is not random, it is not clear ex ante that any sample of never treated segments will provide a valid counterfactual (parallel paths) and (b) most trips use a combination of treated and non-treated roads and drivers may not be cognizant of the exact geographic delineation of the policy, so speed limit reductions could potentially affect driver speeds and accident risk on nearby never-treated roads. We conduct extensive analysis of behavioral spillovers on nearby road segments in the following section and in Appendix C, which indicates that the speed limit reductions reduced both speeding behavior and accident risk on nearby (untreated) roads and that spillovers are limited to 1.6 km of treated roads.[26]

Sample (1) provides a reference sample that includes all never-treated road segments

18 coefficients).

[25]Since C_{it} is estimated conditional on segment-fixed effects, ζ measures the accident reductions associated with the onset of camera-based enforcement in a given month t.

[26]Appendix C describes our analysis of behavioral spillovers using data from electronic speeding tickets on non-treated road segments.

in São Paulo. To address potential bias from behavioral spillovers, sample (2) excludes all control segments located within 1.6 km of any treated road. Sample (3) addresses concerns regarding the non-random selection of never-treated segments by selecting control segments that are highly similar to the treated segments in accident risk. Each treated segment is matched with one control segment using the total number of accidents during the pre-treatment period. Figure 3 maps the segments included in each of the samples 1-3 and plots the corresponding time series of road accidents for treatment and control groups.[27] Comparing sample 1 to sample 2, we see that the 1.6 km restriction in sample (2) excludes all control segments located in the central part of the city. The matching strategy used to generate sample (3) places greater restriction on the control group. Despite these differences, the patterns observed in accident trends are similar across all control groups: both the treatment and control groups present a decreasing trend in accidents per km over the study period, with clear evidence of a larger reduction in accidents on treated segments that coincides with the onset of the speed limit change.

We use the following Poisson event study model with controls:

$$log\left(E\left(y_{it}\right)\right) = \alpha_i + \beta_t + \left(\sum_{q=1}^{7} \gamma_q D_{it}^q\right) + \zeta C_{it} + \eta C_{it} SLR_{it} \tag{2}$$

where terms are equivalent to those in Model 1, except that β_t now measures the average change in accidents observed in each calendar month (t), flexibly controlling for secular trends in accidents.

We conduct independent tests for pre-trends in all models following the approach recommended by Borusyak and Jaravel (2016).[28] Panel A of Figure 4 plots the coefficients γ and α for each of the samples of segments. Results are consistent across specifications. Relative to treated segments, accidents on control segments appear to increase slightly in the period 2 years before the policy change and then exhibit a parallel trend for the year preceding treatment. None of the coefficients associated with periods preceding treatment are statistically different from zero, providing support for the parallel trends

[27] The average number of accidents per km for both groups is normalized using on the values observed in June of 2015, which is the last month before the first speed limit reduction in São Paulo.

[28] See Appendix B for discussion of model specification and Borusyak and Jaravel (2016).

assumption. We also note that the coefficients associated with the quarter immediately preceding the speed limit reduction suggest a small reduction in accidents, which we attribute to anticipatory behavior induced by the installation of banners and signs on treated segments on the weeks preceding the speed limit change in each road. In our baseline specification, we exclude observations from the quarter immediately preceding the speed limit change to address the possibility of confounding effect of anticipatory behavior resulting from announcements and demonstrate that our estimates are robust to that assumption in Appendix A.[29]

Table 2 reports our main estimates of the effect of speed limit reductions. Panel A reports the full set of estimates from each dynamic specification (all 6 quarters). All results indicate a significant reduction in accidents on treated segments after the speed limit reductions and that effects of the reductions increase over time. Results from the base event study specification that omits never-treated segments suggest an immediate effect of 16.9% that grows to 35.5% within 1.5 years of the change in speed limit. Our estimates of cohort-specific average treatment effect (CATT) indicate an immediate effect of 16.7% that grows to 35.3%, indicating that selection in the sequence of treatment does not affect our baseline event study estimates. In specifications with control samples (1-3), we estimate a 11.9-18.8% reduction in the number of road accidents in the first quarter following the policy. The effect grows to 19.5-27.4% over a period of 6 quarters. These reductions correspond to 1,837-2,899 accidents and 101-159 fatalities averted in the first 18 months of the new policy.

We interpret the fact that results are aligned both in magnitude and specific pattern as reassuring evidence that these models yield a credible estimate of policy effects, though we note some key differences. The point estimates from the most general sample of controls (1) are the smallest in magnitude, which is consistent with the possibility of behavioral spillovers on nearby roads. After restricting the set of control segments to those beyond 1.6 km, our estimates indicate a larger effect of 27.4% in the final quarter of

[29]Figure A.10 on Appendix A shows an example of one these signs. In Appendix B, we estimate our empirical model without excluding data from the quarter immediately before the policy adoption to test for the robustness of results. Results that include the final quarter preceding treatment are somewhat attenuated, though this choice does not result in statistical differences in any of our estimates.

the series. Estimates from our preferred (matched) sample of controls indicate an effect of 21.7%, which is smaller than the baseline CATT estimate. Estimates from these two specifications yield a range of 21.7-35.3%, which we interpret as the most credible range of estimates of program effects. Estimates from our preferred matching model are more conservative than the baseline CATT estimate and are used as the basis of calculations reported in the following sections. Given that effects grow over time in all models, we interpret these estimates as the best indication of the effect of the policy over a longer time horizon.

The estimates of treatment effects in the presence of camera-based enforcement also indicate a consistent pattern across specifications, though this smaller sample has less statistical power and yields less precise estimates. Estimates from our preferred models suggest that camera-based enforcement on a given segment had a negligible effect on accident risk *before* the speed limit reduction, but that the onset of camera-based enforcement augmented the impact of the speed limit policy by 11.5-11.8 percentage points.

In Panel B of Table 2, we report separate estimates of the longer-term effect ($\gamma_{q=6}$) of speed limit changes on the two types of urban roadways treated by the program: (1) the Marginais Highways, where speed limits were reduced from 90 km/h to 70 km/h and (2) a large set of arterial roads, where speeds were reduced from 60 km/h to 50 km/h. Exploring heterogeneity in effects is valuable for considering relative benefits on different types of roads (with different base speed limits). This distinction is also important for our comparison of the benefits from the initial reductions to the travel time costs from the reversal in 2017, when speed limits on the Marginais Highways were reversed to pre-reduction levels but the same reversal was not implemented on arterial roads.[30] The estimates from our preferred models in Panel B indicate that speed limit reductions implemented on the Marginais Highways had a substantially larger effect (32.4%-45.9%) than those implemented on arterial roads (19.4%-33.7%). The larger effects on the Marginais Highways are in line with the higher relative speed limit reduction on those roads. These

[30]In Appendix B, we report and discuss findings on the impacts of the 2017 reversal on accidents. These results suggest that the reversal resulted in an increase in accidents on the Marginais Highways, though the precision of estimates is limited by fewer quarters of post-period data and fewer treated segments during the reversal period.

results are consistent with results from Musicant (2016), who reports a 13.3% (13.9) higher average effect per 10 km/h reduction using 28 transport engineering studies in developed countries.

Substitution, Spillovers, and the Displacement of Accident Risk

Our results indicate that a significant reduction in accidents occurred on roads where speed limits were reduced in São Paulo. Our primary hypothesis is that this reduction in accidents was caused by lower accident risk as drivers began driving more slowly. However, if drivers substituted away from treated roads that had become slower as a result of the speed limit reduction, then this substitution would result in fewer vehicles driving on treated roads and consequently, fewer accidents. Route substitution could also potentially displace accident risk to roads that serve as close substitutes for treated segments.[31] We test for evidence of substitution using data on traffic violations resulting from infractions that are unrelated to speeding. We group these violations into two types: (1) Violations of the São Paulo driving restriction, which account for 56.6% of non-speeding traffic tickets and (b) All other non-speeding violations.[32] These two sets of non-speeding tickets serve as proxies for traffic volume per segment, which we consider independently as a check for consistency in patterns across the two proxies.

We examine the effect of the speed limit reduction on traffic volumes using a dynamic event-study model that mirrors our main empirical specification:

$$Z_{it} = \alpha_i + \beta_t + \sum \delta_{it} + \varepsilon_{it} \tag{3}$$

The dependent variable (Z_{it}) in this model is the log of non-speeding tickets issued on segment i during month t. The model tests the hypothesis that the speed limit reduction caused lower traffic volumes on treated segments using the subset of road segments that

[31] van Benthem (2015) finds no meaningful substitution in the context of speed limit changes on interstates in the United States, though São Paulo is a highly distinct setting.

[32] Section A.8 in Appendix A describes the details of the Driving Restriction policy in the City of São Paulo. Other violations include: driving in bus lanes (11.6%), cargo vehicles in restricted areas (3.9%), illegal right-turns (3%), red light violations (2.2%), driving in restricted lanes (4.1%), stopping in a pedestrian crosswalk (0.8%).

possess a monitoring camera during the entire study period. Negative and statistically significant coefficients for δ_{it} provide evidence of reductions.

Table 3 presents the coefficients δ_{it} from Model 3 for (a) driving restriction tickets and (b) other non-speeding tickets. The results indicate that there was no significant change in traffic volume as a result of the speed limit reduction. We cannot rule out a slight (non-significant) decline immediately following the reduction, perhaps as drivers tested routing alternatives or increased their attentiveness, although ticket volumes fully revert to pre-policy levels after the first quarter of the new regime. Assuming that the volume of non-speeding tickets serves as a proxy for the volume of traffic on treated segments, these findings suggest no substantial or persistent patterns of substitution in routing or trip-taking in response to the policy. As a further check on the possible effects of substitution and displacement on accident reductions, we evaluate the net effects of the policy at the city level in Appendix C. Figure C.4 illustrates the evidence of net reductions in accidents on the average road in São Paulo following the implementation of the program.

While we don't find any evidence that the speed limit policy induced route substitution, it may have impacted driver behavior in ways that extend effects beyond the boundaries of the new speed limit regime. The majority of trips made on treated road segments involve passage through untreated segments and drivers may not be aware of the precise boundaries of treatment, creating the potential for behavioral spillovers just before or after drivers pass through a treated zone. If changes in driving behavior were limited to treated segments, then we would expect to see no significant change in the volume of speeding tickets on nearby untreated roads when limits are reduced. On the other hand, if the policy affect driver behavior (reduced speeds) in ways that extend beyond the boundaries of treatment, then we should also observe a decline in the number of speeding tickets issued on untreated roads after the speed limit is reduced. If the policy simply induced route substitution away from treated roads, then we could observe an increase in speeding tickets on nearby roads after the policy. The changes in speeding tickets can shed light on the behavioral mechanism underlying positive spillovers in effects on accidents.

We conduct extensive analysis on the nature and extent of behavioral spillovers in the context of the speed limit reduction (we refer interested readers to Appendix C). This analysis reveals 3 findings: (1) The speed limit policy resulted in reductions in both speeding behavior (tickets) and accident risk on nearby never-treated road segments; (2) Effects on both outcomes diminish as a function of distance from treated roads and both are limited to segments located within 1.6 km of the nearest treated road; (3) While limited geographically, behavioral spillovers may have non-negligible effects on the benefits and the costs of speed limit changes. We provide a comparison of net benefits with and without behavioral spillovers in Section 5.

Speeding Behavior and Compliance on Treated Roads

Our results indicate that the speed limit reduction had a substantial immediate effect on accidents, but also that the effect continued to increase over time. It is not obvious ex ante what causes the change in the impact of the speed limit reduction over time. One hypothesis is that there was a period of adjustment when some drivers did not comply with the new speed limit regime. To test this hypothesis, we estimate Model 3 using the log of speeding tickets as the dependent variable (Z_{it}). The dynamic coefficients (δ_{it}) in the second Panel of Table 3 measure changes in compliance with the new speed limit regime. The results for *speeding* tickets contrast sharply with those for *non-speeding* tickets, indicating a large and immediate **increase** (+81%) in speeding tickets on treated segments following the policy adoption. After the first two quarters, the number of tickets then begins to decline, although it remains well above pre-treatment levels throughout the study period (+75%). These findings illustrate a lag in behavioral adjustment (and a persistent set of non-compliers throughout the study period), suggesting that larger accident reductions could be attained if the rate of compliance continues to increase over a longer horizon, resulting in additional reductions in accident risk.

4 Did São Paulo's Program Affect Travel Times?

This section examines the effect of the January 2017 speed limit increase on the travel times of drivers in São Paulo. Using representative trips simulated on the Google Directions API at the time of the reversal, we compare the estimated duration of trips that utilize the Marginais Highways to those that do not, before and after the policy change. A trip is defined as a pair of origin and destination coordinates queried in real time at a specific time of day.[33] The exact same set of trips was queried continuously throughout the study period, producing a panel of the durations of otherwise identical trips just before and following the reversal.

Since the Google Directions API provides an estimate of the duration of a given trip using the optimal route in real-time conditions, changes in commute times may reflect changes in routes that become optimal as a result of changed speed limits on treated roads or other differences in conditions that may be changing across hours and days.[34] This is important, as we expect drivers to re-optimize based on traffic conditions that may shift with the policy. The primary identification assumption in our dynamic event-study design is that differences in the durations of the same set of trips before and after the change on the Marginais Highways can be attributed to the effect of the change in speed limits. Figure A.7 plots changes in the duration of trips just before and after the reversal, illustrating that treatment and control trips follow similar paths before the reversal and that the duration of trips that use the Marginais Highway tends to fall below the corresponding change in controls after the reversal. We estimate the effect of the reversal on a trip taken in the travel survey using the following equation:

$$ETT_{ihd} = \alpha_{ih} + \beta Marg_i I_d + \delta X_{hd} + \sum_L \phi_L B_{Li} I_d + \varepsilon_{ihd} \tag{4}$$

[33]A query that simulates traveling from point A to point B at 7 am is considered to be distinct from the "trip" from the exact same origin-destination query made at 8 am. All sets of trip-time pairs are queried repeatedly before and after the policy change. Time fixed effects in the regression below flexibly control for differences in the duration of the trip made at 7 am relative to the same trip at 8 am, leaving only differences in the 7 am trips and 8 am trips queried before/after the policy.

[34]Real-time queries are different from queries of past or future trips using the Directions API, which rely heavily upon historical data rather than conditions in real-time to generate a prediction.

where ETT_{ihd} is the log of estimated travel time for each simulated trip i queried at hour h on date d. α_{ih} is a trip-hour fixed effect that controls for trip-specific characteristics such as length, path and departure time. $Marg_i$ measures the fraction of each trip that takes place on the Marginais Highways and measures the intensity of treatment for each trip. I_d indicates if the query was made after the speed limit increase in January 25, 2017. X_{hd} is a vector of other controls, including the occurrence of rain in the moment of the query and if date d was a holiday in São Paulo. We also control for the exact dates and times of queries made during a nighttime motorcycle restriction that was placed on the use of motorcycles between 10pm-5am on the main lanes of the highway four months after the 2017 reversal (May 13, 2017). To account for possible effects of spillovers that may result from changes in congestion near the treated zone, we include a set of terms that identify the fraction of trips that take place within buffers of varying distance (L= 1km, 3km or 5km) from the Marginais Highways.[35]

Table 4 reports our estimates of effects on travel times. The first column corresponds to the simplest version of our empirical model, where we do not include controls for spillovers. The main estimates indicate a reduction of 6.8% in the travel time for a trip made entirely on the Marginais Highways, which reflects an overall reduction of 0.44% in travel time for all trips in the sample. Rain is associated with an average increase of 1.9% and trips on holidays were on average 10% faster. In Column 2, we include covariates that measure the portion of a trip that takes place in spillover zones that may have been indirectly affected by the speed limit change in the Marginais Highways. The inclusion of these variables reduces the main treatment effect to 6.1% for a 20 km increase in the speed limit. Trips taking place within 1 km of the Marginais experienced a reduction of 3.4%, or approximately 60% of the main effect observed on the Marginais. Similarly, the estimated spillover effects within 3 km and 5 km are 30% and 20% of the main effect, respectively. In Column 3, we include date-hour fixed effects that flexibly control for changes in conditions on the different days in our sample. This specification suggests an average effect of 5.5%. Taken together, these results suggest that the speed limit change

[35]Figure A.8 in Appendix A illustrates buffer and control zones.

did affect trip times on the Marginais Highways and also indirectly on nearby roads. Our estimates of direct effects do not change when we control for the impacts of spillovers, though we do find evidence that the speed limit increases affected travel times on nearby roads. In the following section, we show that the costs of spillovers in trip times nearly offset the benefits from spillovers in accident reductions.

Column 4 reports estimates from a specification that estimates treatment effects during peak (7-10 am and 5-8 pm) versus off-peak hours. These results indicate that the average effect was 5.5% during off-peak hours and 5.6% during peak hours. Finally, we estimate a dynamic event-study specification of our model where we interact our main treatment effect component with each quarter after the policy change (closely resembling the dynamic model used for accidents). Panel B of Figure 4 plots these estimates, which indicate that the treatment effect of the policy increased over the first two quarters following the reversal.

5 Cost-Benefit Analysis

In this section, we extend our reduced-form estimates to analyze the social costs and benefits of the speed limit changes that were implemented in São Paulo.[36] We estimate the monetary value of road accidents and travel time using standard parameters from the literature (Viscusi and Masterman, 2017), though we stress the limitations of the procedure given substantial uncertainty underlying both of these parameters. We construct counterfactual scenarios that allow us to compare the social benefits from reduced accidents to the social costs from increased commute time in the context of the 2015 speed limit change in São Paulo. Social costs include effects on private and external accident damages and private plus external travel costs.[37] We test for effects on air pollution externalities in Appendix D, but do not find evidence of effects on the ambient concentrations of any

[36]Results are presented in year 2015 Brazilian Reais (R$). The 2015 exchange rate US$1 = R$3.2551

[37]Speed limit regulations affect the private and external damages related to accidents. Using the same formula as van Benthem (2015) for private versus external damages from accidents, we calculate that 56.7% of the accident damages are external. Speed limits affect travel cost directly as well as the external cost through congestion effects (see discussion in (van Benthem, 2015)) Both are important for evaluating the social cost of the policy. While disentangling private from external costs could yield interesting insights, it also involves restrictive assumptions in this setting.

of the 6 pollutants that we examine. We focus our analysis on the Marginais Highways, where we are able to more directly compare accident gains from the speed limit reduction with travel time gains from the speed limit reversal.

5.1 Social Benefits of the 2015 Speed Limit Reductions

Using the final quarter of data as our best within-sample approximation of the longer-run effects of the 2015 speed limit reduction (1.5 years post-implementation), we calculate the monetary benefits of the program using the design with matched controls sample as preferred estimates and the CATT estimates from the baseline event study (omitting controls) for comparison. We utilize accident-specific data to calculate the monetized damages from accidents using costs to vehicles and to non-fatal victims from IPEA (2016).[38] We value fatalities using the Value of Statistical Life (VSL) of USD$ 1.695 Million[39] estimated for Brazil by Viscusi and Masterman (2017). We construct two discrete policy scenarios: (a) Speed limits were reduced but everything else remained constant and (b) Speed limit reductions were accompanied by an increase in the number of cameras equivalent to the expansion observed between 2015-2017.

Estimates of policy benefits are reported in Panel A of Table 5. In Panel A1, we report benefits from the reduction program as a whole, whereas in Panel A2 we provide a benefits estimate that is comparable to the costs that we estimate using changes in trip times during the reversal. Specifically, we restrict our analysis to accidents observed on business days on the Marginais Highways.[40] For each set of treated segments, the table reports the total number of accidents from a pre-policy baseline (one year immediately preceding policy adoption). For each estimate of policy impacts, we report: (a) a counterfactual estimate of annualized accidents from the post-policy period, (b) an estimate of annualized accidents under the speed reductions program, (c) an estimate of the number of accidents averted as a result of the reductions program, (d) their monetized benefits. Using our

[38] This study from the Brazilian Institute of Economic Research estimates the average cost of road accidents in Brazil by accident severity and status of victims. Appendix E reports all the parameters from IPEA (2016) used in our study.

[39] Value is in 2015 USD.

[40] The household travel survey used to compute travel times is only representative of trips on business days, so we impose same restriction on the accidents data.

preferred event study estimates (matched controls), we find that the speed limit reductions program resulted in 1,317 averted accidents and R$ 439 Million in annual benefits, which is more conservative than the estimate of 2,648 averted accidents and R$ 882 Million using the base event study (CATT).

5.2 Benefits from the 2017 Speed Limit Increase

We calculate the social monetary value of time savings associated with the 2017 increased speed limits using reduced-form estimates of effects on trip durations and two alternate parameters for the value of time (VOT). A first estimate is based on 50% of the after-tax hourly wages of individuals observed in the travel survey, which is consistent with a large body of empirical work (Wolff, 2014).[41] As an alternative parameter that is intended to capture heterogeneity in VOT across different types of trips, we calculate policy benefits using the Victoria Transport Policy Institute (VTPI) guidelines, which suggest assigning a VOT of 150% of travelers' wage for business trips, 50% for commuting (35% if passenger), 25% for personal travel, and 0% for leisure or vacation (VTPI, 2016). The survey records the self-reported income for all travelers and and motivation for all trips in our origin-destination sample. We use this information to calculate the VOT for each trip following the VTPI method. As has been documented throughout the transportation literature, we note that our post-estimation results are highly sensitive to this choice of VOT. In all results that follow, we therefore present two variants of each estimate to illustrate the range of estimates of policy cost given these two different definitions.

Panel B of Table 5 first reports the baseline time in traffic observed in 2016 and cost of time calculations using VTPI VOT and median after-tax hourly wage VOT, accounting for the fact that the speed limit on the Marginais highways was 70 km/h in that year. Columns 2 and 3 report the counterfactual time spent in traffic and the cost of that time for the speed limit increase to 90 km/h. Our reduced-form estimates of effects on trip duration indicate that the speed limit increase of 2017 resulted in 4 Million fewer trip-hours for travelers on treated roads in São Paulo. The value of total travel time savings

[41] The USDOT recommends assigning half of the hourly wage for non-business trips within local urban settings (USDOT, USDOT).

is estimated to be R$ 20.9 Million if we use the 50% median wage VOT and R$ 32.8 Million using the VTPI VOT.

Compared to the existing literature that estimates the welfare impacts of speed limit changes, our study has the distinct advantage of basing our calculations on a representative sample of individuals. As a result, we can identify the characteristics of individuals that were likely to be affected by the policy and estimate their corresponding welfare benefits according to individual-specific parameters. While this approach requires the assumption that the 2012 travel survey is representative of the types of individuals and trips that were taken in 2016/2017, it allows us to account for heterogeneity in effects. In particular, it avoids possible bias associated with unobserved heterogeneity in the travel demand observed for individuals at different income/wage levels.

To illustrate the importance of accounting for this heterogenity, the third cost column in Table 5 provides comparable estimates of travel time benefits from a model that assumes the median VOT for all individuals in our sample. The comparison indicates that by assigning an individual-specific VOT rather than assuming that the median wage is representative, the total estimated benefits from the speed limit increase change from R$ 20.9 to R$ 32.9 Million. This reflects a difference of more than 35% in total monetized impacts. This difference is attributable to the fact that individuals who drive on treated roads tend to be wealthier than the median São Paulo resident. The correlation between income and transport behavior among São Paulo residents introduces substantial heterogeneity in effects and presents a first-order issue in benefit-cost analysis. We note that the our estimates using individual-specific wages (R$ 32.9 Million) are highly consistent with the VTPI-based estimates (R$ 32.8 Million) that utilize individual-specific wages and differentiate values based on trip type. It is also worth noting that about half of households do not own a private vehicle in São Paulo, and are therefore unable to extract much, if any, direct benefit from a speed limit increase.

5.3 Comparing the Costs and Benefits of Speed Limit Changes

We compare the benefits of reduced accidents from the speed limit reductions of 2015 to the reduced costs associated with travel time savings from the policy reversal in 2017, which are different events. Any comparison of these two different events requires the assumption that the travel time effects observed from the speed limit increase of 2017 are symmetric but inverse to the impact that would have been observed during the prior speed limit reduction on the same roads. This is not a testable assumption in our setting due to concomitant safety measures introduced with the 2017 reversal, although other studies have compared the effects of speed limit increases and reductions on travel time. In particular, a meta-study that analyzes the results of 108 events (speed limit changes) that examine driving speeds before and after speed limit increases/reductions in 20 different countries finds that the effects of increases in speed limits are not statistically different from the effects of speed limit reductions (Musicant et al., 2016).[42] The point estimates from this comparison suggest that, if anything, increasing speed limits may have slightly stronger effects than speed limit reductions by the same amount. This evidence suggests that our comparison will likely yield reasonable, if conservative, estimates of net benefits from the speed reduction policy. Changes in speed limits could also generate additional benefits/costs if they affect the reliability of travel along treated roads, though we do not find any evidence of impacts on *uncertainty* in travel times.[43]

To ensure the comparability of policy costs and benefits, we focus on the speed limit changes on the Marginais Highways and utilize the following results: 1) estimates of benefits on business days due to accident reductions (R$ 58.9 Million for our preferred sample with matched controls) from the 2015 policy that excludes the effects of camera enforcement; 2) estimates of travel time savings (R$ 32.8 million using a VTPI VOT) from the 2017 reversal. We estimate a benefit/cost ratio that ranges from 1.77, using

[42]Estimates in (Musicant et al., 2016) are drawn from 28 studies in developed countries. Reported estimates are .0237 (0.09) for the pre/post effect of a 10% increase in the speed limit versus .0133 (0.139) for the pre/post effect of a 10% reduction in a speed limit. It is worth noting that our estimated effect of the São Paulo speed limit increase as measured by the Google API .057 (0.013) is in line with the estimates suggested by the meta-analysis. The speed limit increase in our setting was of 22.2% (90 km/h to 70 km/h), so the extrapolated effect for a 10% increase would be approximately .0256 (0.005).
[43]See Appendix Table B.4 and related discussion.

the VTPI VOT and our preferred estimate of accident effects from our event study with matched controls, to 3.1 using the VTPI VOT and our base event study estimate (CATT) omitting control segments.

We provide the equivalent panel of estimates of benefits and costs using models that account for spillover effects in Appendix Table C.1. Estimates of annual benefits range from R$ 120 to R$ 263 Million, which are more than two times higher than the benefits on treated segments alone. Preferred estimates of total annual costs range from R$ 85.4 to R$ 88.9, which are more than two times higher than the costs on treated segments alone. We calculate a net benefit of R$ 35-177 Million when we account for the benefits/costs of spillovers, which is comparable to but somewhat higher than our range of R$ 25-69 in net benefits on treated roads. When we account for spillovers in benefits and costs, the benefit/cost ratio changes from 1.77-3.1 to 1.41-3.1. This comparison indicates that while spillover effects induced by the speed limit program resulted in large benefits and costs, accounting for these effects results in a similar range of net benefits due to their offsetting effects.

Given the underlying uncertainty in the VSL and the inherent difficulty in obtaining reliable and externally valid values, we also analyze the breakeven VSL for this program – the minimum VSL for which a speed limit reduction would still be socially beneficial. Figure E.1 reports our findings on the breakeven VSL, which yields 2 insights: (a) when we use either the VTPI or the VOT using individual-specific wages, the speed limit reductions on the Marginais Highways yield positive net benefits if the value of a statistical life in our São Paulo sample is greater than R$ 1.28 Million, which is equivalent to 33% of the baseline VSL from Viscusi and Masterman (2017) and (b) the break even VSL increases to R$ 2.21 Million when using the 100% of the median wage as the measure of VOT and to R$ 4.65 Million when using 100% of the individual-specific wage. In Appendix F, we compare these results to estimates of costs and benefits for the United States from van Benthem (2015).

5.4 Distributional Effects of Speed Limit Changes

In this final section, we use our central parameters to evaluate the distribution of policy costs and benefits across different income groups. We proxy for income using the educational attainment of commuters, which are identified in our household travel survey as well as in accident victim reports from our database of road accidents.[44] We report the benefits calculation using the population average VSL parameter for Brazil. For consistency with the assumption of an average VSL, we also report the distribution of costs using single average VOT and note that this likely reflects a conservative depiction of differences in the distribution of costs, since individuals with lower educational attainment are also likely to have a lower VOT. This comparison directly reflects impacts on accident risk and effects on travel times across the income distribution of population in the São Paulo.[45]

Panel A of Figure 5 plots the mean costs and benefits of the speed limit reduction for the individuals in São Paulo as a function of educational attainment. While the benefits from accident reductions are larger for individuals with medium educational attainment (primary education, and secondary education), travel time costs have a disproportionately larger effect on individuals with high educational attainment (college education). The policy delivers net benefits to low and middle income residents in São Paulo and appears to be strongly progressive. In the case of individuals with below primary educational attainment, however, both the costs and benefits are small. It is likely that individuals with very low educational attainment are much less likely to have a driver's license since literacy is a regulatory requirement for obtaining a driver's license in São Paulo.

It stands to reason that motorcyclists and pedestrians could be at greater risk on roadways in developing country cities and would therefore benefit more from policies that achieve meaningful reductions in accident risk. However, this issue has received

[44]While the household travel survey includes information about an individual's income, the accident reports provide educational attainment. Figure A.4 in Appendix A plots the average income of adults (age > 18) from the household survey by educational attainment, illustrating the strong positive correlation between income and educational attainment in the São Paulo sample.

[45]This comparison also assumes that the effects of the speed limit reduction on both road accidents and travel time do not depend on an individual's educational attainment. For example, if two individuals with distinct income levels are observed making the same trip on the Marginais Highways, then we assume that the effects on their travel times after the speed limit reduction were also the same.

little attention in the academic literature and in policy discussions regarding the Decade of Road Safety. We are not aware of any existing empirical evidence or theoretical result that demonstrates that (lower) speed limits disproportionately benefit low-income people. We explore the mechanisms that underlie these distributional effects in Panel B of Figure 5, which plots the share of private vehicle utilization and road accidents at different levels of educational attainment. Values are presented relative to the population of São Paulo, such that negative value indicates that the share of individuals from the corresponding group is lower than the share of individuals from this group in the population. These plots indicate that the progressive effects are explained by: (1) the intuitive fact that a disproportionate share of the costs of longer commutes is born by higher income people who use private vehicles and main transit corridors and (2) a less intuitive fact that low and low-middle income individuals have a much higher incidence of becoming the fatal victims of road accidents. Possible explanations for the differences in fatality rates include differences in motorcycle utilization rates, differences in safety features on vehicles, and differences in driving behavior.

6 Conclusion

This paper evaluates the effect of policies that altered traffic speed limits in one of the most highly congested and dangerous cities for drivers in the world: São Paulo, Brazil. We demonstrate that a series of speed limit reductions in 2015 resulted in a substantial (21.7%) reduction in road accidents on treated road segments, resulting in 1,889 averted road accidents and 104 averted fatalities within the first 18 months of adoption. Our findings provide evidence that camera-based enforcement augmented the effect of the speed limit change on accidents. Evidence from a variety of sources suggests that this reduction in accidents cannot be attributed to road substitution or other factors affecting pre-treatment accident trends. The total accident reductions more than doubles when we account for behavioral spillovers induced by the policy. However, the benefits from positive spillovers are largely offset by spillover impacts on travel costs. Measurements from more than 1 million queries of trip durations using a sample of representative trips and a web API indicate that the estimated travel time for users fell by 5.5% immediately following a speed limit increase adopted in 2017. We do not find evidence of effects on travel time reliability.

Road safety programs are often motivated by concern about road accidents and omit discussion about effects on congestion or other economic costs. The larger (and growing) fraction of damages associated with road injuries in developing country cities have made them a focus of this effort, though it is important to also address concern about the larger potential costs. In São Paulo, deep concern about the effect of speed limit reductions made this a voting issue in the Mayoral election and ultimately resulted in a partial reversal of the policy. This is only the second study that we are aware of that compares the benefits and costs of a speed limit change and the first to do so in a congested urban setting or in the developing world. We combine our reduced form results with standard parameters from the literature to compare the costs and benefits of the speed limit reduction adopted on the urban highways of the city of São Paulo in 2015. Our estimates indicate that the benefits of the policy (reduction in number of accidents) outweighed the costs (increased travel time) even in our most conservative choice of estimates. Our study provides evidence that speed limit reductions can be rationalized in cities that are concerned about severe congestion problems.

Our dataset allows us to disaggregate policy costs and benefits by income and educational attainment. We find evidence that the speed limit reduction likely had strongly progressive impacts. While increased travel times facing car trips on São Paulo's main highways disproportionately affected wealthier individuals, the reduction in fatal accidents disproportionately benefited lower- and middle-income groups. This effect is particularly strong for pedestrians and motorcyclists, which make up the largest group of victims of fatal road accidents in Brazil. Among residents with less than primary education, fatal road accidents in São Paulo are a key contributor to unnatural deaths, representing as large a threat as violence (each of these causes accounts for roughly 20% of unnatural deaths). Our findings suggest that road safety policies such as speed limit reductions have had a considerably larger effect on fatalities than gun control and other policies aimed to reduce violent crimes and have particular benefits for reducing the fatality risk of the urban poor.

Our analysis is limited in several respects. Our representative sample of trips constrains our analysis to trips made by residents on business days, so we do not account for policy impacts on freight transportation and trips made by non-residents. Therefore, we may not be accounting for an important share of social costs associated with a speed limit reduction. We do not find any evidence of policy effects on pollution using data from monitoring stations in São Paulo. The external validity of our results is clearly limited given that we study a single city. Our reduced-form results are surprisingly consistent with those reported for interstate highways in the United States (van Benthem, 2015). Several cities throughout the world are experimenting with stricter speed limit policies and the United Nations has highlighted policy impact evaluation as a goal of the Decade of Road Safety program. This paper suggests that careful evaluation of policy experiments can yield rich and nuanced information about effectiveness, enforcement, benefits, and the distributional implications of driving regulations. We emphasize the potential value of broader comparison of outcomes similar to the ones obtained in our study to evaluate the heterogeneity of policy impacts in different programs and cities.

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Tables

Table 1. Data Descriptives - Accidents and Crawled Trips

Panel A: Traffic Accidents per Year in São Paulo by Road Type (2012-2017)

	Total	2012	2013	2014	2015	2016	2017
São Paulo City Total							
Accidents	125,769	26,928	25,501	23,547	20,258	16,052	13,483
Fatalities	5,997	1,160	1,068	1,177	941	855	796
Treated Highways (Marginais)							
Accidents	5,016	1,039	1,044	1,168	787	509	469
Fatalities	276	53	44	63	50	29	37
Treated Arterial Roads							
Accidents	41,524	8,826	8,712	8,233	6,869	4,916	3,968
Fatalities	1,960	399	354	390	286	282	249
Non-Treated Avenues (Control Group)							
Accidents	47,404	10,027	9,320	8,420	7,619	6,564	5,454
Fatalities	2,242	411	388	457	347	332	307
Local Streets							
Accidents	31,825	7,036	6,425	5,726	4,983	4,063	3,592
Fatalities	1,519	297	282	267	258	212	203

Panel B: Trips Simulated Using Google Directions API

	All Crawled Trips				Trips Using Marginais Highways			
	Obs. (Thousand)	Share	mean	s.d.	Obs. (Thousand)	Share	mean	s.d.
Crawled Trips	1,471.6	1.00			243.7	1.00		
Post Speed Limit Increase	511.5	0.35			84.8	0.35		
Peak	562.2	0.38			96.8	0.40		
Rain	93.6	0.06			15.6	0.06		
Use Marginais Highways	243.7	0.17			243.7	1.00		
Travel Time (Minutes)			20.00	17.11			38.42	19.83
Travel Length (KM)			8.31	9.18			18.49	12.24
Percent on Marginais			0.04	0.12			0.22	0.21

Notes: Panel A - This table was created using data on road accidents compiled by the São Paulo Transit Agency (CET). The datasets were obtained by the authors through a series of LAI (Lei de Acesso à Informacão) requests. Treated Highways include the Marginal Pinheiros and Marginal Tietê Highways. Treated arterial roads are all arterial roads where a reduction was implemented in 2015. Non-treated avenues is the subset of non-treated road segments labeled as either *avenida* or *estrada*. Local Streets are all non-treated road segments.

Panel B: Crawled Trips are real-time observations of trip duration collected from the Google Directions API between July 4, 2016 and September 1, 2017. Trip origin and destination coordinates were taken from the 2012 São Paulo Mobility Household Survey. We collected trip durations for each survey trip at 20 minute intervals within 2 hours of the trip departure time in the survey, resulting in a panel of trip-by-time observations across multiple days of the week that is repeated before and after the policy. The speed limit increase in the Marginais Highways was implemented in January 25, 2017. We identify trips that use the Marginais Highways by comparing the intersection between the optimal trip path suggested by OSRM API and a 200m buffer around the Marginais shapefile. All trips with more than 400m of intersection between the optimal path and the buffer are defined as utilizing the Marginais Highways (treated). Estimated travel times under real-time traffic conditions and travel distance are reported by Google Directions API for each query.

Table 2. Effect of Speed Limit Reduction and Camera Enforcement on Road Accidents

	Dependent variable: number of accidents per segment per month				
	Event Study		Event study with controls		
	Unweighted	CATT	(1)	(2)	(3)
Panel A:					
Quarters after speed limit reduction					
1	-0.169 ***	-0.167 ***	-0.119 ***	-0.188 ***	-0.124 ***
	(0.038)	(0.038)	(0.030)	(0.033)	(0.036)
2	-0.189 ***	-0.194 ***	-0.185 ***	-0.253 ***	-0.184 ***
	(0.040)	(0.039)	(0.030)	(0.033)	(0.037)
3	-0.188 ***	-0.199 ***	-0.144 ***	-0.220 ***	-0.147 ***
	(0.043)	(0.044)	(0.036)	(0.039)	(0.043)
4	-0.274 ***	-0.278 ***	-0.148 ***	-0.234 ***	-0.158 ***
	(0.035)	(0.035)	(0.036)	(0.041)	(0.047)
5	-0.405 ***	-0.409 ***	-0.250 ***	-0.323 ***	-0.260 ***
	(0.032)	(0.033)	(0.035)	(0.040)	(0.045)
6	-0.355 ***	-0.353 ***	-0.195 ***	-0.274 ***	-0.217 ***
	(0.037)	(0.037)	(0.037)	(0.042)	(0.046)
Camera on segment					
camera	0.033	0.031	-0.034	0.005	0.009
	(0.037)	(0.037)	(0.030)	(0.034)	(0.034)
camera × speed limit reduction	-0.128 *	-0.115 **	-0.091	-0.117 *	-0.118 *
	(0.052)	(0.042)	(0.056)	(0.054)	(0.054)
Panel B:					
Speed limit reduction (SLR)					
Marginais highways	-0.458 ***	-0.459 ***	-0.330 ***	-0.383 ***	-0.324 ***
6th quarter after SLR	(0.062)	(0.064)	(0.081)	(0.078)	(0.085)
Arterial Roads	-0.341 ***	-0.337 ***	-0.176 ***	-0.251 ***	-0.194 ***
6th quarter after SLR	(0.040)	(0.040)	(0.039)	(0.042)	(0.046)
Treatment group	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	Matched arterial and highways
Control group	None	None	All non-treated avenues	Non-treated ave. >1.6km away from treatment	Non-treated ave., >1.6km away from treatment, matched to treatment segm.
Segment FE	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	No	Yes	Yes	Yes
Parametric funct. form	Yes	Yes	No	No	No
Observations	100,572	100,572	542,004	254,436	222,108

Notes: All specifications are estimated using a Poisson regression model with coefficients reported as relative incidence ratios. For instance, a coefficient of 0.1 indicates a 10% increase in the incidence of accidents. Standard errors are adjusted using a delta method approximation and are clustered by road (202 clusters). All specifications are estimated using a monthly panel of road segments, though specifications differ w.r.t. the observations included in each regression. The event study specifications only include treated segments. CATT effects are weighted by the share of observations within each treated cohort and the model is saturated with cohort and time fixed effects. Both sets of base event study estimates use a parametric functional form with a linear time trend and two city-wide covariates that change over time (fuel sales and total number of traffic cameras). Specifications with controls use non-treated road segments as controls. These models substitute month fixed effects for the linear time trend and city-wide covariates. Panel A reports estimates of average effects on arterial roads and highways. Panel B separates the speed limit reduction effect by road type (arterial road vs highway). Initial quarter effects (1-5) are also estimated in the model used to construct Panel B, but those coefficients are omitted for conciseness.

Table 3. Effect of Speed Limit Reductions on Traffic Tickets

	Non Speeding Tickets		Speeding Tickets
	Driving Restriction	Other non-Speeding Tickets	
Quarters after Speed Limit Reduction			
1	-0.061	-0.121	0.814 ***
	(0.038)	(0.066)	(0.059)
2	-0.035	0.032	0.831 ***
	(0.044)	(0.078)	(0.068)
3	0.012	0.045	0.818 ***
	(0.046)	(0.084)	(0.071)
4	-0.026	0.071	0.770 ***
	(0.047)	(0.086)	(0.073)
5	-0.013	-0.016	0.751 ***
	(0.047)	(0.086)	(0.072)
6	-0.020	0.000	0.750 ***
	(0.042)	(0.078)	(0.062)
Month Fe	Yes	Yes	Yes
Segment FE	Yes	Yes	Yes
Obs.	2,160	2,124	3,240

Notes: The dependent variables in these regressions are the log of the number of tickets issued by cameras that were continuously active between January 2015 and December 2017. Coefficients indicate relative changes in the number of tickets issued by cameras located on treated segments compared to non-treated ones.

Table 4. Effects of Marginais Speed Limit Increase (2017) on Travel Times

	Changes in Log of Estimated Travel Time			
	(1)	(2)	(3)	(4)
Post SLI - Ratio at Marg.	-0.068 ***	-0.061 ***	-0.055 ***	
	(0.014)	(0.014)	(0.013)	
Post SLI - Ratio at Marg. - Peak				-0.055 **
				(0.017)
Post SLI - Ratio at Marg. - Off-Peak				-0.056 ***
				(0.011)
Post SLI	-0.018 ***	-0.011 ***		
	(0.004)	(0.003)		
Post SLI - spillover area 1km		-0.034 ***	-0.034 ***	-0.034 ***
		(0.008)	(0.008)	(0.008)
Post SLI - spillover area 3km		-0.016 **	-0.016 ***	-0.016 ***
		(0.005)	(0.005)	(0.005)
Post SLI - spillover area 5km		-0.009	-0.011 **	-0.011 **
		(0.004)	(0.004)	(0.004)
Rain	0.019 ***	0.019 ***		
	(0.005)	(0.005)		
Holiday	-0.100 ***	-0.100 ***		
	(0.009)	(0.009)		
Trip-Hour FE	Yes	Yes	Yes	Yes
Motorcycle Late Night Restriction	Yes	Yes	Yes	Yes
Spillover Area Specific Effects	No	Yes	Yes	Yes
Date-Hour FE	No	No	Yes	Yes
Obs.	1,337,555	1,337,555	1,337,555	1,337,555

Notes: Standard errors are clustered by date (191 clusters). Trip origin and destination coordinates were taken from the 2012 São Paulo Mobility Household Survey. We collected trip durations for each survey trip at 20 minute intervals within 2 hours of the trip departure time in the survey, resulting in a panel of trip-by-time observations across multiple days of the week that is repeated before and after the policy. The dependent variable in all regressions is the log of the estimated duration (minutes) of each trip queried on Google Directions API. Post SLI is a dummy that indicates queries made after the speed limit increase on the Marginais Highways in January 25, 2017. Rain is a dummy indicating if there was rain in the hour that each query was made. Trip-Hour fixed effects include a specific intercept for each pair of survey trip origin and destination coordinates queried at a certain time of day, such that estimates reflect differences in the average duration of a trip at a given time before and after the policy.

Table 5. Annual Costs and Benefits of the Speed Limit Program: Marginais Highways

PANEL A: BENEFITS

	Baseline *Before Speed Limit Change*	Counterfactuals: *(6th quarter after change)* Reduced Form Estimates from: Base Event Study CATT		Sample (3) event study w/ matched controls	
		SLR	SLR & Cameras	SLR	SLR & Cameras
A1 - All Days, All Treated Roads					
Accidents					
without policy change	8,568	7,040	7,040	5,709	5,709
with policy change		4,477	4,392	4,466	4,392
Policy Benefits					
Averted Accidents		**2,563**	**2,648**	**1,243**	**1,317**
Benefits from Averted Accidents (R$ million)		**853.9**	**882.2**	**414.2**	**438.8**
A2 - Business Days, Marginais Highways					
Accidents					
without policy change	687	646	646	514	514
with policy change		340	335	340	335
Policy Benefits					
Averted Accidents		**306**	**312**	**174**	**179**
Benefits from Averted Accidents (R$ million)		**101.8**	**103.8**	**58.0**	**59.7**

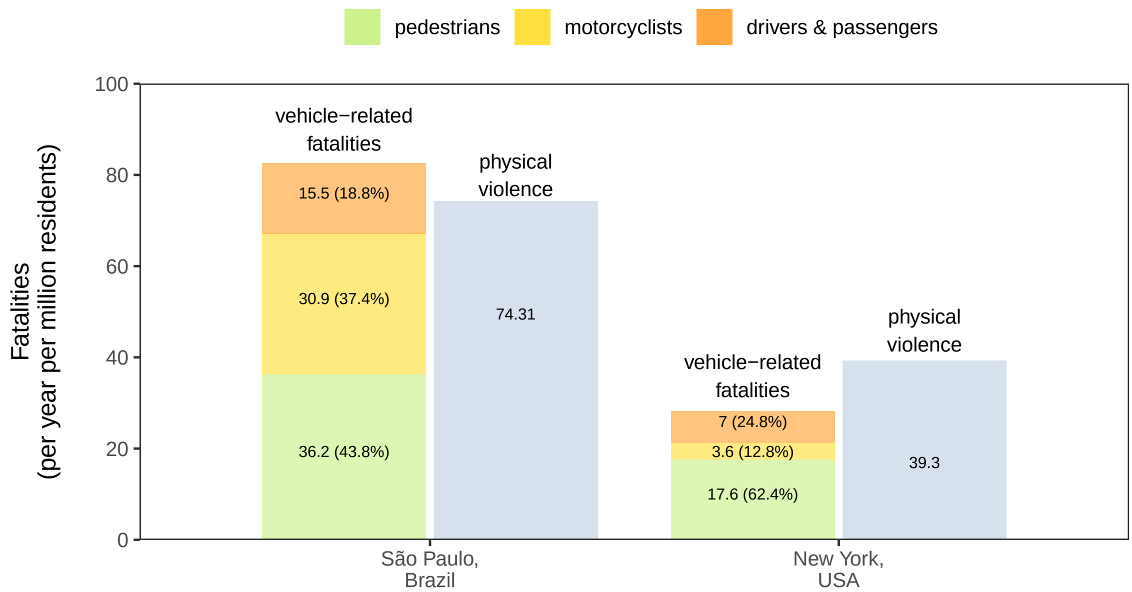
PANEL B: COSTS

	Baseline *Without Speed Limit Change*	Speed Limit Policy	**Policy Cost**
B1 - Business Days, Marginais Highways			
Time Spent in Traffic (million hours)	1,119.1	1,115.1	**-4.1**
Cost of Time Spent in Traffic (R$ million)			
VOT = VTPI individual VOT	7,104.0	7,071.2	**-32.8**
VOT = 50% of median net wage	5,755.9	5,735.0	**-20.9**
VOT = 50% of individual net wage	7,560.1	7,527.1	**-32.9**

Notes: Benefits are calculated using counterfactual scenarios from the CATT event study and the event study with matched control segments in Table 2 Panel B. Cohort-specific CATT estimates weight the counterfactual accident counts by the sample of segments in each cohort. For each model, we compare the counterfactual without any speed limit change to 2 policy scenarios: 1) the speed limit reductions are adopted alone and a 2) the speed limit reduction is accompanied by an expansion of cameras equivalent with what was observed in 2015. Panel A1 includes estimated benefits for the whole year and for all treated roads. In Panel A2, we restrict the calculation to business days and to the Marginais Highways, so values are comparable to the results from Panel B. Costs are calculated using estimates from Table 4 and alternative Value of Time (VOT) parameters. The first column uses 50% of individual after-tax wages as the value of an individual's time spent in transit, taken from a representative survey conducted by the transit authority in the city of São Paulo. The second column uses the VTPI guidelines, which assign different VOT values based on trip motivation, which are also taken from the survey. The third column assigns the 50% of the median after-tax wage as the VOT for all individuals in the sample.

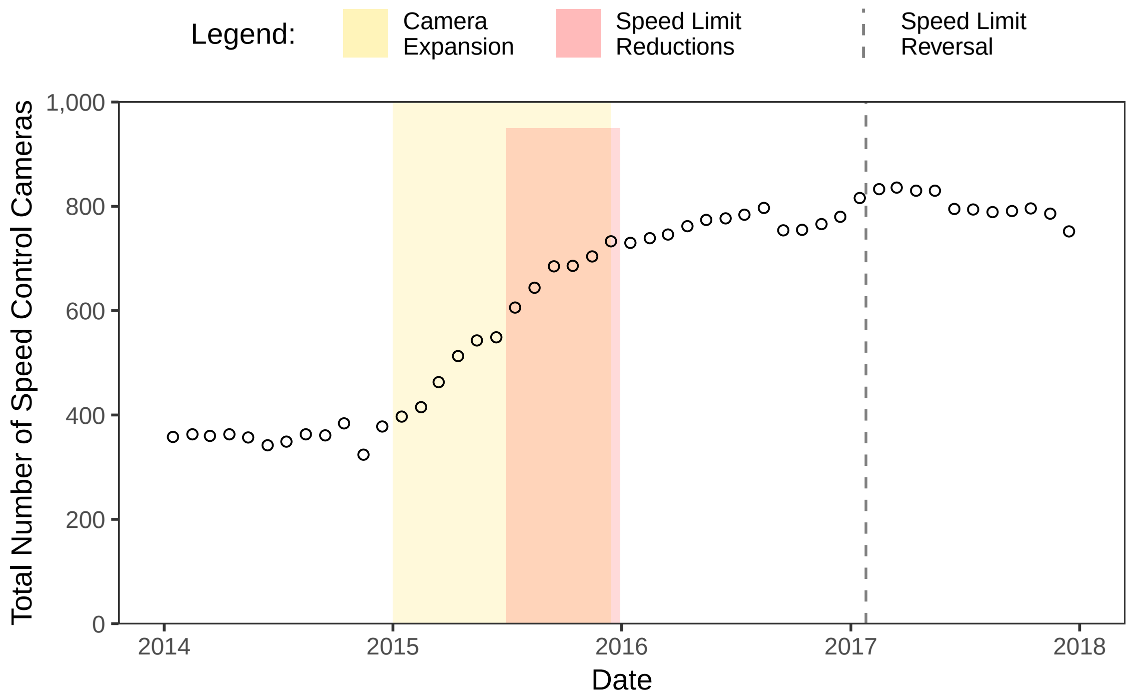
Figures

Figure 1. Road Fatalities and Homicides per Million Residents: São Paulo vs. New York



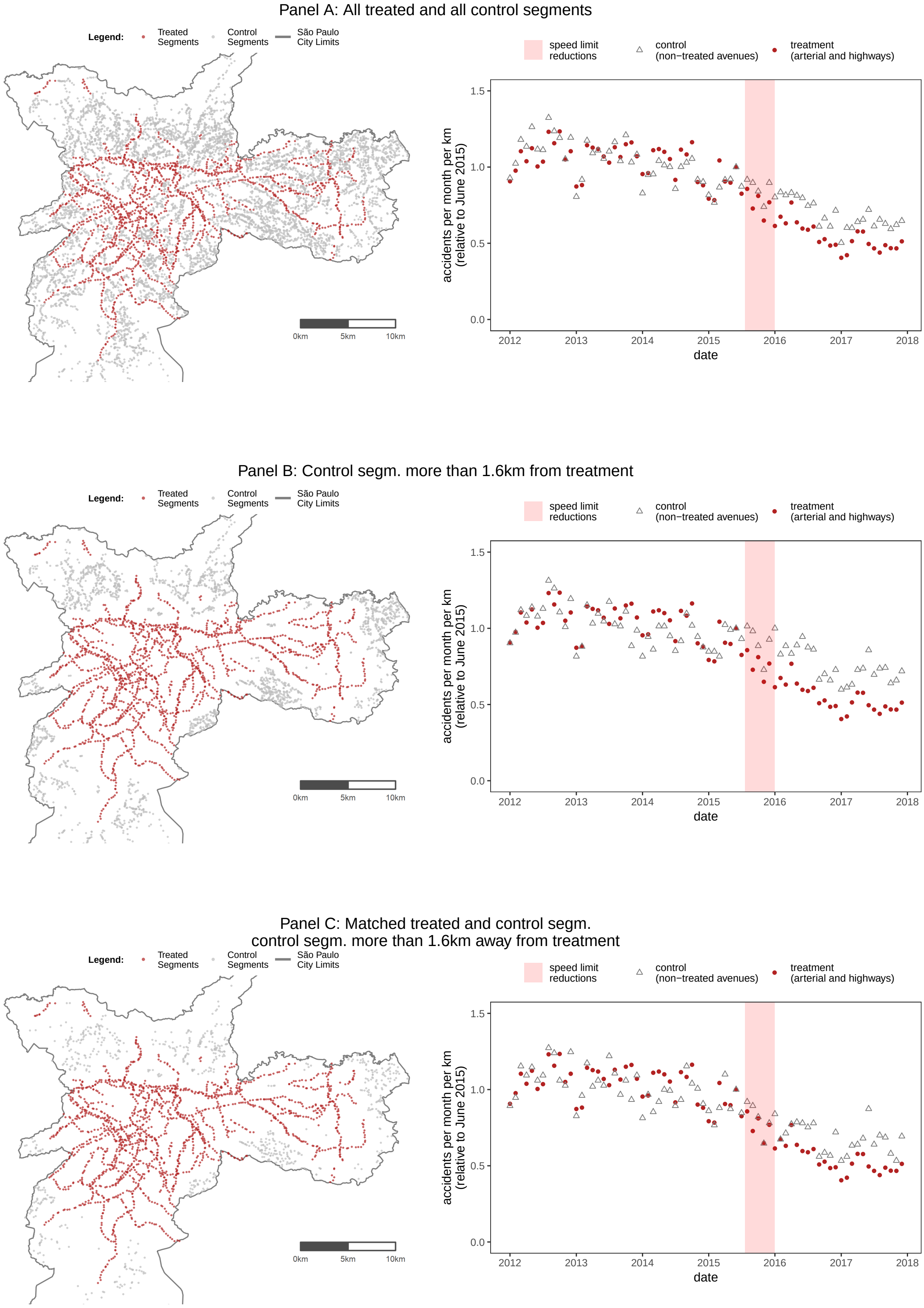
Notes: Data for New York is from the City's Vision Zero Four Year Report (NYC, 2018) and the NYPD Crime and Enforcement Activity Reports. Data for São Paulo comes from reports of road accidents compiled by the Transit Agency of São Paulo (CET, 2017) and the SSP-SP statistical data website.

Figure 2. Growth of Speed Control Cameras in São Paulo



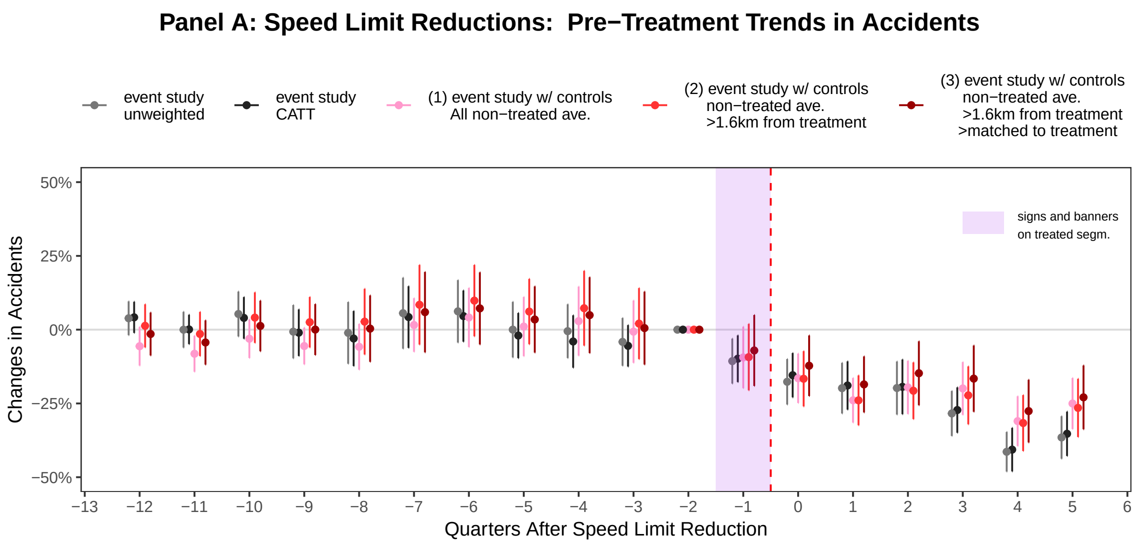
Notes: Monthly observations of speed monitoring cameras compiled from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET).

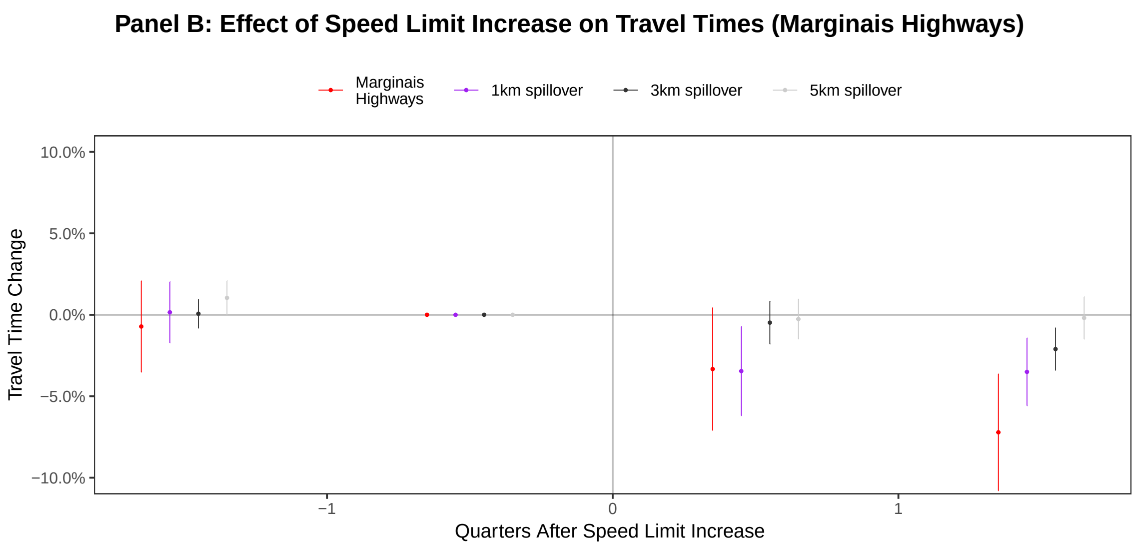
Figure 3. Road Accidents Occurring on Treated and Control Segments (2012-2017)



Notes: In the maps on the left side of the figure, treated segments include all arterial roads and highways where speed limits were reduced in 2015. In Panel A, the control group includes all non-treated avenues and express roads in São Paulo. In Panel B, the control segments are restricted to segments that are more than 1,600m away from any treated road. Panel C restricts the control segments included in panel B using a matching procedure based on total accidents per segment before the beginning of the treatment period. The plots to the right of each map plot the raw data series of accidents on the corresponding treatment and control groups. Each point represents the average number of accidents per km in a given calendar month relative to the values observed in June, 2015, the last month before the implementation of speed limit reductions began.

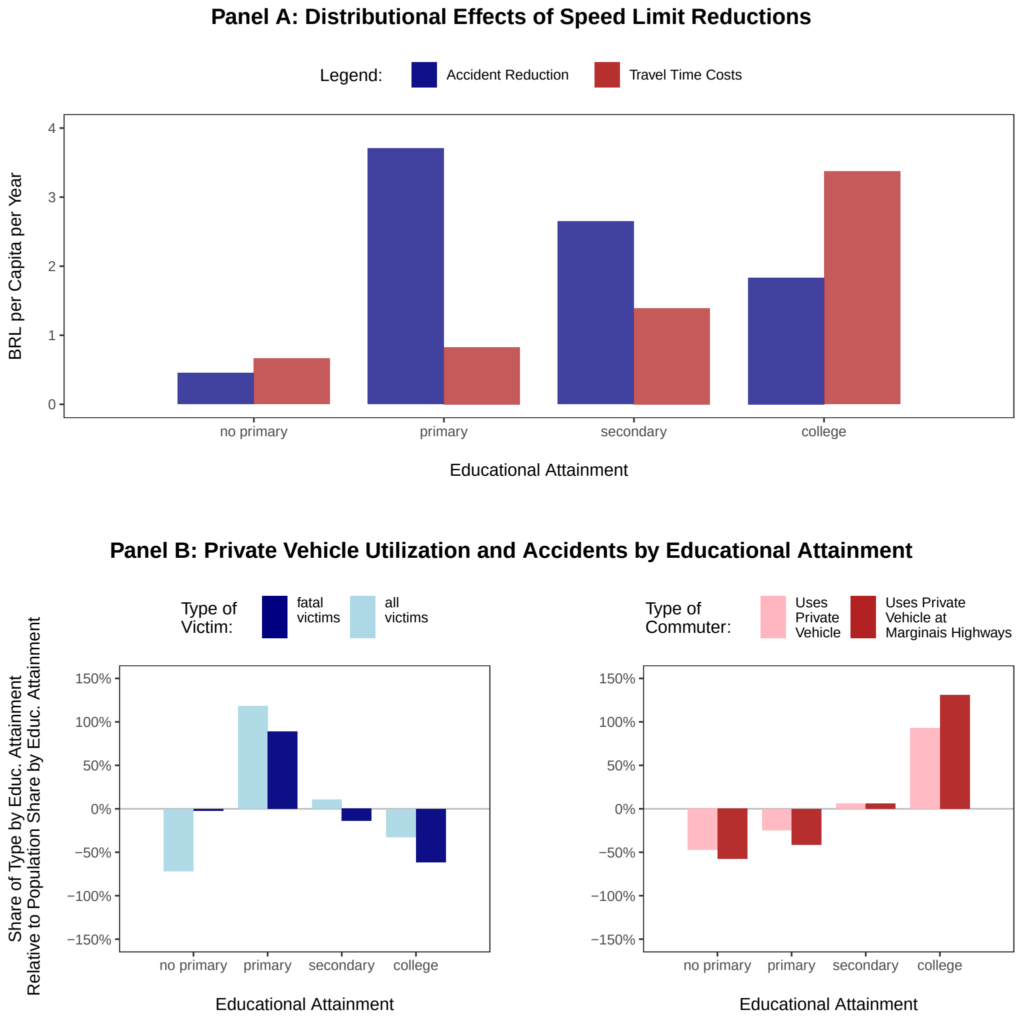
Figure 4. Dynamic Event Study Results: Pre-Treatment Trends





Notes: Panel A - The models reported in the figure are closely related to the models presented in Table 2, though models above include relative quarter leads of treatment and using the second relative quarter before treatment as the reference period as in Borusyak and Jaravel (2016).
Panel B - Coefficients plotted in the figure are closely related to the models presented in Table 4, though models above include relative quarter leads of treatment and using the second relative quarter before treatment as the reference period as in Borusyak and Jaravel (2016). All coefficients correspond to relative changes in travel time relative to trips completely made outside 5km from the Marginais highways.

Figure 5. Distributional Effects of Speed Limit Reductions



Notes: Panel A - The figure compares the average costs and benefits per capita of a speed limit reduction on the Marginais Highways by the level of educational attainment of affected individuals. The values are calculated per year considering business days only. The distribution of benefits from accident reductions is assumed to be proportional to the distribution of accident costs in the baseline period. Travel time costs are assumed to be proportional to the share of each representative trip taken on highways. To make costs and benefits comparable and illustrate differences that come directly from policy effects, we assign all individuals a uniform VOT and VSL using the average value for the population of adults in São Paulo. Panel B - The Y-axis indicates the difference between the share of individuals in each educational attainment group by type relative to the share of each educational group in the population. For example, a value of 50% for accident fatalities with primary educational attainment indicate that the share of fatal victims with primary educational attainment is 50% larger than the share of individuals with primary educational attainment in the whole population.

Appendices

A Background and data appendix

A.1 Treated Road Segments

To construct our panel of treated road segments, we extensively reviewed the online newsletter of the São Paulo Traffic Agency (http://www.cetsp.com.br/noticias). When the speed limit reductions were being implemented, the Agency would post in its newsletter all the upcoming changes that would be adopted in the following days. Table A.1 summarizes all reports we identified in the newsletter that were informing about speed limit changes that took place in 2015 and that were used in our analyses. Additionally, Figure A.1 shows in further details one of these reports. It specifies the date when the change would be implemented, the previous and the new speed limits, and the exact road segments that would be affected by the change.

Based on the information collected from these reports, we constructed a shapefile of treated road segments including the date of treatment and the type of change associated with each road. Figure A.2 maps all treated roads in São Paulo and highlights the Marginais Highways where speed limit was reduced from 90 km/h to 70 km/h.[46] Speed limits on arterial roads were reduced from 60 km/h to 50 km/h.[47] Figure A.3 shows the total length of treated segments throughout the second half of 2015. It is worth noting that policy implementation was evenly distributed throughout that period.

[46]These values correspond to the speed limit reduction on the express lanes of the Marginais Highway. On the intermediary and local lanes of the highways, speed limits were reduced respectively from 70 km/h to 60 km/h and from 70 km/h to 50 km/h.

[47]The only exceptions were the "Corredor Norte-Sul" (North-South Corridor), where the speed limit was reduced from 70 km/h to 60 km/h, and a small number of arterial road segments where the speed limit was reduced to 40 km/h.

Table A.1. 2015 Speed Limit Change Reports

Announcement Date	Speed Limit Change Date	Treated Roads	Link
7/8/2015	7/20/2015	AV MARGINAL DO RIO TIETE, AV MARGINAL DO RIO TIETE	http://www.capital.sp.gov.br/noticia/velocidade-maxima-das-marginais-sera-reduzida-a
7/30/2015	8/3/2015	AV JACU-PESSEGO, AV JACU-PESSEGO	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-na-avenida-jacu-pessego.aspx
7/30/2015	8/3/2015	AV ARICANDUVA, VD ENG ALBERTO BADRA	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-na-avenida-aricanduva.aspx
7/30/2015	8/3/2015	AV S JOAO, AV GAL OLIMPIO DA SILVEIRA, RUA AMARAL GURGEL	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-no-eixo-sao-joao-olimpio-da-silveira-amaral-gurgel.aspx
8/12/2015	8/17/2015	AV ANGELICA, AV ANGELICA, AV NADIR DIAS DE FIGUEIREDO, RUA MAJ NATANAEL, AV DR ABRAAO RIBEIRO, AV PACAEMBU	http://www.cetsp.com.br/noticias/2015/08/12/cet-implanta-cet-implanta-reducao-de-velocidade-maxima-em-mais-duas-vias.aspx
8/17/2015	8/20/2015	AV AFRANIO PEIXOTO, AV VALDEMAR FERREIRA, RUA HENRIQUE SCHAUMANN, AV PAULO VI, AV SUMARE, AV ANTARTICA, AV PROF MANUEL JOSE CHAVES, AV CARLOS CALDEIRA FILHO, AV VER JOSE DINIZ, ES DO CAMPO LIMPO	http://www.cetsp.com.br/noticias/2015/08/17/cet-implanta-reducao-de-velocidade-maxima-em-mais-11-vias-da-cidade.aspx
8/20/2015	8/23/2015	RUA DOMINGOS DE MORAIS, AV GUARAPIRANGA, ES M'BOI MIRIM, AV SEN TEOTONIO VILELA, AV ARNOLFO AZEVEDO, RUA ALM PEREIRA GUIMARAES, RUA DOMINGOS DE MORAIS	http://www.cetsp.com.br/noticias/2015/08/20/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias-da-cidade-(1).aspx
8/24/2015	8/27/2015	AV PEDROSO DE MORAIS, AV PROF FONSECA RODRIGUES, AV DR GASTAO VIDIGAL	http://www.cetsp.com.br/noticias/2015/08/24/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias-na-cidade.aspx
8/27/2015	8/31/2015	PTE ENG ARY TORRES, AV DOS BANDEIRANTES, AV AFFONSO D'ESCRAGNOLLE TAUNAY, CV MARIA MALUF, AV SANTOS DUMONT, AV TIRADENTES, AV PRESTES MAIA, TN PAPA JOAO PAULO II, AV VINTE E TRES DE MAIO, AV RUBEM BERTA, AV MOREIRA GUIMARAES, AV WASHINGTON LUIS, AV INTERLAGOS, AV WASHINGTON LUIS	http://www.cetsp.com.br/noticias/2015/08/27/cet-implanta-reducao-de-velocidade-maxima-em-mais-16-vias.aspx
9/3/2015	9/9/2015	AV SALIM FARAH MALUF, AV JUNTAS PROVISORIAS, RUA MALVINA FERRARA SAMARONE, AV PRES TANCREDO NEVES	http://www.cetsp.com.br/noticias/2015/09/03/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
9/4/2015	9/11/2015	AV FRANCISCO MATARAZZO, VD LESTE-OESTE, AV ALCANTARA MACHADO, RUA MELO FREIRE, AV CD DE FRONTIN, AV ANTONIO ESTEVAO DE CARVALHO, RUA DR LUIZ AYRES, RUA ENG SIDNEY APARECIDO DE MORAES, AV JOSE PINHEIRO BORGES	http://www.cetsp.com.br/noticias/2015/09/04/cet-implanta-reducao-de-velocidade-maxima-no-eixo-leste-oeste.aspx
9/14/2015	9/18/2015	RUA CARMOPOLIS DE MINAS, AV BANDEIRANTES DO SUL, RUA CEL GUILHERME ROCHA, RUA CIRO SOARES DE ALMEIDA, AV OLAVO FONTOURA, AV EDUC PAULO FREIRE	http://www.cetsp.com.br/noticias/2015/09/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-7-vias.aspx
9/18/2015	9/23/2015	AV PEDRO ALVARES CABRAL, AV BRASIL, AV JABAQUARA, AV JABAQUARA	http://www.cetsp.com.br/noticias/2015/09/18/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
9/22/2015	9/25/2015	AV DO ESTADO, AV DO ESTADO, AV ATLANTICA	http://www.cetsp.com.br/noticias/2015/09/22/cet-implanta-reducao-de-velocidade-maxima-em-mais-2-vias.aspx
9/24/2015	9/30/2015	AV VITOR MANZINI, PTE DO SOCORRO	http://www.cetsp.com.br/noticias/2015/09/24/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
9/30/2015	10/2/2015	AV DOM PEDRO I, RUA TEREZA CRISTINA, AV NAZARE, AV DR RICARDO JAFET, AV DR RICARDO JAFET, AV PROF ABRAAO DE MORAIS	http://www.cetsp.com.br/noticias/2015/09/30/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
10/1/2015	10/7/2015	RUA MANUEL DA NOBREGA, AV REPUBLICA DO LIBANO, AV INDIANOPOLIS,	http://www.cetsp.com.br/noticias/2015/10/01/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/6/2015	10/9/2015	AV BRIG FARIA LIMA, RUA DOS PINHEIROS, AV HELIO PELEGRINO, RUA INHAMBU, TN SEBASTIAO CAMARGO, AV PRES JUSCELINO KUBITSCHEK, CV TRIBUNAL DE JUSTICA, RUA ANTONIO MOURA ANDRADE, CV AYRTON SENNA	http://www.cetsp.com.br/noticias/2015/10/06/cet-implanta-reducao-de-velocidade-maxima-em-mais-9-vias.aspx
10/9/2015	10/14/2015	AV PRES WILSON, RUA S RAIMUNDO, RUA S RAIMUNDO, RUA MANOEL PEREIRA DA SILVA, RUA MANOEL PEREIRA DA SILVA, AV DR FRANCISCO MESQUITA	http://www.cetsp.com.br/noticias/2015/10/09/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
10/14/2015	10/16/2015	AV REBOUCAS, AV EUSEBIO MATOSO, TN JORN FERNANDO VIEIRA DE MELO	http://www.cetsp.com.br/noticias/2015/10/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx

Table A.2. 2015 Speed Limit Change Reports (Continuation)

Announcement Date	Speed Limit Change Date	Treated Roads	Link
10/16/2015	10/21/2015	AV PROF FRANCISCO MORATO, AV EMERICO RICHTER	http://www.cetsp.com.br/noticias/2015/10/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/20/2015	10/23/2015	AV DR ARNALDO, AV JORN ROBERTO MARINHO, PTE OCTAVIO FRIAS DE OLIVEIRA, AV JOAO SIMAO DE CASTRO	http://www.cetsp.com.br/noticias/2015/10/20/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/22/2015	10/28/2015	AV ROQUE PETRONI JUNIOR, AV PROF VICENTE RAO, AV VER JOAO DE LUCA, RUA JUAN DE LA CRUZ, AV CUPECE	http://www.cetsp.com.br/noticias/2015/10/22/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
10/26/2015	10/30/2015	AV DR HUGO BEOLCHI, AV ENG ARMANDO DE ARRUDA PEREIRA, AV ENG GEORGE CORBISIER	http://www.cetsp.com.br/noticias/2015/10/26/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias-(1).aspx
10/29/2015	11/4/2015	AV CORIFEU DE AZEVEDO MARQUES, AV VITAL BRASIL, AV DOS TAJURAS, TN PRES JANIO QUADROS, AV LINEU DE PAULA MACHADO	http://www.cetsp.com.br/noticias/2015/10/29/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
11/4/2015	11/6/2015	PTE ENG ROBERTO ROSSI ZUCCOLO, AV CIDADE JARDIM, TN MAX FEFFER, AV EUROPA, RUA COLOMBIA, RUA AUGUSTA, RUA NOVE DE JULHO	http://www.cetsp.com.br/noticias/2015/11/04/cet-implanta-reducao-de-velocidade-maxima-em-mais-7-vias-(1).aspx
11/16/2015	11/19/2015	AV ELISEU DE ALMEIDA, RUA PIRAJUSSARA, AV INTERCONTINENTAL, AV JAGUARE, AV ESCOLA POLITECNICA, AV ESCOLA POLITECNICA, AV DR ANTONIO MARIA LAET, AV DR ANTONIO MARIA LAET, RUA PARANABI, RUA ARARITAGUABA, RUA ARARITAGUABA, AV DO POETA	http://www.cetsp.com.br/noticias/2015/11/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-10-vias.aspx
11/19/2015	11/25/2015	AV S GABRIEL, AV SANTO AMARO, AV JOAO DIAS, AV ADOLFO PINHEIRO, RUA RHONE, AV ADUTORA DO RIO CLARO	http://www.cetsp.com.br/noticias/2015/11/19/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
11/23/2015	11/27/2015	AV MIGUEL IGNACIO CURI, RUA CASTELO DO PIAUI, AV RAGUEB CHOHFI, ES IGUATEMI	http://www.cetsp.com.br/noticias/2015/11/23/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
11/27/2015	12/2/2015	AV PAES DE BARROS, RUA TAQUARI, RUA BRESSER, VD BRESSER, AV BERNARDINO BRITO FONSECA DE CA, AV BERNARDINO BRITO FONSECA DE CA, AV PROF EDGAR SANTOS, AV PROF EDGAR SANTOS, AV ITAQUERA	http://www.cetsp.com.br/noticias/2015/11/27/cet-implanta-reducao-de-velocidade-maxima-em-mais-8-vias.aspx
12/2/2015	12/4/2015	AV PIRES DO RIO, AV DEP JOSE ARISTODEMO PINOTTI, AV DEP JOSE ARISTODEMO PINOTTI, ES DO IMPERADOR, ES DE MOGI DAS CRUZES, RUA EMBIRA, AV S MIGUEL	http://www.cetsp.com.br/noticias/2015/12/02/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
12/4/2015	12/9/2015	RUA DR ASSIS RIBEIRO, AV VER ABEL FERREIRA, RUA BRIG GAVIAO PEIXOTO, RUA MONTE PASCAL, VD DOMINGOS DE MORAES, AV GAL EDGAR FACO	http://www.cetsp.com.br/noticias/2015/12/04/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
12/9/2015	12/11/2015	AV INAJAR DE SOUZA, AV INAJAR DE SOUZA, AV COMEN MARTINELLI, AV ERMANO MARCHETTI, AV MARQ DE SAO VICENTE, RUA SERGIO TOMAS, RUA NORMA PIERUCCINI GIANNOTTI, AV RUDGE, VD ENG ORLANDO MURGEL, AV RIO BRANCO, AV ORDEM E PROGRESSO, PTE JULIO DE MESQUITA NETO, AV NICOLAS BOER, VD POMPEIA, AV ALEXANDRE COLARES, AV MANOEL MONTEIRO DE ARAUJO, AV DOMINGOS DE SOUZA MARQUES, AV ALM DELAMARE, RUA ANCHIETA, RUA FUNCHAL, AV CHEDID JAFET	http://www.cetsp.com.br/noticias/2015/12/09/cet-implanta-reducao-de-velocidade-maxima-em-mais-24-vias.aspx
12/14/2015	12/16/2015	AV SARG MIGUEL DE SOUSA FILHO, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV SERAFIM GONCALVES PEREIRA, AV MORUMBI	http://www.cetsp.com.br/noticias/2015/12/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
12/16/2015	12/18/2015	RUA MANOEL BARBOSA, AV RAIMUNDO PEREIRA DE MAGAL., RUA PRINCIPAL(PERUS), RUA GUIDO CALOI, AV GIOVANNI GRONCHI, ES DO ALVARENGA, RUA DR JOSE MARIA WHITAKER, RUA ALVINOPOLIS, AV ANTONIO BATUIRA, AV QUEIROZ FILHO, RUA CERRO CORA, RUA CERRO CORA, RUA CERRO CORA, RUA CONS MOREIRA DE BARROS, RUA MAUA	http://www.cetsp.com.br/noticias/2015/12/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-14-vias.aspx
12/16/2015	12/18/2015	AV DUQ DE CAXIAS	http://capital.sp.gov.br/noticia/cet-implanta-reducao-de-velocidade-maxima-em-mais-14-vias
12/30/2015	12/29/2015	AV LUIZ GUSHIKEN	http://www.cetsp.com.br/noticias/2015/12/30/cet-implanta-reducao-de-velocidade-na-avenida-luiz-gushiken.aspx

Figure A.1. Traffic Agency Report of an Upcoming Speed Limit Reduction (2015)

» Emergências no Trânsito: Ligue 1188

Companhia de Engenharia de Tráfego

PREFEITURA DE SÃO PAULO MOBILIDADE E TRANSPORTES

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CET » Notícias »
Programa de Proteção à Vida - CET implanta redução de velocidade na Avenida Aricanduva

A- | A+

Notícias

30/07/2015 02:43 Por: CET

Programa de Proteção à Vida - CET implanta redução de velocidade na Avenida Aricanduva

A ação visa aumentar a segurança para usuários e incentivar o melhor compartilhamento do espaço urbano

A Companhia de Engenharia de Tráfego implantará a partir desta segunda-feira (03/08) redução de velocidade na Avenida Aricanduva e no Elevado Engenheiro Alberto Badra, na Zona Leste, onde será regulamentada a velocidade máxima de 50 km/h. Atualmente, a velocidade permitida é de 60 km/h. A medida está inserida no plano de redução de acidentes viários do "Programa de Proteção à Vida". Com isso, pretende-se melhorar a segurança dos usuários mais vulneráveis do sistema viário, pedestres e ciclistas, buscando a convivência pacífica e a redução de acidentes e atropelamentos na área.

Alterações no Sistema Viário

- Regulamentação de velocidade máxima de 50 km/h na Avenida Aricanduva e no Elevado Engenheiro Alberto Badra, em ambos os sentidos, toda extensão (13,4 km), conforme folha informativa anexa.

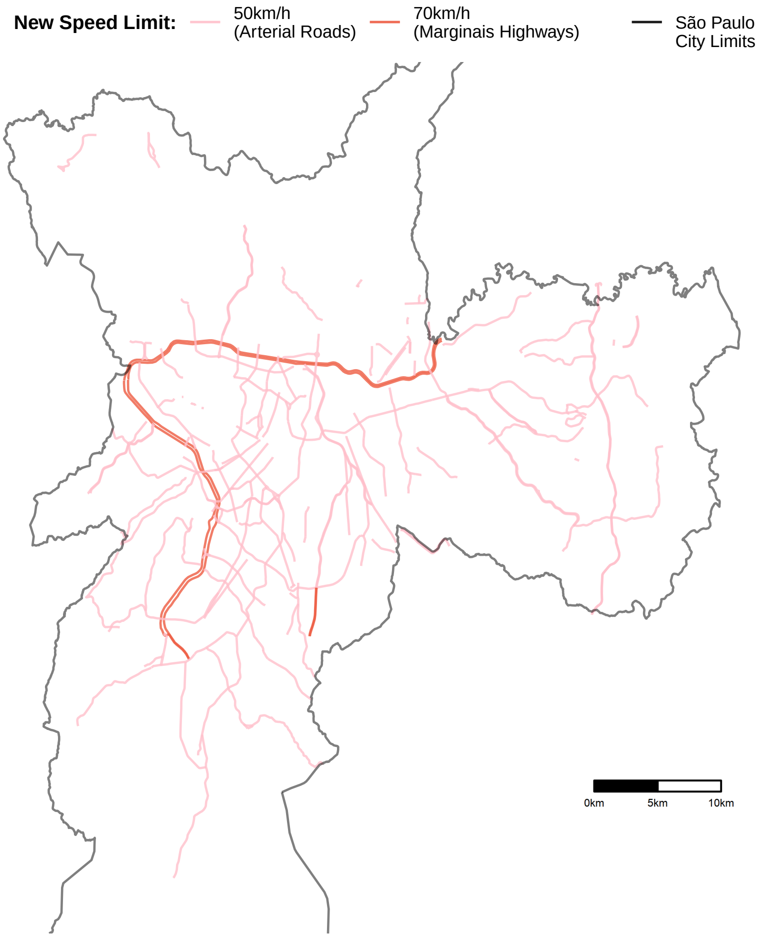
A medida se faz necessária vista que a referida via possui tráfego intenso e grande volume de pedestres. Na Avenida Aricanduva, entre janeiro de 2011 e dezembro de 2014, aconteceram 828 acidentes veiculares com vítimas e 132 atropelamentos, resultando em 1286 pessoas feridas e 45 mortes. Desta forma, velocidades menores são necessárias para promover condições seguras e confortáveis de circulação aos pedestres que ainda representam o maior número de vítimas no trânsito em São Paulo.

Para implantação da redução de velocidade na Avenida Jacu-Pêssego serão utilizadas 246 novas placas de sinalização vertical de regulamentação e advertência, além de nove faixas de vinil e 10 banners nos principais acessos, alertando os motoristas quanto à mudança.

A Engenharia de Campo da CET vai acompanhar o desempenho da nova medida e orientar o tráfego na região.

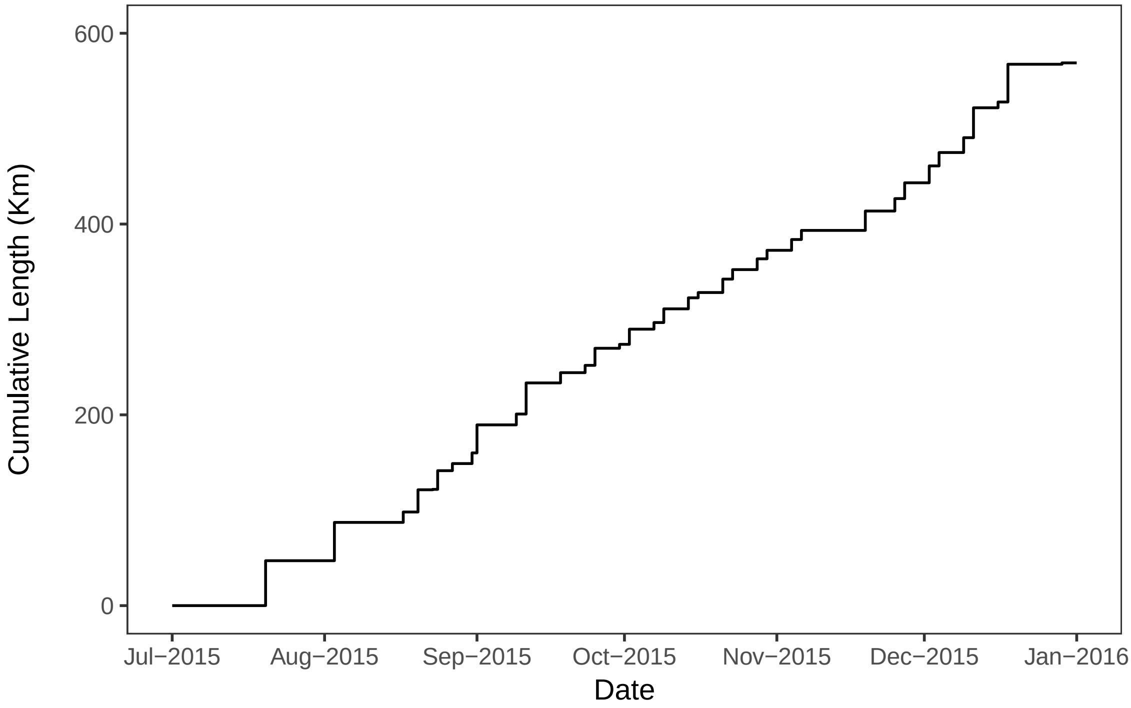
Fale com a CET - Ligue 1188. Atende 24 horas para informações de trânsito, ocorrências, reclamações, remoções e sugestões.

Figure A.2. Road Segments with Speed Limit Reductions by New Speed Limit



Notes: Information about speed limit changes was scraped from the newsletters of the São Paulo Transit Agency Website (http://www.cetsp.com.br/noticias.aspx).

Figure A.3. Cumulative Length of Road Segments with Speed Limit Reductions



Notes: Information about speed limit changes was scraped from the newsletters of the São Paulo Transit Agency Website (http://www.cetsp.com.br/noticias.aspx).

A.2 Characteristics of Motorized Trips and Travelers

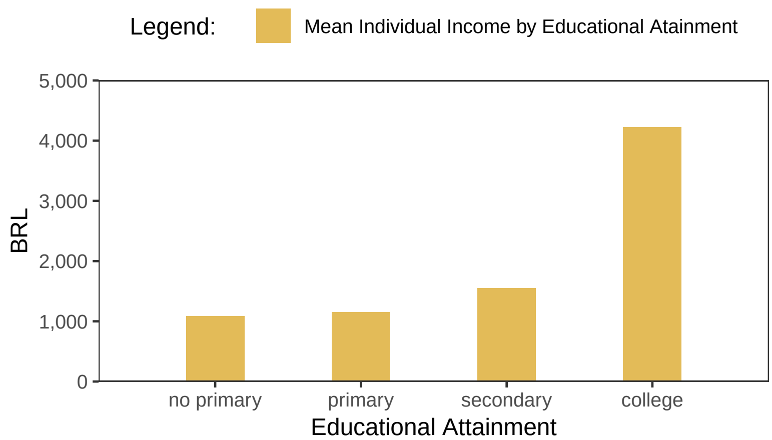
Table A.3 presents basic descriptive statistics for a representative set of motorized trips made on a regular weekday in São Paulo, Brazil. This table was constructed using trip data from the Mobility Household Survey of 2012, which we used as the source of representative trips for which we collected repeated observations of trip durations on the Google Directions API.[48] We also used the data from the household travel survey to plot the relationship between the educational attainment of individuals and their income, which is shown in Figure A.4 below. As expected, the relationship is very clear, with larger average income for individuals with higher educational attainment.

Table A.3. Characteristics of Motorized Trips in a Weekday in São Paulo

	Trips per Day (Million)	Share	Mean Distance (km)	Mean Duration (minutes)
Motorized trips	29.74	1.00	7.99	50.53
By mode				
Bus	11.78	0.40	6.83	58.73
Rail	4.36	0.15	16.61	88.56
Car	12.49	0.42	6.02	31.46
Motorcycle	1.04	0.03	8.50	27.86
By motivation				
Work	16.81	0.57	9.88	60.25
Education	7.51	0.25	4.73	35.72
Other	4.18	0.14	6.90	43.14

Notes: This table was created by the authors based on data from the 2012 Mobility Household Survey of São Paulo (Pesquisa de Mobilidade Urbana 2012).

Figure A.4. Average Individual Income by Educational Attainment



Notes: Based on the sample of adult individuals from the 2012 São Paulo Household Travel Survey.

[48]Further details about the survey can be found at http://www.metro.sp.gov.br/pesquisa-od/resultado-das-pesquisas.aspx

A.3 Cameras and Traffic Tickets

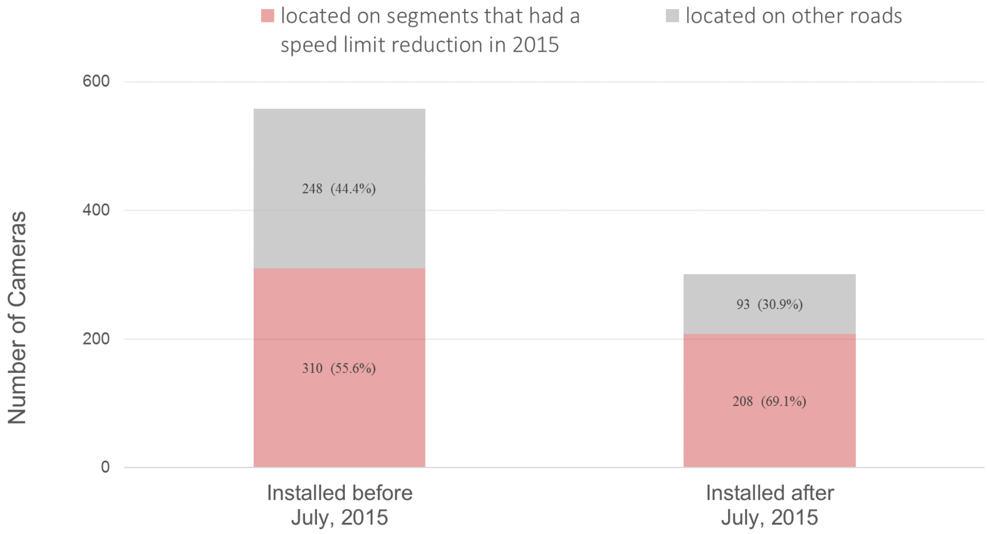
Throughout 2015, there was a substantial expansion in the installation of traffic cameras in São Paulo. However, the installation of these new cameras was not random with respect to location. More speed cameras were installed on road segments where speed limits were reduced. Figure A.5 shows the location of cameras installed before and after July 20, 2015 (the first speed limit change). 55.6% of the cameras installed before July 20, 2015 were installed on segments that were treated with speed limit reductions. That ratio increased to 69.1% as a result of installations occurring after that date. Figure A.6 maps the location of cameras installed after July 20, 2015. From the figure, we can see that more new cameras were installed on treated road segments, particularly the Marginais Highways. This data is available at the website "Painel Mobilidade Segura" (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo traffic agency.

Table A.4. Traffic Control Cameras and Tickets in São Paulo (2014-2017)

	Total	Year			
		2014	2015	2016	2017
A: São Paulo					
Cameras	814	384	712	776	814
Tickets (million)	38.0	6.21	9.61	12.09	10.07
Driving Restriction	9.9	1.76	2.41	3.02	2.69
Speeding	20.5	3.11	5.13	6.74	5.56
Other	7.6	1.34	2.07	2.33	1.81
B: Marginais Highways					
Cameras	98	15	59	81	98
Tickets (million)	9.66	1.40	2.50	3.61	2.15
Driving Restriction	2.31	0.32	0.55	0.77	0.67
Speeding	5.04	0.57	1.33	2.06	1.07
Other	2.30	0.51	0.61	0.78	0.41

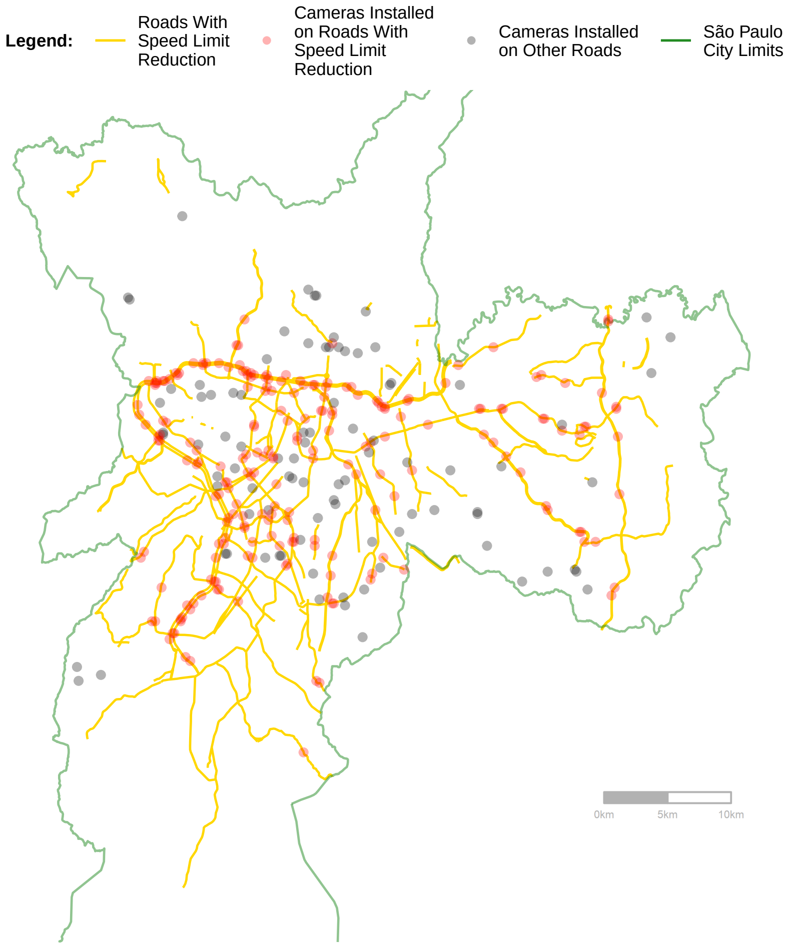
Notes: Table created by the authors based on data scraped from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET). In the case of cameras, the numbers indicate the maximum number of unique camera locations in any specific month. We use that metric because not all locations have camera equipment that is currently in use and so the total number of unique camera locations is not necessarily equal to the total number of cameras monitoring traffic in a given period.

Figure A.5. Speed Control Cameras in São Paulo by Date of Installation and Treatment Group



Notes: Information about cameras and their location was extracted from the website http://mobilidadesegura.prefeitura.sp.gov.br/ which is maintained by the São Paulo City Hall and compiles information about traffic violations in the city. Road segments are defined as ”Treatment Group” if their speed limit was reduced in 2015 and ”Other Roads” if the segment's speed was not altered in that year.

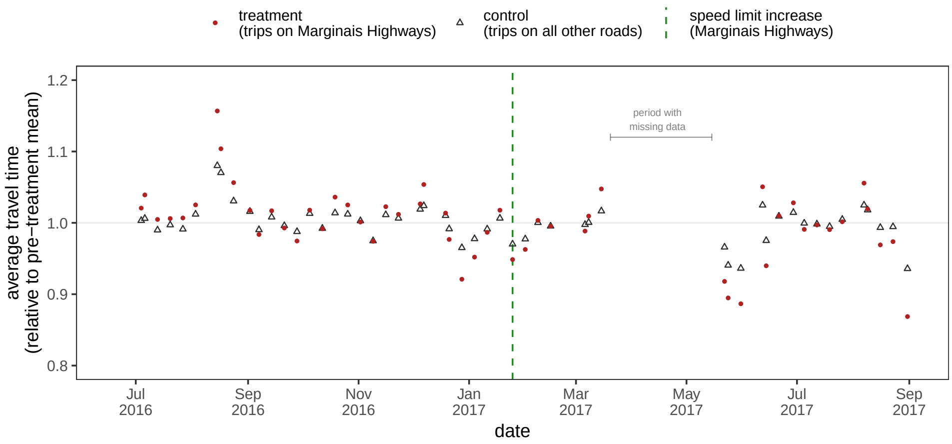
Figure A.6. Segments with Contemporaneous Speed Limit Reductions and Camera Installation



Notes: We identify the date a camera was installed using the earliest date for speeding tickets issued in each location. Data about traffic tickets was scraped from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET).

A.4 Crawled data - Estimated Travel Times

Figure A.7. Average Duration of Queried Trips (by week)

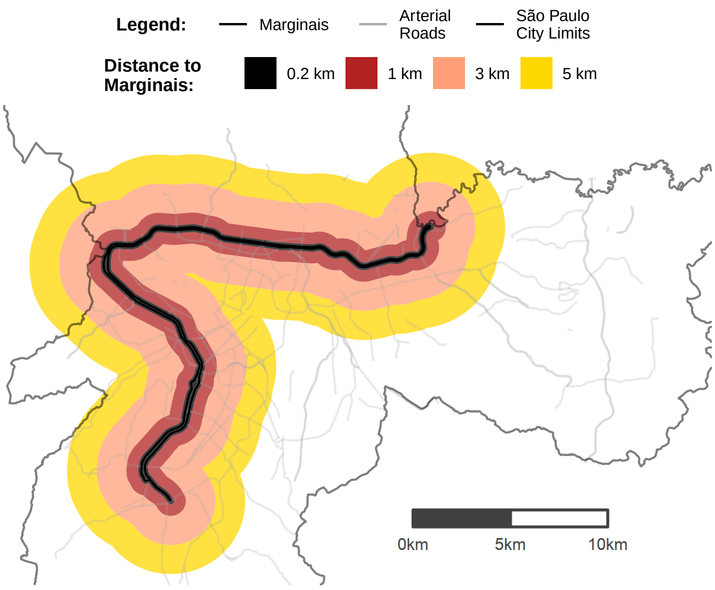


Notes: Each point corresponds to the average estimated travel time of crawled trips relative to its corresponding pre-treatment mean. Due to a server failure, our web-crawler was inactive between March 15, 2017 and May 21, 2017. Therefore, data from that period is missing from the series.

A.5 Spillover Areas of the Travel Time Empirical Model

Figure A.8 maps the road areas which we identified as possible spillover areas for the effects of the speed limit increase in the Marginiais Highways on Travel Time. The areas were constructed using non-overlapping buffers from the Marginais Highways shapefile.

Figure A.8. Marginais Highways: Treated and Spillover Zones

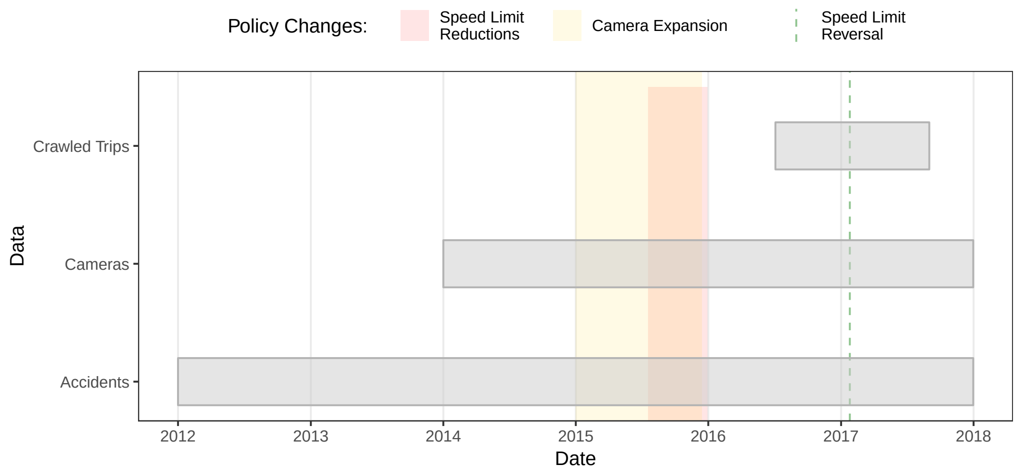


Notes: Buffers were constructed using the shapefile of the Marginais highways.

A.6 Timeline of Policy Changes and Data

Figure A.9 summarizes the timeline of policy changes and the temporal overlap of datasets included in our analysis. For the 2017 speed limit reversal, we have information about accidents, traffic cameras and crawled trips from both before and after the policy change date. However, in the case of the speed limit reductions of 2015, we do not have information about estimated travel times from before the policy because trip queries began in mid-2016. The dataset of road accidents begins in 2012, allowing us to evaluate secular trends in road accidents for both the treatment and control groups.

Figure A.9. Timeline of Speed Limit Changes and Datasets



Notes: Crawled Trips refer to representative trips observed on a household travel survey for which trip durations for the same set of origins and destinations were collected in real time at repeated intervals using Google Directions API. Data about speed monitoring cameras was scraped from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET). The datasets of road accidents were obtained by the authors through a series of LAI (Lei de Acesso à Informacão) requests.

A.7 Signs Announcing Upcoming Speed Changes

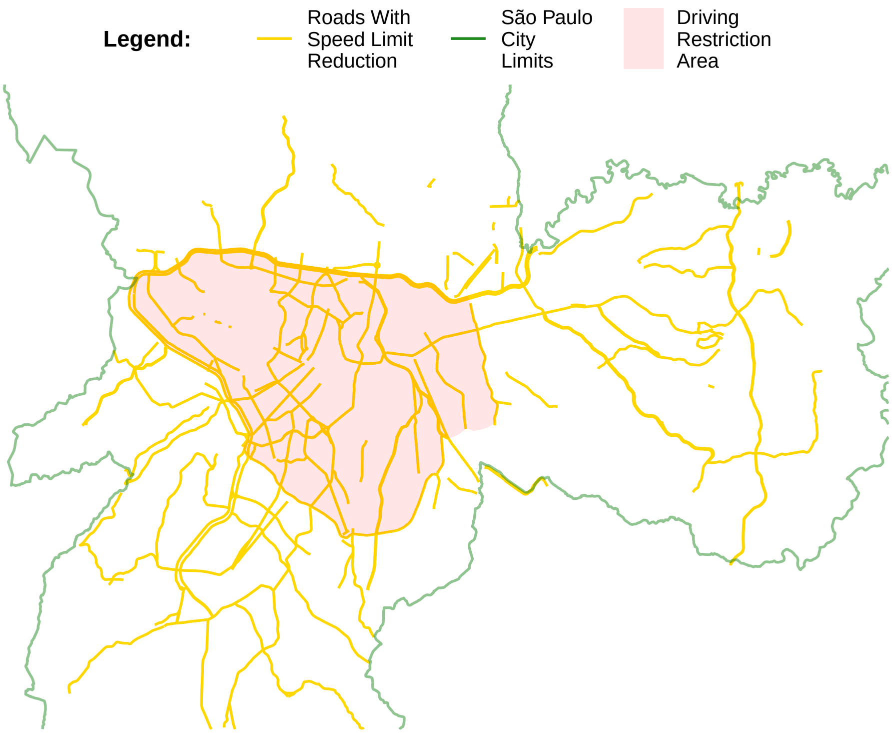
Before the new speed limits were implemented, the Traffic Agency of São Paulo would put up banners and signs along the relevant road segments indicating the upcoming change. Figure A.10 shows an example of these signs. These banners would inform drivers about the new speed limits and the dates when they would go into effect. Unfortunately, the Traffic Agency did not retain records of the exact dates when these banners and signs were placed on each road segment. However, because the signs closely resemble actual speed limit signs, we believe that they could cause drivers to start driving more slowly even before the actual date of speed limit change in each segment. In our robustness analyses, we find evidence of a reduction in accidents on treated segments in the quarter immediately before treatment. To avoid any bias in our main results, our baseline specification excludes observations from the quarter immediately before the speed limit reduction in each road segment.

Figure A.10. Banners Indicating an Upcoming Speed Limit Reduction (2015)

A.8 São Paulo Driving Restriction

The city of São Paulo enforces a driving restriction scheme that limits the circulation of 20% of vehicles during peak-hours in the central area of the city. All Brazilian license plates have a number as their final digit, so the driving restriction of São Paulo limits the circulation of vehicles with license plates ending in 2 different numbers. For example, on Mondays, vehicle license plate numbers ending in 1 and 2 are not allowed to circulate in the São Paulo Downtown Area. Figure A.11 shows the Driving Restriction Area of São Paulo and its overlap with the streets which had their speed limit altered in the period of our analysis.

Figure A.11. Road Segments with Speed Limit Reductions and the Driving Restriction



A.9 Speed Limits and Public Transit Trips

The speed limit reduction affected the city highways and arterial roads and included a large expansion of camera enforcement, while the speed limit reversal of 2017 was restricted to the Marginais Highways and was not accompanied by any substantial changes in enforcement. The estimates do not account for possible increases in travel time on public transit on the Marginais Highways, which account for a small share (1.7%) of public transit trips in the city. A descriptive calculation using city-wide aggregates at http://painel.scipopulis.com/ indicates an average reduction of approximately 4.6% on the average speed of buses if we compare the first half of 2016 (after the speed limit reductions) and the first half of 2015 (before the speed limit reductions). The change was larger on buses that circulate on exclusive lanes (-6.1%), and it was more limited in the case of buses that share the roads with other vehicles (-2.7%). While these differences fall within the range of our estimates of average changes in travel time as a result of the reversal, we cannot infer that they are the direct effect of the speed limit changes without disaggregate data on the speed of public buses that would allow us to account for possible confounding factors. Benefits associated with changes in accidents involving buses are captured in our analysis of accident reductions. 4.2% of victims and 6.4% of fatalities are from passengers of buses, indicating that this is a safer mode of transit that represents a limited share of the benefits from the policy.

B Alternative Specification of Empirical Models

In this Appendix, we verify the sensitivity and robustness of our empirical results. We estimate the policy treatment effect using alternative model specifications and compare their results with the preferred specification reported in the paper.

B.1 Tests for Differences in Pre-treatment Accident Trends

We test the parallel trends assumption in the pre-treatment period by extending our baseline specification with leads of treatment associated with relative-quarters before the speed limit reduction in each segment:

$$log\left(E\left(y_{it}\right)\right)=\alpha_i+\beta_t+\left(\sum_{q=-8,q\neq-1}^{0}\xi_q D_{it}^q\right)+\left(\sum_{q=1}^{7}\gamma_q D_{it}^q\right)+\zeta C_{it}+\eta C_{it}SLR_{it}+\phi SLI_{it} \quad (5)$$

If the parallel trends assumption holds, then all coefficients ξ_q should be equal to zero.[49] Figure 4 plots the coefficients γ and α for the samples of segments used in Models 1 and 2. Results are consistent across specifications. Relative to treated segments, accidents on control segments appear to increase slightly 2 years before the policy change and then exhibit a parallel trend for the year preceding treatment. None of the coefficients associated with periods preceding treatment are statistically different from zero, providing support for the parallel trends assumption. We also note that the coefficients associated with the quarter immediately preceding the speed limit reduction do suggest a

[49]Due to concern about anticipatory effects in the period immediately preceding the policy change on each segment, we include a coefficient that controls for changes during the anticipatory quarter and set the second quarter preceding the reduction to be the reference period.

small reduction in accidents, which we attribute to anticipatory behavior induced by the installation of banners and signs on treated segments on the weeks preceding the speed limit change in each road. Observations from that period are excluded from our preferred estimates of policy effects.

B.2 The Effect of the 2017 Reversal on Accidents

Our primary estimates of the effect of the speed limit program focuses on the speed limit reductions of 2015. We exclude observations from the period after the speed limit reversal of 2017 to simplify interpretation. The 2017 reversal was restricted to a single pair of roads in São Paulo, making it more difficult to isolate the policy effect and to estimate it with sufficient precision. In this section, we report the results of the extended version of our model that includes the reversal period. The model extends equations 1 and 2 from the main text by including observations from 2017 (after the reversal), and an additional indicator I_{it} for segments from the Marginais Highways during the post-reversal period.

$$log\left(E\left(y_{it}\right)\right) = \alpha_i + \beta_t + \left(\sum_{q=1}^{7} \gamma_q D_{it}^q\right) + \zeta C_{it} + \eta C_{it} SLR_{it} + \mu I_{it} \tag{6}$$

where μ gives the average effect of the reversal on the number of accidents on the Marginais Highways. Estimates of μ are reported in Table B.1. Point estimates of the effect of the speed limit reversal suggest an <u>increase</u> of 10.3%-17% in accidents on the Marginais Highways after the speed limit <u>increase</u>. However, fewer segments were affected by this policy and our sample is limited to the first four quarters after the adoption of the policy, both of which affect our ability to precisely estimate longer-term effects. Based on these results, we cannot rule out an effect of the same magnitude observed within the first four quarters of the speed limit reduction (14.8-27.8% from Table 2) or zero effect. We note that point estimates from the reversal period suggest continued declines on arterial roads where the 2015 speed limit reduction was not reversed. If we examine the difference between changes on arterials (which may be the best available counterfactual for previously treated segments on the Marginais highways), then point estimates suggest a range of 17.4-19.6% increases, though none of the effects are significant.

We are cautious about interpreting the specific effect of the policy *reversal* on accidents due to the concomitant adoption of compensating safety measures during the post-reversal period.[50] In particular, four months after the 2017 reversal (May 13, 2017), a restriction was placed on the use of motorcycles between 10pm-5am on the main lanes of the highway. Our analysis of the impact of the 2017 reversal on travel times uses the exact dates and times of queries during this period to control for confounding effect of the nighttime motorcycle restriction. We do not have the ability to add time-specific controls to Model 6 with the accident data. With this limitation in mind, we interpret the opposite effect sign of the point estimates for the speed limit increase as consistent with the expected relationship between speed limit regulation on accident risk and, though highly imprecise, we find the smaller magnitudes suggestive that compensating safety measures implemented at the time of the reversal may have had some mitigating effect on increases in accident risk during the reversal period.

[50]These measures included: the construction of elevated road steps on pedestrian crossing points at Marginais feeder lanes, placement of signs warning drivers about the presence of pedestrians, and placement of traffic agents with speed control pistols along the Marginais Highway (CET, 2016).

Table B.1. Effect of the 2017 Reversal on Accidents (Marginais Highways)

	Dependent variable: number of accidents per segment per month				
	Event Study		Event study with controls		
	Unweighted	CATT	(1)	(2)	(3)
Speed limit increase on Marginais Highways					
(A): Marginais Highways	0.163 *	0.121	0.170 *	0.142	0.103
	(0.079)	(0.241)	(0.080)	(0.087)	(0.086)
(B): Arterial Roads	-0.021	-0.049	-0.026	-0.040	-0.070
	(0.045)	(0.102)	(0.056)	(0.070)	(0.069)
(A) - (B)	0.184	0.170	0.196	0.182	0.174
	(0.108)	(0.186)	(0.109)	(0.110)	(0.110)
Treatment group	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	Matched arterial and highways
Control group	None	None	All non-treated avenues	Non-treated ave. >1.6km away from treatment	Non-treated ave., >1.6km away from treatment, matched to treatment segm.
Segment FE	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	No	Yes	Yes	Yes
Parametric funct. form	Yes	Yes	No	No	No
Observations	100,572	100,572	542,004	254,436	222,108

Notes: The table reports the results of an extended version of our main specification where we include segment observations for the period after the speed limit increase in the Marginais Highways in January 2017. In these specifications, we add additional terms in each model to capture the effect of the post-reversal period on each type of segment. These terms are reported in the table. The table also reports the difference between Highways and arterial roads for each model. Refer to table for the baseline model 2

B.3 Robustness of Accident Results to Alternative Model Specifications

We estimate two alternative versions of our main model: (1) A specification that does not discard observations from the quarter immediately before the policy adoption in each segment; (2) A negative binomial version of the main empirical model; (3) A linear version of the main empirical model. In the latter case, to estimate coefficients directly as a relative change ratio and compare those results directly with our baseline model, we transform the dependent variable by dividing each value by the average number of accident per segment and estimate the following model:

$$\frac{y_{it}}{\bar{y_i}} = \alpha_i + \beta_t + \left(\sum_{q=1}^{7^+} \gamma_q \cdot D_{it} \right) + \zeta \cdot C_{it} + \eta \cdot C_{it} \cdot SLR_{it} + \phi SLI_{it} + \varepsilon_{it} \tag{7}$$

Where $\bar{y_i}$ is the average number of accidents per month on segment i during the baseline period and everything else is equal to equation 1. Table B.2 compares the long term policy effect estimates between the baseline Poisson model used in our main text and the three alternative specifications described above. When we include observations from the last quarter preceding treatment, our estimates fall by 6-7 pp in the event study specifications but are not statistically different. Estimates in models with controls are highly consistent and robust to this choice.

Results from estimates with the negative binomial model are highly consistent with those from the main poisson models. Estimates from the linear specification are substantially smaller in base event study models, which we attribute to the effects of a large number of segment-month observations with 0 accidents. Specifications with controls are consistent and robust.

Table B.2. Long-Term Policy Effect Using Alternative Model Specifications

	Dependent variable: number of accidents per segment per month				
	Event Study		Event study with controls		
	Unweighted	CATT	(1)	(2)	(3)
Long Term Speed Limit Reduction Effect					
Preferred (Poisson) Model	-0.355 ***	-0.353 ***	-0.195 ***	-0.274 ***	-0.218 ***
(omits quarter before treatment)	(0.037)	(0.037)	(0.037)	(0.042)	(0.046)
Alternate Sample	-0.287 ***	-0.283 ***	-0.191 ***	-0.266 ***	-0.215 ***
(includes quarter before treatment)	(0.034)	(0.034)	(0.036)	(0.042)	(0.045)
Negative Binomial	-0.355 ***	-0.354 ***	-0.187 ***	-0.265 ***	-0.208 ***
	(0.037)	(0.036)	(0.036)	(0.040)	(0.044)
Linear Model	-0.247 ***	-0.188 ***	-0.098 ***	-0.158 ***	-0.168 ***
	(0.046)	(0.044)	(0.027)	(0.035)	(0.035)
Treatment group	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	Matched arterial and highways
Control group	None	None	All non-treated avenues	Non-treated ave. >1.6km away from treatment	Non-treated ave., >1.6km away from treatment, matched to treatment segm.
Segment FE	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	No	Yes	Yes	Yes
Parametric funct. form	Yes	Yes	No	No	No
Observations	100,572	100,572	542,004	254,436	222,108

B.4 Robustness of Travel Time Results to Sample Subset

This section evaluates the robustness of our main travel time results to the inclusion of queries made 2 hours before or after the time that representative trips were originally made. In our baseline estimation, we included all API queries regardless of whether or not they matched the original departure time in the survey. Therefore, we compare our baseline estimates to an alternative regression where we subset our crawled trips observations to queries made at the exact time of the day when trips were made. The purpose of this exercise is to test whether the inclusion of those queries made on the interval around actual departure could lead to a significant bias in our results. Table B.4 presents the main policy effect estimates from both models. The differences between results are not statistically or economically different across models.

Table B.3. Effect of Speed Limit Increase on Trip Durations: Exact Trips

	Changes in Estimated Travel Time			
	(1)	(2)	(3)	(4)
Post SLI - Ratio at Marg.	-0.072 ***	-0.066 ***	-0.059 ***	
	(0.015)	(0.015)	(0.014)	
Post SLI - Ratio at Marg. - Peak				-0.056 **
				(0.018)
Post SLI - Ratio at Marg. - OffPeak				-0.062 ***
				(0.013)
Post SLI	-0.026 ***	-0.018 ***		
	(0.005)	(0.003)		
Rain	0.019 ***	0.019 ***		
	(0.006)	(0.006)		
Holiday	-0.104 ***	-0.104 ***		
	(0.010)	(0.010)		
Trip-Hour FE	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	Yes
Obs.	309,648	309,648	309,648	309,648

Notes: Coefficients indicate the average changes of dependent variables with respect to pre-treatment means. For example, a coefficient of -0.5 indicates a reduction of 50%. Standard errors are clustered by Date-Street (191 clusters). Post SLI is a dummy that indicates queries made after the speed limit increase on the Marginais highways in January 25, 2017. Rain is a dummy indicating that there was rain in the hour that the trip duration was collected from the Google Directions API. Trip-Hour fixed effects include a specific intercept for each pair of representative survey trip coordinates queried in a certain hour of the day.

B.5 Effects of Speed Limit Change on Travel Time Uncertainty

This section evaluates the effects of the speed limit reversal on uncertainty in the durations of trips made by private transport. Our main analysis provides evidence that changes in the speed limit impose costs on drivers by increasing the durations of trips made on slower roadways. It is also possible that speed limit changes affect the reliability of transit along treated roads by either increasing or reducing uncertainty in the durations of trips. We test for evidence of changes in uncertainty using a difference-in-differences estimator that computes the standard deviation of the duration of all trips queried for a given origin-destination pair at a given time before and after the reversal. Table B.4 reports the effects by level of treatment intensity. The estimates indicate an overall reduction in uncertainty across the study period but we do not find evidence of differential changes in the uncertainty in trip durations on roads where speed limits increased.

Table B.4. Effects of Speed Limit Increase on Travel Time Uncertainty

	Dependent Variable: Log of Standard Deviation Travel Time (by trip)
post speed limit increase	-0.108 ***
	(0.007)
post speed limit increase * Marginais	
0% to 10% of trip length on Marginais	-0.039
	(0.023)
10% to 20% of trip length on Marginais	0.011
	(0.043)
20% to 50% of trip length on Marginais	-0.005
	(0.031)
more than 50% of trip length on Marginais	-0.073
	(0.047)
Trip-Hour FE	Yes
Obs.	77,313

Notes: Coefficients in the table above are estimated using a difference-in-differences model where the outcome is the standard deviation of the duration of all queries made for a given trip and treatment is defined as trips taken along the Marginais Highways post 2017 reversal. Treatment effects are reported for 4 groups that vary by treatment intensity (share of travel along treated roads), such that coefficients measure the average relative change in standard deviation for each group. The specification includes trip fixed effects.

C Behavioral Spillovers: Accident Reductions and Ticketing on Nearby Roads

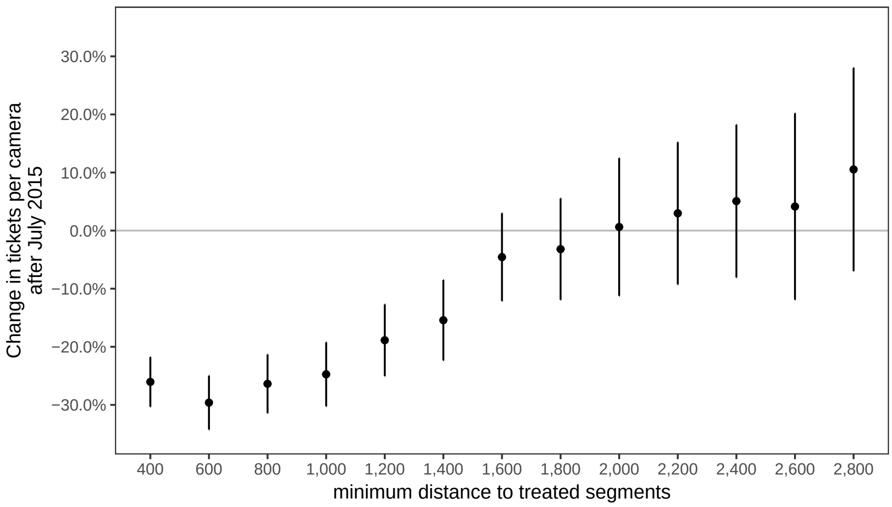
To evaluate possible changes in driver behavior on non-treated segments, we evaluate changes in the number of speeding tickets issued on segments that were nearby treated roads but were not treated. Given that most trips utilize a combination of treated and never-treated road segments, it is likely that any change in driver behavior induced by the policy also affects the untreated portion of a trip. While the speed change was fairly well advertised on treated roads, it could also be the case that drivers were not aware of the extent of the change and altered their behavior more generally. We use the following regression specification to evaluate behavioral spillovers:

$$y_{it} = \alpha_i + \beta \cdot SLR_t + \varepsilon_{it} \tag{8}$$

Where y_{it} is the log of the number of tickets issued by camera i in month t, and SLR_t is an indicator of panel observations from the period after July 2015 when the first speed limit reduction was adopted. Therefore, β indicates the average change in the number of tickets issued by cameras located on never-treated segments. To evaluate the geographic extent of behavioral spillovers, we estimate this model using control groups of never-treated segments at different distances away from treated roads. We begin by defining non-treated observations as any road segments located more than 400m away from treated roads, and we the increase that threshold by increments of 200m.

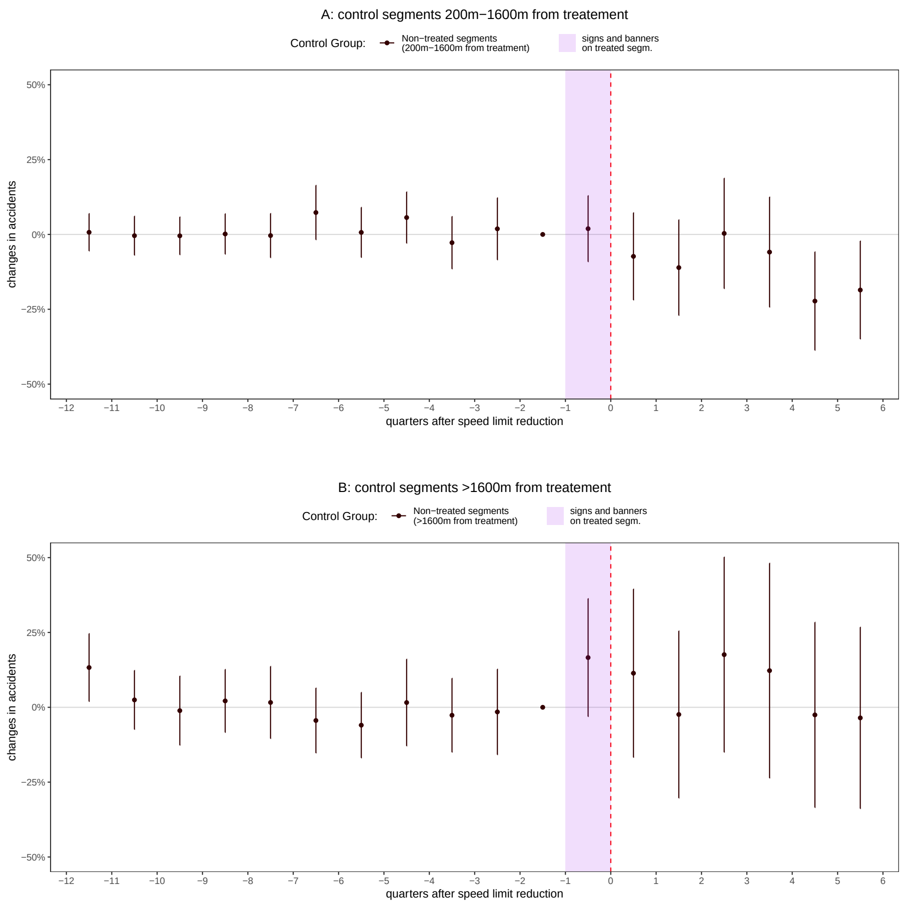
We plot the estimate of β for each of these distance thresholds in Figure C.1. At larger distance thresholds, the estimates become less precise as the number of cameras on never-treated segments becomes smaller. The estimates suggest a clear pattern of reductions in speeding tickets on non-treated roads adjacent to treated segments. The decline in tickets is statistically significant up to 1,600 meters from treated roads. This exercise provides evidence to suggest that drivers adjust their behavior not only on treated road segments, but also on the roads near treated areas.

Figure C.1. Changes in Speeding Tickets on Nearby Control Segments



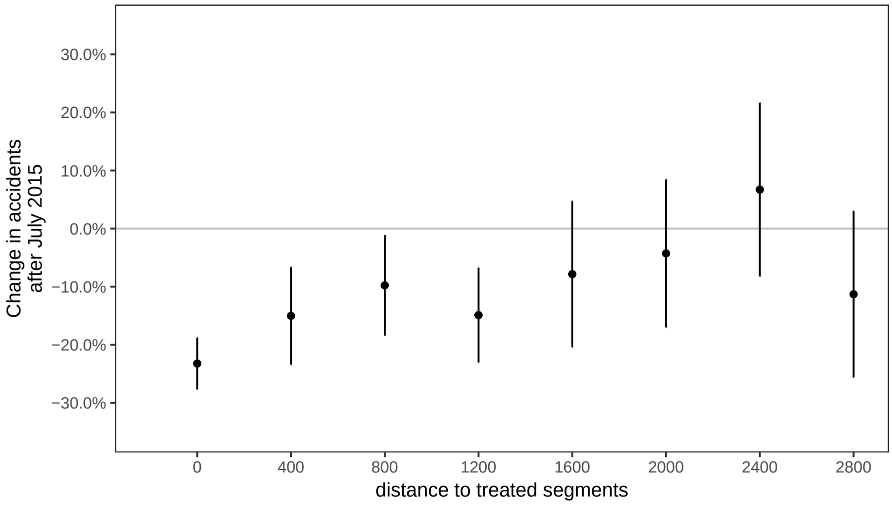
Next, we evaluate changes in accidents on never-treated segments that are close to treated roads. We use an event study model that mirrors our specification based on sample (1) in the main text, except that we are using distance-based control groups of never-treated segments as described above. Figure C.2 plots the result coefficients for each of these groups. Panel A plots the changes in road accidents by relative quarter for non-treated segments located within 1,600 meters of treated roads. This is the zone where behavioral spillovers are identified in Figure C.1. Panel B plots the same estimates using never-treated segments that are located more than 1,600 meters away from treated roads. In both cases, the coefficients associated with the periods preceding the policy change are not statistically different from zero. However, the estimates in Panel A indicate that there was a modest reduction in road accidents on nearby treated segments that was statistically different from what would be expected from secular trends in the period following the speed limit reduction. The same result is not observed in panel B.

Figure C.2. Changes in Accidents on Nearby Control Segments



A final test extends our main regression model to explicitly estimate longer-term changes in road accidents on non-treated segments conditional on their distance to treated roads. Never-treated segments are classified according to rings of increasing diameter around treated roads and we interact the post-treatment period with an indicator for segments located in each of these rings. Figure C.3 plots the estimates as a function of distance to treated segments. The leftmost coefficient (X=0) is the longer-term change in road accidents on treated road segments compared to changes observed on segments located more than 2,800 meters away from any treated road. The next coefficient illustrates the change in road accidents on non-treated segments located between 0-400 meters of treated roads, and so forth. The results suggest reductions in road accidents on never-treated segments located within 1,600 meters of treated roads. This reduction diminishes as distance to the treated roads increases.

Figure C.3. Regression Results: Long Term Changes in Accidents on Control Segments by Distance to Treated Segments



Taken together, these results provide evidence that the effects of speed limit reductions were not restricted to treated roads. Instead, there were significant policy spillover effects on nearby never-treated roads. First, we find a reduction in the number of speeding tickets on these nearby never-treated segments, thus indicating an adjustment in driver behavior. We also observe a decrease in road accidents that was inversely proportional to the distance of segments to treated areas. These results suggest that speed limit reductions may affect driver behavior in ways that extend beyond a specific treatment zone. With respect to the exclusion restriction in our main econometric tests, these results support the exclusion of never-treated segments located within 1,600 meters of treated roads from the control group. By excluding non-treated segments nearby treated areas, we eliminate possible bias due to the confounding effects of spillovers.

C.1 Accounting for Spillovers in Costs and Benefits Estimates

Our primary analysis compares the losses in travel time due to slower trips on the Marginais with the increase in accidents on those same treated road segments. This comparison is preferred because it is direct, utilizes precisely estimated effects, and involves few assumptions regarding the nature of spillovers. However, we provide evidence that accident reductions in 2015 also occur on nearby roads. Our reduced form estimates of the effects of the speed limit increase indicate that trip duration also increased on non-treated roads surrounding the Marginais Highways. Here we extend the cost benefit analysis presented in our main text to account for spillover effects on travel time increases and accident reductions. We acknowledge that these estimates are less precise when compared to the direct effects observed on treated roads, but they provide a valuable approximation of total policy effects, which we can compare with our main estimation of the benefits and costs associated with direct effects of the speed limit change.

To include spillovers in our benefits calculation, we consider a counterfactual scenario where the speed limit is reduced in the Marginais Highways only and not on any other roads. Using the coefficients estimated in Figure C.2, we calculate total averted accidents on roads up to 1.6 km away from the Marginais and their corresponding travel time costs. We then add those values to the total policy benefits presented in Table C.1. For policy costs, we use point estimates from our main model of trip durations that were related to the spillover area around the Marginais. In the most complete version of that model, we estimated that travel time went up by 3.4% on roads that are up to 1 km to the Marginais highways, 1.5% for roads located between 1-3 km of the Marginais, and 1.1% on roads located between 3-5 km. Beyond that distance, estimated changes in travel time are not significantly different from zero.

We calculate a net benefit of R$ 35-177 Million when we account for the benefits/costs of spillovers, which is comparable to but somewhat higher than our range of R$ 25-69 in net benefits on treated roads. When we account for spillovers in benefits and costs, the benefit/cost ratio changes from 1.77-3.1 to 1.41-3.1.

The comparison of policy costs and benefits in the treated area spillover areas is presented in Table 5. Estimates of annual benefits range from R$ 120 to R$ 263 Million, which are more than double the benefits on treated segments alone. Estimates of total annual costs using VTPI and individual-specific VOT range from R$ 85.4 to R$ 88.9 Million, which are more than two times higher than the costs on treated segments alone. We calculate a net benefit of R$ 25-69 Million on treated roads (without spillovers) and R$ 35-177 Million when we include both treated roads and account for the benefits/costs of spillovers. These estimates indicate that (1) spillover effects induced by the speed limit program resulted in large benefits and costs and (2) accounting for these effects results in a similar range of net benefits due to their compensating effects, (3) estimates of direct effects alone appear to be conservative estimates of net benefits.

Table C.1. Annual Costs and Benefits of the Speed Limit Program: Marginais Highways (incl. Spillovers)

PANEL A: BENEFITS

	Baseline *Before Speed Limit Change*	Counterfactuals: *(6th quarter after change)* Reduced Form Estimates from: Base Event Study CATT		Sample (3) event study w/ matched controls	
		SLR	SLR & Cameras	SLR	SLR & Cameras
A1 - All Days, All Treated Roads + Spillover Area					
Accidents					
without policy change	13,532	11,682	11,682	10,221	10,221
with policy change		8,423	8,338	8,350	8,309
Policy Benefits					
Averted Accidents		**3,259**	**3,344**	**1,871**	**1,912**
Benefits from Averted Accidents (R$ million)		**1,085.9**	**1,114.2**	**623.2**	**637.2**
A2 - Business Days, Marginais Highways + Spillover Area					
Accidents					
without policy change	2,326	2,670	2,670	1,848	1,848
with policy change		1,882	1,871	1,488	1,475
Policy Benefits					
Averted Accidents		**788**	**799**	**360**	**372**
Benefits from Averted Accidents (R$ million)		**262.6**	**266.3**	**120.0**	**124.1**

PANEL B: COSTS

	Baseline *Without Speed Limit Change*	Speed Limit Policy	**Policy Cost**
B1 - Business Days, Marginais Highways			
Time Spent in Traffic (million hours)	1,119.1	1,108.0	**-11.2**
Cost of Time Spent in Traffic (R$ million)			
VOT = VTPI individual VOT	7,104.0	7,018.6	**-85.4**
VOT = 50% of median net wage	5,755.9	5,698.3	**-57.6**
VOT = 50% of individual net wage	7,560.1	7,471.2	**-88.9**

Notes: Benefits are calculated using counterfactual scenarios from the CATT event study and the event study with matched control segments 4. Cohort-specific CATT estimates weight the counterfactual accident counts by the sample of segments in each cohort. For each model, we compare the counterfactual without any speed limit change to 2 policy scenarios: 1) the speed limit reductions are adopted alone and a 2) the speed limit reduction is accompanied by an expansion of cameras equivalent with what was observed in 2015. Panel A1 reports estimated benefits for the whole year and for all treated roads. In Panel A2, we restrict the calculation to business days and to the Marginais Highways, so values are comparable to the results from Panel B. Costs are calculated using alternative Value of Time (VOT) Parameters. For the first column, we use 50% of individual after-tax wages as the value of an individual's time spent in transit, taken from a representative survey conducted by the transit authority in the city of São Paulo. In the second column, we use the VTPI guidelines, which assign different VOT values based on trip motivation, which are also taken from the survey. In the third column, we assign the 50% of the median after-tax wage as the VOT for all individuals in the sample.

C.2 Evidence of City-Wide Effects on Accidents

In this section, we test for evidence of city-wide effects of the 2015 speed limit reduction on accidents by aggregating the series of accidents across the city and evaluating how it changed after the adoption of speed limits began in July of 2015. We use data from the pre-treatment period to predict the number of accidents that would be expected in the post-treatment period in the absence of the policy. Therefore, the underlying assumption of this model is that without the speed limit reductions, accidents in São Paulo after July

2015 would follow the same trend as the one observed in the pre-treatment period. We note that this model cannot isolate the specific city-wide impacts of speed limit changes from the city-wide effects of the expansion of cameras. We are also not able to construct a counterfactual using post-treatment control observations from within the city of São Paulo itself as they all become part of the "treatment group." For those reasons, we do not interpret the specific magnitude of the estimates but rather test for evidence that the estimated effects are consistent with our preferred model specifications. We estimate the following model:

$$y_t = \beta X_t + \gamma_t + \varepsilon_t \tag{9}$$

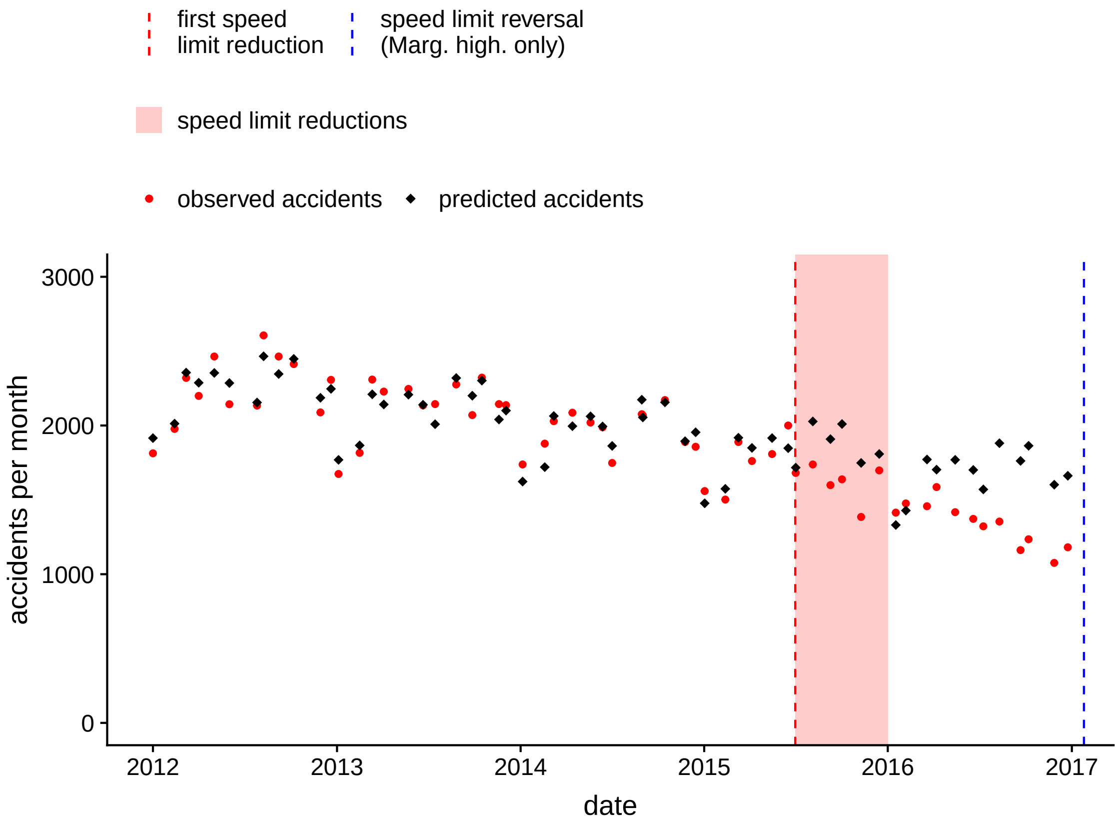
Where Y_t is total road accidents observed on month t. X_t is a linear variable on time, so β is the slope of a linear time trend. γ_t are fixed effects for months of the year, which control for seasonality and other changes occurring during this time.

Accidents in the post treatment period are predicted using the following simple specification:

$$\hat{y}_t = \hat{\beta} X_t + \hat{\gamma}_t \tag{10}$$

Differences between predicted values $\hat{y}_t$ and observed values y_t are interpreted as city-wide effects. Figure C.4 compares observed and predicted accidents according to the above model up to the speed limit reversal of 2017. The plots show that observed and predicted accidents are highly similar (by construction) in the pre-treatment period, but observed accidents begin to decline immediately as the speed limit reductions were adopted. Consistent with the evidence provided in our primary specifications, the effects grow over time. While we note that the the exact magnitudes of these effects involve restrictive functional form assumptions, these findings support our claim that the effects estimated in our primary analysis are not driven by route or trip substitution.

Figure C.4. Predicted and observed accidents in the whole city of São Paulo

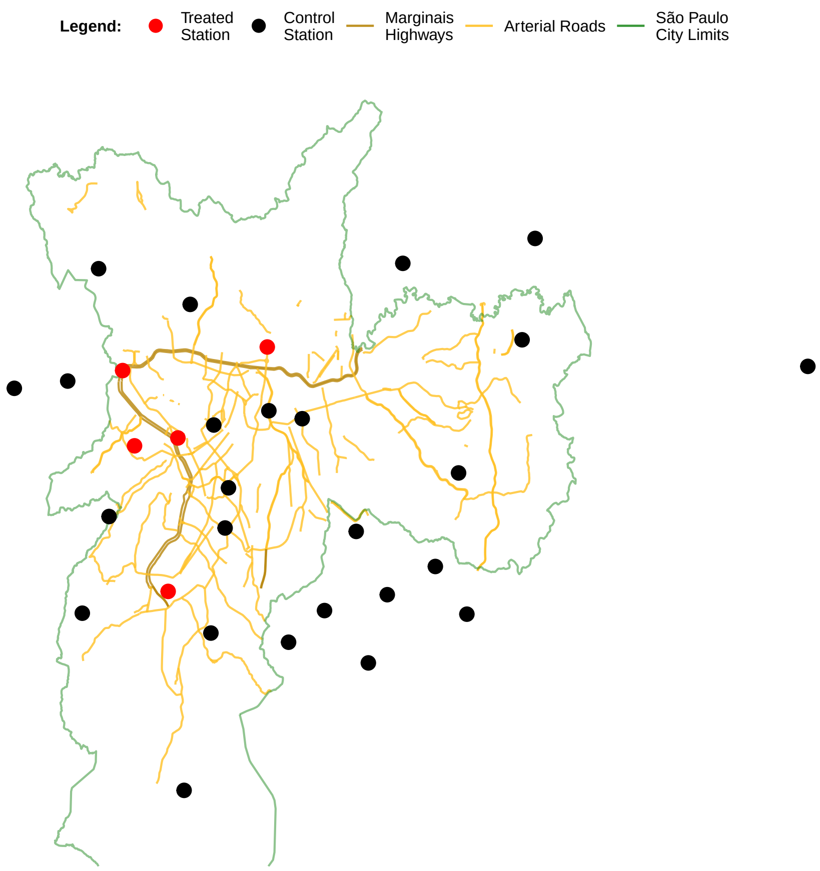


D Policy Effects on Air Pollution

Changes in speed limits may also impact air pollution, as vehicle emissions are a mechanical function of engine speed. However, the expected impacts of the policy are not straightforward, since the relationship between vehicle speed and emissions is non-linear and is specific for different pollutants (van Benthem, 2015). The empirical evidence on the impacts of speed limit changes on air pollution is mixed, with at least one paper finding imprecise effects and the other two papers finding no effect (Folgero et al., 2017, van Benthem, 2015, Bel et al., 2015).

To evaluate the possible impact of the speed limit changes on air pollution in São Paulo, we scraped hourly pollution data from the São Paulo Environment Agency Air Control Website (https://cetesb.sp.gov.br/ar/qualar/). The system includes 30 air monitoring stations distributed throughout the metropolitan region of São Paulo. We divide the monitoring stations into 2 groups: treatment and control. We define the treatment group as the stations located within 3 miles of the Marginais Highways, which is consistent with the threshold used in prior work van Benthem (2015). The control group is comprised of stations located outside that distance threshold. Figure D.1 maps the location of monitoring stations included and their treatment status as defined above.

Figure D.1. Air Monitoring Station by Near-Treatment Status



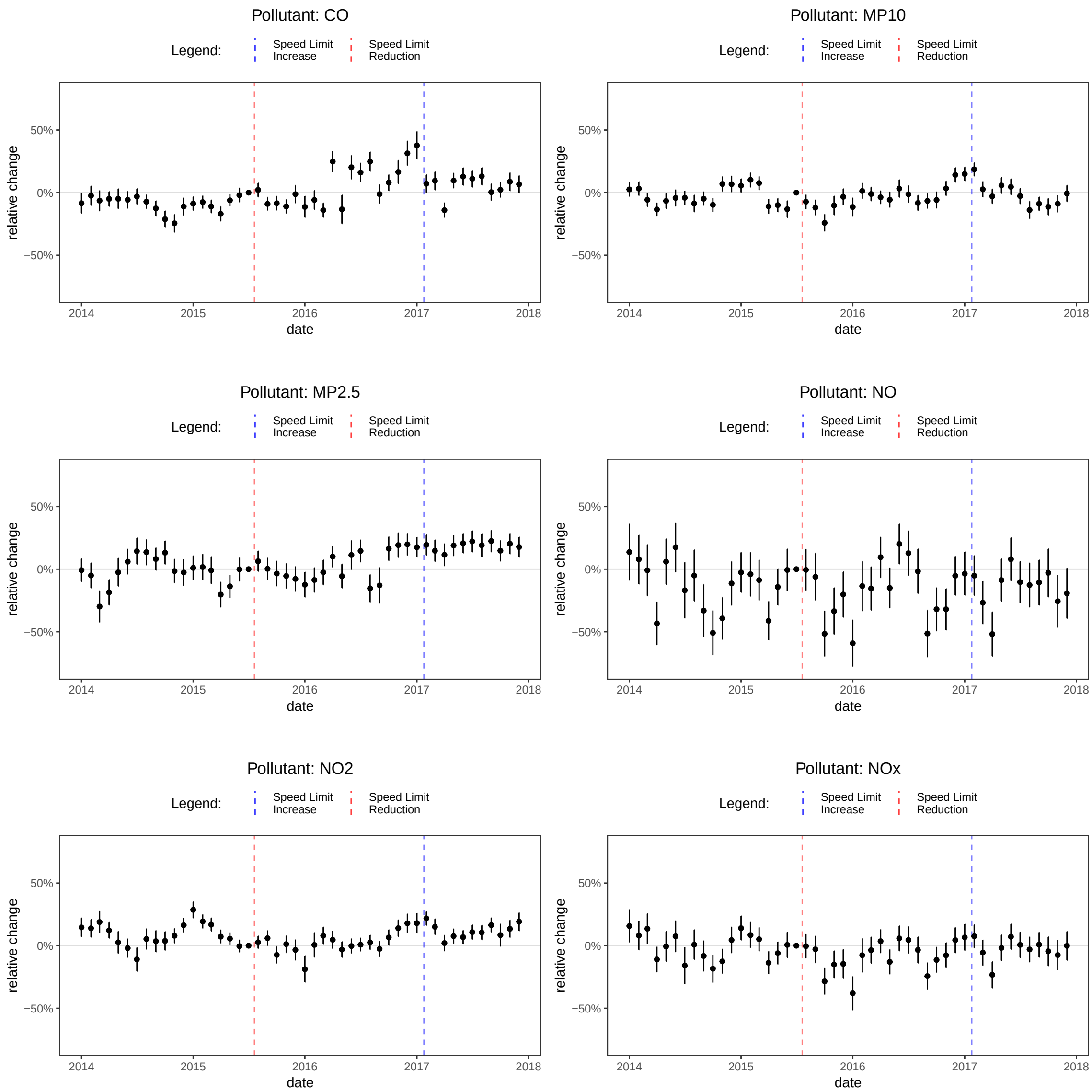
We estimate the following model:

$$log\left(y_{it}\right) = \alpha_i + \beta_t + \left(\sum_{m=2014-01}^{2017-12} \gamma_m \cdot D_{it}^m\right) + \zeta \cdot X_{it} + \varepsilon_{it} \tag{11}$$

Where y_{it} is the concentration of pollutant measured on station i at time t. α_i and β_t are respectively station and time fixed effects. D_{it}^m is an indicator variable that only takes the value of 1 for the treatment group of stations for each month m. The omitted category is June, 2015 – the first month preceding the speed limit reduction. The coefficients γ_m estimate the average changes in pollution measured at treated stations relative to control stations. X_{it} are station time covariates that include air humidity. wind speed, temperature and radiation.

Figure D.2 plots the γ_m coefficients by month for each major pollutant available in our dataset. The results do not indicate any clear or sustained pattern of air pollution effects associated with the treatment stations after any of the policy changes observed in our study period.

Figure D.2. Changes in Air Pollution at Near-Treatment Stations



E CBA Parameters

We calculate the monetary cost of accidents in our database by matching the characteristics of individual accidents to cost parameters found in the literature. In the case of non-fatal accidents we use the parameters estimated by IPEA (2016), a report from the Brazilian Institute of Applied Economics, which includes estimates of the cost of road accidents in Brazil. The report estimates specific parameters based on the type of vehicle and the severity of accidents. The latter is measured in terms of the severity of injuries faced by the victims of accidents. We include these estimates in our cost-benefit analysis to account for heterogeneity in vehicle type and injury severity in our accident data. For example, we calculate that the vehicle cost for a motorcycle accident where the driver was injured is R$ 2,741. For this same accident, the cost of victim injuries is R$ 66,802.

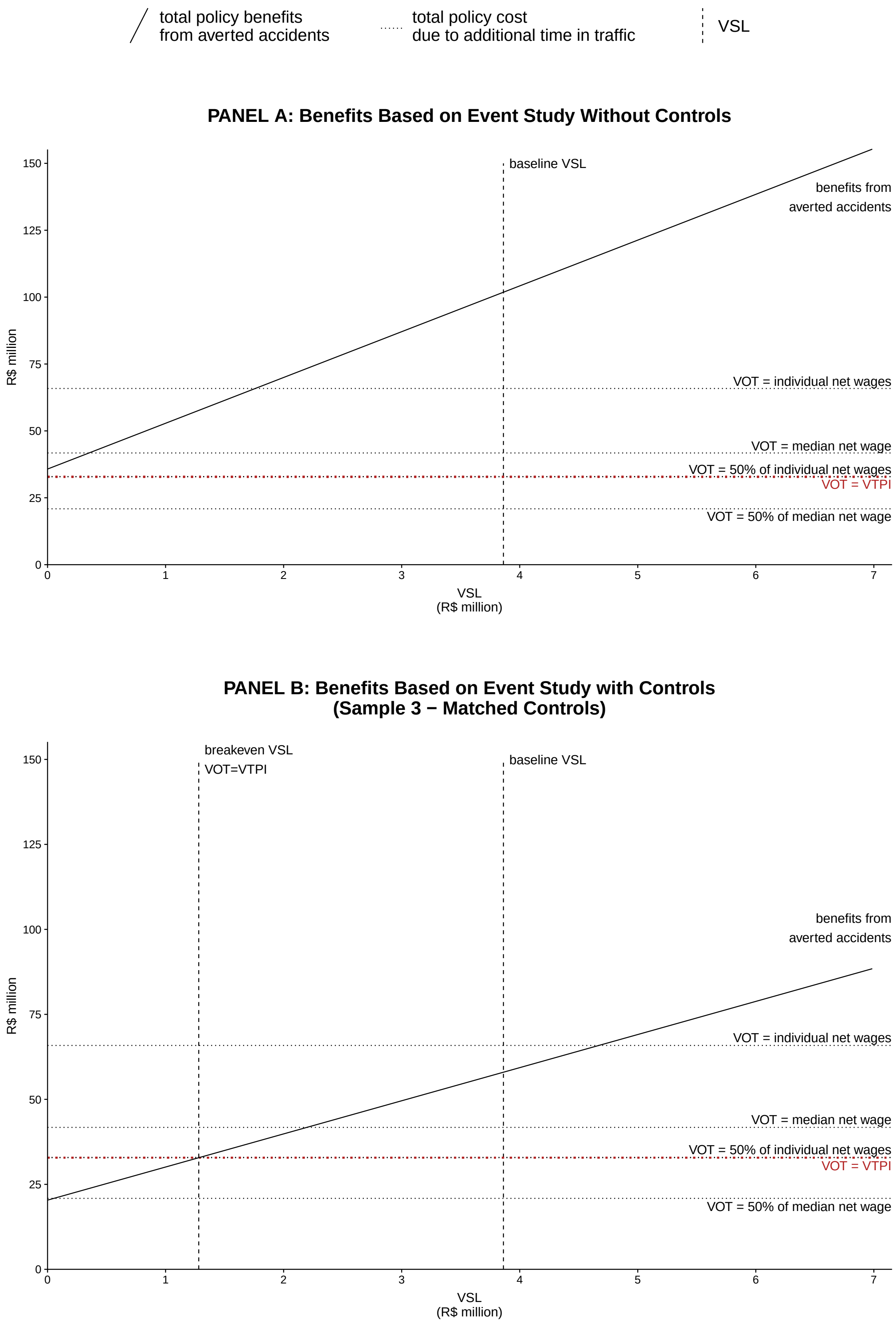
For fatal accidents, we use the Value of Statistical Life (VSL) calculated by Viscusi and Masterman (2017) for Brazil. In this paper, the authors explore the richness of data available for the USA and differences in income between countries to calculate VSL for countries with limited information.

Table E.1. Accident Costs by Type

	Accident Severity		
	No Injuries	With Injuries	With Fatalities
Vehicle Costs			
Auto	7,159	12,127	19,324
Motorcycle	2,473	2,741	4,270
Bike	0	169	124
Utility Veh.	10,570	20,240	35,091
Truck	22,314	65,656	47,825
Bus	16,069	10,537	20,686
Other	10,307	80,109	81,209
Victims Cost			
Unharmed	1,086	4,111	1,840
Injured	-	66,802	74,896
Fatality	-	-	3,862,030

Notes: All values are presented in BRL of 2015. Most parameters were extracted from IPEA (2016), except Fatal Victims Costs, for which the parameter is taken from from Viscusi and Masterman (2017).

Figure E.1. Cost-Benefit Analysis: Sensitivity to VSL Estimates



Notes: The figure plots the estimated total policy benefits using alternative values for the VSL parameter. These results are represented by the dotted diagonal line in the Figure. The horizontal lines indicate the total policy cost due to longer commutes under alternative values for the VOT paramenter. The solid red line shows total policy costs in the case where VOT is equal to 50% of individuals' after tax wages. The dashed red line indicates total policy costs if VOT is calculated using the VTPI guidelines where the value varies by trip motivation. The vertical lines indicate key values for the VSL parameter. The solid black line delineates our baseline value that was calculated by Viscusi and Masterman (2017). The dashed vertical black line delineates the breakeven VSL for which policy costs equal policy benefits under the highest VOT parameter.

F Comparison of Results with the Literature

To the extent of our knowledge, there exists no other study that estimates the reduced-form effects of speed limits in a developing country city and estimates costs and benefits using a welfare framework. The study that most closely approximates our research design is van Benthem (2015), (henceforth VB). Both papers conduct a comprehensive ex-post evaluation of impacts from speed limit changes in the sense that they capture the primary benefits and costs from the policy, which increases the value of a comparison of primary results. However, there are important differences with respect to the setting evaluated in the papers. VB examines speed limit changes from more than 20 years ago on regional freeways in the Western US,[51], whereas our study examines a policy change from 2015 on urban roads in a metropolitan area of the developing world. When comparing the results from both papers, it is important to acknowledge that each of these dimensions may contribute to differences in estimates.

Table F.1 summarizes the comparable results between the two studies.[52][53] We note that the VSL and VOT parameters used in our study were both about 20% of the main parameters used by VB and the similarity in the ratio of VSL/VOT between the US and Brazil facilitates the comparison of results between the two countries with very different economic settings.

With respect to reduced form policy impacts, we find highly consistent estimates of the effect of speed limit changes on travel time, with an average effect of approximately 6% in the same direction as the speed limit change. This is interesting and even somewhat surprising given the differences in measurement and the transportation infrastructure. The primary difference in the results presented between the two studies concerns impacts on accidents. While the present study documents a reduction of 40% on the Marginais Highways, VB identified an average effect of 14% on American highways (he found an effect of 44% in the case of fatalities). In our study, we were not able to isolate the effect of the policy on fatal accidents with sufficient precision since they are observed with less frequency. Therefore, we assume that the effect on fatalities is proportional to the policy effect on total accidents (40%). This assumption is supported by our data, though estimates are imprecise. In both studies, if travel time costs are computed using the VTPI VOT, the breakeven VSL is quite small as the changes in non-fatal accidents alone compensate for the losses due to increased travel time.

Perhaps the most important conclusion regarding the comparison of two studies in very different settings is the clear and consistent conclusion regarding the social benefits of reducing speed limits. In both studies, the benefits related to fewer accidents were found to be substantially larger than the costs of extended commuting times, which lends

[51] The roads evaluated in VB were located in the states of California, Oregon and Washington.

[52] VB evaluated the effects of a speed limit increase, whereas the present paper evaluates a speed limit reduction. Therefore, to align the direction of results and increase comparability, we invert his labels of costs and benefits, so we use "costs" to refer to the value of additional travel time, and "benefits" to refer to reductions in accidents.

[53] In addition to the sources of costs and benefits studied in this paper, van Benthem (2015) estimates the effects of speed limit changes on air pollution and its consequent impacts on health. Our work on the estimation of policy effects on pollution is ongoing. Therefore, when comparing the results between the papers, we exclude VB results associated with pollution and health impacts and compare the effects on road accidents and travel time only.

some confidence to the external validity of both the empirical estimates and the broader conclusions. While in the VB study the central estimate of the benefit/cost ratio was 2.21, the comparable ratio in São Paulo was 1.39. Using our preferred parameter estimates, this comparison would be 2.21 to 1.77. Although the two settings involve highly different VSL and VOT parameters, the relative breakeven VSL is highly consistent between them.

Table F.1. Comparison of Findings with van Benthem (2015)

	Ang, Christensen & Vieira (2018) *Urban highways in São Paulo, Brazil*	Van Benthem (2015) *Western USA freeways*	Ratio *ACV/VB*
Cost-benefit results			
Benefits/costs	1.39	2.21	0.63
Breakeven VSL ratio[a]	0.57	0.50	1.14
Main parameters			
VSL *(U$ million)*	1.72	8.78	0.20
VOT *(U$ per hour)*	3.16	18.31	0.17
Pre-treatment values			
Average vehicle speed *(km/h)*	35.45	96.80	0.37
VKT per year *(billion)*	2.22	4.63	0.48
VHT per year *(million)*	62.60	47.80	1.31
Accidents per year	514	1010	0.51
Fatalities per year	24	24	1.02
Accidents per million VHT	8.2	21.1	0.39
Fatalities per million VHT	0.39	0.50	0.78
Reduced form estimates			
Travel time	0.055	0.059	0.94
Accidents	0.32	0.14	2.31
Fatalities	0.32	0.44	0.74

Notes: All monetary values are in USD of 2016. [a] Breakeven VSL divided by baseline parameter.