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

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Abstract

Road accidents are the leading cause of unnatural deaths worldwide. Cities are experimenting with more stringent speed limits in an effort to reduce them. The impacts of these policies are unclear in many developing country cities, where a disproportionate share of accident damages occur but also where speed regulations could exacerbate already high levels of congestion. 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 find that the program resulted in 1,889 averted accidents within the first 18 months and reduced accidents by 21.7% on treated roads, 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 1.32 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.

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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 recent years, 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 speed limit reductions.

This paper brings together a number of novel data sources to provide estimates of the effects of a major speed limit reduction program in one of the most congested cities of the developing world.[2] In 2015, the government of São Paulo, Brazil reduced speed limits on the main highways in the city from 90 km/h to 70 km/h. However, the policy became highly contentious and in early 2017 the speed limits on urban highways were reverted to pre-reduction levels. Using a semi-dynamic event study design (Abraham and Sun, 2018, Borusyak and Jaravel, 2016), we exploit exogenous variation in the timing of speed limit reductions adopted on each road in São Paulo to identify the effect of changing the speed limit on traffic accidents and travel time. 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

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

switching in response to the policy and analyze changes in individual speeding tickets to learn 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. These impacts are considerably larger than policies aimed at addressing violent crime, which is the other primary cause of unnatural deaths in the city.[3] 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 compare the social cost of increased trip duration from reduced speeds with the benefits of reduced accidents and find that the benefits associated with reduced accident damages are at least 1.32 times larger than the costs of longer drive times.[4]

This 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 on interstate highways in the United States are 2-7 times larger than the benefits, 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 suggest that speed limit reductions are 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

[3]Recent papers have identified an 11-13% reduction in fatalities as a result of a disarmament and conceal carry policies 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.

[4]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[5] estimated for Brazil by Viscusi and Masterman (2017). We use 50% of individual after-tax hourly wages for VOT Wolff (2014) as well as trip-specific VOT VTPI (2016).

event study results (Abraham and Sun, 2018).[6]

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).[7] We combine a representative origin-destination travel survey with repeated observations of real-time trip durations before and after the speed limit change 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. In our setting, we find that failing to account for (otherwise unobservable) heterogeneity in the population of travelers who use 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 motorcycles and other high-risk 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

[6]We express the standard note of caution against extrapolating specific estimates from São Paulo roadways to other cities, where impacts may depend on local conditions.

[7]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.

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. Over 30 million motorized trips are made on an average weekday in São Paulo (METRO, 2013). While the length of the average trip is 8 km, the average duration is 51 minutes. Travel-related injuries are also a major problem in the city. In an average year during the 2005-2014 period, there were 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 the magnitude and composition of road fatalities in São Paulo relative to New York City. 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. However, 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. Speed limit regulations were rationalized on the basis of growing 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.[8] 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 totals, respectively. During the year after the speed limit reductions (2016), the number of accidents and fatalities decreased to 5,425 and 311, or 33.7% and 36.3% of the city totals.

The other 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.[9] Automated traffic cameras register vehicle speeds and issue speeding tickets to vehicles that are traveling above the speed limit (CET, 2016).[10] 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

[8]Figure A.2 in Appendix A maps the roads where speed limits were reduced, indicating the final speed limit in each treated road segment. 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 evenly distributed throughout the second half of 2015.

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

[10]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. Aside from the change in fees in 2016, we are not aware of other major changes to speed limit enforcement in São Paulo that may have coincided with the speed limit program. In Brazil, traffic laws (including ticketing enforcement) are regulated by federal legislation under the Brazilian Traffic Code, making it difficult for municipalities to change ticketing laws as part of a speed limit program (Código de Trânsito Brasileiro: http://www.planalto.gov.br/ccivil_03/leis/l9503.htm).

implemented.[11] 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.[12] We estimate the heterogeneous effects of speed limit reductions with/without automated enforcement by exploiting variation in the timing of onset of camera-based enforcement, thus comparing the effects of speed limit reductions on segments that have camera-based enforcement and those that do not (but will).

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.[13] 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. The Marginais highways represented 3.17% of São Paulo's accidents in 2016 (after the reduction but before the reversal). That fraction increased to 3.47% in the year following the reversal.

[11] The introduction of camera-based enforcement began earlier than speed limit reductions and continued to grow on treated and untreated road segments throughout the study period. A resolution from the National Traffic Consil (CONTRAN) defines the requirements for technical analysis underlying the placement of cameras: https://infraestrutura.gov.br/images/Resolucoes/RESOLUCAO_CONTRAN_396_11.pdf. Local governments are required to conduct technical studies that consider the type of road, inclination, urban density, VMT, pedestrian and cyclist circulation, nearby speed limits, traffic accidents and other factors that may be considered relevant by local traffic engineers. 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. 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 69.1% as additional cameras were installed.

[12] In other ongoing work, we examine the specific effect of camera-based enforcement on driver behavior (Christensen et al., 2019)

[13] 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.

2 Data

This section describes the data that we use to evaluate the impacts of speed limit changes on accidents, commuting time, traffic volume and traffic violations.[14]

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 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.[15] 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.

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 (CET, 2017).[16] 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.).

[14] Figure A.10 in Appendix A summarizes these datasets and their temporal coverage.

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

[16] 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 requests 21,151, 25,968 and 30,818.

Between 2012 and 2017, 125,769 accidents were recorded in São Paulo, involving 146,991 injured victims and 5,997 fatalities (Table 1, Panel A). 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 (Panel A of Figure 4 illustrates pre-policy trends).[17]

Travelers and Trips in São Paulo

In order to measure the effects of the speed limit changes on the duration of trips, we simulate a set of representative motorized journeys using the Google Directions API.[18] 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 change, 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.

To our knowledge, ours is the first study to examine policy effects using trip queries that are representative of *actual decisions* made in a transportation market. We imple-

[17] A variety of factors have been proposed to explain the overall downward trend in traffic accidents, including improved safety features in vehicles and stringent drinking and driving legislation (*"Lei Seca"*), which was implemented in 2008 and updated in 2012 with more stringent rules (Campos et al., 2013).

[18] 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).

ment this by first obtaining a set of origin and destination coordinates from a travel survey that 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.[19] 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. For each origin-destination pair, we then queried the Open Source Routing Machine (OSRM) API to obtain a set of optimal trip routes before the policy change.[20] Each surveyed trip is assigned a value for the proportion of the trip that intersects treated road segments.

From July 2016 to September 2017, we then queried Google's Directions API using origin-destination pairs from the travel survey at 20 minute intervals within 2 hours of the time that the surveyed trip was originally taken in order to obtain real-time estimates for trip duration.[21] For each surveyed trip, we obtain approximately 75 observations of real-time trip durations collected over the course of a week. Since treatment intensity is assigned based on the traveler's optimal route at baseline, this method allows us to obtain repeat observations of the duration of each trip before and after treatment. Google Maps provides a new optimal route as a result of changing real-time traffic conditions, allowing us to then estimate 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.[22]

[19] The survey was conducted by the São Paulo Subway Company in 2012. Trips in the survey are summarized in Table A.3.

[20] We intersect this set of optimal trip routes from the (pre-policy) baseline with a shapefile of São Paulo roads to determine to what extent each survey trip intersects with treated road segments. The OSRM API queries were made on March 21, 2017 and the speed limits were updated in Open Street Maps road network database on March 28, 2017, such that the routes assigned to each trip reflect the (pre-reversal) baseline exposure to treatment. We assign a trip to the treatment group if the trip utilizes at least 400 m of treated road segments.

[21] 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 Section A.9 of Appendix A.

[22] In Appendix C.3, we examine the implications of different assumptions about re-routing behavior using a unique feature of the OSRM routing platform that allows us to alter speed limits for road segments in the São Paulo transportation network and then query the duration and paths of optimal routes on that

Panel B of Table 1 describes the characteristics of the 1.47 million trips that we simulated between July 2016 and September 2017.[23] The average trip in the sample spans 8.31 km and 20 minutes. On average, trips occurring on the Marginais Highways (treated roads) were considerably longer in length and tended to have a higher average speeds. According to our OSRM measure of trip length along treated roads, these trips spent about 22% of their total trip duration on the treated segments. One third of all observations were queried after the speed limit reversal and about 40% of the queries were made during peak hours.

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. 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.[24] Speeding tickets received by mail are the main form of speed limit enforcement in the city. In total, more than 35 million traffic tickets were issued during the study period, with an average of 9.46 million tickets per year. Camera-based enforcement became increasingly important. 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%.

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 exact timing that a speed monitoring camera was installed or discontinued on all segments included in our analysis, which allows us to estimate the interaction between the effect of speed

altered network.

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

[24]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. Table A.4 summarizes the sample of all traffic tickets issued by cameras in São Paulo between 2014 and 2017. Traffic tickets can also be issued manually by the police and by traffic agents. However, speeding tickets are only issued by automated cameras. About 7% of license plate driving restriction violations are issued through manual policing.

limit reductions and the effect of contemporaneous introduction of camera-based enforcement on the same segment. 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, using panel variation in non-speeding tickets to test for evidence of substitution away from treated roads after the speed limit reductions. In this test, 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.[25]

3 Did São Paulo's Program Reduce Accidents?

We estimate the effects of speed limit changes on road accidents using a semi-dynamic event study design that makes use of exogenous variation in the timing of speed limit reductions. 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.[26] 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

[25]In Appendix C.2, we report the results of two additional validation tests that show that flows of non-speeding tickets predict changes in traffic delays and total VMT from an OSRM simulation.

[26]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.

(Borusyak and Jaravel, 2016).[27]

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 treatment, which was likely true of the roll-out of the speed limit program.[28] The primary identification assumption 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. In order to attribute the differential effects of speed limits on treated roads where cameras were additionally placed on a treated segment as an estimate of the differential effect of the speed limit reduction in the presence of camera-based enforcement, we also assume that the timing of onset of camera-based enforcement on a given segment as random with respect to accidents.

We use the following Poisson event study model[29] 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

[27] Borusyak and Jaravel (2016) show that estimates in static event study designs are under-identified when treatment effects change over time and that time fixed effects do not provide a viable solution in the event study setting because absolute time is not separable from time measured relative to treatment. We apply both of their proposed solutions solutions below: (1) imposing parametric assumptions that define how the counterfactual would change over the study period and (2) including a never-treated sample as a time-varying control group.

[28] 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.

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

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. Standard errors are clustered at the road level (202 clusters).

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).[30] C_{it} is an indicator for the presence of a speed monitoring camera on 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.[31] 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. Because the policy was partially reversed in January 2017, 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

[30] 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.

[31] 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.

clear ex ante that any sample of never treated segments will provide a valid counterfactual (parallel trends) 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 as well as 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.1, 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.[32]

Sample 1 provides a reference sample that includes all never-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.[33] 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 to estimate the policy

[32] Appendix C.1 describes our analysis of behavioral spillovers using data from electronic speeding tickets on non-treated road segments. The 1.6 km buffer is also supported by independent evidence from simulations of possible re-routing using OSRM in Appendix C.3.

[33] 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.

effect:

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

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).[34] 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 on 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. We demonstrate that our estimates are robust to that assumption in Appendix B.[35]

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

[34] See Appendix B for discussion of model specification and Borusyak and Jaravel (2016). None of the coefficients associated with periods preceding treatment are statistically different from zero, providing support for the parallel trends assumption (Figure 4, Panel A; Figure A.7).

[35] In Appendix B, we estimate our empirical model without excluding data from the quarter immediately before the policy adoption. 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. Figure A.11 in Appendix A shows an example of one the warning signs.

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.

The fact that results are aligned in both magnitude and specific pattern provides 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 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 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 view these estimates as the closest approximation to the policy's longer-run effect.

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 baseline 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.[36] 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.[37]

Substitution, Spillovers, and the Displacement of Accident Risk

Substitution

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.[38] We test for evidence of substitution using data on traffic violations resulting from infractions that are unrelated to speeding. We group the non-speeding violations into two types: (a) violations of the São Paulo driving restriction, which account for 56.6% of non-speeding traffic tickets and (b) all other non-speeding violations.[39] These two sets of non-speeding tickets serve as proxies for traffic volume per segment. We evaluate them independently as a check for consistency in patterns across the two proxies.

[36]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.

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

[38]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.

[39]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%).

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 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. 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.[40] While we don't find any evidence that the speed limit policy induced route substitution, we also simulate the possible effects of the policy on re-routing and find that VMT on treated segments could decline by up to 12.1%, though the median trip would save only 23 seconds by re-optimizing. This suggests that the benefits of adjusting away from treated roads (which are main corridors) may not have provided a large enough incentive to induce an effect.[41]

Spillovers

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.1). If changes in driving behavior were limited to treated segments, then we would expect to

[40] 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.1. Figure C.7 illustrates the evidence of net reductions in accidents on the average road in São Paulo following the implementation of the program.

[41] We examine the implications of re-routing for our reduced form estimates and subsequent benefits calculations in Appendix C.3.

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 affected driver behavior 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. Our 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 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.[42] 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.[43] 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.8 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:

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

where ETT_{ihd} is the log of estimated travel time for each simulated trip i queried at hour

[42] A query that simulates travelling 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 flexibly control for differences in the duration of the trip made at 7 am relative to the same trip at 8 am.

[43] 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.

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.[44] 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 intervals of varying distance ($L=$ 1km, 3km or 5km) from the Marginais Highways.[45]

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 10% faster. In Column 2, we include covariates that measure the fraction 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

[44]Route paths were collected for each pair of origin and destination using the OSRM API before the speed limit reversal and the exposure of each trip to treatment reflects the (pre-policy) baseline route for a trip.

[45]Figure A.9 in Appendix A illustrates buffer and control zones.

did affect trip times on the Marginais Highways and also indirectly 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.[46] We estimate the monetary value of road accidents and travel time using standard parameters from the literature (Viscusi and Masterman, 2017). We acknowledge the limitations of this procedure given substantial parameter uncertainty in this literature. 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.[47] 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.

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

[47]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.

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 the matched controls sample as our preferred estimates. We separately report benefits based on 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).[48] We value fatalities using the Value of Statistical Life (VSL) of USD$ 1.695 Million[49] 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.

Panel A of Table 5 reports estimates of policy benefits. 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.[50] 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 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).

[48] 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.

[49] Value is in 2015 USD.

[50] 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.

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 by applying the reduced form estimates of effects on trip durations to trips taken by São Paulo travelers observed in the origin-destination survey. Our estimates of the effects of the 2017 increase on travel time depend upon the length of travel along treated segments. For each traveler, we compute the reduction in time spent in travel as a result of the 2017 speed limit increase and use survey expansion factors to obtain estimates of population-level benefits from the policy change. The survey also records the self-reported income for all travelers and and motivation for all trips in our origin-destination sample. We use information from the survey to construct two alternate parameters for the value of time (VOT).

A first estimate is generated using 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).[51] 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). We use trip motivation reported in the survey 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 policy cost given these two different definitions. Our reduced-form estimates of effects on trip duration indicate that the speed limit increase of 2017 resulted in 5.4 Million fewer trip-hours for travelers on treated roads in São Paulo. The value of total travel time savings is estimated to be R$ 27.9 Million if we use the 50% median wage VOT and R$ 43.6 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 represen-

[51] The USDOT recommends assigning half of the hourly wage for non-business trips within local urban settings (USDOT, 2014).

tative sample of travelers. 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$ 27.9 to R$ 43.8 Million. This reflects a difference of more than 55% 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$ 43.8 Million) are highly consistent with the VTPI-based estimates (R$ 43.6 Million) that utilize individual-specific wages and assign different VOT 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, 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).[52] 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.[53]

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$ 58.0 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$ 43.6 million using a VTPI VOT) from the 2017 reversal. We estimate a benefit/cost ratio that ranges from 1.32, using the VTPI VOT and our preferred estimate of accident effects from our event study with matched controls, to 2.33 using the VTPI VOT and our baseline 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.5. 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

[52]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).
[53]See Appendix Table B.6 and related discussion.

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 1.32-2.33 to 1.39-3.04. 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.

We examine the sensitivity of estimates of costs and benefits to assumptions about routing adjustments in Appendix C.3. While our empirical tests do not suggest evidence of re-routing, we explore the potential gains from re-optimization using routing simulations on the Open Street Map road network. We calculate the benefits from a speed limit change under a scenario where we include all trips where there is a time saving from re-routing ("full re-routing") and a baseline scenario where all drivers stick to their baseline trips ("zero re-routing") to better understand the implications of such behavior for our policy analysis. Under full re-routing, our benefits-costs estimates range from 1.23-2.16.[54] Using the baseline assumption of zero re-routing, our benefits-costs estimates range from 1.32-2.33.

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$ 2.4 Million, which is equivalent to 62% of the baseline VSL from Viscusi and Masterman (2017) and (b) the break even VSL increases to R$ 3.64 Million when using the 100% of the median wage as the measure of VOT. In Appendix G, we compare these results to estimates of costs and benefits for the United

[54] We use extensive margin elasticity parameters from Akbar and Duranton (2017) to additionally account for the effect of forgone trips (0.36%) that could result from the speed limit reduction.

States from van Benthem (2015).

Prior literature has found evidence of additional health benefits from air pollution reductions resulting from speed limit changes on US highways (van Benthem, 2015). We are aware of two engineering studies that estimate the effects of the speed limit reduction on ambient air pollution. Ibarra-Espinosa (2017a) estimates that the speed limit reductions led to an increase in CO emissions by approximately 2% per year. A more recent modeling exercise by the same authors concludes that total emissions could decline by approximately -3.99% for CO (Ibarra-Espinosa, 2017b). We discuss these results and empirical tests for effects on air pollution externalities in Appendix D, where we do not find evidence of effects on the ambient concentrations of the 6 major pollutants emitted by vehicles. Given ambiguity in the direction of effects from the atmospheric science literature and evidence that they are likely relatively small in magnitude and smallest in magnitude for pollutants that have serious health-related damages such as PM and NOx, we are cautious about claims regarding their effects on the net benefits of the São Paulo program and instead focus on clearly identified time costs and accident reductions. We emphasize that this is an important area for further research given the health effects identified in prior economic literature and note that any reductions consistent with the direction found in van Benthem (2015) would increase the net benefits from the policy.

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.[55] We construct estimates of costs for each group by matching individual-specific cost calculations for individuals in the survey to their self-reported educational attainment. Accident-specific damages are divided into the same categories using the educational attainment informa-

[55] 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.

tion in victim reports. 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.[56]

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 appears to be strongly progressive, delivering net benefits to low and middle income residents in São Paulo.[57]

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 little attention and 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.[58] These plots indicate that the progressive effects are explained by: (1) the intuitive fact that a disproportionate share of the costs

[56]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.

[57]In the case of individuals with below primary educational attainment, both the costs and benefits are small. Individuals with very low educational attainment are less likely to drive since literacy is a regulatory requirement for obtaining a driver's license in São Paulo. We examine the additional effect of the incidence of ticket fees in Appendix F and find that it further magnifies the progressive impact of the policy.

[58]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.

of longer commutes is born by higher income people who use private vehicles and main transit corridors and (2) a less recognized fact that the incidence of fatal accidents is much higher for low and low-middle income individuals. 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. Total accident reductions more than double when we account for behavioral spillovers induced by the policy, though 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 also important to consider 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. 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. The external validity of our results is also limited given that we study a single city, although 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 speed limit 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 that capture travel times before and after the speed limit change. The speed limit increase on the Marginais Highways was implemented on January 25, 2017. We identify trips that intersect with 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 with respect to 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. Statistical significance: ***=0.1%,**=1%,*=5%.

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. Statistical significance: ***=0.1%,**=1%,*=5%

Table 4. Effects of the 2017 Marginais Speed Limit Increase (SLI) 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 Household Mobility 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 different days. 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. The ratio of each trip on the Marginais Highways (treatment exposure) was calculated by simulating the fastest route between survey trip origin and destination on OSRM API during the (pre-policy) baseline. 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. Statistical significance: ***=0.1%,**=1%,*=5%.

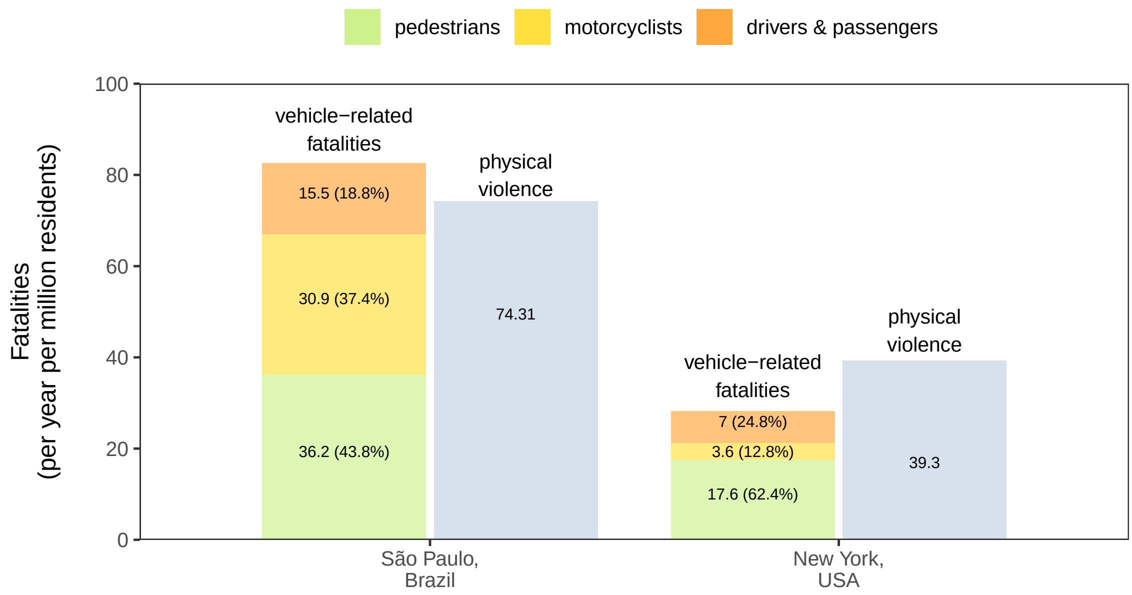
Table 5. Annual Costs and Benefits of the Speed Limit Program: Marginais Highways

tables/5_cba/cba_rerouting.pdf

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 number of segments in each cohort. For each model, we compare the counterfactual without any speed limit change to 2 policy scenarios: 1) only the speed limit reduction is implemented and 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 survey trips taken on business days and to survey trips which intersect with 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 line uses uses the VTPI guidelines, which assign different VOT values based on trip motivation. The second line assigns the 50% of the median after-tax wage as the VOT for all individuals in the sample. The third line uses 50% of individual after-tax wages as the value of an individual's time spent in transit.

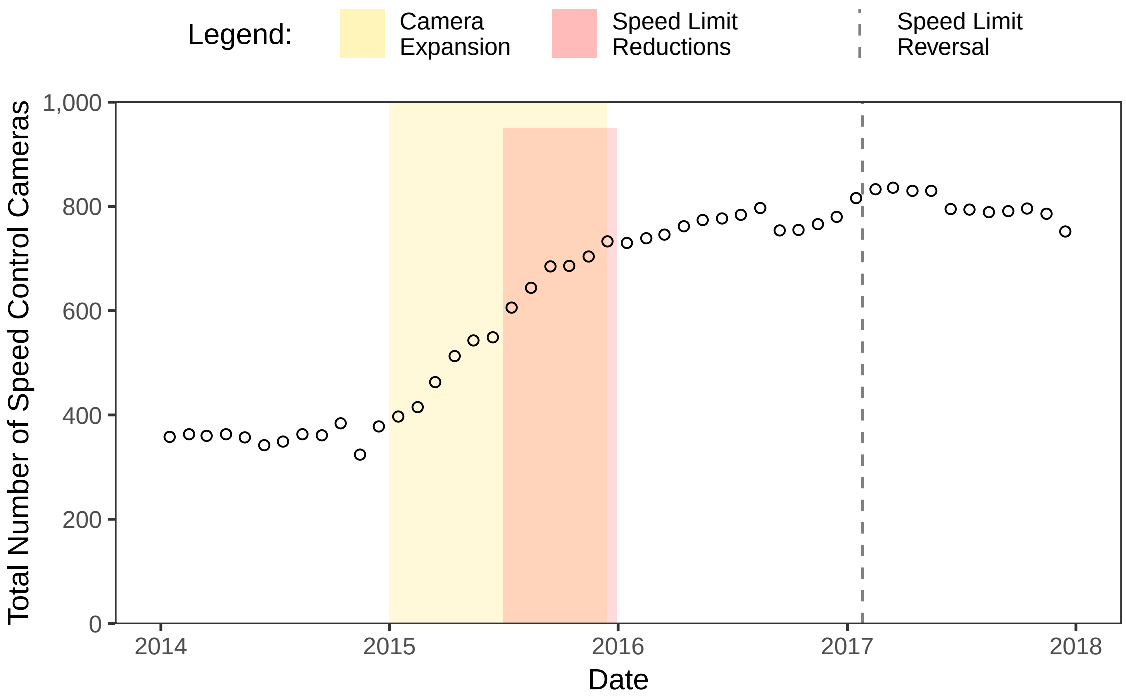
Figures

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



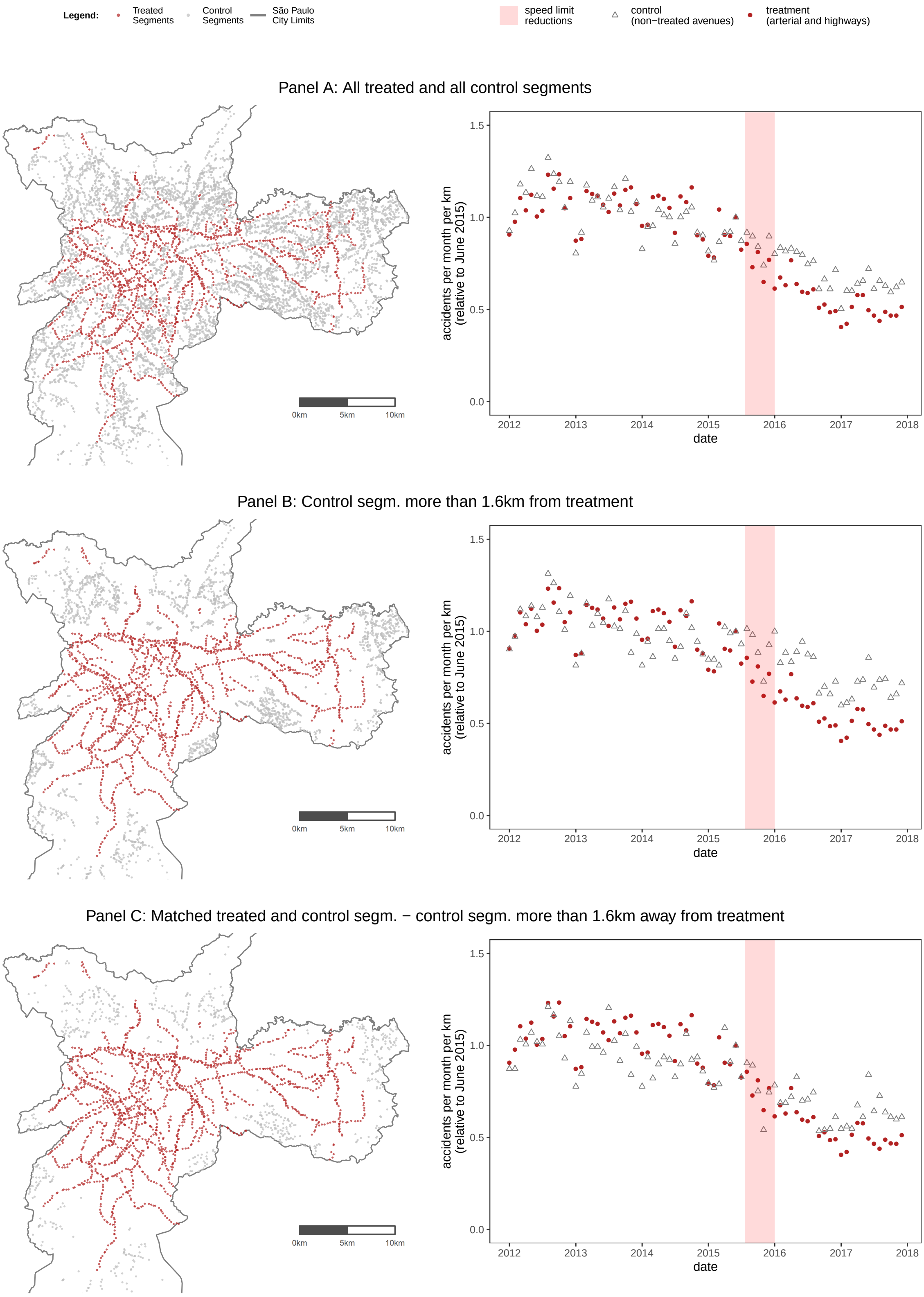
Notes: Data for New York is from the City's Vision Zero Four Year Report (NYC, 2018) and the NYPD Crime and Enforcement Activity Reports. Data for São Paulo comes from reports of road accidents compiled by the Transit Agency of São Paulo (CET, 2017) and the SSP-SP statistical data website.

Figure 2. Changes to Speed Limits and Control Cameras in São Paulo



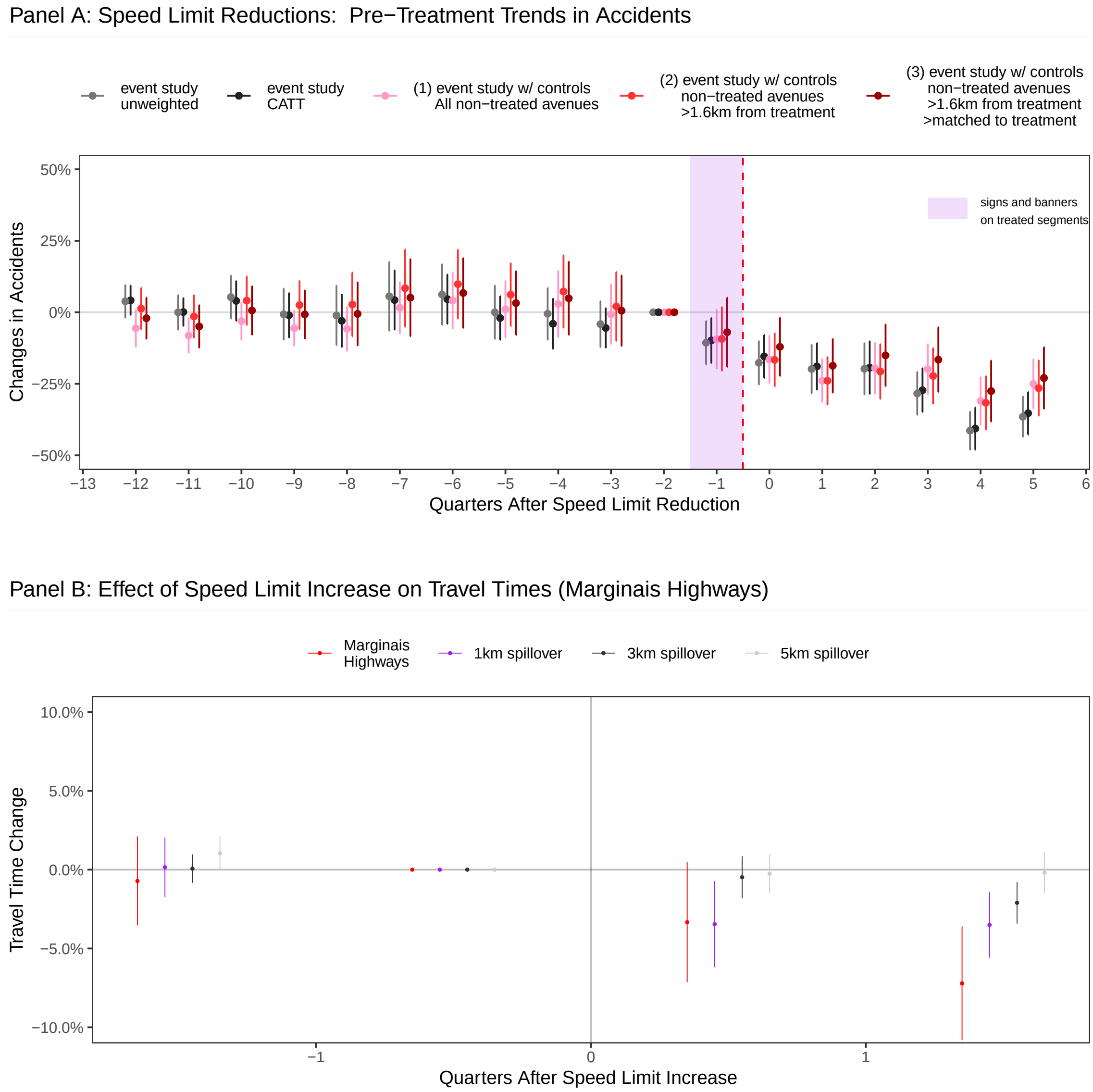
Notes: Monthly observations of speed monitoring cameras compiled from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET).

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



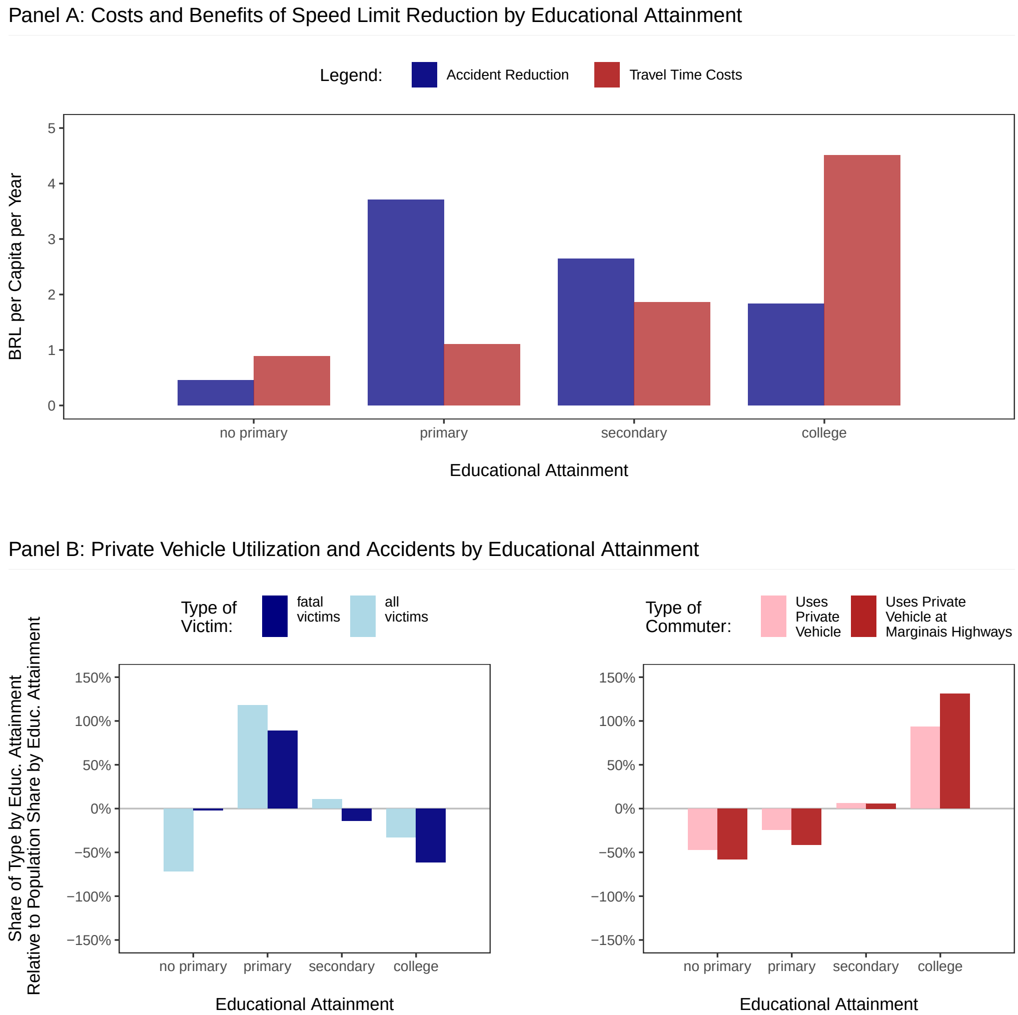
Notes: In the maps on the left side of the figure, treated segments include all arterial roads and highways where speed limits were reduced in 2015. In Panel A, the control group includes all non-treated avenues and express roads in São Paulo. In Panel B, the control segments are restricted to segments 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: The models reported in Panel A are closely related to the models presented in Table 2. The models reported in Panel B are closely related to the models presented in Table 4. Though, For both Panel A and B, models above include relative quarter leads of treatment and use the second relative quarter before treatment as the reference period as in Borusyak and Jaravel (2016).

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 conducted an extensive review of 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 provided information about speed limit changes that took place in 2015 and that were used in our analyses. Figure A.1 depicts an example of one of these reports. Reports specify 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.[59] Speed limits on arterial roads were reduced from 60 km/h to 50 km/h.[60] 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.

[59] 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.

[60] 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)

appendix/tables/A2_CET_speed_changes/cet_reports_2.pdf

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

» Emergências no Trânsito: Ligue 1188

Companhia de Engenharia de Tráfego

Página Inicial | Perguntas Frequentes | Mapa do Site | Acesso à Informação | Participação Social |

PREFEITURA DE SÃO PAULO MOBILIDADE E TRANSPORTES

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.

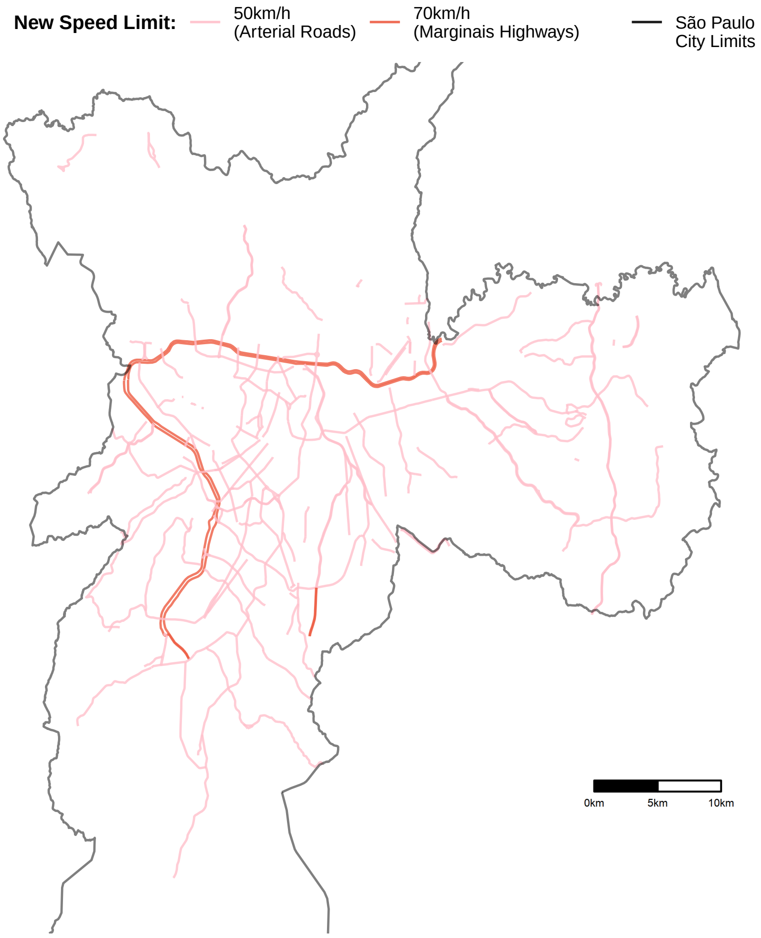
A medida se faz necessária vista que a referida via possui tráfego intenso e grande volume de pedestres. Na Avenida Aricanduva, entre janeiro de 2011 e dezembro de 2014, aconteceram 828 acidentes veiculares com vítimas e 132 atropelamentos, resultando em 1286 pessoas feridas e 45 mortes. Desta forma, 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.

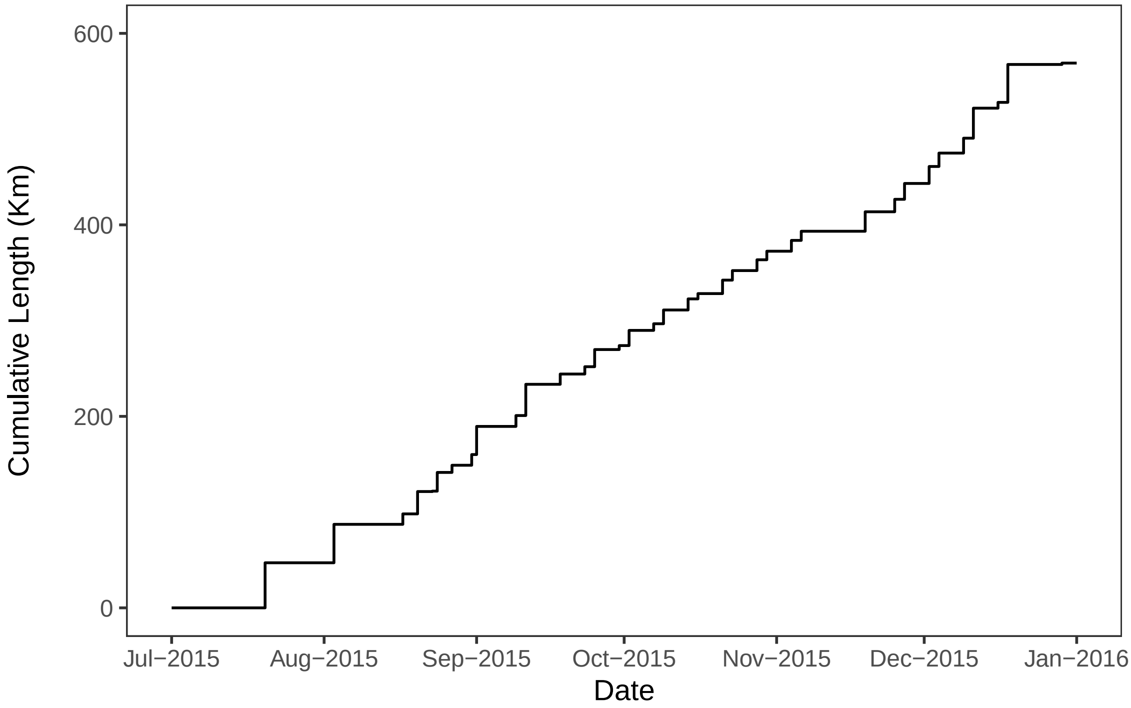
Fale com a CET - Ligue 1188. Atende 24 horas para informações de trânsito, ocorrências, reclamações, remoções e sugestões.

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

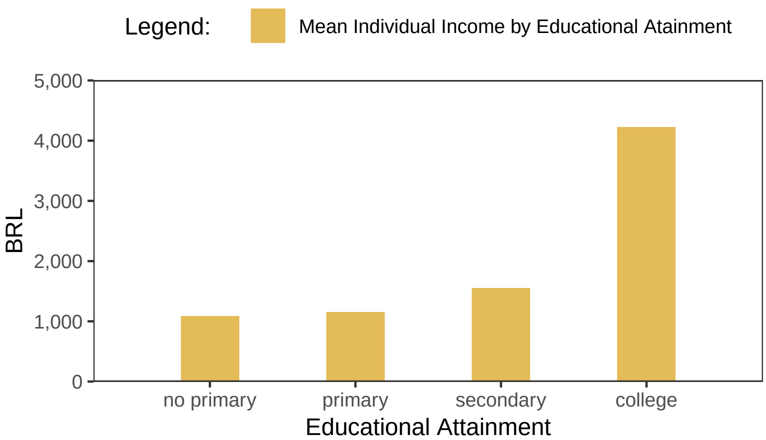
Table A.3 presents basic descriptive statistics for a representative set of motorized trips made on a regular weekday in São Paulo, Brazil. This table was constructed using trip data from the Household Mobility Survey of 2012, which we used as the source of representative trips for which we collected repeated observations of trip durations on the Google Directions API.[61] 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.

[61] 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 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. After the speed limit change, the proportion of cameras installed on treated segments increased to 69.1 %. 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.

A central aim of this paper is to compare the costs and benefits of speed limit regulations in a developing country city and understand their effects under different conditions. We are interested in accounting for the effects of camera-based enforcement in our estimates of the effects of speed limit changes, which is important since road segments that receive camera-based enforcement likely have higher baseline accident risk. Our event study design controls for fixed differences in the probability of accidents on road segments that receive speed limits reductions and camera-based enforcement relative to road segments where speed limits are reduced but that never receive camera-based enforcement. We are also interested in the heterogeneous effects of speed limit reductions on roads where the new limits are clearly enforced. To do this, we exploit variation in the timing of onset of camera-based enforcement, allowing us to compare the effects of speed limit reductions on segments that have camera-based enforcement and those that do not (but will).

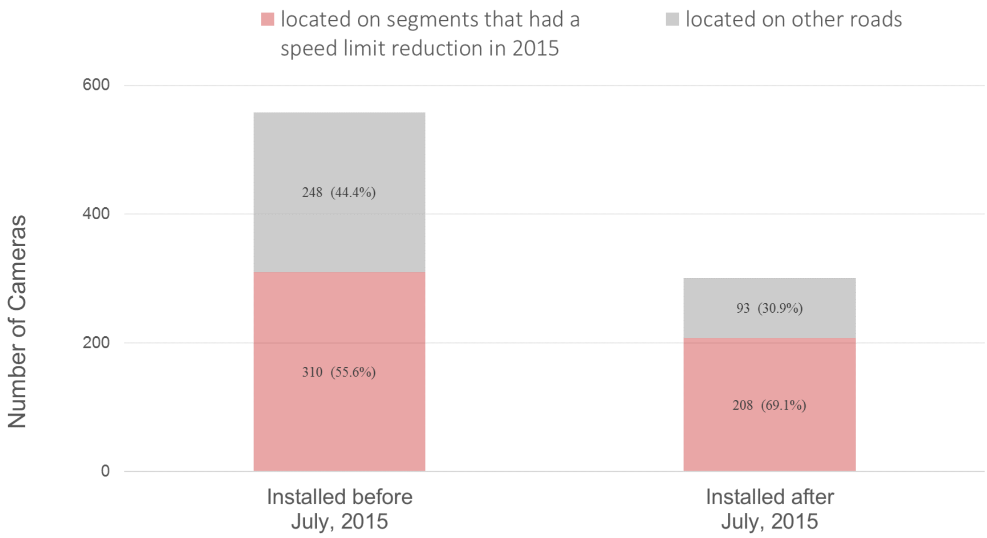
Our models flexibly control for differences in accident risk across segments using segment fixed effects. We do, however, treat the timing of onset of camera-based enforcement on a given segment as random with respect to accidents. We interpret a change in accidents when a camera is placed on a treated segment as evidence of a differential effect of the speed limit reduction in the presence of camera-based enforcement. Figure A.7 plots trends in accidents before and after the placement of a new camera. We see some evidence of fluctuations in accidents during the pre-period, but no evidence of a differential pre-trend. In the post period, we see evidence of declines in accidents (with a lag in response). Note: These are not estimates of the differential effect of speed limit reductions with camera-based enforcement, which we provide in the main analysis.

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

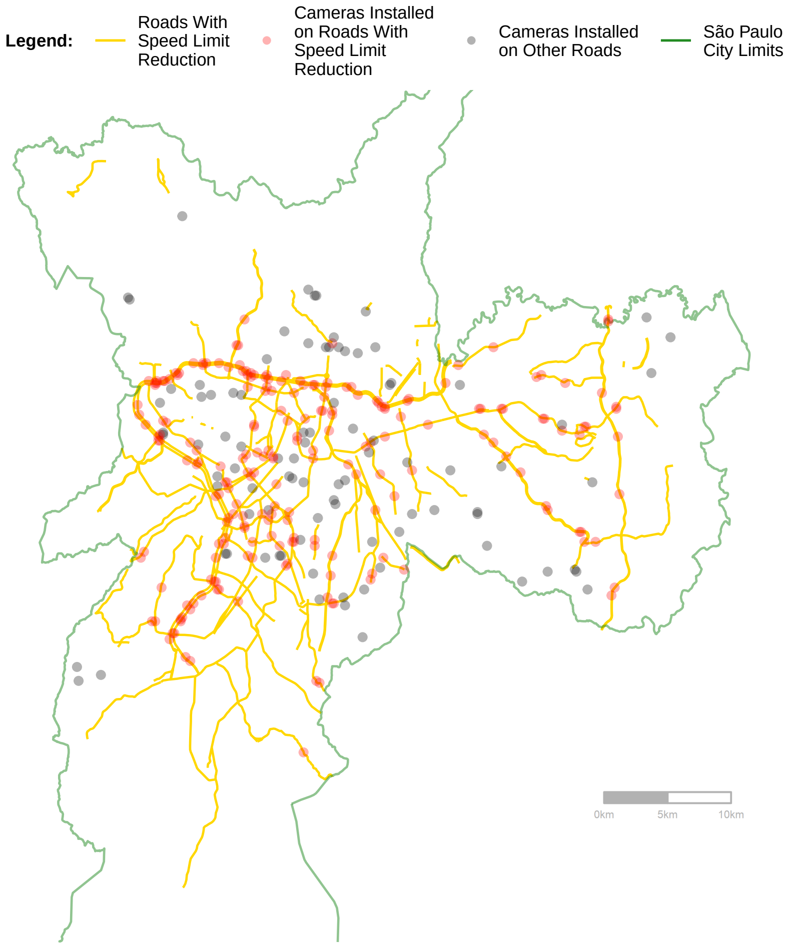
Notes: Table created by the authors based on data scraped from the website Painel de Mobilidade Segura (http://mobilidadesegura.prefeitura.sp.gov.br), which is maintained by the São Paulo Transit Agency (CET). In the case of cameras, the numbers indicate the maximum number of unique camera locations 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 Cameras in São Paulo by Installation Date and Treatment Group



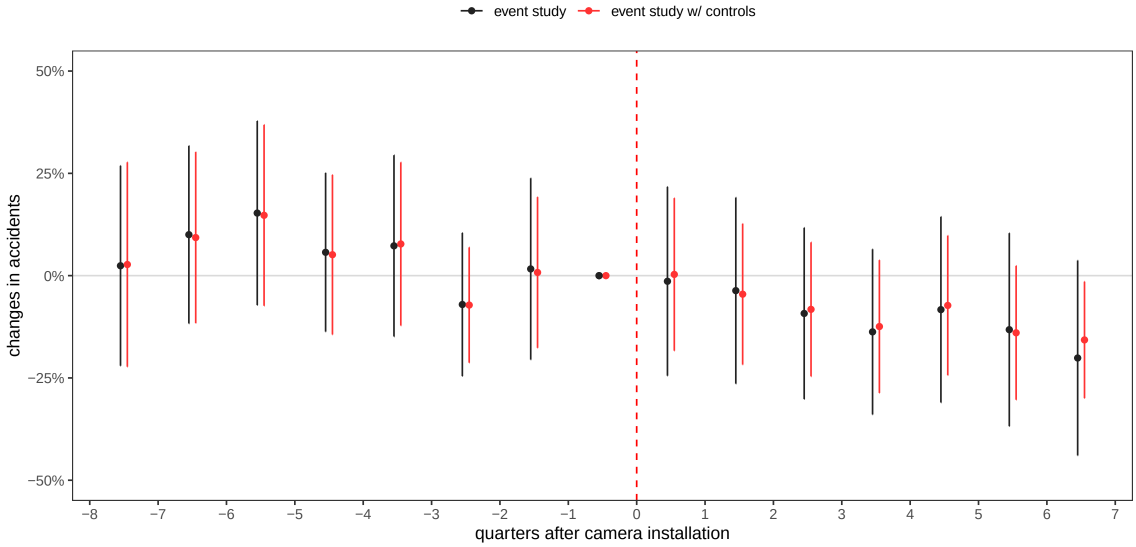
Notes: Information about cameras and their location was extracted from the website http://mobilidadesegura.prefeitura.sp.gov.br/ which is maintained by the São Paulo City Hall and compiles information about traffic violations in the city. Road segments are defined as "Treatment Group" if their speed 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 on which 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).

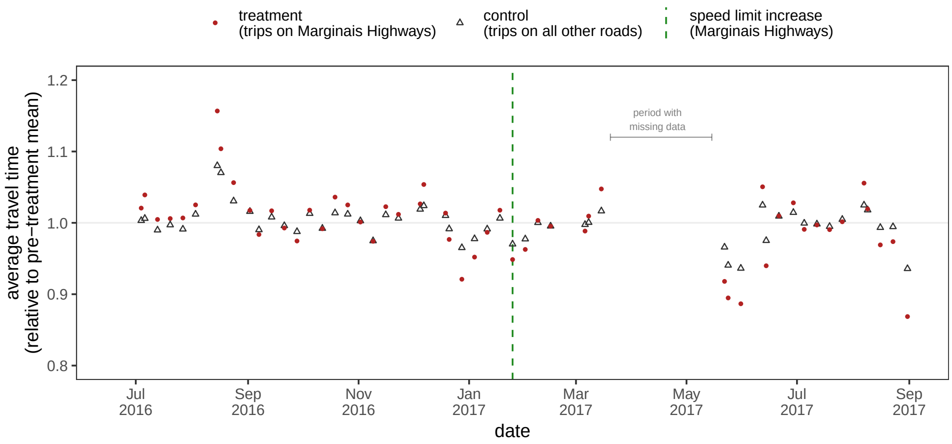
Figure A.7. Trends in Accidents Pre/Post Camera Installation



Notes: All coefficients correspond to average changes in accidents relative to the quarter immediately before the placement of traffic cameras in each segment. The specification without controls includes a linear time trend, fuel sales and total number of cameras as covariates. The specification with controls (segments that never had a traffic camera) substitute those covariates with calendar time fixed effects. Error bars represent 95% confidence intervals of coefficients.

A.4 Crawled data - Estimated Travel Times

Figure A.8. Average Duration of Queried Trips (by week)

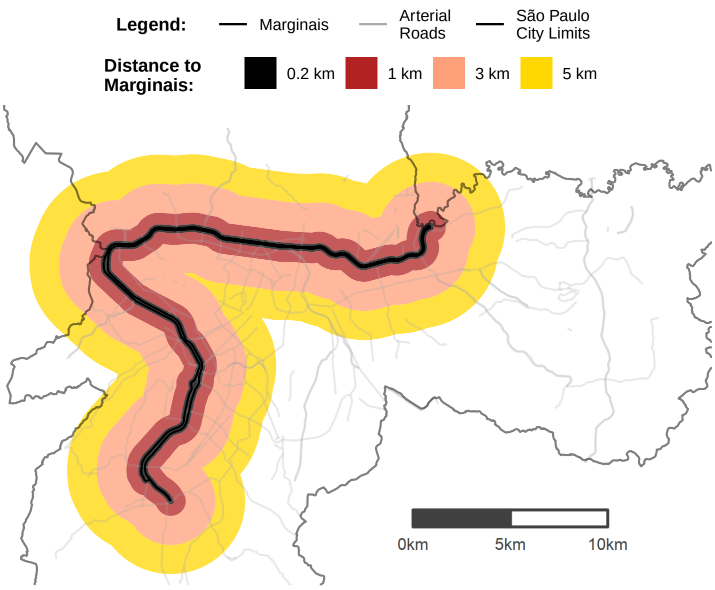


Notes: Each point corresponds to the average estimated travel time of crawled trips relative to its corresponding pre-treatment mean. Due to a server failure, our web-crawler was inactive between March 15, 2017 and May 21, 2017. Therefore, data from that period is missing from the series.

A.5 Spillover Areas of the Travel Time Empirical Model

Figure A.9 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.9. Marginais Highways: Treated and Spillover Zones

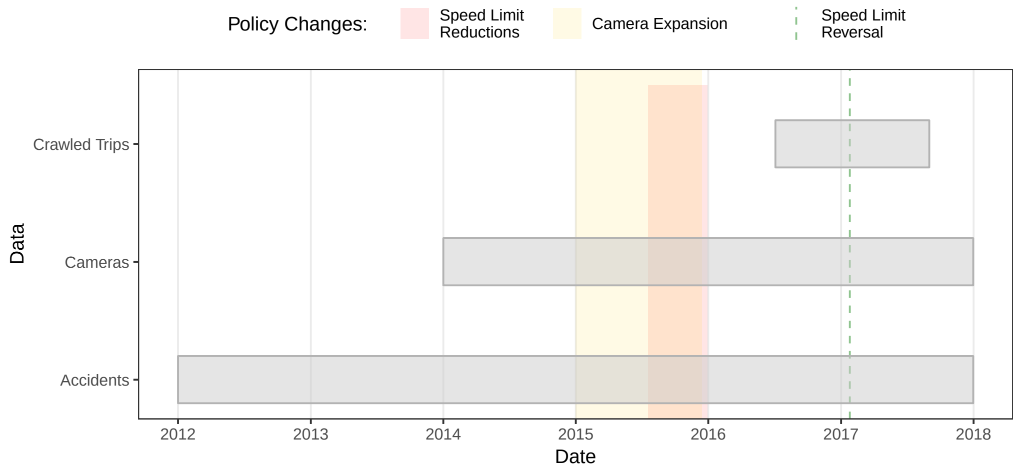


Notes: Buffers were constructed using the shapefile of the Marginais highways.

A.6 Timeline of Policy Changes and Data

Figure A.10 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.10. Timeline of Speed Limit Changes and Datasets



Notes: Crawled Trips refer to trip durations for a representative set of origins and destinations that 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

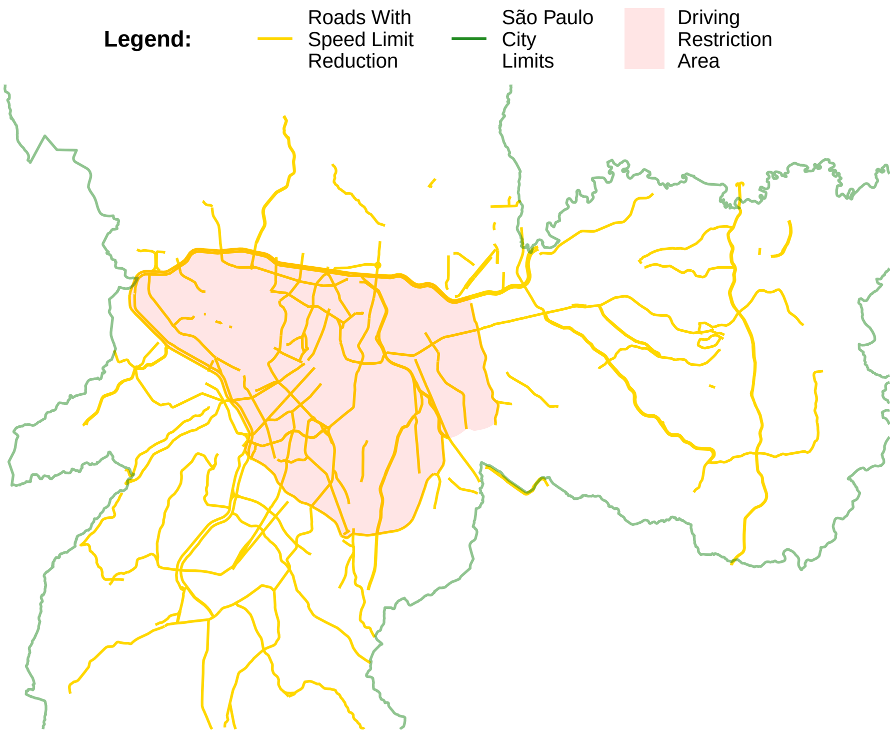
Before the new speed limits were implemented, the Traffic Agency of São Paulo would put up banners and signs along the relevant road segments indicating the upcoming change. Figure A.11 shows an example of these signs. These banners would inform drivers about the new speed limits and the dates when they 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. 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.11. 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.12 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.12. 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. Our 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 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.[62] 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

[62]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 (speed limit increase) 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.[63] 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

[63]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).

in accident risk during the reversal period.

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. Statistical significance: ***=0.1%,**=1%,*=5%.

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.3 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.

B.4 Robustness of Accident Results to the Inclusion of Time-Varying Covariates

We evaluate the robustness of our main model to the selection of time-varying controls by estimating a version of each specification that omits them. We report estimates in Table B.2, with columns 1 and 2 corresponding to the main event study estimates in the paper and columns 3 and 4 replicating these results while omitting controls. We find that failure to control for secular trends in accidents by omitting time-varying covariates can increase the magnitude of our estimates by 5-10 percentage points.

Table B.2. Effect of Speed Limit Reduction and Camera Enforcement on Road Accidents: Event Study Models with and without Time Varying covariates

	Dependent variable: number of accidents per segment per month			
	Event Study with Controls		Event Study without Controls	
	Unweighted	CATT	Unweighted	CATT
	(1)	(2)	(3)	(4)
Panel A:				
Quarters after speed limit reduction				
1	-0.169 ***	-0.167 ***	-0.216 ***	-0.219 ***
	(0.038)	(0.038)	(0.029)	(0.028)
2	-0.189 ***	-0.194 ***	-0.287 ***	-0.291 ***
	(0.040)	(0.039)	(0.029)	(0.026)
3	-0.188 ***	-0.199 ***	-0.266 ***	-0.280 ***
	(0.043)	(0.044)	(0.030)	(0.028)
4	-0.274 ***	-0.278 ***	-0.331 ***	-0.336 ***
	(0.035)	(0.035)	(0.027)	(0.023)
5	-0.405 ***	-0.409 ***	-0.470 ***	-0.478 ***
	(0.032)	(0.033)	(0.027)	(0.024)
6	-0.355 ***	-0.353 ***	-0.457 ***	-0.453 ***
	(0.037)	(0.037)	(0.030)	(0.027)
Camera on segment				
camera	0.033	0.031	0.026	0.022
	(0.037)	(0.037)	(0.037)	(0.038)
camera × speed limit reduction	-0.128 *	-0.115 **	-0.126 *	-0.110 **
	(0.052)	(0.042)	(0.051)	(0.042)
Panel B:				
Speed limit reduction (SLR)				
Marginais highways	-0.458 ***	-0.459 ***	-0.516 ***	-0.518 ***
6th quarter after SLR	(0.062)	(0.064)	(0.045)	(0.045)
Arterial Roads	-0.341 ***	-0.337 ***	-0.451 ***	-0.337 ***
6th quarter after SLR	(0.040)	(0.040)	(0.034)	(0.040)
Treatment group	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways
Control group	None	None	None	None
Segment FE	Yes	Yes	Yes	Yes
Year-month FE	No	No	No	No
Parametric funct. form	Yes	Yes	Yes	Yes
Time Varying Covariates	Yes	Yes	No	No
Observations	100,572	100,572	100,572	100,572

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 with respect to 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. All sets of event study estimates use a parametric functional form with a linear time trend. Columns (1) and (2) also include two city-wide covariates that change over time (fuel sales andtotal number of traffic cameras), columns (3) and (4) do not include those covariates. Panel A reports pooled 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. Statistical significance: ***=0.1%,**=1%,*=5%.

Table B.3. 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

Notes: All estimates correspond to 6th quarter. Poisson coefficients are 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 were estimated using a monthly panel of road segments, however specifications differ with respect to the observations that were 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. 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 different sets of non-treated road segments as controls and month fixed effects. Statistical significance: ***=0.1%,**=1%,*=5%.

B.5 Robustness of Accident Results to Alternative Segment Lengths

Road segments are the basic spatial unit of analysis in our main accidents model. However, the definition of road segments with 400m of length is not determined on the basis of any specific model. We test the sensitivity of our results to a design using road segments with 800m of length below. The results from this exercise are reported in Table B.4. It shows that estimates and standard errors are highly consistent.

Table B.4. Effect of Speed Limit Reduction and Camera Enforcement on Road Accidents: Robustness to Alternative Segment Length

	Dependent variable: number of accidents per segment per month			
	Event Study (CATT)		Event study with controls (Sample 3)	
	400m segments	800m segments	400m segments	800m segments
Panel A:				
Quarters after speed limit reduction				
1	-0.167 ***	-0.166 ***	-0.124 ***	-0.119 ***
	(0.038)	(0.039)	(0.036)	(0.036)
2	-0.194 ***	-0.193 ***	-0.184 ***	-0.188 ***
	(0.039)	(0.039)	(0.037)	(0.036)
3	-0.199 ***	-0.197 ***	-0.147 ***	-0.159 ***
	(0.044)	(0.044)	(0.043)	(0.042)
4	-0.278 ***	-0.276 ***	-0.158 ***	-0.179 ***
	(0.035)	(0.036)	(0.047)	(0.046)
5	-0.409 ***	-0.407 ***	-0.260 ***	-0.282 ***
	(0.033)	(0.033)	(0.045)	(0.045)
6	-0.353 ***	-0.352 ***	-0.217 ***	-0.233 ***
	(0.037)	(0.037)	(0.046)	(0.047)
Camera on segment				
camera	0.031	-0.005	0.009	-0.031
	(0.037)	(0.031)	(0.034)	(0.028)
camera × speed limit reduction	-0.115 **	-0.085 *	-0.118 *	-0.082 *
	(0.042)	(0.034)	(0.054)	(0.038)
Panel B:				
Speed limit reduction (SLR)				
Marginais highways	-0.459 ***	-0.459 ***	-0.324 ***	-0.346 ***
6th quarter after SLR	(0.064)	(0.066)	(0.085)	(0.086)
Arterial Roads	-0.337 ***	-0.336 ***	-0.194 ***	-0.217 ***
6th quarter after SLR	(0.040)	(0.040)	(0.046)	(0.044)
Treatment group	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways	All treated arterial and highways
Control group	None	None	All non-treated avenues	Non-treated ave. >1.6km away from treatment
Segment FE	Yes	Yes	Yes	Yes
Year-month FE	No	No	Yes	Yes
Parametric funct. form	Yes	Yes	No	No
Observations	100,572	62,490	222,108	138,018

Notes: The table reports the estimates from the CATT event study and the event study with the most restrictive controls (sample 3) from Table 2. For each of these two specifications, the table presents the coefficients estimated using alternative definitions of road segments: baseline specification using 400m road segments and 800m road segments. In both cases, standard errors are clustered by road (202 clusters). Statistical significance: ***=0.1%,**=1%,*=5%.

B.6 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. In this section, 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 in the interval around actual departure could lead to a significant bias in our results. Table B.6 presents the main policy effect estimates from both models. The differences between results are not statistically or economically different across models.

Table B.5. 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 change 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. Statistical significance: ***=0.1%,**=1%,*=5%.

B.7 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 and two distinct measures of uncertainty: 1) 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, and 2) the ratio between the 90th and 50th percentile of those same trips. Table B.6 reports the effects by level of treatment intensity. The estimates indicate an overall reduction in uncertainty across the study period as measured by both variables, but we do not find evidence of differential changes in the uncertainty in trip durations on roads where speed limits increased.

Table B.6. Effects of Speed Limit Increase on Travel Time Uncertainty

	Dependent Variable:	
	Log of Standard Deviation Travel Time (by trip)	Log of p90/p50 (by trip)
post speed limit increase	-0.089 ***	-0.012 ***
	(0.013)	(0.001)
post speed limit increase * Marginais		
0% to 10% of trip length on Marginais	-0.074	-0.006
	(0.046)	(0.003)
10% to 20% of trip length on Marginais	-0.051	-0.007
	(0.081)	(0.007)
20% to 50% of trip length on Marginais	-0.002	-0.002
	(0.060)	(0.005)
more than 50% of trip length on Marginais	-0.066	-0.012
	(0.090)	(0.009)
Trip-Hour FE	Yes	Yes
Obs.	334,308	334,308

Notes: Coefficients are estimated using a difference-in-differences model where the outcome in the first column is the standard deviation of the duration of all queries made for a given trip, and the 90th/50th percentile of trip duration in the second column. Treatment is defined as trips taken along the Marginais Highways after the 2017 speed limit 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. Both specifications include trip fixed effects. Statistical significance: ***=0.1%,**=1%,*=5%.

C Evaluating the Possible Effects of Behavioral Spillovers and Re-Routing

C.1 Accident Reductions and Ticketing on Nearby Roads

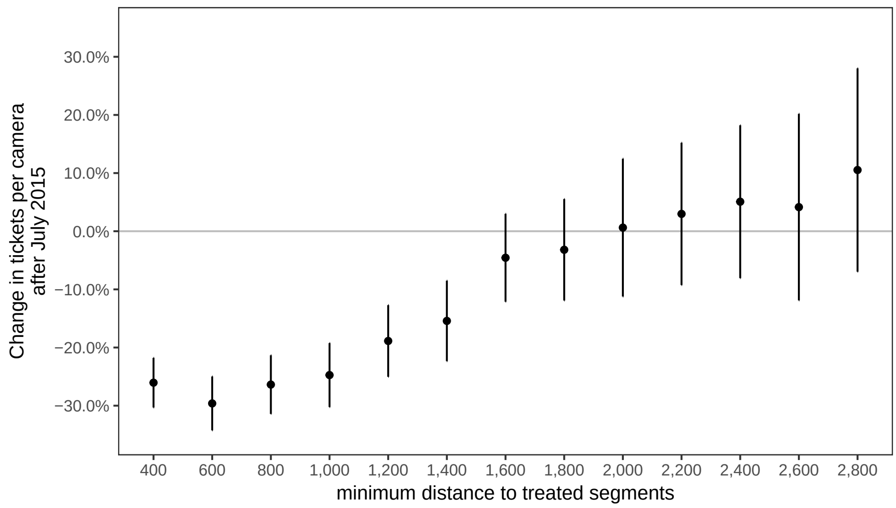
To evaluate possible changes in driver behavior on non-treated segments, we evaluate changes in the number of speeding tickets issued on segments that were nearby treated roads but were not treated. Given that most trips utilize a combination of treated and never-treated road segments, it is likely that 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 speeding 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.

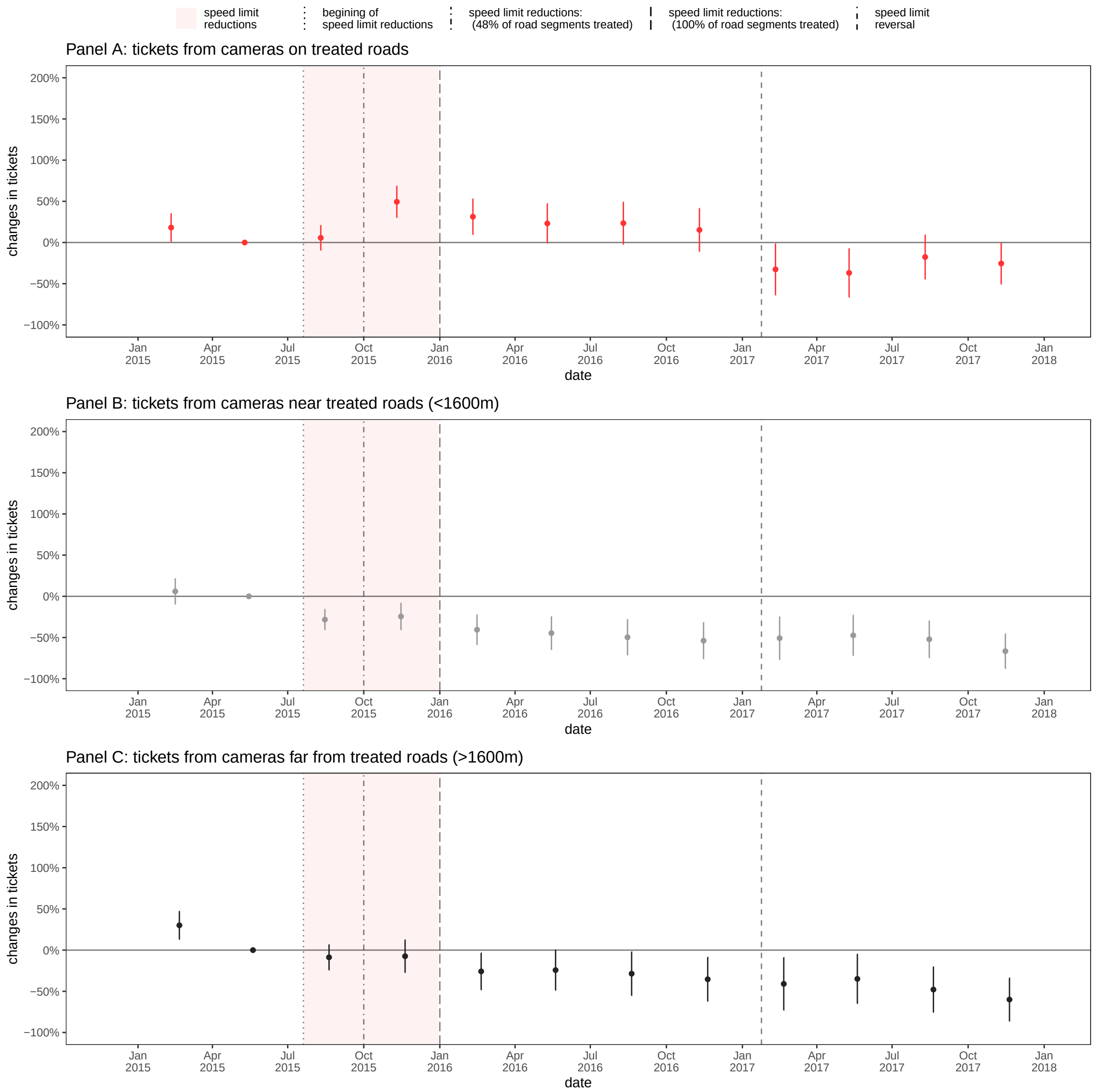
We plot the estimate of β for each of these distance thresholds in Figure C.1. At larger distance thresholds, the estimates become less precise as the number of cameras on never-treated segments becomes smaller. The estimates suggest a clear pattern of reductions in speeding tickets on non-treated roads 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



We then plot event study estimates of changes in speeding tickets for treated segments, for never-treated segments within 1600 meters of a treated segment, and for never-treated segments beyond 1600 meters of a treated segment. Estimates reported in Figure C.2 illustrate a continuous downward trend in the study period across all three groups. This secular trend is evident in the pre-period in all classes of ticketing and also for accidents. The top panel plots speeding tickets issued on treated segments (red), where we observe a discontinuity in ticketing in response to the speed limit reduction (increase in ticketing) and in response to the reversal (reduction in ticketing). We note that timing of treatment during the speed limit reduction is not respected when we use calendar time, so the pronounced increase in tickets on treated roads appears to occur in October 2015, which is the point when speed limits on approximately 48% of treated segments had been reduced. The middle panel plots speeding tickets on nearby segments where drivers do not need to adjust to a new regulatory regime but where spillovers may have occurred. We observe a discontinuous reduction in ticketing on these roads. As discussed in the section "Substitution, Spillovers, and the Displacement of Accident Risk" in the main text, this evidence is consistent with a spillover in the behavioral effect of the policy, where drivers continue to drive at reduced speeds even after transitioning from treated to untreated roads on a trip. We find a similar reduction in accidents on nearby roads, which is also consistent with a behavioral spillovers explanation. We interpret this evidence as inconsistent with a significant amount of re-routing away from treated roads and onto nearby roads, which would tend to lead to an increase in VMT and likely result in a net increase in the flow of speeding tickets (and accidents). Of course some combination of both effects is possible. In the bottom panel, we plot speeding tickets on the group of control segments that we identify as unimpacted by the speed limit changes (segments beyond 1600 meters from a treated segment). We do not find any evidence of a discontinuity in ticketing among cameras in the control group.

Figure C.2. Changes in Speeding Tickets after Speed Limit Reductions by Distance of Cameras to Treated Road Segments

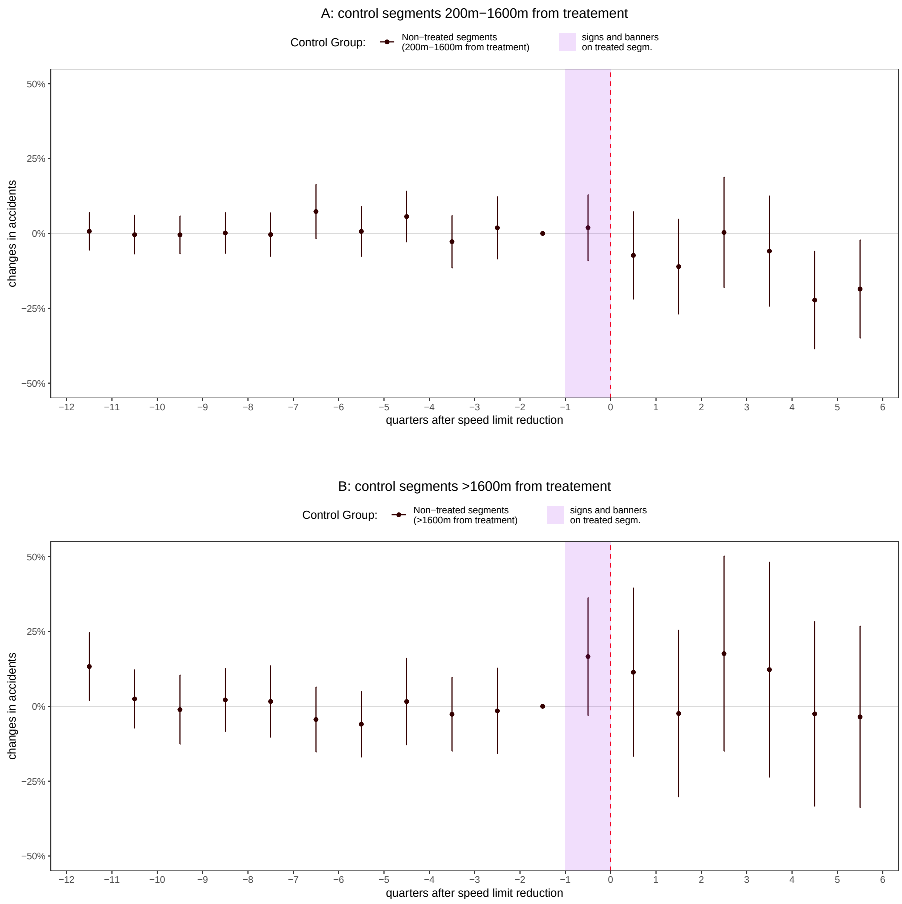


Notes: All models are restricted to the cameras that were in place during the second quarter of 2015 (the quarter immediately before the reductions). Each model was estimated separately using different sets of cameras. The series in red includes the coefficients associated with the cameras located on treated road segments. The series in grey includes the coefficients associated with cameras located on non-treated road segments located up to 1600m from treated roads. The series in black includes the coefficients associated with cameras located on non-treated road segments located more than 1600m from treated roads. Error bars represent 95% confidence intervals of coefficients.

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.3 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.

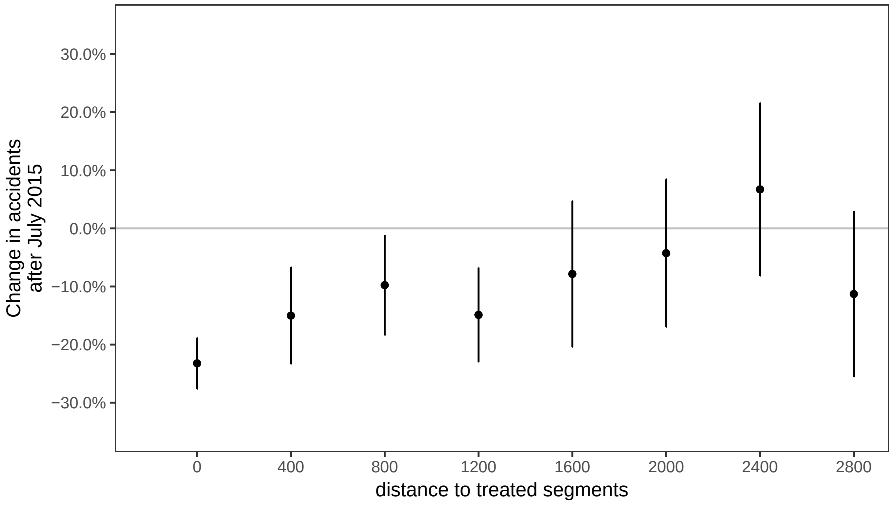
In both cases, the coefficients associated with the periods preceding the policy change are not statistically different from zero. However, the estimates in Panel A indicate that there was a modest reduction in road accidents on nearby treated segments that was statistically different from what would be expected from secular trends in the period following the speed limit reduction. The same result is not observed in panel B.

Figure C.3. 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.4 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.4. 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.2 Validating non-speeding tickets as proxies for traffic volume

In our analysis, we use non-speeding tickets as a measure of traffic flow to test for evidence of route substitution or changes in trip-taking on treated roads. Here, we present a validation exercise for the use of non-speeding tickets as a proxy for traffic flows. We analyze two types of non-speeding tickets: (1) tickets issued for the violation of the São Paulo driving restriction and (2) all other non-speeding tickets as defined in the main text. Our validation compares these two types of non-speeding tickets with (1) traffic delays (congestion) by region of the city from the São Paulo Traffic Agency for the 2016-2018 period and (2) cross-sectional variation in VMT from our simulation exercise with OSRM data. The results in Table C.1 indicate that flows of non-speeding tickets predict cross-sectional variation in simulated VMT as well as panel variation in observed traffic delays. We note that our test for evidence of substitution using non-speeding tickets requires the assumption that a change in VMT results in a detectable change in flows of non-speeding tickets on treated roads. The results of these tests suggest that changes in traffic flows would likely result in changes in flows of non-speeding tickets.

Table C.1. Correlation Between Non-Speeding Tickets with Simulated VMT and Traffic Congestion

	Dependent Variable:			
	Driving Restriction Tickets (log)		Other non-Speeding Tickets (log)	
	(1)	(2)	(3)	(4)
Simulated VMT (log)	0.299 **		0.365 ***	
	(0.100)		(0.072)	
Km of Congestion (log)				
1st quartile		0.531 *		0.349 ***
		(0.267)		(0.054)
2nd quartile		0.553 *		0.358 ***
		(0.256)		(0.051)
3rd quartile		0.618 *		0.376 ***
		(0.248)		(0.049)
4th quartile		0.604 *		0.351 ***
		(0.239)		(0.047)
Obs.	228	1,214	352	1,649

Notes: Columns (1) and (3) report the results of regressions that include the simulated daily VMT on segments as explanatory variables for the average number of non-speeding tickets per segment during the year of 2017. Segments without cameras are not included. Simulated VMT is calculated based on OSRM API queries of private vehicle trips reported in the 2012 household travel survey. Columns (2), and (4) are the results of regressions that include the total daily congestion recorded in each region of São Paulo (South, West, North, East and Central) during the year of 2017 as explanatory variables for the total number of non-speeding tickets recorded in each of those regions in each date. These panel models include region fixed effects. Columns (1) and (3) are specified with a single coefficient for the relationship between tickets and simulated VMT. Columns (2) and (4) use a more flexible model allowing different slopes for each quartile level of congestion. Statistical significance: ***=0.1%,**=1%,*=5%.

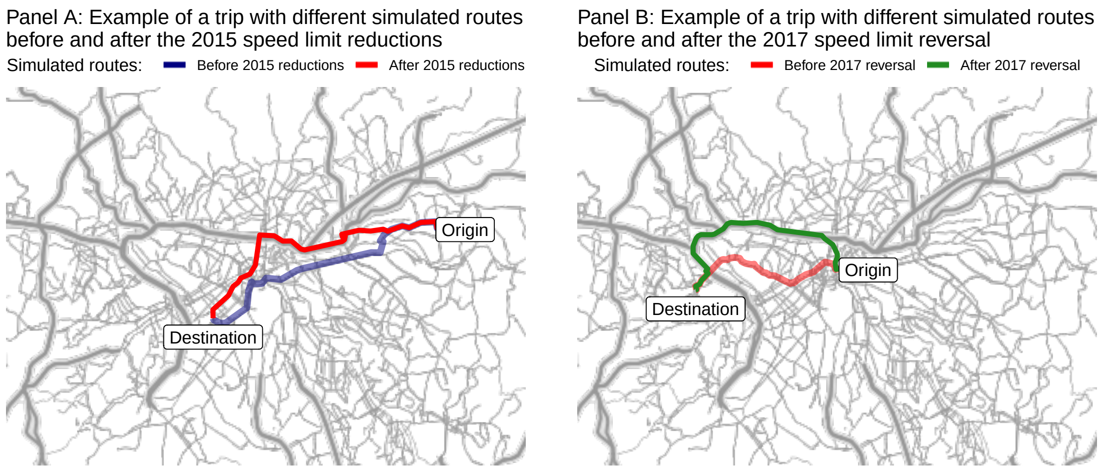
C.3 Possible Effects of Re-Routing

In this section, we evaluate the potential effects of re-routing by simulating optimal routing behavior in response to the speed limit changes. This simulation is conducted using the OSRM API, which allows an analyst to adjust the speed limit on any road in the Sao Paulo network and then identify the optimal (lowest time cost) route for a given trip. We utilize the full set of representative trips from the travel survey and conduct three sets of simulations: (1) during the pre-policy baseline (before July 20, 2015), (2) post speed limit reduction (July 20, 2015 - January 25, 2017) and (3) post-reversal (January 25, 2017)

Route simulations

Figure C.5 illustrates an example of two survey trips: one that had net benefits from re-routing under our simulation of the speed limit reduction (2015) and the other under our simulation of the reversal (2017).

Figure C.5. Route Simulations with OSRM API



Notes: Both figures show examples of trips with different routes according to the queries made on OSRM API using different speed limits for the roads that were treated in each year. Panel A shows an example of a trip with a different optimal route after the 2015 speed limit reductions. Panel B shows an example of a trip with a different optiimal route after the 2017 reversal.

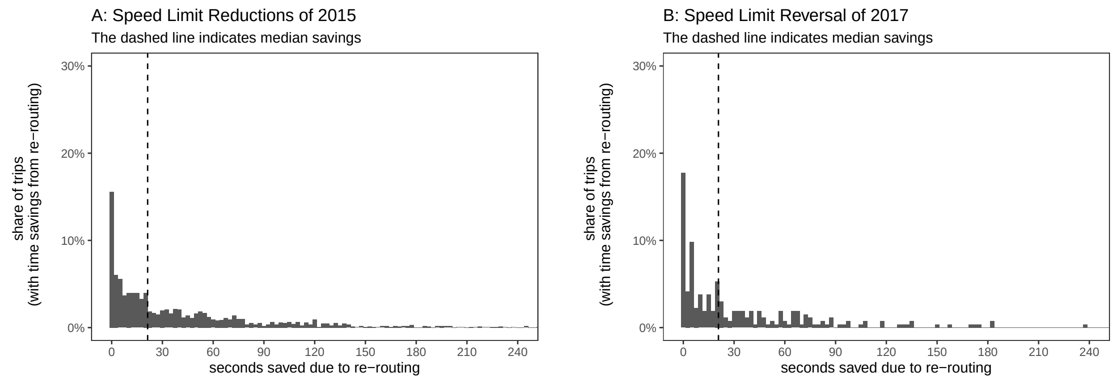
Speed Limit Reduction of 2015

In the first simulation, we identify the optimal routes and duration of trips in the survey using speed limits from the baseline (pre-reduction) period. We then simulate the exact same set of trips after adjusting the speed limits on all treated roads to their post-reduction speed limits. We use the routing data from these simulations to obtain a measure of the vehicle miles traveled (VMT) on each road segment in our analysis. A comparison of the pre-reduction to post-reduction simulated routes suggests that VMT on treated roads could decline by up to 12.1%, VMT on untreated roads within the spillover area could increase by up to 13.9% and that VMT on untreated segments beyond the spillover area could increase by up to 1.8%. There are three implications of this simulation for our empirical results:

(1) The effects of re-routing may have resulted in up to a 12.1% reduction in VMT on treated roads, potentially reducing the number of potential accidents occurring there. In order to better understand incentives to re-route, we examine the distribution of time savings that would result from re-routing for the trips that our OSRM simulations suggest would re-optimize and present them in Figure C.6. This suggests that the median trip in our sample would save 21 seconds by re-routing away from treated routes and that

only a small number of trips would save more than 60 seconds by re-routing. These magnitudes suggest that while travelers may have had room to re-optimize, the potential gains from routing away from the Marginais highways and major arterial roads may not have provided a large enough incentive for most users. This could explain why we find no evidence of changes in the flows of tickets administered for infractions that are unrelated to speeding in reduced form tests reported in Figure C.1.

Figure C.6. Distribution of Travel Time Savings due to Re-routing for Trips with Different Simulated Routes when Speed Limits Change



Notes: Travel time savings due to re-routing are calculated by comparing two sets of simulations for each policy change: 1) simulated travel times for trips using optimised routes and speed limits from after the policy change, and 2) simulated travel times using the speed limits from after the policy change but using the the optimal routes from before the policy change. The difference between those simulated values are defined as the time savings due to re-routing. In both panels, the histogram includes only the trips that have different routes after each policy change.

(2) Do possible changes in routing behavior explain accident reductions or affect our event-study estimates? In Table C.2, we use the simulation exercise to examine the robustness of our main event study estimates (Panel A) to the inclusion of the segment-specific change in VMT from the OSRM simulation (Panel B). These specifications yield a set of estimates of treatment effects conditional on potential changes in VMT that occur as a result of re-routing. A comparison of the main estimates in Panel A to estimates in Panel B indicates that there are no differences in any of the main treatment effects when accounting for these changes. Estimates reported in the final row of Panel B indicate that there is no evidence of an association between simulated re-routing and reductions in accidents.

Table C.2. Changes in the Estimation of Policy Effects on Accidents After Controlling for Simulated VMT Changes on Segments

	Dependent variable: number of accidents per segment per month, not controlling for VMT change					Dependent variable: number of accidents per segment per month, controlling for VMT change				
	Event Study		Event study with controls			Event Study		Event study with controls		
	Unweighted	CATT	(1)	(2)	(3)	Unweighted	CATT	(1)	(2)	(3)
Panel A:										
Quarters after speed limit reduction										
1	-0.169 ***	-0.167 ***	-0.119 ***	-0.188 ***	-0.124 ***	-0.169 ***	-0.167 ***	-0.120 ***	-0.188 ***	-0.114 **
	(0.038)	(0.038)	(0.030)	(0.033)	(0.036)	(0.037)	(0.038)	(0.030)	(0.033)	(0.036)
2	-0.189 ***	-0.194 ***	-0.185 ***	-0.253 ***	-0.184 ***	-0.190 ***	-0.195 ***	-0.187 ***	-0.253 ***	-0.177 ***
	(0.040)	(0.039)	(0.030)	(0.033)	(0.037)	(0.040)	(0.039)	(0.030)	(0.033)	(0.037)
3	-0.188 ***	-0.199 ***	-0.144 ***	-0.220 ***	-0.147 ***	-0.189 ***	-0.200 ***	-0.147 ***	-0.221 ***	-0.142 **
	(0.043)	(0.044)	(0.036)	(0.039)	(0.043)	(0.042)	(0.044)	(0.036)	(0.039)	(0.044)
4	-0.274 ***	-0.278 ***	-0.148 ***	-0.234 ***	-0.158 ***	-0.275 ***	-0.279 ***	-0.150 ***	-0.235 ***	-0.154 **
	(0.035)	(0.035)	(0.036)	(0.041)	(0.047)	(0.035)	(0.035)	(0.037)	(0.042)	(0.048)
5	-0.405 ***	-0.409 ***	-0.250 ***	-0.323 ***	-0.260 ***	-0.406 ***	-0.410 ***	-0.253 ***	-0.324 ***	-0.256 ***
	(0.032)	(0.033)	(0.035)	(0.040)	(0.045)	(0.032)	(0.033)	(0.036)	(0.041)	(0.046)
6	-0.355 ***	-0.353 ***	-0.195 ***	-0.274 ***	-0.217 ***	-0.355 ***	-0.354 ***	-0.198 ***	-0.276 ***	-0.211 ***
	(0.037)	(0.037)	(0.037)	(0.042)	(0.046)	(0.037)	(0.036)	(0.038)	(0.042)	(0.047)
Camera on segment										
camera	0.033	0.031	-0.034	0.005	0.009	0.033	0.031	-0.034	0.005	0.009
	(0.037)	(0.037)	(0.030)	(0.034)	(0.034)	(0.037)	(0.038)	(0.030)	(0.035)	(0.034)
camera × speed limit reduction	-0.128 *	-0.115 **	-0.091	-0.117 *	-0.118 *	-0.129 *	-0.116 **	-0.092	-0.117 *	-0.118 *
	(0.052)	(0.042)	(0.056)	(0.054)	(0.054)	(0.052)	(0.042)	(0.056)	(0.054)	(0.054)
VMT change after all reductions						-0.031	-0.031	-0.014	-0.015	-0.012
						(0.054)	(0.052)	(0.026)	(0.044)	(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	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.	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	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Parametric funct. form	Yes	Yes	No	No	No	Yes	Yes	No	No	No
Observations	100,572	100,572	542,004	254,436	222,108	100,572	100,572	542,004	254,436	222,108

Notes: VMT changes per segment were calculated with OSRM API using two alternative settings for the road networks of São Paulo: first, the speed limits of treated roads were set using the limits from before the 2015 changes; second, the speed limits on treated segments were redefined according to the values that were in place in 2016, that is, after the reductions and before the reversal of January 2017. Statistical significance: ***=0.1%,**=1%,*=5%.

(3) The simulation suggests that any re-routing would occur within the spillover zone (1600 meters) that we define using empirical evidence from accidents and ticketing behavior. This result supports the use of control segments from beyond the spillover zone in the event study analysis of the speed limit reduction. Our event study evidence in Figure C.3 indicates that, if anything, the speed limit reduction resulted in net reductions in accidents on control segments located within the 1600 meter spillover area. We interpret this as evidence that either the rate of re-routing that actually occurred was lower than predicted by the simulation, which could occur if the benefits from re-routing were small (as shown above). It is also possible that the countervailing effect of behavioral spillovers on driver behavior were larger than our estimates suggest, making our estimates of net benefits with spillovers a lower bound.

Speed Limit Reversal of 2017

We then use OSRM simulations to evaluate our estimates of treatment effects in the speed limit reversal of 2017. We begin by testing the sensitivity of our exposure variable (fraction of a trip taken on the Marginais Highways, which were the only treated roads during the reversal) to whether exposure is defined using survey trips that are simulated using pre-reversal (Jan 2017) speed limits or post-reversal speed limits. We note that while our simulation exercise allows us to hold everything on the São Paulo road network constant while only changing speed limits on treated roads, it is possible that other changes on the network have occurred between our initial period of route collection and the timing of the simulation exercise. We test the robustness of treatment effects estimates across three different measures of exposure to Marginais: Panel A measures exposure using routes collected in 2017 during the post-reversal period; Panel B measures exposure using data collected in 2019 using pre-reversal speed limits; Panel C measures exposure using data collected in 2019 using post-reversal speed limits. Table C.3 reports estimates of main effects across the three different measures. We don't find evidence of statistical differences in effects.

Table C.3. Changes in the Estimation of Policy Effects on Travel Time Using Alternative Measures of Exposure to Treatment

	Changes in Log of Estimated Travel Time											
	Treatment exposure calculated based on routing queries made on OSRM online API on March/2017				Treatment exposure calculated based on routing queries made on a local OSRM server on August/2018							
	Speed limits from 2017 *(Marginais = 90 km/h)*				Speed limits from 2016 *(Marginais = 70 km/h)*				Speed limits from 2017 *(Marginais = 90 km/h)*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post SLI - Ratio at Marg.	-0.068 ***	-0.061 ***	-0.055 ***		-0.068 ***	-0.062 ***	-0.057 ***		-0.061 ***	-0.058 ***	-0.053 ***	
	(0.014)	(0.014)	(0.013)		(0.014)	(0.014)	(0.013)		(0.013)	(0.013)	(0.012)	
Post SLI - Ratio at Marg. - Peak				-0.055 **				-0.056 **				-0.050 **
				(0.017)				(0.018)				(0.017)
Post SLI - Ratio at Marg. - Off-Peak				-0.056 ***				-0.059 ***				-0.056 ***
				(0.011)				(0.011)				(0.010)
Post SLI	-0.018 ***	-0.011 ***			-0.018 ***	-0.011 ***			-0.018 ***	-0.011 ***		
	(0.004)	(0.003)			(0.004)	(0.003)			(0.004)	(0.003)		
Post SLI - spillover area 1km		-0.034 ***	-0.034 ***	-0.034 ***		-0.036 ***	-0.034 ***	-0.034 ***		-0.035 ***	-0.033 ***	-0.033 ***
		(0.008)	(0.008)	(0.008)		(0.008)	(0.008)	(0.008)		(0.008)	(0.008)	(0.008)
Post SLI - spillover area 3km		-0.016 **	-0.016 ***	-0.016 ***		-0.015 **	-0.015 ***	-0.015 ***		-0.015 **	-0.015 **	-0.015 ***
		(0.005)	(0.005)	(0.005)		(0.005)	(0.005)	(0.005)		(0.005)	(0.005)	(0.005)
Post SLI - spillover area 5km		-0.009	-0.011 **	-0.011 **		-0.009 *	-0.012 **	-0.012 **		-0.009 *	-0.011 **	-0.011 **
		(0.004)	(0.004)	(0.004)		(0.004)	(0.004)	(0.004)		(0.004)	(0.004)	(0.004)
Rain	0.019 ***	0.019 ***			0.019 ***	0.019 ***			0.019 ***	0.019 ***		
	(0.005)	(0.005)			(0.005)	(0.005)			(0.005)	(0.005)		
Holiday	-0.100 ***	-0.100 ***			-0.100 ***	-0.100 ***			-0.100 ***	-0.100 ***		
	(0.009)	(0.009)			(0.009)	(0.009)			(0.009)	(0.009)		
Trip-Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Motorcycle Late Night Restriction	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spillover Area Specific Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Date-Hour FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555	1,337,555

Notes: Columns 1-4 replicate the main results from Table 4. Columns 5-8 replicate those same models using exposure to treatment defined according to the optimal routes from OSRM API using a road network where the speed limits in the Marginais highways were set according to the values in place before the 2017 reversal. Columns 9-12 run that same exercise with speed limits in the Marginais highways from after the 2017 reversal. Statistical significance: ***=0.1%,**=1%,*=5%.

Accounting for Re-routing in Costs and Benefits Estimates

Although we do not find evidence that re-routing resulting from the 2015 speed limit reduction affected accidents on treated road segments, we provide calibrated estimates of the possible effects of comprehensive re-routing on our benefits estimates by adjusting our counterfactual to reduce accidents by 12.1% (the simulated reduction in VMT) on treated road segments. We account for possible forgone trips using extensive margin elasticity parameters from Akbar and Duranton (2017), which indicate that the speed limit reduction could lead to a reduction of up to 0.36% in the total number of trips on treated roads. Assuming a direct relationship between the number of trips and the number of accidents, changes in the extensive margin could reduce the effect on accidents by 1.1%. The calibrated results in Table C.4 indicate that these adjustments reduce total benefits from R$ 101.8 Million to R$ 75 Million in the event study with CATT and from R$ 58 Million to R$ 42.7 Million in the event study with matched controls.

Table C.4 also reports estimates of policy costs that are calibrated to examine the effects of re-routing on our estimates of policy costs. Our main estimates of policy costs assume that travelers re-routed when an optimal routing change was available after the speed limit reversal.[64] We assign the additional time cost for the small number of forgone trips (0.36%) in this scenario, which provides an upper bound on the cost of the speed limit reduction policy. If we instead assume that travelers did not optimally re-route, then our estimates of policy costs would increase from R$ 32.8 Million to R$ 42.6 Million using the VTPI calculation. If we assume that all travelers optimally re-route during these policy changes, our benefits-costs estimates range from 1.23-3.42. If we instead assume zero re-routing, our benefits-costs estimates range from 1.32-3.65. These estimates indicate that re-routing can materially affect impacts of a speed limit policy even when the benefits to any individual traveler may save less than a minute of their journey, suggesting an important area for empirical work. In the case of our analysis of the São Paulo speed limit program, however, assumptions about re-routing do not meaningfully affect the overall conclusions regarding the sign of net benefits from the policy.

[64] We note that re-routing behavior in the main estimates in the paper incorporates in real-time traffic conditions, which are not possible in the simulation exercise.

Table C.4. Costs and Benefits of a Speed Limit Reduction in the Marginais Highways. Comparison of Scenarios with and without Re-routing.

PANEL A: BENEFITS	without rerouting	with rerouting	
Benefits from Averted Accidents (R$ million)			
Base Event Study CATT	101.8	74.7	
Event Study w/ Matched Controls	58.0	42.5	

PANEL B: COSTS	without rerouting	with rerouting	
	simulated in August 2019	simulated in March 2017	simulated in August 2019
Cost of Additional Time Spent in Traffic (R$ million)			
VOT = VTPI individual VOT	-43.6	-32.8	-34.5
VOT = 50% of median net wage	-27.9	-20.9	-21.8
VOT = 50% of individual net wage	-43.8	-32.9	-34.2

Notes: **A: Benefits** The scenario without re-routing is the same as defined in the baseline model (5). It assumes no changes in VMT on treated roads. The scenario with re-routing assumes changes in VMT as simulated by OSRM API. In this case, the calculation of benefits assumes a direct and linear relation between simulated VMT changes and accidents. That is, a 12.1% reduction in simulated VMT would explain 12.1%p.p. of the reduction in accidents. **B: Costs** The scenario with re-routing is based on exposure to treatment as defined by the OSRM simulations using post-reduction speed limits. That is, the policy effect is imputed to all travellers whose optimized routes use treated road segments after the speed limit reduction. The scenario without re-routing is based on exposure to treatment as defined by the OSRM simulations using pre-reduction speed limits. That is, the policy effect is only imputed to travellers whose optimized routes used treated road segments before the speed limit reduction. The OSRM simulation based on pre-treatment speed limits was carried out using both the Open Street Maps road network of São Paulo from March 2017 and from August 2019, the simulation based on post-treatment speed limits was only carried in August 2019.

C.4 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.3, 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.5. 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$ 33-204 Million when we account for the benefits/costs of spillovers, which is comparable to but somewhat higher than our range of R$ 14-73 Million in net benefits on treated roads. When we account for spillovers in benefits and costs, the benefit/cost ratio changes from 1.32-2.33 to 1.39-3.04.

The comparison of policy costs and benefits in the treated area spillover areas is presented in Table C.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$ 86.2 to R$ 89.6 Million, which are more than two times higher than the costs on treated segments alone. We calculate a net benefit of R$ 14-73 Million on treated roads (without spillovers) and R$ 33-204 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, (2) accounting for these effects results in a similar range of net benefits due to their compensating effects and (3) estimates of direct effects alone appear to be conservative estimates of net benefits.

Table C.5. 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	1,107.8	**-11.3**
Cost of Time Spent in Traffic (R$ million)			
VOT = VTPI individual VOT	7,104.0	7,017.8	**-86.2**
VOT = 50% of median net wage	5,755.9	5,697.8	**-58.1**
VOT = 50% of individual net wage	7,560.1	7,470.5	**-89.6**

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.5 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:

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

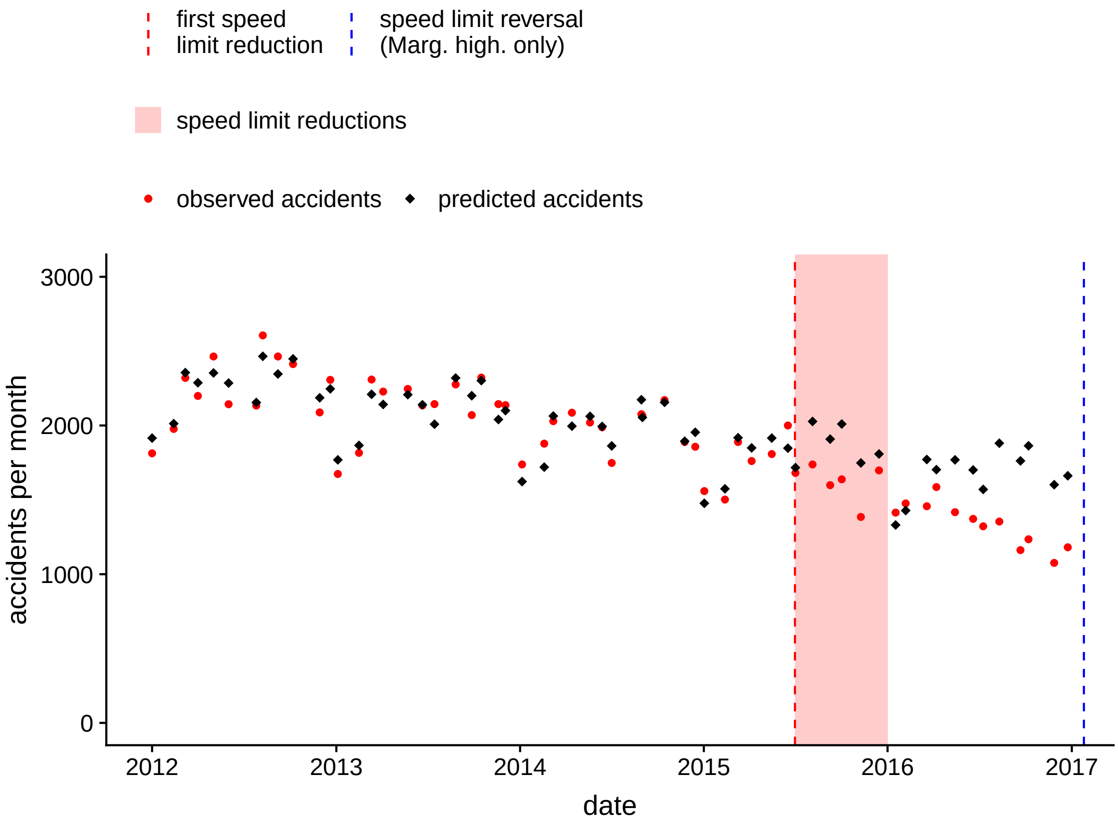
Where Y_t is total road accidents observed on month t. X_t is a linear variable on time, so β is the slope of a linear time trend. γ_t are fixed effects for months of the year, which control for seasonality and other changes occurring during this time.

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.7 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.7. Predicted and observed accidents in the whole city of São Paulo

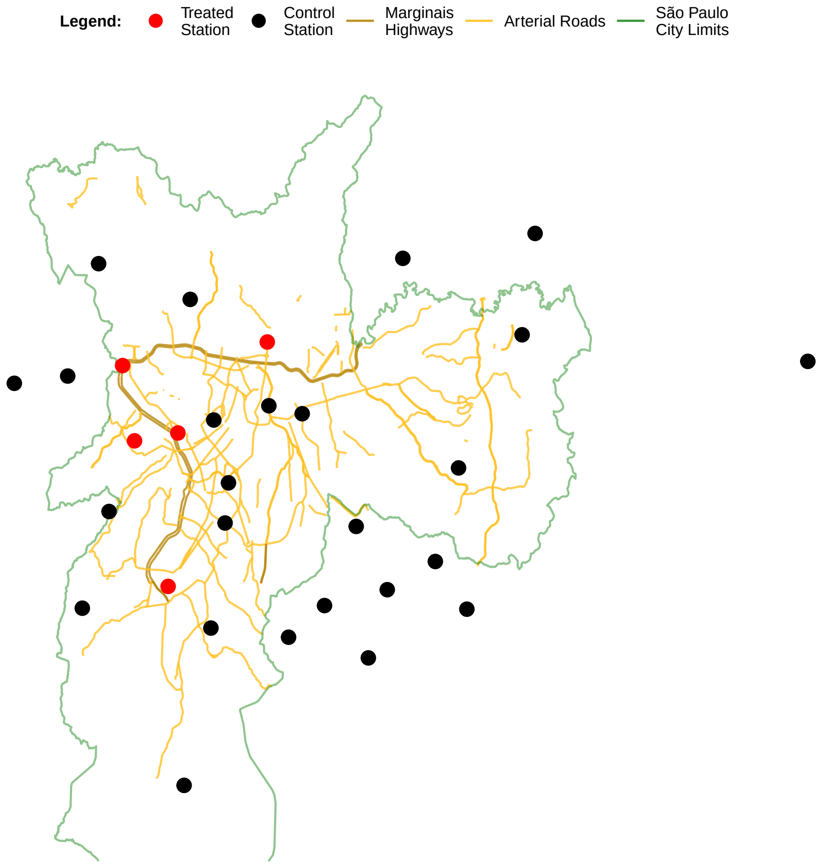


D Policy Effects on Air Pollution

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



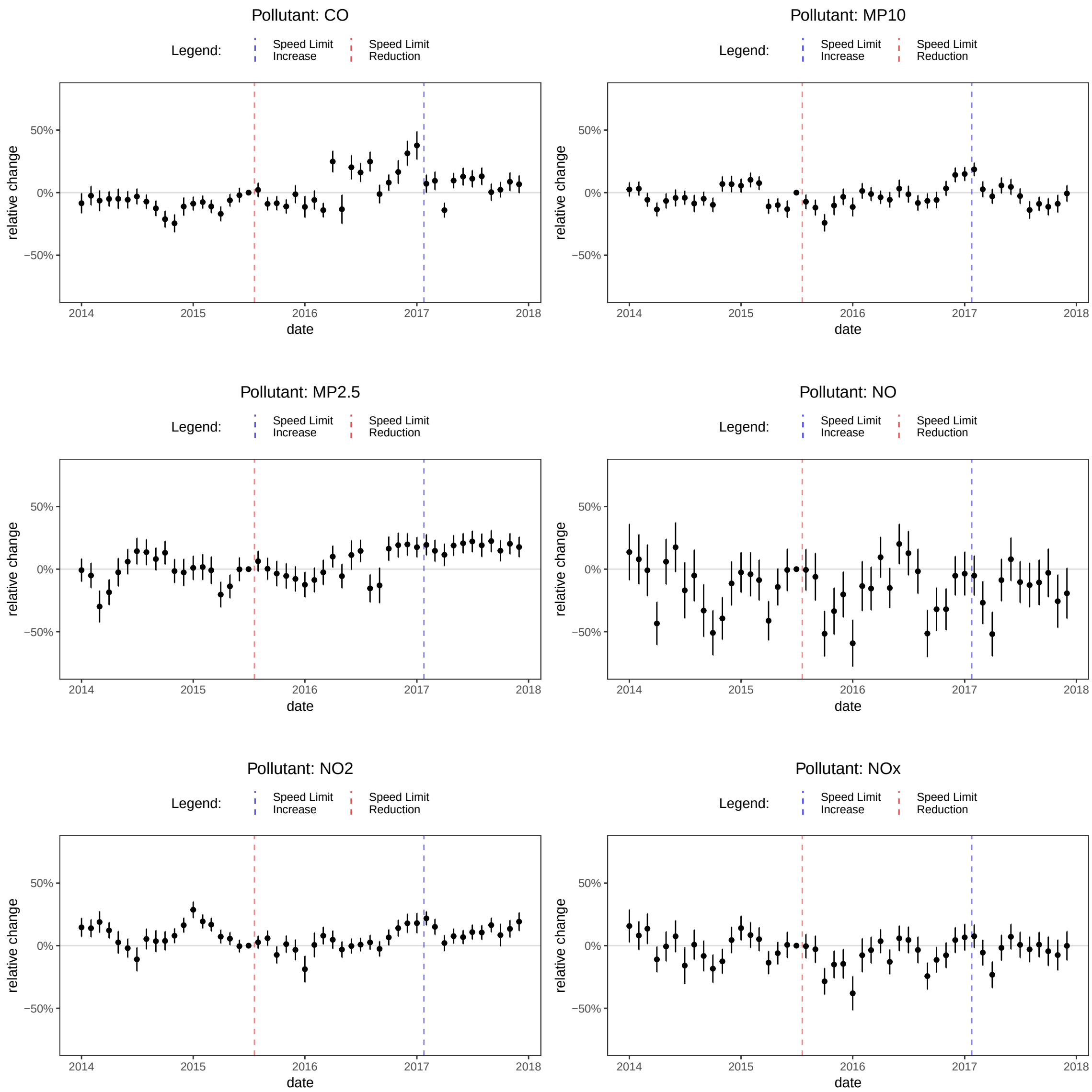
We estimate the following model:

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

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



These estimates do not reveal evidence of an effect of the program on health-related emissions concentrations. Evaluating the pollution-related health effects of the São Paulo speed limit reduction is complicated by: (1) the high existing levels of pollution concentrations in São Paulo, (2) the relatively small effect of the policy (which is specific to treated segments) in total vehicle and non-vehicle related emissions in the city, and (3) the ambiguity in direction and magnitude of the effect of speed limit changes on vehicular emissions. According to Parthak et. al. (2016): "Higher average speeds have positive correlation with higher emission levels; however, the driving load depends on speed as well as acceleration rates. Constant high speeds might lead to lower emissions than lower speeds with more accelerations and decelerations."

According to engineering studies (Ibarra-Espinosa, 2017a), the expected effects on pollution depend not only on the actual changes in traffic speed but also on several other factors such as: the interaction between vehicle speed, fleet age, type of vehicles, and fuel type used by vehicles that circulate in the affected area, atmospheric conditions, and traffic patterns in different periods of the day and different days of the week. We are aware of two engineering studies that use distinct simulation methodologies in an attempt to model these interactions and evaluate the expected effects of the São Paulo speed limit reductions of 2015 on emissions. An earlier study estimated that the speed limit reductions led to an increase in CO emissions by approximately 2% per year (Ibarra-Espinosa, 2017a). This result is due to the fact that, for most vehicles, the ratio of emissions per VMT tends to increase when speeds become lower within the range of speeds usually observed in urban areas (up to 90km/h). More recently, a separate modeling exercise by Ibarra-Espinosa (2017b) concludes that total emissions could decline by approximately -3.99% for CO. Estimates for other pollutants are not available in the original study, but are all smaller in magnitude in the latter (-1.45% for CO2, -3.49% for HC, -1.29 for NOx and would go up by 0.06% for PM). Given the variance observed in concentrations as measured by monitoring stations and all other concomitant factors that affect emissions in one of the world's largest cities, it is unlikely that any changes in emissions caused by the speed limit changes (regardless of direction) would be identified by any before/after analysis of those concentration series from monitoring stations. For CO, we identify statistically significant reductions by of as much as -24.5% and increases of as much as 37.7% in the pre and in the post periods, suggesting that variation due to other factors may be substantially larger than detectable effects resulting from the speed limit program.

We also note that the treatment evaluated in our benefit/cost analysis corresponds to the speed limit change in the Marginais highways only, which includes a much smaller set of treated roads (65 km) than the reduction analyzed by Ibarra-Espinosa studies (570 km). If we were to consider the range of results from these studies on emissions (from -3.99% to +2.00%) the effects of the speed limit reduction in the Marginais highways would represent a small fraction of that range. Given ambiguity in the direction of effects from the atmospheric science literature and evidence that they are likely relatively small in magnitude and smallest in magnitude for pollutants that have serious health-related damages such as PM and NOx, we are cautious about any claims regarding their effects on the net benefits of the São Paulo program. However, given the health effects identified in prior economic literature (albeit in a very different setting), we emphasize that this is an important area for further research. We note that reductions consistent with the direction found in (van Benthem, 2015) would increase the net benefits from the policy, making our estimates a lower bound.

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.

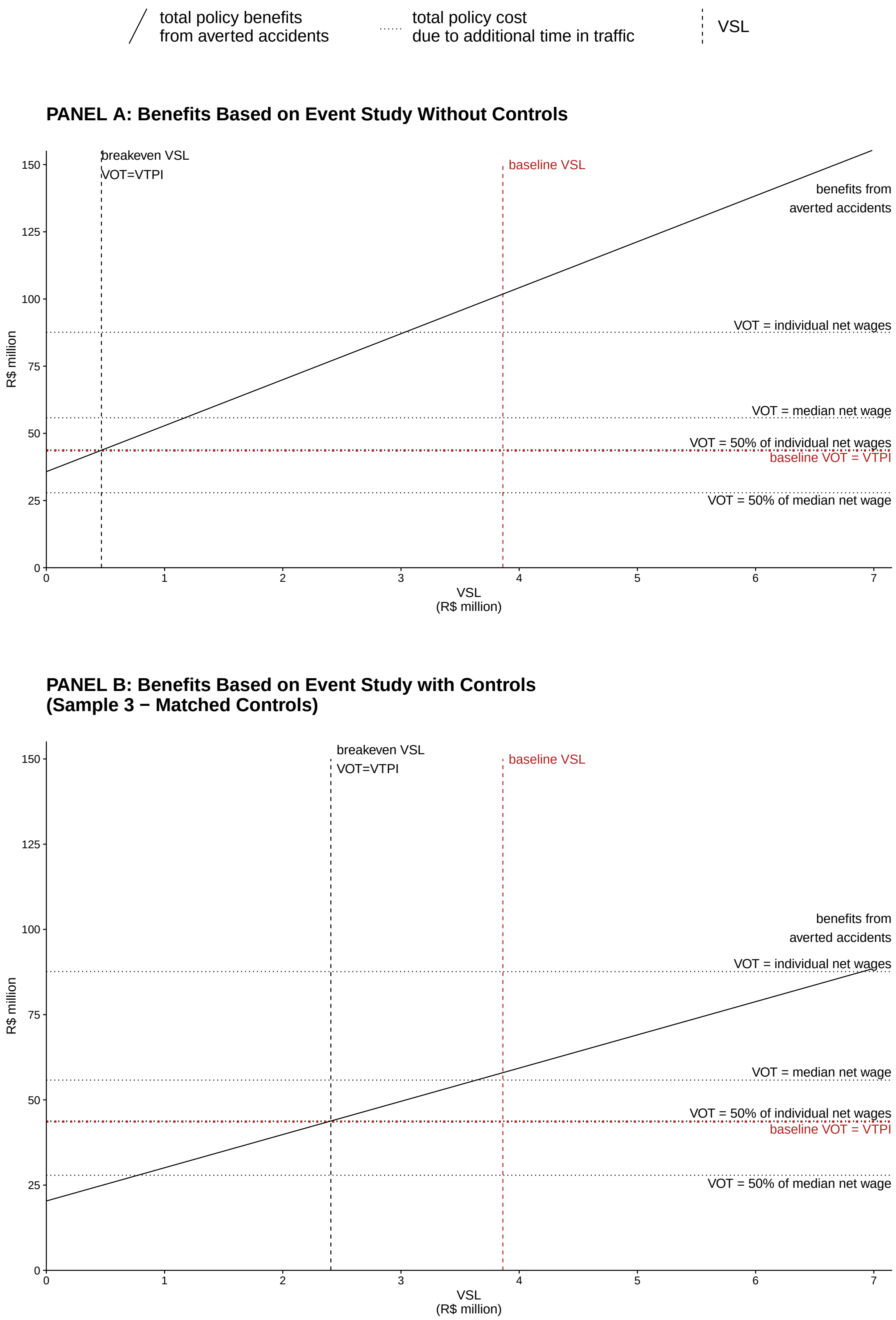
For fatal accidents, we use the Value of Statistical Life (VSL) calculated by Viscusi and Masterman (2017) for Brazil. In this paper, the authors explore the richness of data available for the USA and differences in income between countries to calculate VSL for countries with limited information.

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

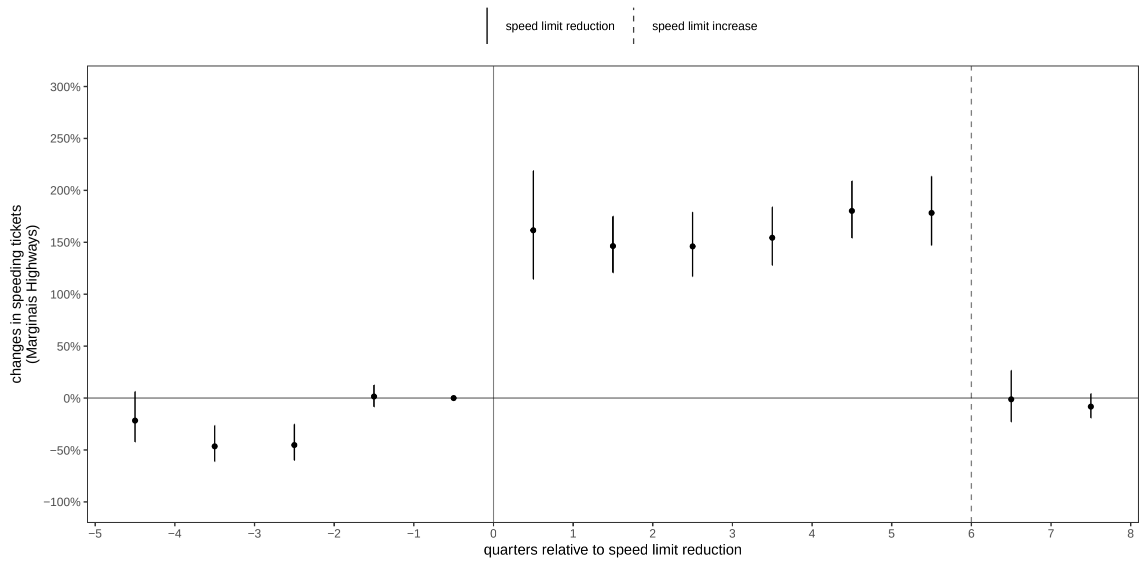


Notes: The figure plots the estimated total policy benefits using alternative values for the VSL parameter. These results are represented by the solid diagonal line in the Figure. The horizontal lines indicate the total policy cost due to longer commutes under alternative values for the VOT paramenter. The red horizontal line indicates our baseline 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 red vertical line delineates our baseline VSL that was calculated by Viscusi and Masterman (2017). The black vertical black line delineates the breakeven VSL for which policy costs equal policy benefits under the baseline VTPI VOT parameter.

F Distributional Analysis of Speed Limit Reductions: Effect of Ticket Revenue

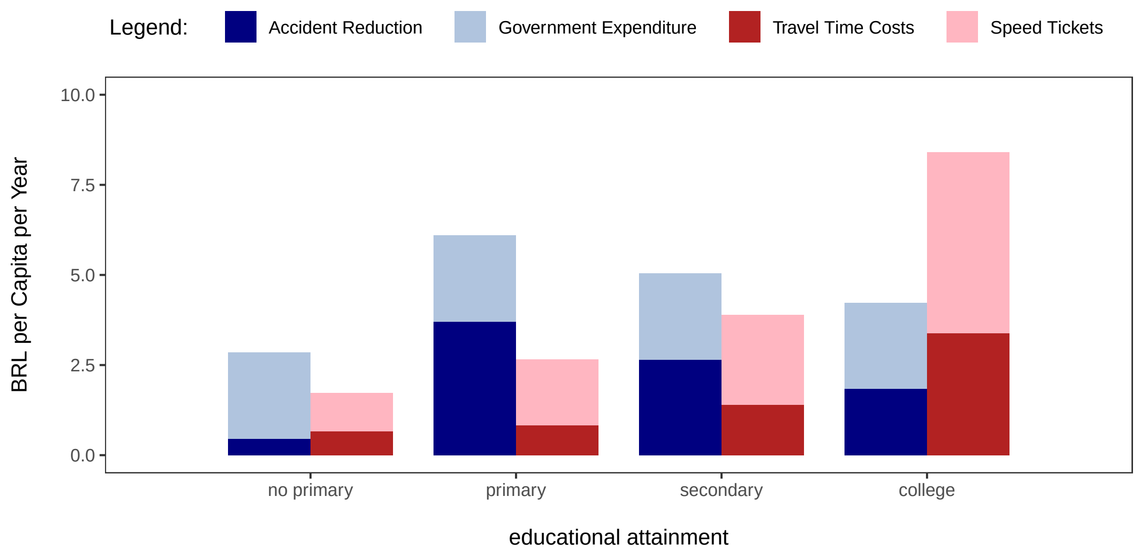
In the analysis of the distributional effects of the policy in the main paper, we focus on the distribution of the benefits and costs that are directly attributable to accidents and travel cost. However, given the camera-based enforcement regime that was also in place during the speed limit program, it is important to recognize that the penalties associated with speeding tickets also had distributional impacts as would the redistribution of the associated revenue. We first identify the impact of the speed limit reduction in the number of speeding tickets in the Marginais highways, which are plotted in Figure F.1. Speeding tickets on existing cameras tripled following the policy change, representing an additional revenue of approximately R$ 100 million per year (using an average cost of R$ 88 per ticket). We do not have information about the incomes of individuals who received speeding tickets and distribute the policy-related revenue assuming an equal incidence of ticketing risk for each driver in the sample. Higher income brackets have a higher share of drivers in the sample, resulting in a higher cost per capita for wealthier groups. We assume that ticket revenue is redistributed equally across income groups, such that all individuals receive the same benefit (R$ 6.79). Figure B.2 illustrates the implications of speeding ticket fees/revenues. The magnitude of tickets revenues is considerably larger than the costs and benefits from travel time losses and averted accidents in the short period between the speed limit reduction and subsequent reversal, thereby magnifying the distributional impacts of the speed limit program.

Figure F.1. Distributional Effects of Speed Limit Reductions Including the Policy Impact on Speeding Tickets



Notes: The figure shows the coefficients from an event study model of speeding tickets based on cameras located on the Marginais Highways. All values refer to average changes in the number of tickets per camera relative to the quarter immediately before the speed limit reductions. Error bars indicate the 95% interval with standard error clustered by month.

Figure F.2. Distributional Effects of Speed Limit Reductions Including the Policy Impact on Speeding Tickets



Notes: 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 excluding Saturdays and Sundays. 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. Ticket costs are assumed to be uniformly distributed accross all drivers. The government revenue from tickets is assumed to be distributed as an equal lump sum payment to all residents.

G 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,[65], 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.

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

Table G.1 summarizes the comparable results between the two studies.[66][67] 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 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 1.04. Using our preferred parameter estimates, this comparison would be 1.32 to 2.33.

[66]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.

[67]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.

Table G.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	1.04	2.21	0.47
Breakeven VSL ratio[a]	0.93	0.50	1.85
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	28	24	1.18
Accidents per million VHT	8.2	21.1	0.39
Fatalities per million VHT	0.45	0.50	0.90
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.