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CAN PUBLIC TRANSIT SUBSIDIES BE WARRANTED FOR IMPROVING AIR QUALITY AND HEALTH OUTCOMES? EVIDENCE FROM THE SAO PAULO METROPOLITAN AREA FOR A SECOND-BEST POLICY

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ABSTRACT

This research investigates the impacts of public transit on air pollution and health in the Sao Paulo Metropolitan Area. Subsidies for public transport systems are highly controversial and there is no consensus in the literature on their efficiency. One of the reasons advanced to support public transit subsidization refers to the negative externalities that automobile use generates, such as air pollution and its adverse health effects, assuming that these external costs are not fully internalized by automobile users through other policies. However, the empirical question remains as to whether increasing transit provision is welfare-enhancing in this respect, especially in megacities of developing countries. To be able to infer a causal relationship, we adopted a quasi-experimental approach using public transit strikes to identify exogenous variations in the outcomes of interest. The results suggest significant benefits for providing public transit, especially with regard to impacts on traffic-related air pollutants and respiratory illness in young children, with stronger effects for those residing near more congested arterial roads. It is expected that the findings will inform policy makers’ efforts to evaluate existing fare subsidies and potential pricing reforms, especially in times of fiscal stress.

Keywords: transit subsidies, transit strike, automobile externalities, air pollution, health

RESUMO

Esta pesquisa investiga os impactos do transporte público na poluição do ar e saúde na Região Metropolitana de São Paulo. Os subsídios para sistemas de transporte público são altamente controversos e não há consenso na literatura sobre sua eficiência. Um dos argumentos a favor ao subsídio do transporte público refere-se às externalidades negativas que o uso do automóvel gera, como a poluição do ar e seus efeitos adversos à saúde, assumindo que esses custos externos não são totalmente internalizados pelos usuários de automóveis por meio de outras políticas. No entanto, permanece uma questão empírica saber se a crescente oferta de transporte público é benéfica ao bem-estar neste aspecto, especialmente nas megacidades dos países em desenvolvimento. Para ser capaz de inferir uma relação causal, adotamos uma abordagem quase experimental usando greves do transporte público para identificar variações exógenas nos resultados de interesse. Os resultados sugerem benefícios significativos da oferta de transporte público, especialmente no que diz respeito aos impactos sobre os poluentes atmosféricos relacionados ao tráfego e doenças respiratórias em crianças pequenas, com efeitos mais fortes para aqueles que residem perto de vias arteriais mais congestionadas. Espera-se que as conclusões dessa pesquisa informem os formuladores de políticas em seus esforços de avalição dos subsídios ao transporte público existentes e potenciais reformas de preços, especialmente em tempos de estresse fiscal.

Palavras-chave: subsídios ao transporte público, greve de transporte público, externalidades de automóveis, poluição do ar, saúde

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

1. Introduction

Passenger fares for public transit are heavily subsidized in most cities in the developed world. The Sao Paulo Metropolitan Area (SPMA) in Brazil, however, is one of the few exceptions in Latin America to have its public transportation system partially subsidized. The two classic rationales for transit subsidies postulate that mass transit provision involves increasing returns to scale (MOHRING, 1972), and that they may induce substitution from automobile travel to transit ridership, thereby reducing external costs due to traffic congestion, accidents, and air pollution (PARRY; SMALL, 2009). The latter is a second-best argument, since it admits that automobile externalities cannot be fully internalized by superior road pricing policies. However, there is no consensus in the literature regarding the efficiency of transit subsidization, especially in respect to its potential to mitigate the external costs of automobile use.

A growing empirical literature – anchored in credible research designs enabling causal inference – aiming at investigating whether subsidies to public transit is justified on grounds of traffic congestion relief (ADLER; VAN OMMEREN, 2016; ANDERSON, 2014) and air quality benefits (CHEN; WHALLEY, 2012; GENDRON-CARRIER et al., 2018; GOEL; GUPTA, 2017; LALIVE; LUECHINGER; SCHMUTZLER, 2017) has provided support for transit subsidies as a second-best policy – even though there is also evidence for the counter-argument of induced demand, or the so-called "fundamental law of road congestion" (DOWNS, 1962, 1992), in which a latent demand for more automobile travel will most likely occupy freed road capacity brought about by automobile-transit substitution (BAUM-SNOW; KAHN, 2005; DURANTON; TURNER, 2011).

Insofar as transportation policies impact well-being through influencing travel patterns, and these, in turn, alter external costs of automobile travel, our goal is to contribute to this literature by providing further evidence of the impact of public transit on local air pollution and, assessing more directly, its adverse effects on the human health. Even though there is a broad consensus that traffic-related air pollution adversely impacts health (e.g., CHAY; GREENSTONE, 2003; CURRIE; NEIDELL, 2005; CURRIE; NEIDELL; SCHMIEDER, 2009), and many studies have focused on directly estimating the associated health costs of traffic congestion (e.g., CURRIE; WALKER, 2011; HE; GOUVEIA; SALVO, 2018; KNITTEL; MILLER; SANDERS, 2016; ZHONG; CAO; WANG, 2017), to the best of our knowledge, there has not been an empirical study of the effects of public transit on contemporaneous health outcomes in the environmental-health economics literature.

In fact, the epidemiology literature (e.g., DOMINICI et al., 2006, among many others) has long focused on the daily effects of short-term air pollution variations on morbidity and mortality outcomes. In contrast, within the health economics discipline, which complements public health literature by attempting to address issues regarding avoidance behavior, unobserved confounding factors, and measurement errors, there are only a few studies investigating the effects of short-run variations in air pollution exposure on daily hospital admissions and emergency room visits (MORETTI; NEIDELL, 2011; SCHLENKER; WALKER, 2016) – even though many others have looked at health outcomes over longer periods of time (e.g., CURRIE et al., 2014).

A core assumption to evaluate the effects of public transit supply on automobile externalities is about commuter willingness to substitute between private vehicle travel and transit ridership. Particularly in regard to air quality and health externalities, it also relies on the assumption of a net reduction in total air pollution, stemming from the automobile-to-transit emission substitutability rate (RIVERS; SABERIAN; SCHAUFELE, 2017). Any automobile externality mitigation by transit supply depends on both these elasticities of substitution. Therefore, the empirical analyses of transit impacts on air pollution test the behavioral and technological hypotheses jointly (LALIVE; LUECHINGER; SCHMUTZLER, 2017).

Nevertheless, the air pollution effect in the first stage is not easily translated to the reduced-form of transit impacts on health outcomes, even if air pollution levels are as good as randomly assigned, exposure to pollution may remain endogenous (ZIVIN; NEIDELL, 2009, 2013). Moreover, weather conditions directly affect health outcomes as well as air pollution concentration levels, making it difficult to disentangle the impact factors (DESCHÊNES; GREENSTONE, 2011; KNITTEL; MILLER; SANDERS, 2016). Lastly, measurement errors arise from the differences between monitored air pollution concentration levels and actual individual air pollution exposure, which stem from the mechanism of assigning air pollution to individuals (LLERAS-MUNEY, 2010).

Above all, a critical issue for transit impact evaluations is contrafactual identification, as transit infrastructure and service supply cannot, in general, be randomly assigned. Recognizing the challenges of implementing field experiments in the realm of urban phenomena (BAUM-SNOW; FERREIRA, 2014), quasi-experimental research designs should deliver more credible estimates of the parameters of interest than the more typical observational studies in this area (DOMINICI; GREENSTONE; SUNSTEIN, 2014). Of central importance, however, is the degree to which one can be confident that the causal variable of interest is independent of

confounding factors, in which there would be unobserved variables that both influence the outcome of interest and are correlated with the treatment variable. Even so, these empirical approaches are often not implemented without limitations, such as: the size of the study population is beyond the control of researchers; the empirical methods are data demanding, requiring sufficient variation in spatial and temporal dimensions; the number of air-quality monitors are often limited relative to the geographic distribution of the population; the assignment of air pollution concentrations to individuals requires strong assumptions about individual mobility and population spatial distribution; and the hospital admission and mortality rates are a lower bound of the full impact (CURRIE; HEEP; NEIDELL, 2011; ZIVIN; NEIDELL, 2013).

In this work, we exploit public transit strikes as a source of exogenous supply shock in public transit provision. This is not a new approach, as strikes in other sectors have been widely used to identify exogenous variations in air pollution and health outcomes, assuming that other factors remain constant (e.g., LAVAINE; NEIDELL, 2017; POPE, 1989; RANSOM; POPE, 1992, 1995). Transit strikes per se, not only in the economics literature, but also in transportation engineering and public health studies – despite the latter's use of a more before-and-after framework – have also been employed to assess the effects of transit on traffic congestion (ADLER; VAN OMMEREN, 2016; ANDERSON, 2014; LO; HALL, 2006) and on air quality (BASAGAÑA et al., 2018; CHALOULAKOU et al., 2005; MEINARDI et al., 2008; RIVERS; SABERIAN; SCHAUFELE, 2017), including health outcomes (SILVA et al., 2012).

There is evidence suggesting that transit strikes induce a significant fraction of public transit riders to switch to private vehicles (VAN EXEL; RIETVELD, 2001, 2009). Moreover, in the case of the SPMA, transit strikes have the effect of suspending the city driving restriction scheme (the so-called "*rodizio*") for the days in which the strikes take place, and as such, they have the potential to increase the car fleet on the streets during strike days by up to 20%. Nevertheless, the estimated impacts of transit strikes are not allusive about the actual elasticities of substitution between modals. That is, the transit strike strategy allows credible recovery of causal public transit impacts on automobile externalities only in the short run, and based on a halt of transit service provision. Thus, it is not informative regarding long-run effects or marginal changes of transit supply.

Our two main databases are hourly ambient air pollution concentration measures (CETESB) of the pollutants carbon monoxide (CO), nitrogen oxides (NOx), nitrogen dioxide (NO_2),

particulate matter with a diameter between 2.5 and 10 micrometers (PM10), ozone (O_3), and sulfur dioxide (SO2), and daily hospital admission counts (DATASUS) due to respiratory and circulatory system diseases from 1998 to 2017, for the Sao Paulo Metropolitan Area and four other metropolitan areas in the state of Sao Paulo. For the entire period of study, we self-collected information on 21 public transit strikes, totaling 34 transit strike days. We also obtained weather data for controls.

We first present an event study analysis – in the spirit of recent applied microeconomics research (ALMOND; HOYNES; SCHANZENBACH, 2011; FUEST; PEICHL; SIEGLOCH, 2018; HOYNES; MILLER; SIMON, 2015; HOYNES; SCHANZENBACH; ALMOND, 2016; MACHADO; SZERMAN, 2018) – in order to visually inspect the dynamics of the outcomes of interest around transit strike episodes. We then move on to actually evaluate the effects of transit strikes on air pollution and health outcomes by employing a difference-in-differences approach, similar to BAUERNSCHUSTER; HENER; RAINER (2017) and RIVERS; SABERIAN; SCHAUFELE (2017), leveraging on the large dataset comprised of hourly air pollutant concentration measurements and daily hospital admission counts in more than one metropolitan area across multiple years. We compare outcomes in affected and non-affected metropolitan areas on strike and non-strike days in order to control for unobserved, but fixed omitted variables, in time and space dimensions. That is, in the absence of treatment, the outcomes of interest are determined by the sum of a time-invariant location effect and a time effect that is common across locations (ANGRIST; PISCHKE, 2009) – but we also flexibly interacted location with time fixed effects and included a vector of location- and time-varying weather covariates.

Additionally, to more precisely estimate the population dose-response function to variations in air pollution exposure, we accounted for potential spatially heterogeneous effects of public transit supply and, thus, allowed the marginal effect of transit strikes to differ based on the relative distances of cardio-respiratory inpatient places of residence to more congested roads. Therefore, we show how increases in traffic-related air pollution, due to public transit stoppages, disproportionately affect people closer to tailpipe emission sources than residents further away, in an empirical strategy similar to that adopted by HE; GOUVEIA; SALVO (2018), and SCHLENKER; WALKER (2016).

We interpreted the estimated coefficients as the effects of exogenous variation in public transit supply on the SPMA air quality and on the health of its residents. We found statistically significant and positive effects on major traffic-related atmospheric pollutant concentration

levels. Carbon monoxide (CO) concentrations increased by 8.8 percent, and both nitrogen oxide (NOx) and nitrogen dioxide (NO_2) concentrations increased by 13.2 and 8.9 percent respectively, on average, during the hours of transit strike days in the SPMA. As expected, metro strikes had a higher impact on air pollution than bus strikes. Our findings of the effects of public transit on urban air pollution are in line with those suggested in previous studies in the economic literature (BAUERNSCHUSTER; HENER; RAINER, 2017; CHEN; WHALLEY, 2012; GOEL; GUPTA, 2017; LALIVE; LUECHINGER; SCHMUTZLER, 2017). Additionally, as a robustness check, falsification tests using fake transit strikes as treatment yielded statistically insignificant estimates for the main traffic-related pollutants (CO, NOx and NO_2). However, different from what is customary in this literature, sulfur dioxide (SO_2) concentrations could be taken as a placebo (BAUERNSCHUSTER; HENER; RAINER, 2017; CHEN; WHALLEY, 2012; LALIVE; LUECHINGER; SCHMUTZLER, 2017), given much higher levels of sulfur content in the running diesel of heavy-duty vehicles in the SPMA. Additionally, with the difference-in-differences model we did not find evidence of a positive effect of public transit on cardio-respiratory daily hospital admission rates.

In contrast, with the panel data restricted to city of Sao Paulo observations and taking the distance between inpatient zip code of residence centroids and the nearest point on congested inner-city roads (proxied by key inner-city arterial roads, which arguably, have the greater probability of presenting the highest upsurge in traffic volume during transit strike days), we found that transit strikes did increase hospital admission rates due to respiratory illness, with a greater effect on young children on the day following the end of transit strike episodes. Furthermore, the estimated coefficients suggested that there is indeed spatial heterogeneity in the marginal effects of transit strikes, as indicated by the negative and statistically significant coefficients on the interaction of transit strikes with the shortest distance of zip code centroids to inner-city arterial roads. In particular, respiratory-related hospitalization rates for all ages increased on average by 2.2 percent on transit strikes days with effects attenuating at an increasing distance from inner-city arterial roads. Children under five years of age were most affected, with respiratory hospital admission rates for this subgroup increasing by 5 percent on the day following the end of the transit strike occurrences. Moreover, children living closer to arterial roads were more affected than those residing in more distant locations.

We accounted for spatial and serial correlation concerns on statistical inference (ABADIE et al., 2017; BERTRAND; DUFLO; MULLAINATHAN, 2004; COLIN CAMERON; GELBACH; MILLER, 2011) by alternatively clustering standard errors by location and time

dimensions and, hence, allowing for hospital admission observations across neighborhoods to be serially correlated within a day and hospital admission observations within neighborhoods to be spatially correlated over time. The main result on young children hospitalization rates due to respiratory illness, lineally declining with distance, is statistically significant across all model specifications and standard errors clustering levels. Furthermore, in the interaction distance model, both placebo health outcome – hospital admissions for non-traffic related injuries of external causes – and placebo strikes are all statistically insignificant.

These results are relevant in the context of a major metropolitan area in the developed world. The Sao Paulo Metropolitan Area (SPMA)[1] is one of the largest urban agglomerations in the world, with around 21 million inhabitants. According to the Origin-Destination (O-D) Survey (METRO, 2007), on an average weekday, there are around 38 million trips within the SPMA, of which 25 million happen within the city of Sao Paulo alone. Of the daily trips, 31% are made in private vehicles, 37% by public transportation, and 32% are non-motorized. In contrast, 44.6% daily commuting trips are completed by public transportation, but only 20% of them include metro rides, even though the subway system carries around 4 million passengers a day. The municipal bus system, on the other hand, numbers 15,000 vehicles and is the main transit mode. The city is dominated by a vehicular fleet of 7.5 million units (12 million in the SPMA), of which 70% are private vehicles. For equity considerations, spatial mismatch is greater for low-income residents, overwhelmingly concentrated in the peripheral areas. Higher income levels tend to commute more by private vehicles, with an average travel time of 31 minutes, whereas the greater percentage of the population using transit is in the income bracket of 2–5 minimum wages with an average travel time of 67 minutes. The wealthier travel 2.5 times more than the poorest 20%, highlighting mobility inequalities within the city, which result in severe disparities in accessing job opportunities and essential public services.

In addition to this introductory chapter, the remainder of this dissertation is organized as follows. Chapter 2 reviews the literature, mainly in the economic fields, on transit subsidy efficiency, on the linkages between transit, air pollution, and health, and on other transportation policies. Chapter 3 provides institutional background on SPMA urban mobility and travel patterns, emission and air pollution standards and driving restriction policy. Chapter 4 outlines the core assumptions within the theoretical framework and intuitions for interpreting the

[1] In this dissertation, we use, to some extent, the reference to the Sao Paulo Metropolitan Area and to the city of Sao Paulo interchangeably, in the sense that in many circumstances our datasets are geographically constrained to the city of Sao Paulo, but, given the interconnections within the metropolitan area, it is relatively safe to assume that the effects found for the city of Sao Paulo can, to some degree, be extended to the entire metropolitan area.

empirical findings later. Chapter 5 presents the empirical approach, first describing the identification strategy, the data, and the econometric methods, and then, the findings of this research. Lastly, Chapter 6 carries final discussions and conclusions.

2. Literature Review

This work relates to different strands of the economic literature. It draws from urban, transport, environmental and health economics. The references are predominantly empirical studies that seek to adopt credible research designs able to recover causal relationships between transit, air pollution and health outcomes. The transmission mechanisms by which transportation policies impact well-being start with the policy instruments influencing transportation patterns. The changes in travel patterns affect pollutant emissions, that, in turn, alter air pollution levels, which adversely affects human health. These final variations in the quality of services provided by the environment (i.e. air quality) correspond to the welfare effects of interest in the economic analysis. The economic literature investigates how transportation instruments affects behavior and evaluates the welfare losses from air pollution-related health effects, leaving many of the (technological and biological) mechanism issues to engineering and public health scholars. Our paper aims to contribute to the existing empirical literature in economics based primarily on quasi-experimental research designs, providing evidence of public transit benefits to mitigate the environment-health externalities of automobile use.

2.1 Transit subsidy efficiency

There is no consensus in the literature regarding the efficiency of transit subsidization. There are compelling economic arguments on both sides, as summarized by BASSO; SILVA (2014) and PARRY; SMALL (2009)[2] in the rationales reproduced below. Nevertheless, the empirical literature on optimal transit fare pricing has delivered conflicting results, particularly in respect to its second-best argument, as a response to relieve automobile externalities (ADLER; VAN OMMEREN, 2016; ANDERSON, 2014; BAUM-SNOW; KAHN, 2005; DURANTON; TURNER, 2011).

On the one hand, there is the economy of scale justification arising from fixed costs and, more importantly, the so-called “Mohring effect” (MOHRING, 1972), in which increased service frequency or route density induces a decline in access costs or wait times for all users. The

[2] As noted by the authors, the arguments could also be used to assess the desirability of transit infrastructure improvements. However, transit investments are mostly beyond the scope covered by the literature on transit fare pricing, which has a particular focus on transit subsidization, given existing infrastructure and underlying sunk capital costs.

costs of transit operators also display economies of scale, as higher passenger density allows vehicles to be operated with higher occupancy. Furthermore, there is a second-best argument in favor of lowering transit fares, based on the substitution of automobile use by transit ridership. By reducing the price of transit, by fare subsidization, the unpriced negative externalities of automobile travel (i.e. traffic congestion, air pollution, and road accidents) would be reduced. However, this argument assumes that these external costs cannot be fully internalized by charging these automobile externalities more directly through appropriate road pricing. Finally, there is also the equity argument, which generally plays a significant role in political decisions regarding transit subsidization across different transit modes. For distributional considerations, it might be optimal to increase subsidies for high-density bus service, which is heavily utilized by the lower-income population, and to reduce it for rail service, which often benefits higher-income riders and home owners living near rail stations.

On the other hand, there is plenty of evidence of the rather low cross price-elasticity between private vehicle use and transit price[3]. Moreover, transit subsidies are financed by distortionary taxes, such as income taxes, causing efficiency losses (e.g. by discouraging work effort; though there is a counteracting effect, as lower transportation costs are deflationary to real prices, raising real wages and, thus, inducing work effort). Furthermore, public funds have a non-negligible cost. Therefore, the public funds necessary to cover transit subsidies may produce welfare losses. Finally, transit subsidies may potentially deliver rather soft incentives for cost minimization by transit operators in monopoly settings. However, this is not a caveat of the transit subsidy per se, but of the incentive structure provided by the transit management system in place, which could be overcome, for example, by user-side subsidy schemes.

Deciding what is the optimal transit subsidy level, based on these economic rationales, is problematic due to several factors. First, the power of each argument might substantially vary according to peak and off-peak hours, transit mode, location, vehicle size, frequency and design. Second, some transit modes generate themselves the same motor vehicle externalities (i.e. traffic congestion, air pollution, and road accidents). Third, transit riders also impose external costs on each other by crowding. Fourth, automobile travel externalities are at least partly internalized by fuel taxes. Fifth, it depends on how transit agencies respond to increases in passenger demand at the margin. Lastly, transit subsidies might operate alongside with other policies, such as congestion pricing or dedicated bus lanes, with complementary effects.

[3] Particularly in the case of Sao Paulo, JACOB; BIDERMAN (2017) suggested that bus demand is inelastic with respect to price both in the short and long terms, corroborating previous findings.

There is a growing empirical literature aiming at evaluating whether subsidies to public transit is justified by their ability to mitigate auto travel externalities, predominantly by their congestion relief benefits alone. ANDERSON (2014) exploited a sharp discontinuity in transit provision, brought about by a 35-day bus and rail strike of transit workers in the Los Angeles Metropolitan Area. Using hourly traffic speed data, he found a significant congestion relief benefit, with average driving delays increasing by 47% (0.19 minutes per mile) on L.A. freeways during peak periods when transit service provision ceases. One important assumption in his estimates was that driving delays were not homogeneous across roads within the same metropolitan area and across different hours of the day. Thus, he was able to demonstrate that public transit has a much larger impact on alleviating traffic congestion than predicted by models that do not incorporate these heterogeneities in driving delays. Along these lines, ADLER; VAN OMMEREN (2016) used multiple transit strikes that took place in Rotterdam to conclude that the congestion-relief benefit of public transit can be substantial even in a mildly congested city. They especially showed that the effect of transit service supply is larger for inner-city roads than for freeway ring roads.

By contrast, DURANTON; TURNER (2011), mainly to test a variation in the "fundamental law of road congestion"[4], found no evidence that public transit provision affects auto travel volume (measured by vehicle-kilometers traveled) in several metropolitan areas of the U.S. They used data from daily average peak service of large buses and public transit supply instrumented with metropolitan areas' share of Democratic vote in the previous presidential election (which, in the political agenda of the time, indicated support for transit subsidization). Consistent with the theory of induced demand, BAUM-SNOW; KAHN (2005), investigating the effects of rail improvements on the share of transit ridership in large and dense U.S. cities, also found no reduction in congestion levels, as increased rail supply generally lead to commuters substituting bus for rail ridership, with insignificant effects on auto travel. The authors employed a difference-in-differences design using variation in rail service accessibility over time within census tracts. Thus, they were able to demonstrate heterogeneous responses of commuting mode choice, which mainly depended on metropolitan area structure (e.g. population density), both within and between metropolitan areas.

[4] The "fundamental law of road congestion" states that changes in transit accessibility may induce some substitution from car driving to transit ridership, however a latent demand for more car travels will soon occupy the freed road capacity (DOWNS, 1962, 1992).

2.2 Transit, air pollution and health

There are several empirical studies focusing on the relationship between auto travel and air pollution, and, most recently, looking at the linkage between congestion and different health outcomes, interested in directly estimating the associated health costs of traffic congestion (e.g., CURRIE; WALKER, 2011; HE; GOUVEIA; SALVO, 2018; KNITTEL; MILLER; SANDERS, 2016; ZHONG; CAO; WANG, 2017). Supporting this literature, there is a broad consensus that traffic-related air pollution adversely impacts human health (e.g., CHAY; GREENSTONE, 2003; CURRIE; NEIDELL, 2005; CURRIE; NEIDELL; SCHMIEDER, 2009). There is also an extensive recognition of its effects on other economic outcomes of interest, such as educational (e.g., BHARADWAJ et al., 2017; CURRIE et al., 2009; SANDERS, 2012) and labor outcomes (e.g., HANNA; OLIVA, 2015; ISEN; ROSSIN-SLATER; WALKER, 2017; ZIVIN; NEIDELL, 2012).

Nonetheless, there are only a few empirical studies assessing the causal impact of transit supply on air pollution, and, to the best of our knowledge, there has not been an empirical study on the effects of public transit on contemporaneous health outcomes in the environmental-health economics literature[5,6]. Most of these studies exploit sharp discontinuities in ridership on rail transit infrastructures' opening days. CHEN; WHALLEY (2012) used the inauguration date of a new urban rail system in Taipei to identify the air pollution effects of transit infrastructure investments. Their findings suggested a reduction between 5 and 15% of carbon monoxide (CO) ambient air concentrations, and a less precise decline in nitrogen oxides (NOx) levels. In the same fashion, GOEL; GUPTA (2017) investigated the impact of New Delhi's subway extension lines on monitored CO and NO_2 concentrations. The results also gave an indication of the benefits of mass transit for air pollution abatements. GENDRON-CARRIER et al. (2018) looked at 43 subway system expansions around the world and using remote sensor air pollution images, point to a 4% drop in airborne particulate. Using a different research design, LALIVE; LUECHINGER; SCHMUTZLER (2017) showed that higher frequency rail service

[5] In the BAUERNSCHUSTER; HENER; RAINER (2017)'s analysis, they estimated the impact of transit strikes on hospital admissions (interested in the impact of transit strikes themselves) indirectly given evidence for the effect of transit service provision on health outcomes. LALIVE; LUECHINGER; SCHMUTZLER (2017) estimated the effects of improvements in rail service frequency on yearly infant mortality, but did not include any contemporaneus morbidity outcome.

[6] In the public health literature, there are many studies looking at the correlation between the variations in air pollution levels and traffic volumes due to public transit strikes (e.g., CHALOULAKOU et al., 2005; MEINARDI et al., 2008; BASAGAÑA et al., 2018). Moreover, SILVA et al. (2012) exploited two single-day subway strikes in Sao Paulo aiming at evaluating the air quality and health benefits of the city subway system. These studies will be presented in more detail in Chapter 5, when we discuss similar identification strategies adopted in both literatures.

(instrumented with procurement mode) led to lower concentrations of CO and NOx across Germany. By contrast, RIVERS; SABERIAN; SCHAUFELE (2017), in focusing on the existing North American bus-dominated transit system (which typically runs on diesel, while subways are powered by electricity), found that transit work stoppages led to reductions in NOx ambient air levels (due to a decrease in bus emissions from diesel fuel), and had a meaningless effect on CO and $PM_{2.5}$ concentrations.

Among the studies concerned with the impact of ambient air pollution on human health, there is already a large epidemiology literature (e.g., DOMINICI et al., 2006) focusing on the daily effects of short-term air pollution variations on morbidity and mortality outcomes in the overall population. By contrast, research in health economics, which complements the public health literature[7] by addressing issues regarding avoidance behavior, unobserved confounding factors, and measurement errors, have focused much more on infant health over longer periods of time (e.g., CURRIE et al., 2014). In fact, only a few studies in economics aim at investigating the effects of short-run variations in pollution exposure explicitly recognizing the often non-random assignment of air pollution levels to individuals' health (e.g., MORETTI; NEIDELL, 2011; SCHLENKER; WALKER, 2016).

Instrumenting with airport runway congestion driven by network delays that began in large airports elsewhere, SCHLENKER; WALKER (2016) sought to identify how short run, daily variations in air pollution affected health outcomes of different population subgroups. They looked at daily hospital admissions and emergency room visits of inpatients living near California airports over a two-year period and found significant effects on respiratory and circulatory hospitalization counts. Even though the study confirmed that infants and the elderly are more sensitive to air pollution fluctuations in relative terms, it demonstrated that, for respiratory diseases, the adult population accounted for the largest share of total impact. Likewise, MORETTI; NEIDELL (2011) focused on the effects of short-term ozone variation on Los Angeles hospital admissions for seven years in the months from April-October. They instrumented air pollution variation with daily boat traffic at the port of Los Angeles and found significantly large estimates of the effect of ozone levels on hospital admissions related to respiratory diseases.

[7] For references for state-of-the-art epidemiological models, see, e.g., BELL; SAMET; DOMINICI (2004); DOMINICI et al. (2008); DOMINICI; MCDERMOTT (2002); DOMINICI; SAMET; ZEGER (2000); PENG; DOMINICI; LOUIS (2006); SACKS et al. (2012).

Central to these research designs is the acknowledgement that even if air pollution levels are as good as randomly assigned, exposure to pollution might remain endogenous (ZIVIN; NEIDELL, 2009, 2013). This is because optimizing agents may compensate for rises in pollution by cutting exposure – i.e. more vulnerable individuals can reduce short-term increased pollution exposure by staying indoors (NEIDELL, 2007). Another source of bias in the estimates stems from environmental confounders, as weather conditions directly affect health outcomes as well as air pollution concentration levels. Although in principle they can be controlled for, it is difficult to fully control for all related factors with the correct functional form (KNITTEL; MILLER; SANDERS, 2016). Lastly, measurement errors arise from the differences between monitored air pollution concentrations and actual individual pollution exposure levels. The common approach to assign data from air pollution monitors to nearby residential locations is unlikely to overcome this problem (LLERAS-MUNEY, 2010).

2.3 Other transportation policies

Insofar as air pollution and its well-known adverse health effects are among the main automobile externalities, transit subsidies are only a second-best response to them (PARRY; SMALL, 2009). One should expect much larger welfare gains from pricing these externalities more directly (PARRY; SMALL, 2005). On an entirely different scale, national policies affecting these auto externalities include fuel taxes, emission standards, fuel economy and alternative fuel policies. At the local level, there are superior pricing policies, such as congestion tolls, to be exploited.

Nevertheless, as noted by PARRY; WALLS; HARRINGTON (2007), more recent studies, with better controlling for fuel economy, regulations, and fleet age, have shown that the behavioral response to fuel taxes is less elastic than previously thought. Moreover, there are several obstacles to raising fuel taxes. On equity grounds, most studies find that fuel taxes are regressive. Politically, collective action from powerful interest groups (e.g., auto manufacturers and oil companies) might prevail. In respect to externality mitigation actions, fuel tax revenues might not be used for environmental or other socially desirable spending, such as R&D for cleaner or alternative fuels.

In fact, many countries, moving in the opposite direction, continue to subsidize fuel. However, there has been much more pressure for reform recently, as current lower crude oil prices are reducing government revenues in oil producing countries, which largely subsidize fuel consumption. Arguably, fuel subsidy reforms would not only balance government budgets, but

would also contribute to the alleviation of auto externalities. For instance, DAVIS (2017) quantified substantial external environmental costs of global fuel subsidies on carbon dioxide emissions and local pollutants, besides the external costs of traffic congestion and accidents.

Moreover, fuel economy standards are subject to the so-called rebound effect[8], by which improved fuel efficiency causes additional motor vehicle travel (as fuel becomes cheaper to consume), partially offsetting the intended energy saving[9]. Additionally, emission standards and fuel content regulations might impose substantial costs on consumers and, in some settings, were found to have a limited effect on air pollution (e.g, AUFFHAMMER; KELLOGG, 2011)[10]. Furthermore, PIERCE; SNYDER (2012) discussed the flaws in regulatory enforcement of such policies by emission inspection.

Government investments, through carbon taxes and research subsidies, towards a transition to clean technology, are also recognized as important policies in reducing fossil fuel emissions. ACEMOGLU et al. (2016) particularly suggested that, even though the carbon tax policy has been intuitively advanced as more appropriate for directly mitigating the carbon externality, research subsidies might be the optimal policy. Their quantitative model demonstrated that relying only on carbon taxes or delaying intervention has significant welfare costs.

As a complement to national instruments, local transportation policy has a role to play, but their efficiency to mitigate automobile externalities need to be assessed in local contexts (SCHMUTZLER, 2011). Congestion pricing at peak-periods has been supported above other local policies, such as transit subsidies, to alleviate traffic congestion, as it directly tackles the congestion externality, optimally redistributing travel to other transportation modals and to off-peak hours. However, in practice, the welfare effect of the policy can be rather different from those predicted by a first-best analysis (LIPSEY; LANCASTER, 1956), which ignored

[8] SMALL; VAN DENDER (2007) estimated the rebound effect for passenger-vehicle use at the U.S. state level and, over the period 1966-2001, at 4.5% for the short run and 22.2% for the long run. They also estimated the long-run price-elasticity of fuel demand at -0.43, and the proportion of which is caused by mileage changes at 52%.

[9] See GREENSTONE; SUNSTEIN; ORI (2017) for suggestion of a more efficient policy (i.e. the controversial cap-and-trade system) to reduce fuel consumption.

[10] AUFFHAMMER; KELLOGG (2011) demonstrated how some types of emission regulation are more effective than others, and in which some locations are more likely to benefit. They found that the effectiveness of federal gasoline standards varies considerably with the flexibility with which refiners are permitted to respond. When refiners are allowed to choose the compliance mechanism, the regulation does not lower air pollution. An interesting case, however, is California, where precisely targeted, inflexible regulations significantly improved air quality.

spillovers and unpriced externalities or other preexisting distortions within the transportation system (PARRY; BENTO, 2002)[11].

There is, nevertheless, already some robust evidence[12] of the impacts of road pricing policy on traffic congestion, air quality, and health. PERCOCO (2013, 2014), investigating the introduction and later 50-day suspension due to a court injunction of the Milan's road pricing scheme, found that the tax shifted users away from more polluting vehicles, but the air quality benefits of the policy were limited as there was a substantial increase in motorbike usage. By contrast, GIBSON; CARNOVALE (2015), evaluating exactly the same quasi-natural experiment, demonstrated intertemporal and spatial substitution effects toward unpriced times and roads. But net of these responses, they shown that Milan's road pricing policy significantly decreased air pollution, generating large welfare gains. SIMEONOVA et al. (2018) carried out the first study to investigate congestion pricing effects on health outcomes. Exploring the implementation of the congestion tax in Stockholm, they revealed that a reduction in ambient air pollution was associated with a significant reduction in the rate of acute asthma among young children[13]. However, the variations in health outcomes were more gradual than the variations in air pollution levels, suggesting that short-run estimates of pollution abatements may understate the long-run health effects of policies.

Nonetheless, for political reasons, it has been very difficult for traffic authorities to implement road pricing across congested and polluted cities around the world. DE BORGER; PROOST (2012) noted that in the few cases of successful introduction of congested pricing, revenues were secured towards subsides for transit or to financing transit service improvements, increasing public acceptance for the policy. BASSO; SILVA (2014) also suggested that there is sizable efficiency substitutability between congestion pricing and transit subsidization. In particular, in the absence of other policies, transit subsidies may have the potential for reducing auto negative externalities, at least if the marginal cost of public funds and induced cost inefficiencies are not very large. However, if road pricing, and also dedicated bus lanes, are implemented first, the marginal contribution of increased transit subsidies diminishes

[11] PARRY; BENTO (2002) shed some light on first-best congestion taxes in the presence of these distortions (i.e. reallocation of traffic to competing routes, indirect effects on tailpipe emissions of certain air pollutants, exacerbation of fuel tax distortions etc.). Multiple unpriced externalities are particularly prevalent in the transportation system, in which unpriced congestion and air pollution interact across congested locations and peak-hours. A key insight, however, is that revenue-raising instruments, such as congestion tolls, can mitigate some of the welfare losses associated with these interaction effects.
[12] See TSEKERIS; VOS (2009) for an earlier review of the literature.
[13] It is worthy to note that SIMEONOVA et al. (2018) were able to identify adverse health effects of air pollution in a context of already, even by developed country standards, low air pollutant concentration levels.

substantially. In fact, they argue that dedicated bus lanes should be preferred, as they may increase frequencies and decrease transit fares without the need of extra public funds.

Finally, congestion pricing is also seen as more efficient than driving restriction schemes, as the latter do not account for differing auto travel types and they tend to lose effectiveness over time. DAVIS (2008), employing a regression discontinuity design, was one of the first attempts to provide empirical evidence on the effects of license-plate-based driving restrictions ("*Hoy No Circula*") introduced in Mexico in 1989. The study demonstrated that the driving restriction policy led to a surge in the total number of vehicles and a change in fleet composition towards high-emission vehicles. Moreover, it did not find a decrease in gasoline sales or an increase in transit ridership. As a result, there was no improvements in air quality across pollutants and periods of the week. GALLEGO; MONTERO; SALAS (2013) additionally examined how policy responses vary among income groups and how fast households adjust their stock of vehicles in the short- and long-run. GUERRA; MILLARD-BALL (2017), in turn, complementing the analysis using a household travel survey, rejected the dominant hypothesis that the policy fails to reduce overall vehicle travel due to perverse incentives for increasing car ownership. There are a range of other, less costly ways that people adjust behavior to avoid the driving restriction (e.g., shuffling travel periods or days etc.). Furthermore, ZHANG; LIN LAWELL; UMANSKAYA (2017), investigating the introduction and subsequent changes of driving restrictions ("*Pico y Placa*") in Colombia, found that the driving restriction scheme might have distinctive effects on different air pollutants, reflecting fleet composition changes induced by the policy. However, even if there is no consensus about the exact behavior mechanism, driving restriction policies, adopted mainly by Latin American cities[14], have not achieved its objective of reducing congestion and air pollution by inducing transit ridership in substitution for automobile usage.

3. Background

In most large cities in the developed world, the public transit system is heavily subsidized[15]; much different from what is found in developing countries. Nevertheless, the city of Sao Paulo is one of the few exceptions in Latin America, currently subsiding its bus system at around 32% of operational costs (JACOB; BIDERMAN, 2017). In more recent years, however, transit

[14] VIARD; FU (2015) studied Beijing's driving restriction scheme and found that it reduced air pollution and labor supply.
[15] Transit subsidies are, on average, around 70% of operational costs in the largest 20 cities in the United States and similar figures are found in other cities of the developed world (PARRY; SMALL, 2009).

subsidies have become a politically sensitive issue in the city as operational costs have soared and there are strong social pressures to maintain transit fares stagnated at real prices. Fare subsidies increased threefold in the last ten years, reaching almost one billion dollars in 2016, representing nearly 7% of the total city budget (JACOB; BIDERMAN, 2017). Therefore, in this context, the question of whether transit subsidies can deliver additional welfare benefits is becoming even more relevant and demands further empirical investigation.

3.1 SPMA urban mobility and travel patterns

The Sao Paulo Metropolitan Area (SPMA) is one of the largest urban agglomerations in the world, with around 21 million inhabitants, corresponding to 10% of the total population of Brazil. It is composed of 39 municipalities, of which the city of Sao Paulo is the main core metropolitan totaling 9.9 million people in 1998 and 12.1 million in 2017. It covers an area of 7947 km^2 and has a demographic density of 2653.9 inhabitants km^2 with a radial road structure and a marked urban sprawl pattern.

A comprehensive characterization of the travel patterns in the SPMA is provided by the Origin-Destination (O-D) Survey of the SPMA[16], which has been carried out every ten years since 1967 by the Metropolitan Company of Sao Paulo (Metro). According to the findings of the last O-D Survey in 2007, on an average weekday, there were around 38 million trips within the SPMA, of which 25 million were in the city of Sao Paulo alone – 31% of daily trips were made in private vehicles[17], 37% in collective transportation[18], and 32% were non-motorized[19] trips. Between 1997–2007, the total number of daily trips increased by 19%, but closely maintained the same modal share distribution.

The broadly-defined central business district (CBD) of the SPMA concentrates the larger proportion of jobs, even though some employment decentralization is observed in the metropolitan area. Households are predominantly located in the surroundings of the main center and population density decreases towards the boundaries of the territory. Spatial mismatch is greater for low-income residents, overwhelmingly concentrated in the peripheral areas. Therefore, the majority of commuters flow from the peripheries to the extended-CBD.

[16] See the O-D Survey at http://www.metro.sp.gov.br/pesquisa-od/resultado-das-pesquisas.aspx. There are many studies which summarize the survey's main findings and we did not repeat this exercise here. We, instead, drew on HADDAD et al. (2015, 2017) and JACOB; BIDERMAN (2017)'s analysis of the O-D Survey as the main references in this section.
[17] Mainly automobiles as drivers or passengers with just a small percentage on motorcycles.
[18] Mainly buses and subway, but also including metropolitan trains, school and chartered buses, among others.
[19] Mainly walking.

There were, on average, 15.7 million daily commuting trips in the SPMA in 2007, of which 44.6% were completed by public transportation, but only 20% of them included subway rides (7.6% of total daily trips). The subway network is restricted to a length of 46.2 miles (five lines, totaling 64 stations) within the limits of the city of Sao Paulo, even though it carries 4 million passengers a day, making it one of the most productive in the world in terms of passengers per kilometer. The subway system is interlinked with the suburban rail system, which is responsible for only 1.3% of daily trips in the SPMA. The municipal bus system, on the other hand, numbers 15,000 vehicles and is the main transit mode (24.2% of total daily trips) in the metropolitan area. Nevertheless, even though there have been recent investments to expand the bus corridor system, and also the bikeways, the city is dominated by a vehicular fleet of 7.5 million units (12 million in the SPMA), of which 70% are private vehicles, and a road network of 10,500 miles.

The commuting patterns are largely defined by income strata. Higher income levels tend to commute more by private vehicles, with an average travel time of 31 minutes. The lower the income, the higher the transit and non-motorized modal shares, but the greater percentage of the population using transit is in the income bracket of 2–5 minimum wages, as the very poor (1 minimum wage or below) mainly walk to work. The average travel time by public transit is very high, around 67 minutes, as the low-income population resides in the peripheral areas and average commuting time increases as the distance to the CBD rises. Additionally, the wealthier travel 2.5 times more than the poorest 20%, highlighting the mobility inequalities within the city, which result in severe disparities in accessing job opportunities and essential public services.

3.2 SPMA emission and air pollution standards

The vehicular fleet of the SPMA is the main emission source of air pollution, as in the 1980s and early 1990s many polluting industries moved their operations to other sites with less stringent regulations. Mobile sources in the SPMA currently account for 97.5% of carbon monoxide (CO) emissions, 79% of hydrocarbons (HC) emissions, 67.5% of nitrogen oxides (NO_x) emissions, 22% of sulfur oxides (SO_x) emissions and 40% of particulate matter (PM_{10}) emissions in the atmosphere (CETESB, [s.d.]). Despite substantial decreases in air pollutant concentrations in the last 30 years, average air pollution levels in the SPMA still do not comply

with World Health Organization (WHO) guidelines[20]. Furthermore, the greatest current challenge is to control secondary pollutants, such as ozone (O_3) and fine particulate ($PM_{2.5}$)[21,22], for which no reductions were observed in the same period (ANDRADE et al., 2017).

The air quality in the SPMA has been monitored by the State of Sao Paulo Environment Agency (CETESB), which, for most years, followed national air quality standards set in 1990 by the Brazilian National Council for the Environment (CONAMA), covering seven main air pollutants. Since then, national standards have not been updated in line with international guidelines and with more recent empirical evidence of the adverse effects of air pollution on the human health[23]. However, the state of Sao Paulo decided to revised the standards in 2013 to include limits for fine particles ($PM_{2.5}$) and to adopt more restrictive values for other pollutants[24]. These standards are still relatively relaxed compared with other metropolitan areas of the world, both for developed (e.g., London) and developing countries (e.g. New Delhi) (KUMAR et al., 2016). Moreover, the air pollution standards currently adopted by the State of Sao Paulo are only focused on the first interim targets recommended by the WHO and two more intermediate targets must be met before state standards meet WHO reference levels.

Motorized vehicles in the SPMA use a variety of fuels, including gasohol[25] (20–25% ethanol and 80–75% gasoline), hydrated ethanol, compressed natural gas (CNG), and diesel. The total fleet is composed of approximately 85% light-duty vehicles (LDVs) running mainly on gasohol or hydrated ethanol, 12% motorcycles running on gasohol, and 3% heavy-duty vehicles (HDVs) running on diesel (CETESB, [s.d.]). LDVs account for about 90% of CO emissions and HDV are responsible for nearly 50% of NOx emissions. Of all the LDVs, 55% are gasohol-driven, 38% are flex-fuel vehicles, 4% ethanol and 2% diesel (CETESB, [s.d.]). All diesel fuel (mainly for buses and trucks) is blended with 8% biodiesel (from soybeans). Thus, the vehicle

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[20] For instance, WHO standards for PM10 is 20 µg/m3. However, the average PM10 concentration in São Paulo has been above the national legislation level of 50µg/m3 in the late 90s and around 40 µg/m3 since the late 2000s (SILVA et al., 2012).

[21] Particulate matter can be both released directly from vehicle engines as well as formed through chemical reactions in the atmosphere from primary pollutants.

[22] Note, however, that fine particles with a diameter less than or equal to 2.5 mm ($PM_{2.5}$), unlike other pollutants, has been monitored in the SPMA only since 2005 (ANDRADE et al., 2017).

[23] The association between high air pollution levels and health outcomes in Brazil, in general, and in the SPMA, in particular, has long been demonstrated in several epidemiological studies, corroborating extensive international literature. See a brief literature review section at SILVA et al. (2012).

[24] CETESB's Air Quality Annual Reports – http://cetesb.sp.gov.br/ar/publicacoes-relatorios/ – contain all air pollution standards applied to the SPMA.

[25] The National Ethanol Program, known as "Proalcool", started in 1975 to stimulate the use of ethanol as fuel, mandating the mixture of ethanol and gasoline, creating gasohol, which led to reductions in carbon monoxide (CO) emissions by the vehicular fleet.

fleet operating exclusively on biofuel blends[26] makes the SPMA a unique biofuel-driven metropolis to be studied (KUMAR et al., 2016).

Emission standards for the SPMA vehicular fleet is determined by the National Program for the Control of Air Pollution Emissions by Motor Vehicles (PROCONVE), and were established in 1983, and since then have been implemented in phases, aiming at reducing air pollutant emissions, promoting large-scale use of biofuels, and decreasing the sulfur content in fuels (CARVALHO et al., 2015). Currently, the PROCONVE for LDVs, first imposed in 1986, is in its sixth phase, increasingly restricting CO, HC and NOx emissions by light-duty vehicles. By contrast, PROCONVE regulations for HDVs have been delayed for its third phase in the 1990s, only then imposing legal limits to CO, HC, NOx and PM emissions on heavy-duty vehicles. Moreover, the seventh phase was only implemented in 2012, making the use of S-10 diesel fuel (with reduced sulfur content) mandatory for new diesel motors (although old trucks and buses can still run on S-500)[27]. To complement PROCONVE, the National Program for the Control of Emissions by Motorcycles (PROMOT) began setting limits for motorcycle emissions only from 2003 onwards. Nevertheless, a motorcycle can still emit ten times more pollutants than a car manufactured in the same year and collectively they are responsible for 22% of CO emissions in the SPMA (CETESB, [s.d.]).

Motor vehicles manufactured before the first phases of PRONCOVE composed 15% of the vehicular fleet, but are responsible for approximately 35% of the pollutant emissions in the SPMA. By contrast, the 35% of the fleet in compliance with the fifth phase of PRONCOVE accounts for 18% of total emissions (CETESB, [s.d.]). Although the total fleet and fuel consumption have experienced high growth rates in the years since the implementation of the first phases of the emission control programs, due to the introduction of new and more efficient technologies and alternative fuels, total emission levels have dropped for most traffic-related pollutants in the SPMA, except for some secondary pollutants. However, if total trips by private vehicles continue to rise, an increase in total emissions can be expected in the near future (CARVALHO et al., 2015).

[26] Ethanol-based vehicles are supposedly less polluting than gasoline-based vehicles. However, it is possible that they increase ozone pollution, even though they might decrease $PM_{2.5}$ (SALVO et al., 2017; SALVO; GEIGER, 2014; SALVO; WANG, 2017).
[27] ANDRÉ et al. (2012) evaluated the impact of the delay in implementing the Cleaner Diesel Technologies policy (based on EU standards) on health outcomes and costs.

3.3 SPMA driving restriction policy

In 1996, concerns about air pollution led the state of Sao Paulo to partially ban the use of passenger cars in the SPMA for a month (the so-called "*rodizio*", meaning literally "rotation")[28]. The policy restricted car use based on the last digit of the license plate number one day per week (covering 20% of the car fleet on each weekday) in most of the SPMA (it included the city of Sao Paulo and another nine municipalities) between 7a.m.–8p.m. The scheme was also enforced in the following two years from May to September, which are the months that present the worst air pollution levels in the region due to unfavorable weather conditions for the dispersion of pollutants (cold/dry months).

After the first year of implementation, a household survey showed that the driving restriction policy was favored by the general population, especially among those who did not own a car[29]. However, the main reason for the "*rodizio*" policy support was due to improvements in traffic congestion (93% of cars adhered to the scheme), although the air quality benefits had been acknowledged by the population (MAHENDRA, 2008). At the time, the vehicular fleet in the city of Sao Paulo was around 4.5 million private vehicles and 12,000 municipal buses, with an estimated traffic speed of 12.5 miles per hour (JACOBI; SEGURA; KJELLEN, 1999).

In 1997, the scheme was redesigned by the city of Sao Paulo traffic authority to mainly tackle traffic congestion (although the reduction of air pollution was also mentioned in the proposed plan). The new traffic policy covered just the extended-CBD of the city of Sao Paulo and restricted driving within the limited area during peak hours, between 7a.m.–10a.m. and 5p.m.–8p.m., on weekdays, and was enforced for the entire year. In contrast, the state's "environmental" driving restriction scheme was terminated in 1999[30].

The fixed schedule of the driving restriction policy adopted by the city of Sao Paulo, still in place after 20 years, is easy to bypass, similar to the scheme in Mexico City[31] (DAVIS, 2008). Nevertheless, due to lack of data, there is no robust empirical evidence of the actual impact of

[28] A voluntary pilot was run a year before, in 1995, before the scheme was formalized in 1996 by the State authority. Similar driving restrictions have been in place in Santiago, Chile, since 1986, and in Mexico City, since 1989.

[29] Nevertheless, the State Secretary for the Environment, Fabio Feldmann, who first implemented the rodizio scheme in the SPMA, was not re-elected for a following term, receiving half the votes he received in the previous election, even though the State Governor, Mario Covas, had been re-elected for the 1999-2003 term. It is argued that, with the driving restriction policy, Feldmann lost much of his support among the largely middle class car-driving constituency (MAHENDRA, 2008).

[30] The state and municipal "*rodizio*" schemes overlapped from May to September of 1997 and 1998.

[31] The driving restriction schemes implemented later in Bogotá and Beijing periodically change the days on which vehicles with a given last digit are restricted from driving.

the policy on congestion and air pollution outcomes in Sao Paulo, beyond simple before-and-after comparisons[32]. Nonetheless, given the theoretical and empirical literature reviewed in the previous chapter on traffic policies, in general, and on driving restrictions in particular, there has been interest by many city hall administrations in substituting the rodizio scheme by a congestion pricing policy, but no real proposal has been put forth so far, mainly for the political reasons already addressed by the literature.

4. Theoretical Framework

The core assumption in the studies regarding the effects of transit supply on automobile externalities is about commuter willingness to substitute between private vehicle travel and transit ridership. Particularly regarding air quality and health externalities, it also relies on the assumption of a net reduction in total air pollution, stemming from the auto-to-transit emission substitutability rate. In this fashion, the analytical model formalizes the overall effect of public transit provision on air pollution as a function of the auto-transit emission rates per passenger transported and the magnitude of relevant elasticities of substitution. It specifically emphasizes how any automobile externality mitigation by transit supply depends on these elasticities of substitution. In this sense, it provides intuition for interpreting the empirical findings later, even though the econometric estimations in the following empirical chapter do not parallel the stylized model presented here.

Additionally, the outline of an analytical model for the health effects of air pollution helps interpret underlying estimates that are, in general, complicated by several factors[33]. The existence of avoidance behavior means that, even for random variations in air pollution, if estimates do no account for behavioral responses, they should understate the health impact of air pollution, as the most at-risk individuals have the greatest incentive to avoid exposure to higher pollution concentration levels. Moreover, the estimated impact can be confounded by a wide range of unobserved environmental, socioeconomic and other behavioral factors. Further, measurement error typically arises in assigning pollution exposure to individuals. The most common approach is to assign measurements of pollutant concentration readings to individuals' residential location from the nearest air-quality monitoring station. This is subject to considerable measurement error, given distance to measurement points, non-uniform

[32] LIN; ZHANG; UMANSKAYA (2011) attempted to collect pollution data around the time of the policy implementation in Sao Paulo to estimate the impact of the driving restriction on pollution using a RD design, but were not successful.
[33] See MORETTI; NEIDELL (2011) for a discussion of some salient factors in the empirical literature.

pollutant dispersion, and individuals' allocation of time across space. Therefore, the empirical estimates in the next chapter should be understood within these conceptual frameworks and underlying caveats.

4.1 Transit price-transportation emission analytical model

In order to express the change in air pollutant emissions that results from a change in transit price (in transit service provision or in any other condition of transit supply), RIVERS; SABERIAN; SCHAUFELE (2017) derived the following equation of a typical commuter utility maximization problem, subject to a standard budget constraint[34]:

$$\frac{dE}{dp_t} = \frac{\theta_t}{\zeta_t}\frac{\partial T(p_t, p_v, p_x)}{\partial p_t} + \frac{\theta_v}{\zeta_v}\frac{\partial V(p_t, p_v, p_x)}{\partial p_t} \tag{1}$$

In maximizing utility, a representative commuter has preferences over consumption of a composite good X, of price p_x, and travel, either by private vehicle, V, or public transit, T, with corresponding prices p_v and p_t – noting that the prices of travel comprise both pecuniary and non-pecuniary costs (time, access, comfort, reliability etc.). The θ_t and θ_v are the per-mile air pollutant emission parameters from transit and private vehicles, and ζ_t and ζ_v are the occupancy rate parameters of each modal vehicle, respectively. For ease, these parameters are assumed to be exogenous. The price of private vehicle use, p_v, is also assumed to be exogenous and, thus, does not account for endogenous variations in congestion levels[35]. The first term in the right-hand side is the product of a change in the demand for transit, $\partial T(p_t, p_v, p_x)$, due to the change in the transit price, ∂p_t, multiplied by the emission rate of transit, θ_t/ζ_t. Likewise, the second term is the change in private vehicle travel demand, $\partial V(p_t, p_v, p_x)$, due to the change in transit price, ∂p_t, multiplied by the emission rate of private vehicle travel, θ_v/ζ_v. As long as private vehicles and public transit are substitutes, the sign of the left-hand side term of the equation (1), which is the overall change in emissions, dE, due to a change in transit price, dp_t (in other words, the effect of transit supply on air pollution, although the empirical approach taken in the next chapter is based on a non-marginal supply change), depends on the corresponding rates of emission by private vehicles and public transit and the magnitude of the

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[34] See ANDERSON (2014) for a complete set-up of a typical commuter utility maximization problem or SCHMUTZLER (2011) for an alternative discussion to an analogous reduced-form framework, but which was sketched to be applicable to a wider variety of local transportation policies.

[35] The price of private vehicle use could be endogenized by allowing it to be a function of traffic volume, which would be assumed to be affected by the auto-to-transit substitutions induced by changes in public transit price.

underlying elasticities of substitution, between transport and other goods, and between private vehicle and public transit.

Regardless of the coefficient estimations of average emissions per passenger-mile, provided by engineering studies – based on vehicle age and size, fuel-type and timing of emission standard applications, travel frequency and speed, capacity utilization and occupancy rates etc.[36] –, the overall impact of transit supply on air pollution is ambiguous as it additionally relies on marginal measures and commuter behavior responses, which are difficult to predict. For instance, if buses are under capacity, a decline in transit price that motivates commuters to swap from auto travel to bus riding may lead to a reduction in CO emissions (mainly emitted by private vehicles), but relative smaller changes in NOx levels (mainly emitted by diesel-driven buses). By contrast, if the bus system is at-capacity, new buses can be added to meet the increased demand, with a potential increase in NOx. Moreover, if any freed road resulting from auto-to-bus substitution is occupied with latent demand for more private vehicles travel (i.e. the manifestation of the fundamental law of road congestion), there might be no reduction in CO. Considering a different scenario, if the elasticity of substitution between transport modals is small (i.e. commuters are unwilling to switch between private vehicle and public transit), a rise in transit price could produce a reduction in total travel, which could lead to improvements in air-quality if the bus system adjusts to keep running at-capacity.

In sum, the stylized model highlights how total air pollutant emissions and, hence, air pollutant concentrations (noting that in the analytical model the focus is on transport sector emissions, whereas in the econometric models in the next chapter the predicted variables are air pollution concentrations[37]) depends on both technological and behavioral parameters. The empirical literature concerning the transit impact on air pollution typically test whether the two hypotheses hold jointly – public transit implies lower emissions than private vehicles per passenger transported, and public transit supply induces enough substitution from private vehicle to transit (LALIVE; LUECHINGER; SCHMUTZLER, 2017). The former is less of a concern when subway and light rail account for the largest share of total public transportation, as they usually run on electricity[38] (CHEN; WHALLEY, 2012; GOEL; GUPTA, 2017;

[36] Additionally, emission coefficients are highly dependable on context-specific characteristics, such as road type, topography, driving behavior, traffic conditions etc.
[37] Although transport sector emissions are the main source of air pollution concentrations in most large cities, they are not the only source. Moreover, and most importantly, there are other variables, such as weather conditions, which affect the levels of air pollution. Thus, pollutant emissions and air pollutant concentrations are only imperfectly correlated.
[38] Electricity can be generated by power plants with no relevant traffic-related pollutant emissions or by emitting mainly SO_2. However, they are in general, outside the urban area and, thus, beyond the scope of investigation.

LALIVE; LUECHINGER; SCHMUTZLER, 2017). However, when buses are the principal mode of transit, the total emission for some specific pollutants (e.g., NOx emission by diesel driven buses vis-à-vis CO emission by gasoline-driven private vehicles) might not be necessarily lower even if the elasticity of substitution between transport modals is sufficiently large (RIVERS; SABERIAN; SCHAUFELE, 2017).

4.2 Air pollution-health outcome analytical model

Air pollution levels, exposure and dose-response are held as independent and quasi-exogenous in the broadly scientific literature describing the process through which air pollution impacts the human health[39]. The conceptual models taken in economics depart from this framework in order to account for the endogenous behavior of individuals (i.e. it is not assumed to be fixed) in the face of air pollution. In this fashion, building upon the "Grossman model" (GROSSMAN, 1972), which depicts health as a capital good, ZIVIN; NEIDELL (2013) present an analytical model that explicitly recognizes the role of individuals in influencing their own health:

$$(2) \qquad \frac{d\mathrm{H}}{d\mathrm{P}} = \left(\frac{\partial\varphi}{\partial\mathrm{P}} + \frac{\partial\varphi}{\partial\mathrm{A}}\frac{\partial\mathrm{A}}{\partial\mathrm{P}}\right) \cdot \left(\frac{\partial\mathrm{H}}{\partial\mathrm{M}}\frac{\partial\mathrm{M}}{\partial\varphi} + \frac{\partial\mathrm{M}}{\partial\varphi}\right) = \frac{d\varphi}{d\mathrm{P}} \cdot \frac{d\mathrm{H}}{d\varphi}$$

The stylized model is derived from a health production function of a representative individual in a nonconventional form, $\mathrm{H} = \mathrm{H}(\varphi(\mathrm{P}, \mathrm{A}), \mathrm{M}(\varphi))$, making the distinction between the individual health status, H, and illness episodes, φ. The latter depends on both air pollutant concentrations, P, and avoidance behavior, A (which mitigates exposure)[40]. Health is also a function of medical care, M, which ameliorates the negative effects of air pollution exposure and, in turn, depends on illness episodes. Thus, the individual health depends jointly on illness episodes and medical care. Following a standard individual's utility maximization problem, in which individual utility is also contingent on the consumption of a composite good, the optimal avoidance behavior and medical care will depend on air pollution levels, the dose-response function (which translates air pollution exposure to illness episodes), and the costs of avoidance behavior (including both expenditure on defensive measures and disutility associated with reallocation of time), medical treatment, and all other consumption goods. As a result, equation (2), which is the reduced form effect of air pollution on health, depends on the relationship between air pollution levels and illness episodes, $d\varphi/d\mathrm{P}$, and, between illness episodes and

[39] See ZIVIN; NEIDELL (2013) for further discussion.
[40] Avoidance behavior is a term typically employed in the literature concerning short-term air pollution exposure, whereas residential sorting is an example of long-term avoidance behavior.

health status, $d\mathrm{H}/d\varphi$. Noting that the first right-hand side term expresses the net effect of air pollution on illness episodes accounting for the individual exposure to pollution, in which $\partial\varphi/\partial\mathrm{P}$ is the individual physiological response to pollution, whereas $(\partial\varphi/\partial\mathrm{A}\cdot\partial\mathrm{A}/\partial\mathrm{P})$ is the avoidance behavior based on air pollution levels in averting illness episodes. By the same token, the second right-hand side term of equation (2) describes the ratio by which medical care mitigates the negative effects of air pollution on health, $(\partial\mathrm{H}/\partial\mathrm{M}\cdot\partial\mathrm{M}/\partial\varphi)$, and the degree to which health responds to illness episodes, $\partial\mathrm{H}/\partial\varphi$. Different empirical approaches will estimate different variants of equation (2), but, mostly constrained by data limitations, the empirical literature in this area is only able to partially estimate this total derivative, as we shall also attempt to in the next chapter.

5. Empirical Approach

Transit supply has been most likely supported in densely populated cities with higher traffic congestion and, hence, worse air quality and health conditions. Thus, without a credible research design accounting for potential endogeneity issues, the estimates should be biased. A critical issue for transit impact evaluations is contrafactual identification, as transit infrastructure and service provision cannot be, in general, randomly assigned[41]. Recognizing the challenges of implementing field experiments in the realm of urban phenomena, researchers in this area have been able to successfully recover credible estimates of the parameters of interest by exploiting nonexperimental variations in the treatment variables[42]. Of central importance to these empirical strategies is the degree to which researchers can be confident that the causal variable of interest is independent of confounding factors, in which there would be unobserved variables that both influence the outcome of interest and are correlated with the treatment variable.

In particular, quasi-experimental research designs may permit more credible inferences about the relationship between air pollution and human health, as observational studies mainly rely on controls for observed confounders[43]. Nevertheless, even if these empirical approaches are

[41] As noted by BOARNET (2007), random experiments could be pursued in a few instances, for instance, in the specific case of direct transit subsidies to individuals.
[42] See BAUM-SNOW; FERREIRA (2014) for a review of causal inference in urban economics with nonexperimental data.
[43] Since many determinants of health are unobserved, for instance, people who are exposed to different levels of air pollution frequently have differing initial levels of health (e.g., lower income populations living in more polluted areas), there might be locational determinants of health that differ across space and are correlated with air pollution levels (e.g., hospital quality), and people may avoid pollution exposure and/or invest more in self-protection depending on health susceptibility in ways that are unobserved. See DOMINICI; GREENSTONE;

devoted to identify exogenously treatment-induced variations that convincingly mitigate confounding or omitted variables in the effect estimates, they are often not implemented without limitations[44]. Firstly, in these settings, the study population is determined by the available treatment and, hence, is beyond the researchers' control. Second, the empirical methods adopted are usually data demanding, requiring sufficient variation in spatial and temporal scales. Third, even though air quality monitors typically measure pollutant concentrations at very high frequencies at a fixed location, they are limited in numbers relatively to the geographic distribution of the population. The assignment of air pollution concentrations to individuals are not trivial and require strong assumptions about individual mobility and population spatial distribution. Fourth, hospital admission and mortality rates are the commonly observed health outcomes, but they do not capture many of the health effects of air pollution and, as such, should always be considered as the lower bound of the full impact. Even when data of other healthcare forms are available, such as emergency room and doctor visits and medication use, they might be subject to sample selection bias. Moreover, adverse health effects might be manifested in other ways[45] other than healthcare encounters. Last, but not least, different quasi-experimental identification strategies differ in their effectiveness to eliminate, or considerably reduce, the time-varying behavioral responses to changes in air pollution concentrations. There may be unobserved variables influenced by the treatment that also affect individuals' exposure levels or health outcomes more directly[46]. Therefore, even if air pollution variations are as good as randomly assigned, exposure to air pollution might remain endogenous.

5.1 Identification Strategy

Transit strikes have been exploited in the literature as a credible source of exogenous supply shocks and, hence, seen as a quasi-experiment[47]. They have been particularly used to provide

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SUNSTEIN (2014) for a review of the limits of observational studies and the quasi-experimental alternatives in the assessment of the effects of air pollution on health outcomes.

[44] See CURRIE; HEEP; NEIDELL, (2011) and ZIVIN; NEIDELL, (2013) for further discussions about the empirical challenges involved in quasi-experimental studies analyzing the relationship between environmental externalities and human health.

[45] An emerging economic literature has focused on these other health effects, such as school absenteeism and labor productivity (e.g., CURRIE et al., 2009; ZIVIN; NEIDELL, 2012).

[46] In addition to optimizing behavior causing endogenous air pollution exposure, changes in environmental conditions are also a concern for omitted variable bias. In particular, weather conditions interact with pollutant emissions to form air pollution concentrations, but they might also directly impact health (DESCHÊNES; GREENSTONE, 2011).

[47] Strikes in other sectors have been largely exploited in the economics literature as a "shock" to air quality that push the market temporarily out of equilibrium holding other characteristics constant. In fact, one of the earliest examples is the use a temporary closing of a steel mill due to a labor strike to access variations in respiratory-related hospital admissions, school absences, and mortality outcomes in difference-in-differences models

evidence that subsidies to public transit are justified by their congestion relief benefit alone. While ANDERSON (2014) implemented a sharp regression discontinuity design in time using a single 35-day-transit strike (bus and rail lines) in the Los Angeles area on 640 hourly traffic delay detectors, ADLER; VAN OMMEREN (2016) examined the effect of 16 multiple one-day strikes of various transit modes (including bus, light rail and metro) spread over a period of ten years in the city of Rotterdam on 24 pneumatic tubes measuring hourly car or bicycle travel flows and 4 speed points for travel time measurements.

However, to the best of our knowledge, there is only one working paper, yet unpublished, in the economics literature using transit strikes for identification of the effects of transit provision on air pollution and none on contemporaneous health outcomes. RIVERS; SABERIAN; SCHAUFELE (2017) deployed a difference-in-differences model, as they observed a total of 105 multiple-day transit strikes[48] in several cities of Canada, comparing outcomes in 18 cities for CO between 1974–2011, 13 cities for NOx between 1980–2011, and 9 cities for $PM_{2.5}$ between 1997–2011. However, they found that the current configuration of public transit in North America (which are dominated by buses and have a low transit mode share) did not contribute to improving local air quality. Their empirical approach closely followed BAUERNSCHUSTER; HENER; RAINER (2017), who investigated the impact of 71 one-day transit strikes in five German cities on congestion, car crashes, air pollution, and hospital admissions between 2002 and 2011. It is worthy to note, though, that the latter was ultimately interested in estimating the impact of the transit strikes themselves, concluding that transit strikes impose substantial costs also on non-involved third parties.

Looking beyond the economics literature, the use of transit strikes is also found in transportation engineering and public health studies. The seminal work of LO; HALL (2006) was the first to investigate the effect of the Los Angeles transit strike on congestion. However, the before-and-after and control group comparisons employed delivered much larger estimates relative to ANDERSON (2014)'s regression discontinuity design[49]. Furthermore, there are quite a few studies examining the correlation between variations in air pollution levels and traffic volumes due to public transit strikes. For instance, CHALOULAKOU et al. (2005),

comparing affected and unaffected communities (POPE, 1989; RANSOM; POPE, 1992, 1995). More recently, LAVAINE; NEIDELL (2017) investigated the effects of energy production on air quality and on asthma and bronchitis related hospital admissions and birth weight and gestational age of newborns using a month-long strike that affected several oil refineries in France.

[48] The multiple-transit strikes used for econometric identification in this paper are much longer than generally found in this literature, lasting several weeks each on average.

[49] In fact, ANDERSON (2014) replicated the LO; HALL (2006)'s study, but using the RD design, and found statistically insignificant results.

studying air pollution concentration levels in central Athens, Greece, compared the average concentrations of pollutants in the 21 days in which taxi or public bus drivers went on full-day strikes with the average concentration levels 3 days before and 3 days after each isolated event. MEINARDI et al. (2008) collected air pollutant samples a few days before and on the day of a transit strike in Milan, Italy, during which the driving restriction scheme in place in the downtown area was suspended. They argued that comparison between samples were possible as the weather conditions did not vary much during the short study period. BASAGAÑA et al. (2018), analyzing a larger number of transit strikes on air pollution levels, observed 208 strike days of the metro, train or bus systems over 11 years in Barcelona, Spain. They employed linear regression models using 24-hour average pollutant concentrations and controlling for meteorological conditions, temporal and seasonal trends, including lagged dependent variables in the time series.

Yet, in the public health literature, the paper most closely related to our work was carried out by SILVA et al. (2012), who exploited two 24 hour single-day metro strikes[50] in Sao Paulo aiming at evaluating air quality and health benefits of the city subway system. Using PM10 average daily concentrations for all monitoring stations located in the central districts of Sao Paulo and daily mortality data on cardio-respiratory diseases for the elderly (age 65 and over), they compared strike day outcomes with outcomes of pre- and post-strike days and with "manually" selected control days chosen by similar weather conditions on the same day of the week and month of the respective strikes. They further estimate the associated economic burden using a value of statistical life.

Above all, for transit strikes to be exploited as a credible identification strategy, they have to be unrelated to other variations in air pollution and health outcomes and they should only affect these outcomes through induced upsurges in automobile use. Most studies assume that conditional on the covariates (i.e. mainly weather and time controls), the timing of the strikes is orthogonal to the outcomes of interest. That would not be the case if transit worker unions systematically planned strike actions to produce maximum disruption[51], in which the transit strikes would frequently coincide with periods of worse congestion and air quality conditions[52].

[50] They coded the metro strike in June 2003 as a 24h single-day metro strike, whereas we coded it as a two full-day metro strike (starting on the first hours of June 17 and finishing on June 18 around 7:30p.m.). The other 24h single-day metro strike used in their study in August 2006 is coded similarly in our analysis.

[51] For instance, on the evening prior to holidays or particularly bad weather days, which could be controlled for if the underlying confounding factors are always observed.

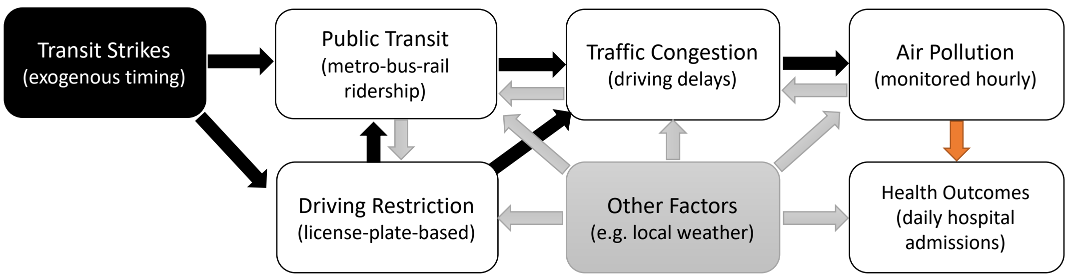
[52] To eliminate doubts regarding these issues, it has become standard in this literature to apply falsification tests with both placebo strikes (i.e. using non-strike days with similar timing) and placebo outcomes (i.e. observing non-traffic related pollutants and diseases).

Nevertheless, it might be unlikely that transit strikes and health outcomes are uncorrelated, either unintentionally due to changes in time allocation, in direct response to changes in mobility conditions, or due to intentional avoidance behavior.

Moreover, one important caveat in this identification strategy is that, even though there is suggestive evidence that transit strikes induce a significant fraction of public transit riders to switch to private vehicles (VAN EXEL; RIETVELD, 2001, 2009)[53], it is not allusive about the actual elasticities of substitution between modals, as discussed in the previous theoretical chapter. The transit strike strategy allows credible recovery of the causal public transit impact on automobile externalities only in the short run, since it is not informative regarding long-run effects. It ignores long-term adjustments, such as land-use development patterns, residential and commercial locational choices, and private vehicle ownership. Furthermore, the identification strategy contrafactual is based on a halt of transit service provision and, hence, it is not revealing about marginal changes of transit supply.

Having said this, and given its strengths and limitations, we built on this literature to estimate the impact of transit provision on air pollution and health in the SMPA. Figure 1 sums up the research design, in which transit strikes are assumed to be an exogenous source of variation in public transit supply and, without it, the relationships between variables would flow in both directions. That is, transit strikes, as exogenously determined, allow us to estimate the level of traffic-related air pollution and illness that would arise in the counterfactual scenario without public transit provision.

Figure 1 – Transit strike identification strategy



Source: Elaborated by the authors.

[53] The VAN EXEL; RIETVELD (2001) reviewed earlier studies of strikes in the public transport sector and used a survey to confirm the main results of this literature: on average 10–20% trips were cancelled in transit strike days and the majority of travelers switched to automobile use, either as driver or passenger. However, in an updated study, VAN EXEL; RIETVELD (2009) found that as many as 44% of the people who had planned to travel by train cancelled their trips on a given strike day, but 38% still switched to automobile use (24% as drivers and 14% as passengers). As expected, commute and business trips were less likely to be cancelled and more likely to be completed by car. Moreover, LARCOM; RAUCH; WILLEMS (2017) confirmed that transit strike episodes are unlikely to produce a permanent modal shift, but induced commuters to find more optimal transit routes.

In this overview, it is important to highlight some additional salient features and caveats of the research design. First, the driving restriction effect cannot be disentangled from public transit provision (i.e. when the latter is ceased, the former is suspended), which reinforces the substitution effect from transit riding to automobile use. Second, we assume that public transit implies lower emission levels than private vehicle per passenger transported, but metro strikes should have a higher impact than bus strikes. Third, the comparisons between strike and non-strikes outcomes are conditional both on observed weather conditions (since traffic emissions and air pollution levels are imperfectly correlated and are confounded by environmental factors as well as the relationship between air pollution levels and illness episodes), and on timing (i.e. day of the week, season, year etc.), as strikes are not evenly distributed across time. Finally, we only observe illness episodes, which do not only depend on air pollutant concentrations, but also on pollution exposure, which can be mitigated by endogenous changes in behavior. With these points in mind, we present our data followed by the econometric methods to estimate the effects of interest.

5.2 Data and Descriptive Statistics

We obtained data on hourly air pollution concentrations and daily hospital admissions from 1998 to 2017 for the SPMA and four other metropolitan areas of the state of Sao Paulo. The data on transit strikes are for the SPMA only and were self-collected. Additionally, we obtained hourly driving delays in the SPMA for the period of study. The data for controls includes hourly weather variables with the same coverage as the air pollution data[54].

5.2.1 Transit strikes

The list of public transit strikes observed between 1998 and 2017 in the Sao Paulo Metropolitan Area was obtained from the city of São Paulo's traffic authority – the Sao Paulo Traffic Engineering Company (CET) –, which catalogued all transit strikes occurring in the period of analysis[55], as they had the effect of suspending the driving restriction scheme (the so-called "*rodizio*"), enforced by the city since 1997. Additional information about transit strikes were self-collected from transport workers union's newsletters, public transit operators' websites, and newspapers' online archives.

[54] All datasets and R codes used in this research is available at: https://drive.google.com/open?id=1REw-ychmksGUILAaKPjI5FhrLP5BUA6c
[55] See CET listing of all "rodizio" suspensions, including transit strike episodes at: http://cetsp1.cetsp.com.br/institucional/rodizio/default.asp

We were able to double-check all transit strikes listed by CET, with information from at least one independent source[56]. We found that of the twenty-five transit strikes listed by CET for the period, three were not confirmed by other sources and, hence, were excluded from our sample[57]. Moreover, one of the listed strikes happened simultaneously with many other strikes across different sectors and thus was also excluded from our sample, as it possibly had the opposite expected effect, reducing the probability of commuters of substituting from transit to private vehicle (since the majority of people had not gone to work or school on the day of the "general strike")[58].

Therefore, we focus on a total of 21 transit strikes within the period of study, including either one-day or multiple-day strikes, adding up to 34 strike days. About half of the transit strikes last for multiple full-days, while the other half ended within 24 hours, but the majority impacted both morning and evening driving restriction periods (between 7am–10am and 5pm–8pm). The observed transit strikes affected either metro, bus or rail networks. As summarized in Table 1, we observed 11 metro strikes (totaling 17 strike days), nine bus strikes (16 strike days in total), and just one full-day rail-only strike. During four metro strikes, we observed either partial-network or part-time bus and rail strikes happening at the same time, but we classified them under the metro strike type, as it was the dominant modal carrying strike actions on those days.

Table 1 – SPMA total transit strikes by mode, 1998–2017

Type	#Strike	#StrikeDays
Bus	9	16
Metro	11	17
Rail	1	1
Total Strikes	**21**	**34**

Note: The total strike days includes one-day and multiple-day strikes.
Source: Elaborated by the authors. Data by CET, SPTrans, Metro, CPTM, Transport Workers Union, and newspapers' online archives

Table 2 provides detailed information about the 21 transit strikes by type, date, and mode. It reports whether any other strike happened in the same period. Two transit strikes coincided with other sector strikes (public servants and public school teachers), but all other economic

[56] Before 2003, there are just a few independent sources available to double-check information about the transit strikes. Particularly, for this period we used the Folha de Sao Paulo newspaper's online archive.
[57] We could not find additional information on the bus strike of April 7, 1998 besides what was registered by CET, thus we cannot be sure whether the strike indeed took place. We also believe CET miscoded a transit strike on October 7, 2000, since it was Saturday and there is no other strike observed beginning on weekends (and no other source has mentioned it). There was also a transit strike recorded by CET on November 11, 1999, but the union claimed they had canceled it and no other source confirmed that the strike had indeed happened. All transit strikes excluded from our sample were included as controls since they had the effect of suspending the driving restriction anyway.
[58] This "general strike" took place on April 28, 2017.

sectors maintained their daily routines on those days. It additionally includes notes about some of the responses by the traffic authority or other transit operators triggered by the transit strike, such as suspension of parking restrictions (the so-called “Zona Azul”), increased rail frequency, and/or deployment of the bus company emergency plan (known as “PAESE”). Moreover, it points to any abnormal pattern that might affect traffic, such as an evening prior to a holiday or protests in the city.

Table 2 – SPMA list of transit strikes, 1998–2017

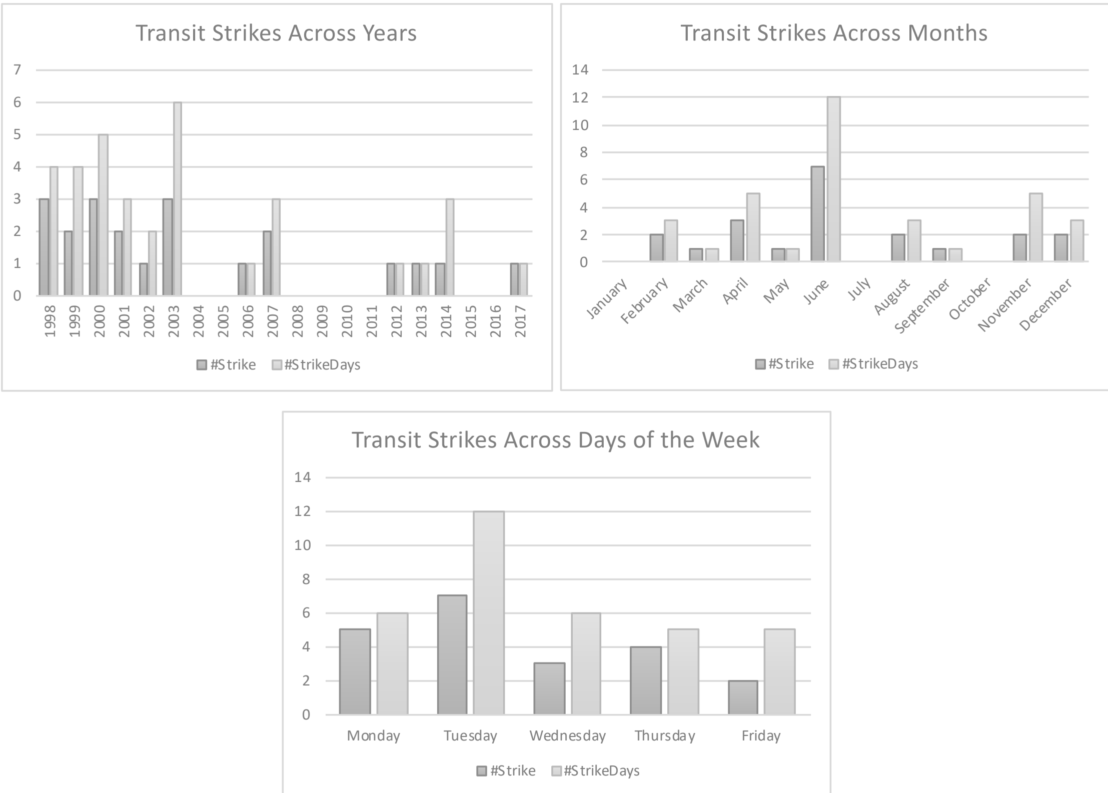
Strike Type	Start and End Dates	Time	Notes
Bus Strike			
Partial network	11/17/98 Tuesday 11/18/98 Wednesday	full-day	Only southern part of the city affected; few bus companies adhere
Partial network	12/08/98 Tuesday	full-day	Only northern part of the city affected; few bus companies adhere
Total network	04/06/99 Tuesday 04/07/99 Wednesday	full-day	Parking restriction canceled; about 80% of bus companies adhere
Total network	09/22/00 Friday	12am-7am	Before dawn, driving restriction suspended anyway
Partial network	11/06/00 Monday 11/08/00 Wednesday	full-day	Only eastern and southern parts of the city affected; few bus companies adhere
Partial network	02/06/01 Tuesday	full-day	PAESE; about half of the bus companies adhere
Total network	06/24/02 Monday 06/25/02 Tuesday	full-day	Increased rail frequency and minibus provision
Total network	02/03/03 Monday 02/04/03 Tuesday	full-day	Parking restriction canceled
Total network	04/07/03 Monday 04/08/03 Tuesday	full-day	PAESE and increased rail frequency; parking restriction canceled
Metro Strike			
Total network	04/28/98 Tuesday	5am-6am	Also total network bus strike from 3am to 6am; before dawn, driving restriction suspended anyway
Total network	12/09/99 Thursday 12/10/99 Friday	full-day	Also partial network bus strike in various periodos of the day, but few bus companies adhere
Total network	06/02/00 Friday	full-day	Coinciding with few strikes in other sectors (teachers and public servants)
Total network	06/25/01 Monday 06/26/01 Tuesday	full-day	PAESE and increased rail frequency; parking restriction canceled
Total network	06/17/03 Tuesday 06/18/03 Wednesday	full-day	PAESE and increased rail frequency; parking restriction canceled; holiday eve; protests
Total network	08/15/06 Tuesday	full-day	PAESE and increased rail frequency
Total network	06/14/07 Thursday	5am-11am	PAESE and increased rail frequency
Partial network	08/02/07 Thursday 08/03/07 Friday	full-day	PAESE and increased rail frequency
Partial network	05/23/12 Wednesday	full-day	Also partial network, full-day rail strike; PAESE
Partial network	06/05/14 Thursday 06/09/14 Monday	full-day	PAESE and increased rail frequency; few days before World Cup; protests
Partial network	03/15/17 Wednesday	full-day	Also total network bus strike from 12am to 8am; coinciding with few strikes in other sector (teachers, public servants, and banking); protests
Rail Strike			
Partial network	06/13/13 Thursday	full-day	PAESE; protests

Source: Elaborated by the authors. Data by CET, SPTrans, Metro, CPTM, Transport Workers Union, and newspapers' online archives

Figure 2 shows the distribution of transit strikes across time. The variable “#Strike” counts the total number of strikes, either one-day or multiple-day strikes. By contrast, the “#StrikeDays” counts the actual number of days when we observe transit strikes in the SPMA. In the last panel

of Figure 2, the "#Strike" counts the day of the week in which the strike begins and "#StrikeDays" counts again the actual number of strike days. There was a clear concentration of transit strikes in the first years of the study period. There was no strike activity in 8 out of the twenty years. Of all months of the year, only January, July (the summer and winter school vacations' months, respectively), and October registered no transit strikes. The spike in June was due to metro strikes (5 out of 11 metro strikes took place in that month)[59]. We observed transit strikes happening in all weekdays. There was no transit strike during weekends (except a metro strike in June, 2014, that lasted from Thursday to Monday, but we did not compute that weekend in our count of strike days) or during holidays (there was just one metro strike that ended on an evening prior to a holiday). It is interesting to note that all bus strikes happened between 1998 and 2003, while metro strikes are more well-distributed across the study period.

Figure 2 – SPMA transit strike frequency distribution across time



Note: #Strike appoints to the day of the week in which the strike begins
Source: Elaborated by the authors. Data by CET, SPTrans, Metro, CPTM, Transport Workers Union, and newspapers' online archives

We were able to verify that 18 transit strikes were confirmed in advance, at least one day prior to the strike occurrence. For the other three transit strikes, all happening before 2001, we could

[59] Reflecting subway worker union disagreements on annual wage adjustments due every May.

not find this information but which does not mean that these strikes were not confirmed[60]. Hence, we assume that in all strikes, commuters had time to adjust their travel behavior, even though the information provided beforehand is not usually very clear about the time and coverage of the strike.

As transit strikes have the effect of suspending the driving restriction ("*rodizio*") for the days in which they take place[61], they have the potential to increase the car fleet on the streets during strike days by up to 20 percent[62]. The city traffic operator also facilitates private vehicle use on transit strike days by suspending paid on-street parking, and by opening bus-only lanes to passenger. Almost all metro strikes observed in the period of study had the effect of increasing bus provision – by activating the emergency bus plan (PAESE)[63] – and rail frequency.

The motivations for transit strike actions vary from labor rights, pay increases, working conditions and safety, service privatization and automation, to more general policy claims, such as pension and labor law reforms. During four transit strikes in our sample, we also observed protests in some central points of the city, of which two had the same general policy motivation of the transit strikes (pension and labor law reforms). In the other two cases, the protests were not directly related to the transit strike motivation[64].

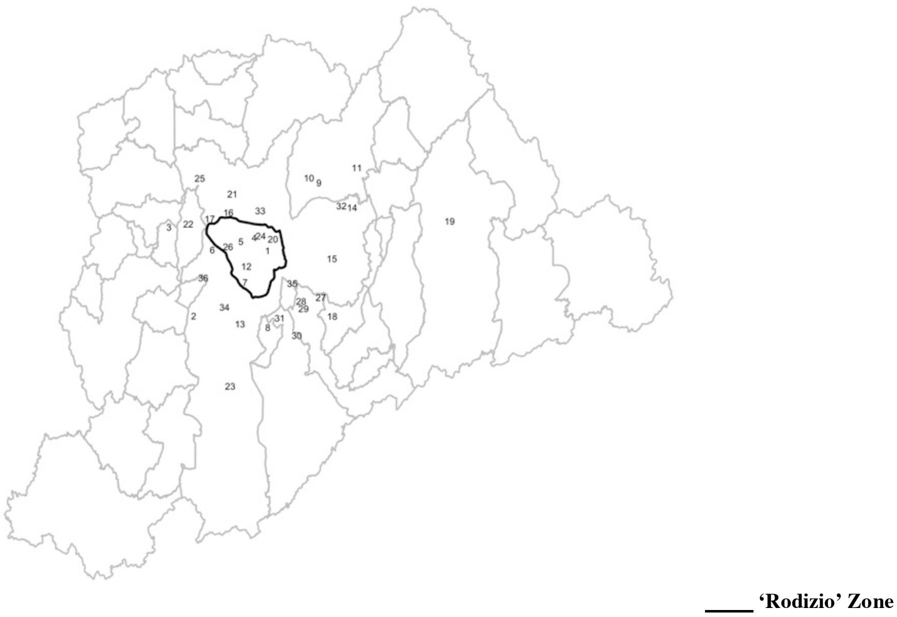
Finally, it is worthy to explicitly recognize that our transit strike data is subject to measurement error. We could have missed days that were indeed affected by strikes or we might have incorrectly coded a strike day which had in fact it did not happened. However, in either case, the miscoded transit strike will have the effect to bias our estimates downward. The former would supposedly increase air pollution on days coded as non-strike days and the latter would make air pollution levels on strike days lower.

[60] In fact, the transit worker unions are obligated to announce a strike at least one week in advance, however these announcements do not carry much meaning as many times they are announced as threats as part of negotiating strategies, but are never carried out. Thus, the "last minute" confirmation on the night before the strike is the most informative one.
[61] Driving restrictions for heavy trucks and chartered buses are maintained even in transit strike days.
[62] Only the last transit strike in our sample, in March 2017, happened in the "Uber era", in which the option to substitute transit by automobile use even when one does not own a car is more widespread.
[63] Nevertheless, with PAESE activated, the bus fleet on the streets should increase by no more than 5 percent (about one thousand extra buses).
[64] The strike of June, 2014 also saw violent clashes between strike workers and the police. The partial rail strike on June 13, 2013 happened just after days of violent protests regarding bus fare rise, in which some buses were set on fire.

5.2.2 *Air pollution*

We obtained hourly data on air pollutants for the period between 1998–2017 from the State of Sao Paulo Environment Agency (CETESB)[65], which operates air quality-monitoring stations across the state[66]. We observed 36 air-quality monitoring stations in the SPMA within the period of study. However, not all stations measure all selected pollutants and not all stations were in operation over the entire time of our study period. Figure 3 shows the location of all air-quality monitoring stations operating at some point between 1998 and 2017 in the SPMA. It also highlights the driving restriction zone (or "rodizio zone"), which covers the extended central business district (CBD) of Sao Paulo.

Figure 3 – SPMA air-quality monitoring stations and driving restriction zone



1	Cambuci	10	Guarulhos-Paco Municipal	19	Mogi das Cruzes	28	S.Andre-Centro
2	Capao Redondo	11	Guarulhos-Pimentas	20	Mooca	29	S.Andre-Paco Municipal
3	Carapicuiba	12	Ibirapuera	21	N.Senhora do O	30	S.Bernardo-Centro
4	Centro	13	Interlagos	22	Osasco	31	S.Bernardo-Pauliceia
5	Cerqueira Cesar	14	Itaim Paulista	23	Parelheiros	32	S.Miguel Paulista
6	Cid.Universitaria-USP-Ipen	15	Itaquera	24	Parque D.Pedro II	33	Santana
7	Congonhas	16	Lapa	25	Pico do Jaragua	34	Santo Amaro
8	Diadema	17	Marg.Tiete-Pte Remedios	26	Pinheiros	35	Sao Caetano do Sul
9	Guarulhos	18	Maua	27	S.Andre-Capuava	36	Taboao da Serra

Note: The numbers in the subtitle were attributed to the air-quality monitoring stations by us, and, thus do not correspond to the actual registration numbers of CETESB's stations
Source: Elaborated by the authors. Data by CETESB, CET, and IBGE

[65] Data available at: http://ar.cetesb.sp.gov.br/qualar/
[66] The annual CETESB reports give a full account of all monitoring stations (CETESB, [s.d.]).

Table 3 – Descriptive statistics for air pollutant concentrations, 1998–2017

Air Pollutant	Station	MA	unit	N	Mean	Std.Dev.	Min	Median	Max
CO	Cerqueira César	RMSP	ppm	118320	1,28	0,98	0,1	1,0	13,2
	Congonhas	RMSP	ppm	118320	1,89	1,34	0,1	1,5	20,9
	Ibirapuera	RMSP	ppm	118320	0,79	0,72	0,1	0,6	13,9
	Parque D.Pedro II	RMSP	ppm	118320	1,05	0,90	0,1	0,8	16,9
	Osasco	RMSP	ppm	118284	1,84	1,18	0,1	1,6	14,9
	São Caetano do Sul	RMSP	ppm	118113	1,10	1,04	0,1	0,9	20,9
	Santo Amaro	RMSP	ppm	108659	0,82	0,70	0,1	0,6	9,6
	Campinas-Centro	RMC	ppm	105922	1,18	0,70	0,1	1,1	13,7
MP10	Cerqueira César	RMSP	µg/m3	118320	38,0	27,3	1	32	820
	Congonhas	RMSP	µg/m3	118320	48,9	33,6	1	41	796
	Diadema	RMSP	µg/m3	116042	39,9	27,7	1	33	356
	Mauá	RMSP	µg/m3	118320	38,9	31,6	1	31	629
	N.Senhora do Ó	RMSP	µg/m3	112512	36,3	26,5	1	29	353
	Osasco	RMSP	µg/m3	118320	54,6	38,3	1	45	705
	Parque D.Pedro II	RMSP	µg/m3	118320	43,3	34,7	1	35	602
	S.André-Capuava	RMSP	µg/m3	118320	36,3	26,2	1	30	563
	S.Bernardo-Paulicéia	RMSP	µg/m3	118320	42,4	35,4	1	33	761
	São Caetano do Sul	RMSP	µg/m3	118320	40,4	29,4	1	33	557
	Taboão da Serra	RMSP	µg/m3	118320	41,2	33,6	1	32	859
	Mooca	RMSP	µg/m3	118296	38,7	30,7	1	31	462
	Santo Amaro	RMSP	µg/m3	108870	39,1	32,8	1	31	609
	Campinas-Centro	RMC	µg/m3	106272	35,9	21,7	1	31	567
	Paulínia	RMC	µg/m3	105488	36,0	27,9	1	28	561
	Sorocaba	RMS	µg/m3	106038	33,1	26,6	1	25	540
	S.José dos Campos	RMVP	µg/m3	104553	27,5	23,4	1	22	541
	Cubatão-Centro	RMBS	µg/m3	118320	35,2	26,9	1	30	985
NOx	Cerqueira César	RMSP	ppb	118320	83,62	71,71	1	64	917
	Congonhas	RMSP	ppb	118224	150,71	121,88	1	108	1632
	Mauá	RMSP	ppb	118320	26,30	28,95	1	18	484
	Osasco	RMSP	ppb	118320	105,83	75,41	1	86	741
	Parque D.Pedro II	RMSP	ppb	118187	66,55	67,97	1	44	1034
	São Caetano do Sul	RMSP	ppb	118112	59,20	60,57	1	43	1040
	Ibirapuera	RMSP	ppb	118056	37,63	47,82	1	24	907
	Paulínia	RMC	ppb	105488	24,66	29,00	1	15	569
	Cubatão-Centro	RMBS	ppb	118320	42,61	37,49	1	30	592
NO2	Cerqueira César	RMSP	µg/m3	118320	59,78	32,47	1	54	394
	Congonhas	RMSP	µg/m3	118224	76,39	37,29	1	68	392
	Mauá	RMSP	µg/m3	118320	31,07	20,37	1	27	327
	Osasco	RMSP	µg/m3	118320	57,72	26,84	1	53	355
	Parque D.Pedro II	RMSP	µg/m3	118185	55,17	30,22	1	49	391
	São Caetano do Sul	RMSP	µg/m3	118112	51,07	28,13	1	46	354
	Ibirapuera	RMSP	µg/m3	118056	41,95	26,16	1	36	397
	Paulínia	RMC	µg/m3	105487	27,72	20,74	1	22	247
	Cubatão-Centro	RMBS	µg/m3	118320	28,62	17,87	1	26	260
O3	Ibirapuera	RMSP	µg/m3	118320	38,02	37,04	1	27	350
	Mauá	RMSP	µg/m3	118320	39,15	33,05	1	32	326
	Parque D.Pedro II	RMSP	µg/m3	118320	28,33	28,86	1	19	282
	Mooca	RMSP	µg/m3	118296	31,49	31,55	1	22	339
	São Caetano do Sul	RMSP	µg/m3	118114	36,33	32,14	1	27	381
	S.José dos Campos	RMVP	µg/m3	106076	34,08	29,94	1	26	258
	Sorocaba	RMS	µg/m3	106039	39,66	31,35	1	34	283
	Paulínia	RMC	µg/m3	105397	45,81	35,93	1	36	294
	Cubatão-Centro	RMBS	µg/m3	118320	31,30	29,72	1	23	279
SO2	Congonhas	RMSP	µg/m3	118223	14,34	13,04	1	10	266
	Osasco	RMSP	µg/m3	118319	9,50	9,15	1	7	352
	Cerqueira César	RMSP	µg/m3	118295	8,67	9,36	1	6	187
	São Caetano do Sul	RMSP	µg/m3	117851	9,71	10,45	1	6	344
	S.José dos Campos	RMVP	µg/m3	106391	4,02	6,72	1	2	279
	Paulínia	RMC	µg/m3	105487	9,69	17,72	1	4	548

Notes: The five metropolitan areas (MA) included are: Sao Paulo Metropolitan Region (RMSP), Campinas Metropolitan Region (RMC), Sorocaba Metropolitan Region (RMS), Vale do Paraiba Metropolitan Region (RMVP), and Baixada Santista Metropolitan Region (RMBS). The numbers are in Brazilian format, which uses comma rather than dot as decimal separator, and dot rather than comma as thousand separator.
Source: Elaborated by the authors. Data by CETESB

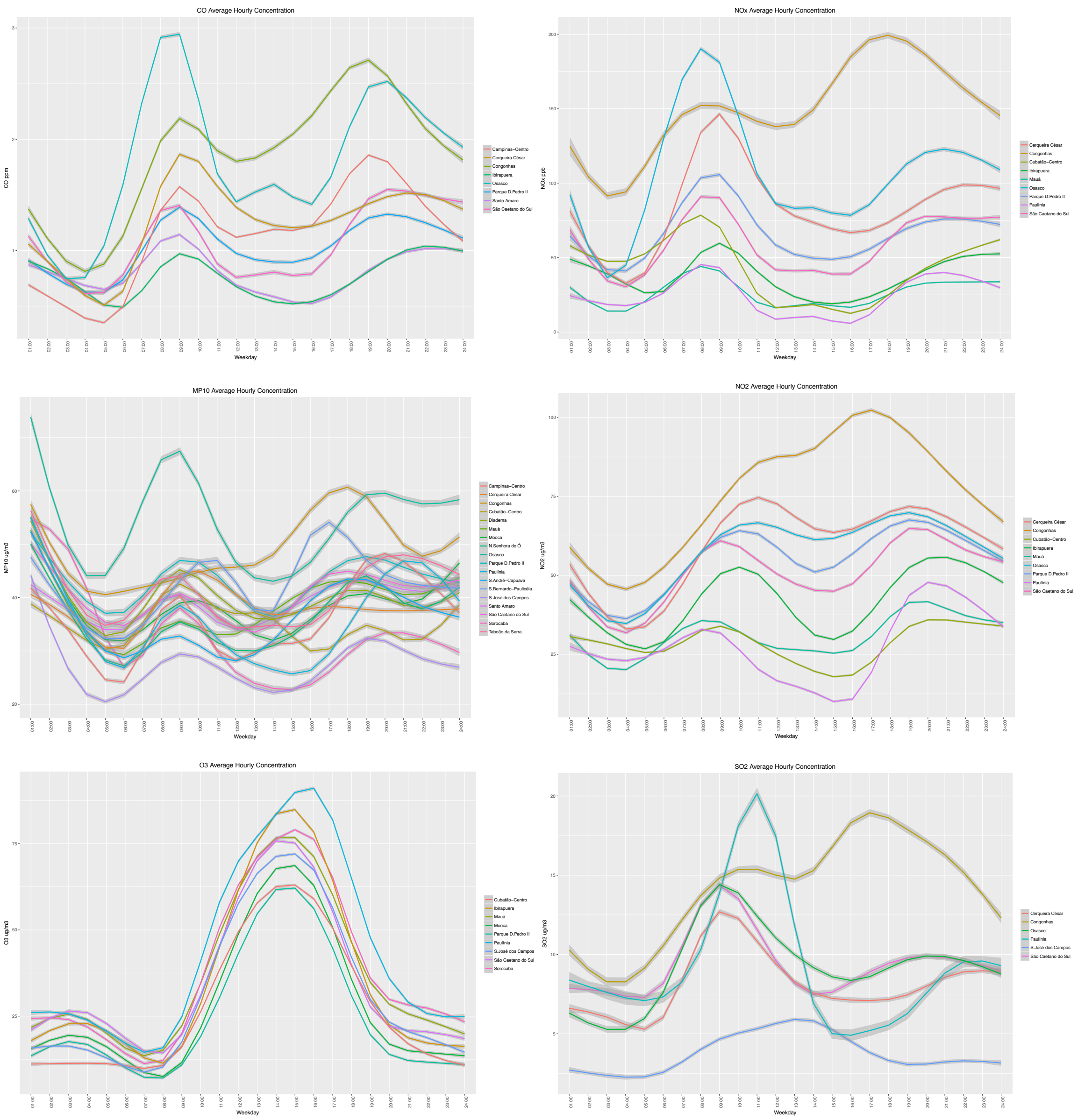
We focused on six types of air pollutant: carbon monoxide (CO), nitrogen oxides (NOx), nitrogen dioxide (NO_2), particulate matter with a diameter between 2.5 and 10 micrometers

(PM10)[67], ozone (O_3), and sulfur dioxide (SO2). Our sample includes observations from four to thirteen stations (depending on the pollutant) operating in the SPMA for the entire period of study (we excluded observations from the stations that were added or removed from the network within the study period to prevent compositional changes). For a descriptive comparison, we also observed one to five stations (depending on the pollutant) in another four metropolitan areas in the State of Sao Paulo[68]. We excluded weekends and holidays (including holiday extension days) from our pollutants sample[69], since transit strikes are observed only on workdays. We also dropped missing values (as well as zeros) from the analysis[70]. Table 3 presents descriptive statistics for all air-quality monitoring stations and respective monitored pollutants.

Figure 4 plots hourly mean concentrations in ambient air for the six selected pollutants over the course of 24 hours for workdays. The spikes in CO, MP10, NOx, NO_2, and SO_2 hourly mean concentration levels are within driving restriction periods (morning and/or afternoon) in the majority of the observed stations. O_3, which is a secondary pollutant formed in sunlight by the reaction between NOx and volatile organic compounds, peaked between the two periods. The Congonhas and Osasco stations have the highest mean concentrations for most of the selected air pollutants. However, it is important to note that the averages were taken over the entire period of study and these two locations have experienced significant decreases in air pollutant concentrations, especially for NOx, with some convergence, but they are still much more polluted than other measured sites (as shown by Figure 6).

[67] We did not include $PM_{2.5}$, as we only have data from 2011 onwards for this pollutant. However, HE; GOUVEIA; SALVO (2018)showed pairwise correlations between mean daily $PM_{2.5}$ and NOx ranging from 0.63 to 0.68.
[68] In the SO_2 sample, we excluded two stations located in the city of Cubatao in the Baixada Santista Metropolitan Area, as it is an industrial area and, hence, the means were many times above the measurement points of the other stations kept in the sample.
[69] Municipal holidays are kept in the dataset and should be controlled for in the econometric models.
[70] We also excluded any station with a too high percentage of missing information. Thus, all selected air pollutants have more than 100 thousand observations measured per station within the study period.

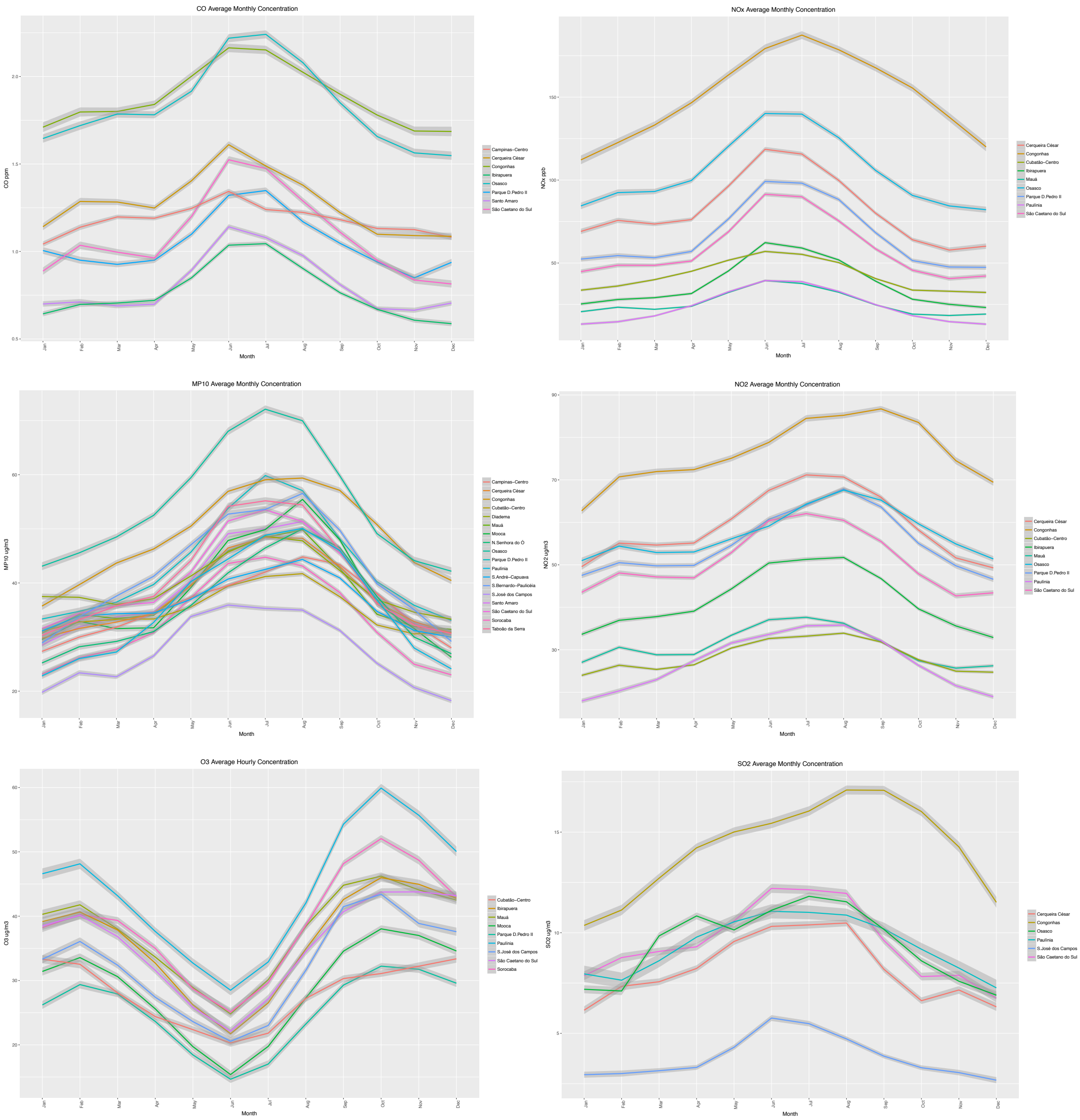
Figure 4 – Hourly mean air pollutant concentrations, 1998–2017



Source: Elaborated by the authors. Data by CETESB

Figure 5 highlights the seasonal variation in the data by plotting the monthly mean concentrations of the observed air pollutants. Weather conditions during cold/dry seasons are unfavorable for the dispersion of pollutants, thus worsening pollutant concentration levels in the ambient air. Thus, the air-quality deteriorates in the middle of the year, during the winter months. The exception again is O_3 concentration levels, which peaks in October and has its lowest mean point in June.

Figure 5 – Monthly mean air pollutant concentrations, 1998–2017

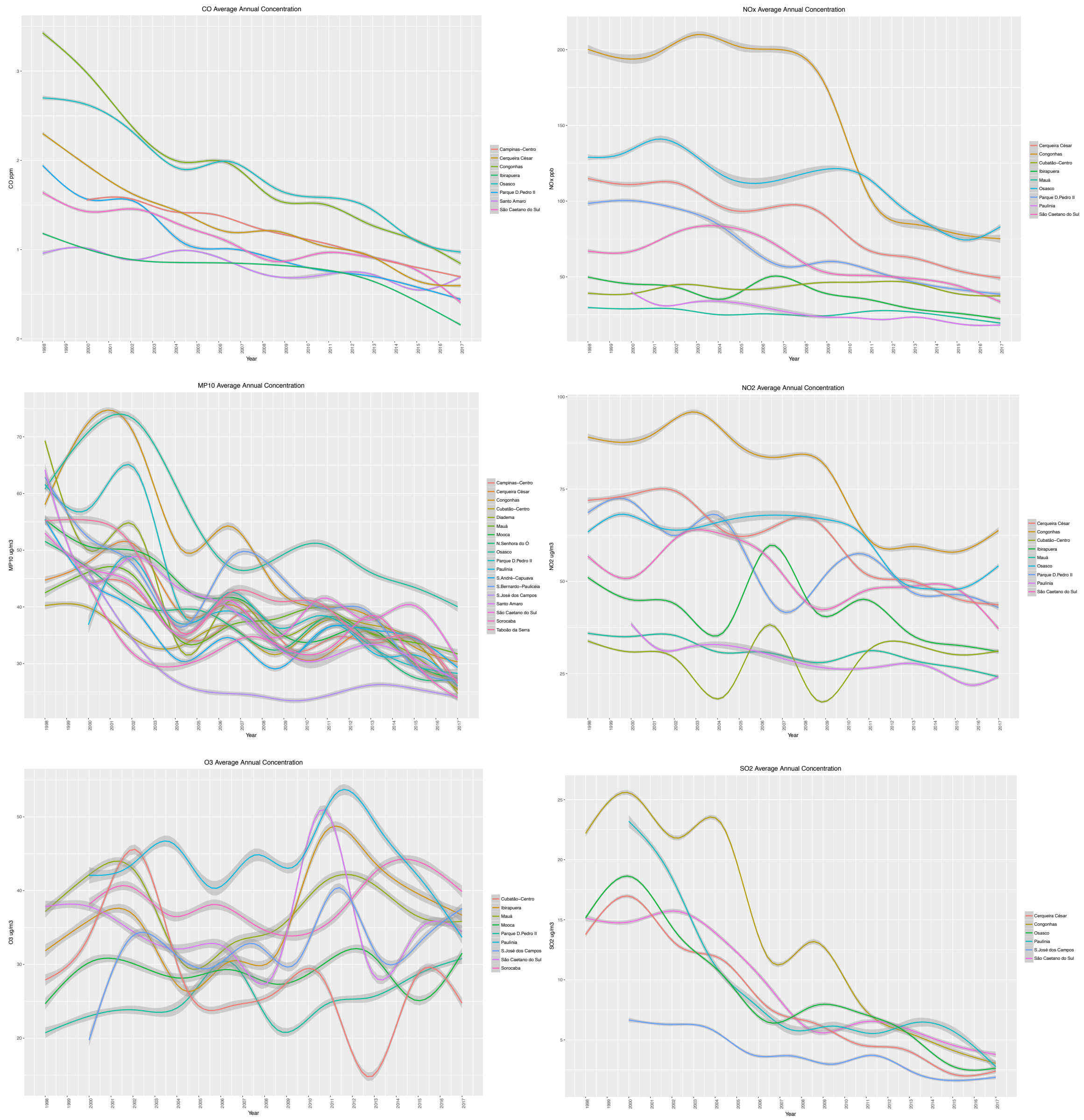


Source: Elaborated by the authors. Data by CETESB

Figure 6 shows annual mean concentration levels of selected pollutants between 1998 and 2017 at all stations included in our analysis[71]. There are clear downward trends for all observed air pollutants, except for O_3, during the study period.

[71] For some pollutants in some metropolitan areas, there are concentration measures from 2000 onwards only, and thus do not cover three bus strikes and two metro strikes in the SPMA, adding up to eight strike days observed between 1998 and 1999.

Figure 6 – Annual mean air pollutant concentrations, 1998–2017



Note: Some air-monitoring stations in other metropolitan areas only started measurement from 2000 onwards
Source: Elaborated by the authors. Data by CETESB

In sum, air pollutants measured by all monitoring stations in different sites of the SPMA and also in other metropolitan areas of the state of Sao Paulo follow somewhat similar patterns. Therefore, one station per metropolitan area can be seen to be representative for air pollution levels within the entire area and to be comparable among areas, with perhaps the exception of the Congonhas station, which is heavily influenced by airport traffic, adding to the usual traffic-related air pollution emissions observed in other sites.

5.2.3 Health

We used daily data from the Hospital Information System (SIH) of the National Health System (SUS) made available by the Brazilian Ministry of Health (MS/DATASUS)[72], which provides information about all inpatients in all Brazilian public hospitals. In particular, we collected the following information for the period between 1998–2017: main diagnosis[73] (by ICD-10 code), dates of admission and discharge (or death), place of residence (by zip code and city of residence), date of birth, gender, as well as the total cost for hospital stay (in local currency and in US dollars).

In order to examine pollution-related health outcomes, we focused on hospital admissions for respiratory system (ICD-10 codes J00–J99) and circulatory system (ICD-10 codes I00–I99) diseases. Additionally, as a placebo, we collected hospital admissions for non-traffic related injuries of external causes (ICD-10 codes T20-T98). For each type of diagnosis, we aggregated the number of hospitalizations by day of admission and inpatient zip code or city of residence, per 100,000 inhabitants[74]. Hence, we obtained daily hospital admission rates, which we examined for both the entire population, as well as for population subgroups of those under 5 years of age and over 64 years of age.

Table 4 presents descriptive statistics with the daily averages of hospital admission counts per 100,000 inhabitants for respiratory and circulatory diseases and non-traffic related injuries, as coded by the ICD-10, in the city of Sao Paulo. For comparison, we also report mean daily hospital admission rates, and respective standard deviations, for the main cities of the other four metropolitan areas of the state of Sao Paulo. We also computed the average daily diagnosis-specific reimbursement values, as claimed by the hospitals to the National Health System (SUS), in US dollars (which represents a lower bound of the associated total health costs).

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[72] The microdata, without nominal identification, is available at: ftp://ftp.datasus.gov.br/dissemin
[73] We did not use secondary diagnosis to complement the data due to a large percentage of missing information.
[74] We did not have population by zip code to scale the data accordingly, thus the hospitalization rate is by cities' annual estimated populations, available at: https://ww2.ibge.gov.br/home/estatistica/populacao/estimativa2017/estimativa_tcu.shtm

Table 4 – Descriptive statistics for daily hospital admission rates, 1998–2017

Main diagnosis	City	MA	Mean daily rates	Std.Dev.	Mean daily cost (US$)	Std.Dev. (US$)
Respiratory diseases	São Paulo	RMSP	1,25	0,39	$ 55.316,82	$ 34.444,46
	Santos	RMBS	1,38	0,66	$ 2.850,48	$ 3.405,68
	Campinas	RMC	1,29	0,49	$ 7.116,64	$ 6.554,48
	Sorocaba	RMS	1,53	0,77	$ 2.752,04	$ 2.610,64
	São José Dos Campos	RMVP	1,44	0,73	$ 2.749,56	$ 3.005,23
Circulatory diseases	São Paulo	RMSP	1,41	1,26	$ 164.996,54	$ 158.397,33
	Santos	RMBS	1,53	0,75	$ 4.684,67	$ 4.872,51
	Campinas	RMC	1,28	0,47	$ 13.889,22	$ 10.875,06
	Sorocaba	RMS	1,40	0,66	$ 5.352,11	$ 4.837,13
	São José Dos Campos	RMVP	1,32	0,60	$ 7.116,97	$ 6.835,83
Non-traffic related external causes	São Paulo	RMSP	0,20	0,19	$ 17.120,58	$ 22.034,81
	Santos	RMBS	0,37	0,20	$ 991,75	$ 1.747,11
	Campinas	RMC	0,21	0,12	$ 1.624,52	$ 2.848,33
	Sorocaba	RMS	0,27	0,15	$ 1.315,65	$ 2.186,58
	São José Dos Campos	RMVP	0,29	0,18	$ 1.320,38	$ 2.151,65

Note: Hospital admission rates per 100,000 inhabitants based on cities' annual estimated populations. The main cities of the five metropolitan areas (MA) included are: Sao Paulo (RMSP), Campinas (RMC), Sorocaba (RMS), S. José dos Campos (RMVP), and Santos (RMBS). The numbers are in Brazilian format, which uses comma rather than dot as decimal separator, and dot rather than comma as thousand separator.

Source: Elaborated by the authors. Data by DATASUS and IBGE

Table 5 shows the data for the city of Sao Paulo by gender and age subgroups. Children under 5 years old are the most vulnerable group for respiratory illness episodes. They accounted for 43% of all hospital admission due to respiratory causes. The elderly population, by contrast, had a higher percentage of circulatory illness compared to respiratory episodes.

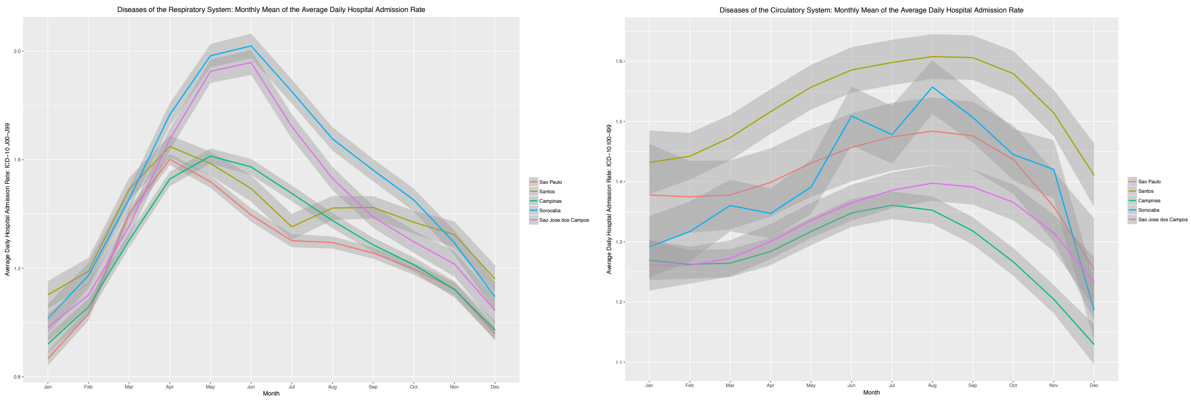
Table 5 – Hospital admission by gender and race in the city of Sao Paulo, 1998–2017

Main diagnosed disease		Respiratory	Circulatory
Gender	Male	54%	51%
	Female	46%	49%
Age	4 years or less	43%	1%
	5 to 64 years	39%	57%
	65 years or more	18%	42%

Source: Elaborated by the authors. Data by DATASUS

Figure 7 plots the monthly mean hospital admission rates by month for the city of Sao Paulo and the main cities of the other four metropolitan areas of the state of Sao Paulo. The hospitalization for the diseases of the respiratory system (left-hand side panel) peaks between April and June, and for the diseases of the circulatory system (right-hand side panel) between July and September. Higher hospital admission rates during the winter season was associated with higher air pollution levels, but also with other environmental factors, such as allergens.

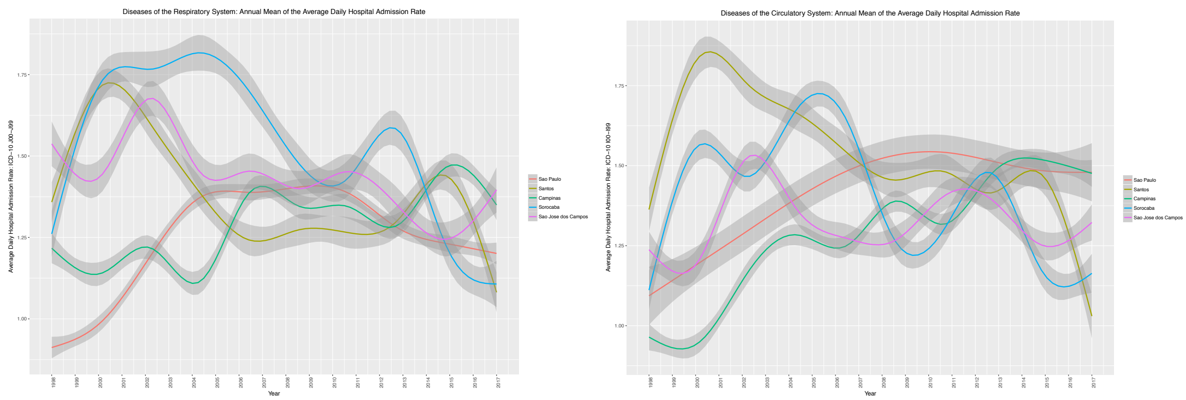
Figure 7 – Monthly mean hospital admission rates, 1998–2017



Source: Elaborated by the authors. Data by DATASUS and IBGE

However, differently from the most air pollutant trends, there was no clear decline in hospital admission counts over time for respiratory and circulatory system diseases, as demonstrated in Figure 8. On average, they varied between 1.00 and 1.75 daily hospitalizations per 100,000 inhabitants due to respiratory and circulatory illness episodes.

Figure 8 – Annual mean hospital admission rates, 1998–2017



Source: Elaborated by the authors. Data by DATASUS and IBGE

It is important to note that we were not able to obtain daily emergency room visits (only available by Datasus on a monthly basis), which would better capture the health effects of air pollution, since a large fraction of, especially respiratory-related, diseases do not require overnight stays in hospitals. SCHLENKER; WALKER (2016) demonstrated that estimates based on more commonly used inpatient admission or discharge datasets substantially underestimate the morbidity impacts of air pollution, relative to estimates that combine information on hospital admissions with emergency room visits. Therefore, the daily hospital admission rates represent a lower bound of the health impact, as it ignores illness episodes that do not result in hospitalization.

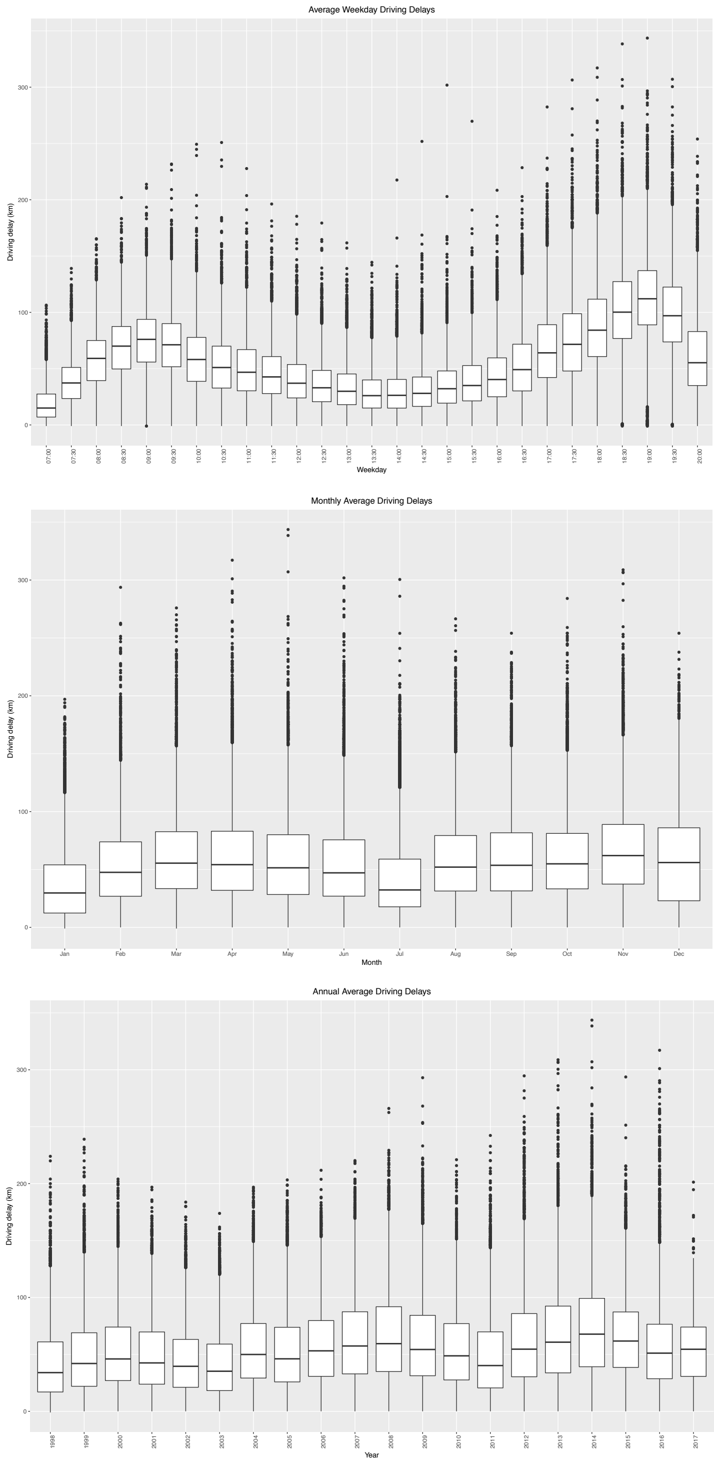
5.2.4 Congestion

We obtained data on road congestion, as a proxy for traffic volume, from the city of São Paulo's traffic authority – the Sao Paulo Traffic Engineering Company (CET)[75]. The traffic authority currently monitors 868 kilometers[76] of the city's total road grid, recording measurements of driving delays every half-hour between 7am–8pm on weekdays. The CET's operational agents measure the length, in kilometers, of driving delays in the monitored road lanes. Thus, a null value means free-flow in the monitored roads. On the freeways, any stoppage in the flow is recorded as driving delay. On the arterial and collector roads, driving delays are considered when the green traffic light time is insufficient for traffic dispersion. For the study period, we only have an average of all measurements for the whole city and, thus, do not have data by road segment[77]. The driving delay variable aims to quantify the overall traffic condition in the city.

The panels in Figure 9 show driving delay averages in the city of Sao Paulo over the course of a weekday, over the months of the year, and over the years of the study period. On an average weekday, there are two traffic congestion peaks, at 9am. and 7pm. Both peaks are within driving restriction periods, but evening traffic congestion averages and maximums are much more pronounced. The driving delays during rush hours are, on average, nearly 100 km, but on a particularly traffic congested day they can reach as much as 350km. Traffic congestion is lower during the school holiday months (January and July). Over the years, overall driving delay averages have increased slightly, but the maximums recorded have been reaching higher levels more recently.

[75] We requested the data directly from the CET under the law that guarantees public access to state administrative information (i.e. freedom of information law).
[76] In 1997, there were only 250 kilometers of monitored road lanes, thus the total length of monitored road grid has been steadily increased over the study period, reaching 840 kilometers in 2007.
[77] See HE; GOUVEIA; SALVO (2018) for the use of Sao Paulo congestion data by segment from 2008 to 2013.

Figure 9 – Driving delay averages across time in the city of Sao Paulo, 1998–2017



Source: Elaborated by the authors. Data by CET

The congestion data adds to the descriptive analysis in the event study of the next section. However, this dataset will not be used in our econometric models aimed at estimating the causal impact of transit strikes on the outcomes of interest.

5.2.5 *Controls*

We used temperature, relative humidity, wind speed, and precipitation in the empirical analysis to control for the direct effects of weather conditions on air pollution levels and health outcomes. Many of the CETESB's air-quality monitoring stations across metropolitan areas measure these weather variables hourly, except for precipitation, which were obtained from the Water and Electricity Department (DAEE)[78] at a daily frequency. Since the assignment of weather conditions to zip code residential location is subject to substantial measurement error[79], we instead took averages across all stations in the metropolitan area to obtain hourly mean temperature, humidity and wind speed variables[80]. Likewise, we averaged the daily total rainfall across DAEE's observatory stations that had measures for the entire period of study.

Additionally, we control for the days in which the driving restriction scheme ("*rodizio*") were suspended, except those due to transit strikes. Besides transit strike days, the traffic authority suspends driving restrictions during summer school holidays (end of December to mid-January), major events (e.g. World Cup), and other city disruptions caused by floods, blackouts, protests, general strikes, public security occurrences etc. In the 20-year study period, the driving restriction was cancelled on 451 weekdays (in addition to the 34 days of transit strikes). Furthermore, we controlled for the 109 days in 1998 in which the State's more restricted driving restriction scheme (from 7am–8pm covering the city of Sao Paulo and other nine municipalities of the SPMA) overlapped with municipal driving restrictions (from 7am–10am and 5pm–8pm applied only to the extended CBD of the city of Sao Paulo). We also control for national and local holidays, adding up to 331 days.

5.3 Event Study

Before we move on to actually estimate the effects of interest employing econometric methods in the next section, we present an event study analysis in order to visually inspect the existence of the "events" themselves – i.e. the discontinuities in the outcomes brought about by the transit strikes. It is worth noting that event study analyses have been recently employed in applied microeconomics researches (e.g., ALMOND; HOYNES; SCHANZENBACH, 2011; FUEST; PEICHL; SIEGLOCH, 2018; HOYNES; MILLER; SIMON, 2015; HOYNES;

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[78] Available at: http://www.hidrologia.daee.sp.gov.br/ . Total daily precipitation is measured by one millimeter of rainfall (which is the equivalent of one liter of water per square meter) accumulated in 24-hour period.
[79] For example, the assignment of weather conditions to inpatient residency zip codes has the potential to improve accuracy, but may also worsen it if people travel beyond their zip code locations (MORETTI; NEIDELL, 2011).
[80] Some studies use hourly maximum and minimum instead, and adjust different function forms for weather controls (see, e.g., KNITTEL; MILLER; SANDERS, 2016; MORETTI; NEIDELL, 2011; RIVERS; SABERIAN; SCHAUFELE, 2017; SCHLENKER; WALKER, 2016).

SCHANZENBACH; ALMOND, 2016; MACHADO; SZERMAN, 2018) in order to provide further evidence of the validity of the research design. In general, they are used to tackle distrust regarding the presence of pre- or post-trends that could lead to spurious results.

However, in the economics literature, the event study methodology has a long history and has been widely used for empirical research in the accounting and finance literature[81]. Becoming especially ubiquitous in capital markets research, the method essentially measures the impact of a specific event on the value of a firm, based on the underlying assumptions that capital markets are sufficiently efficient and the event cannot be anticipated (i.e. it is exogenous)[82]. Thus, the effect of the event is reflected immediately on the security price of the firm over a relatively short period of time. These events often are mergers and acquisitions, issues of new debt or equity, or earnings and macroeconomic variables announcements, but they have increasingly included news about firms' environmental performance, such as toxic release, pollution inventories, accidents and disasters, or disclosure of superior environmental practices (e.g., CAPELLE-BLANCARD; LAGUNA, 2010; DASGUPTA; LAPLANTE; MAMINGI, 2001; HAMILTON, 1995; KONAR; COHEN, 1997; OBERNDORFER et al., 2013).

Nevertheless, as noted by SANDLER; SANDLER (2014), the event study methodology borrowed from the finance literature[83] has not been rigorously modified to meet the different contexts of applied microeconomics disciplines, which widely depart from the origins of the method. In particular, they aim at looking into the dynamics around the given event, similar to another descriptive technique found more often in the public health literature referred as interrupted time series[84]. The interrupted time series is graphed along event time, and the

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[81] For literature and methodology reviews, see CORRADO (2011) and MACKINLAY (1997).

[82] To be more precise, in finance, an event study tracks the following general course of analysis (MACKINLAY, 1997): (i) define the event of interest and the corresponding "event window" (which might range from a few hours to many days subsequent of the event); (ii) delimit the "estimation window" (common choice is using the period prior to the event window); (iii) set the null hypothesis in which the given event has no impact on the behavior of the security price of the firm; (iv) estimate the abnormal security price return (i.e. the actual ex-post return over the event window minus the normal return over the event window, in which the normal return is the excepted return without conditioning on the event taking place). In the model, for firm i and event window t, $AR_{i,t} = R_{i,t} - E(R_{i,t} |X_t)$, $AR_{i,t}$ is the abnormal return, $R_{i,t}$ is the actual return, $E(R_{i,t} |X_t)$ is the normal return, and X_t is the conditioning information for the normal return model. The normal return is often modelling in two ways, either with a constant mean return model where X_t is a constant, or with a market model where X_t is the market return (which should increase precision by removing the portion of the return that is related to variation in the market's return).

[83] The event study method has been applied also in other areas of economics and in a variety of disciplines, including history, law, management, marketing, and political science (CORRADO, 2011).

[84] Further reading on interrupted time series designs, see the textbook "Experimental and Quasi-experimental Designs for Generalized Causal Inference" by SHADISH; COOK; CAMPBELL (2002) and "Interrupted Time Series Regression for the Evaluation of Public Health Interventions: A Tutorial" by BERNAL; CUMMINS; GASPARRINI (2017).

observations on the graph are equal to the averages of the outcome at each time period. When these averages are regression-adjusted, the interrupted time series is equivalent to an event study. In this fashion, the event study approach is also similar to the graphical representation found in the regression discontinuity design using time as the running variable. However, even though a graphical event study demonstrating the existence of a clean event is informative and provides visual presentation of the outcome at the time of the event, and its dynamics around that time, it is not a substitute of a more rigorous identification of the impact of the event. In fact, in the applied microeconomics fields, event study analyses are often presented alongside with more robust techniques used for causal inference, such as difference-in-differences, regression discontinuity design or instrumental variables and, thus, is primarily deployed as a graphical descriptive procedure.

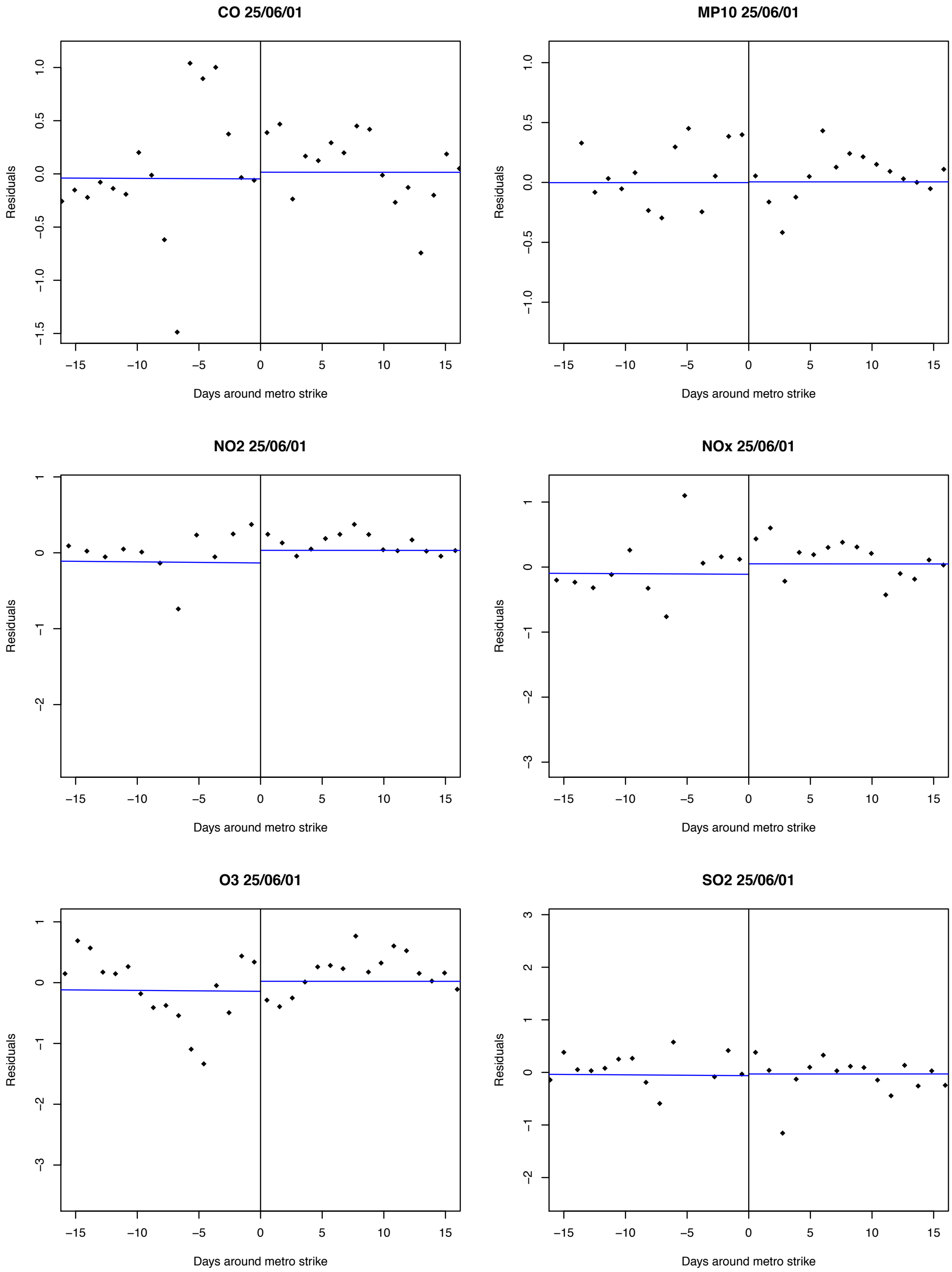
5.3.1 Event Study Analysis

We regressed the daily average of the six selected air pollutants (CO, NOx, NO_2, MP10, SO_2, O_3), the daily hospital admission rates (by respiratory and cardiovascular conditions, those that terminated in death, and age group), and the daily average of driving delays with the most extensive set of control variables, including weather, driving restrictions, holidays, and time fixed effect controls for all the observations during the period of study in the SPMA. We then plotted the residuals of the average daily outcomes against the fifteen days before and fifteen days after a metro or bus strike normalized to zero. Thus, data points represent daily averages of unexplained variations in air pollution concentrations, hospital admissions and congestion levels.

The panels in Figures 10–15 present all residuals of the outcomes of interest around metro or bus strike events. We would expect to see the observations just on the right-hand side of the vertical line that marks the beginning of the strike to jump upwards for one or two days, depending on the duration of the strike and persistence of the effect. As the treatment is turned off, the time trend curve should be flatter once exposure to the shock fades. However, the outcomes seem to be influenced by other variables that we do not observe and, hence, cannot control for. In fact, the effects of the different strikes are far from homogeneous and even though in some instances, we do see the expected spikes in the residuals, in many others we cannot observe any effect or even effects in the opposite direction that were predicted[85].

[85] In an earlier version of this analysis, we created air pollution measures for morning and evening peaks by taking the averages of the hourly pollutant readings between 7am–10am and 5pm–8pm and plotted them by day for the entire study period. However, as we do not observe tailpipe emissions directly, but rather pollutant concentration

Figure 10 – Air pollution outcomes around a metro strike event in the SPMA



Notes: The panels in the figure plot the residuals from the regression of the six selected pollutants' daily means (in logarithms) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected metro strike.
Source: Elaborated by the authors.

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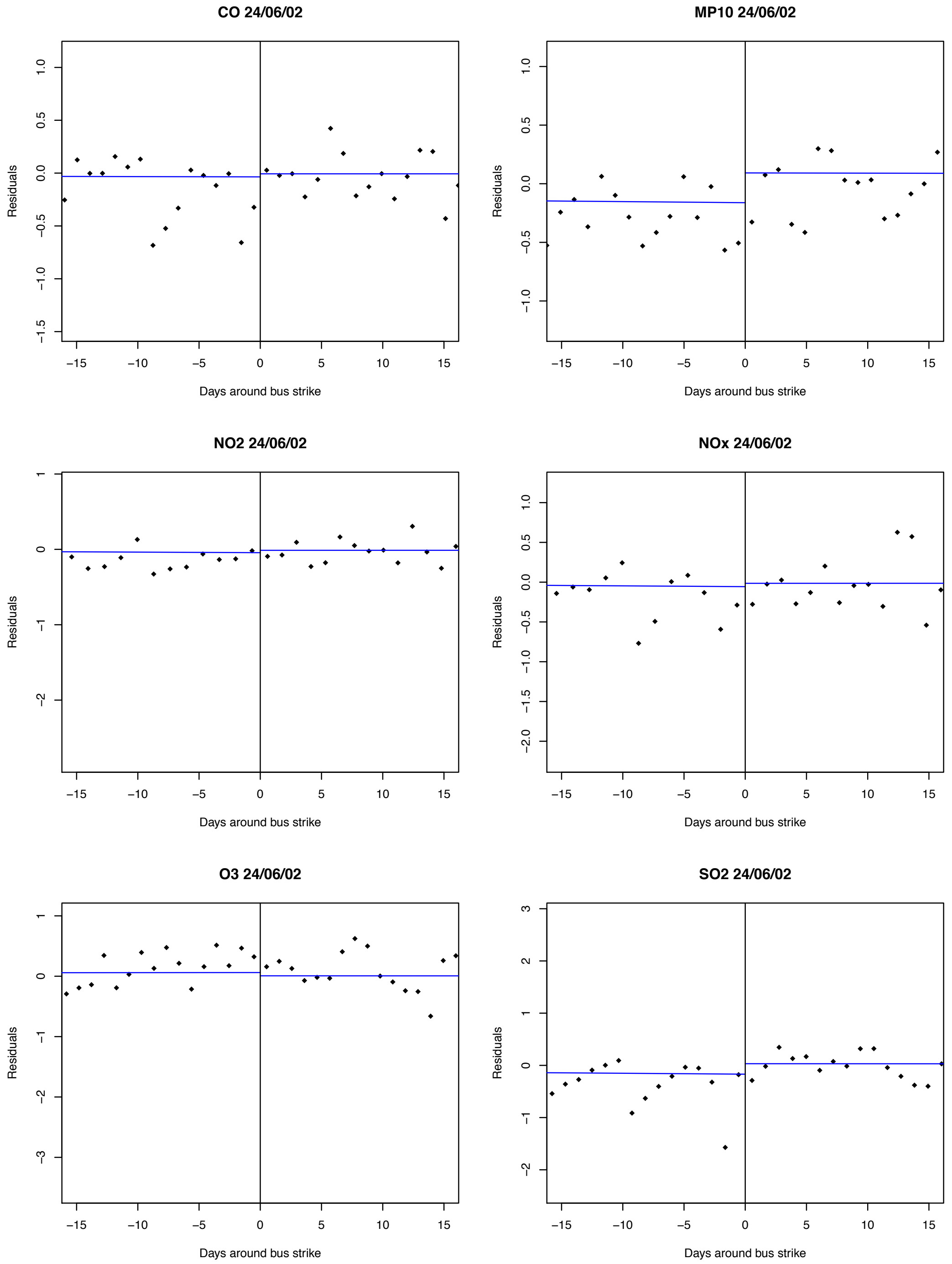
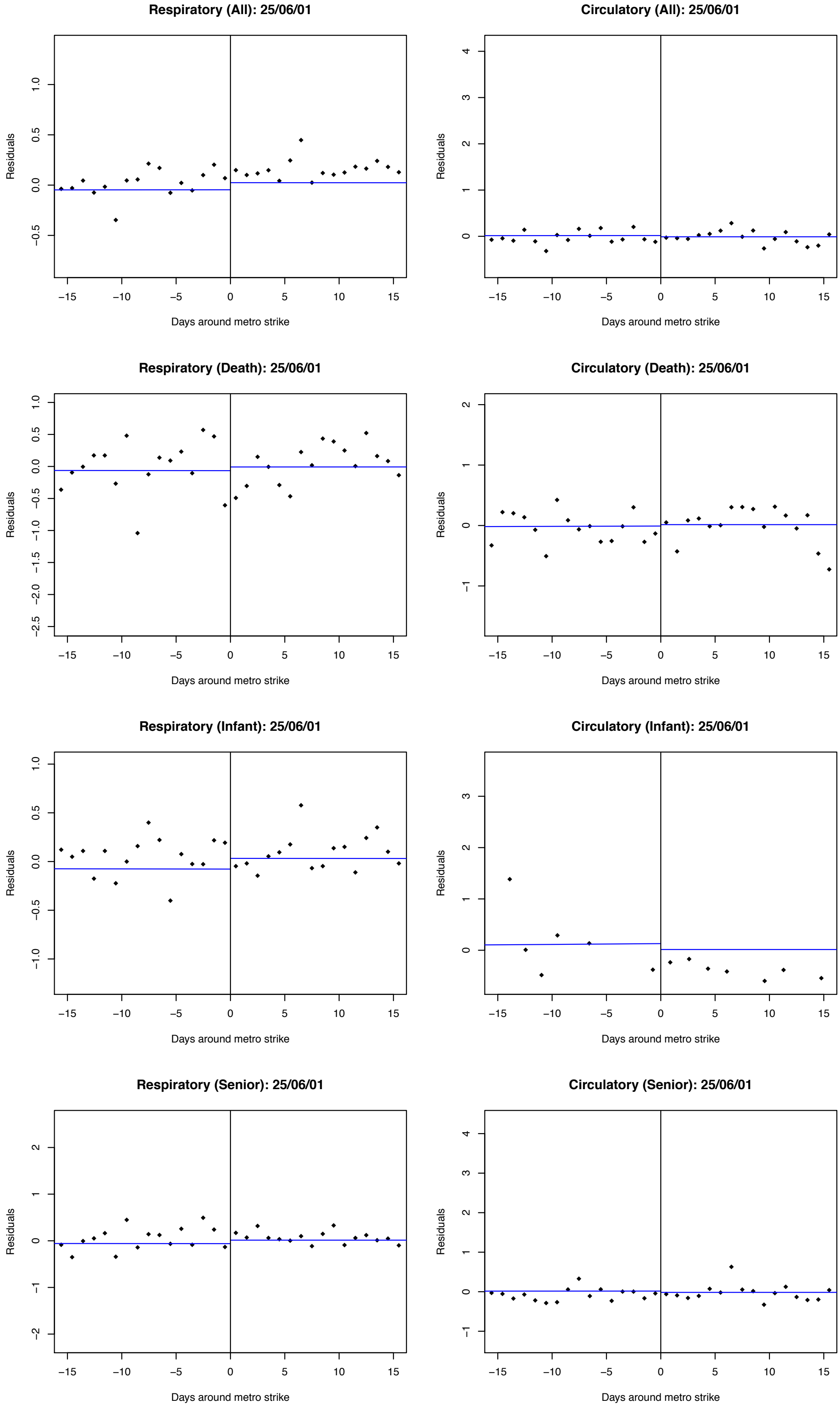
levels in ambient air, which is mostly affected by weather and dispersion conditions, we were not able to check for the transit strike effects in graphical inspections before controlling for weather conditions and other observable factors that might affect urban air pollution concentrations. Figures 16–19 in the Appendix display the morning and evening peak mean pollutant concentrations in the SPMA and show all transit strike episodes with vertical lines.

Figure 11 – Air pollution outcomes around a bus strike event in the SPMA



Notes: The panels in the figure plot the residuals from the regression of the six selected pollutants' daily means (in logarithms) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected bus strike.
Source: Elaborated by the authors.

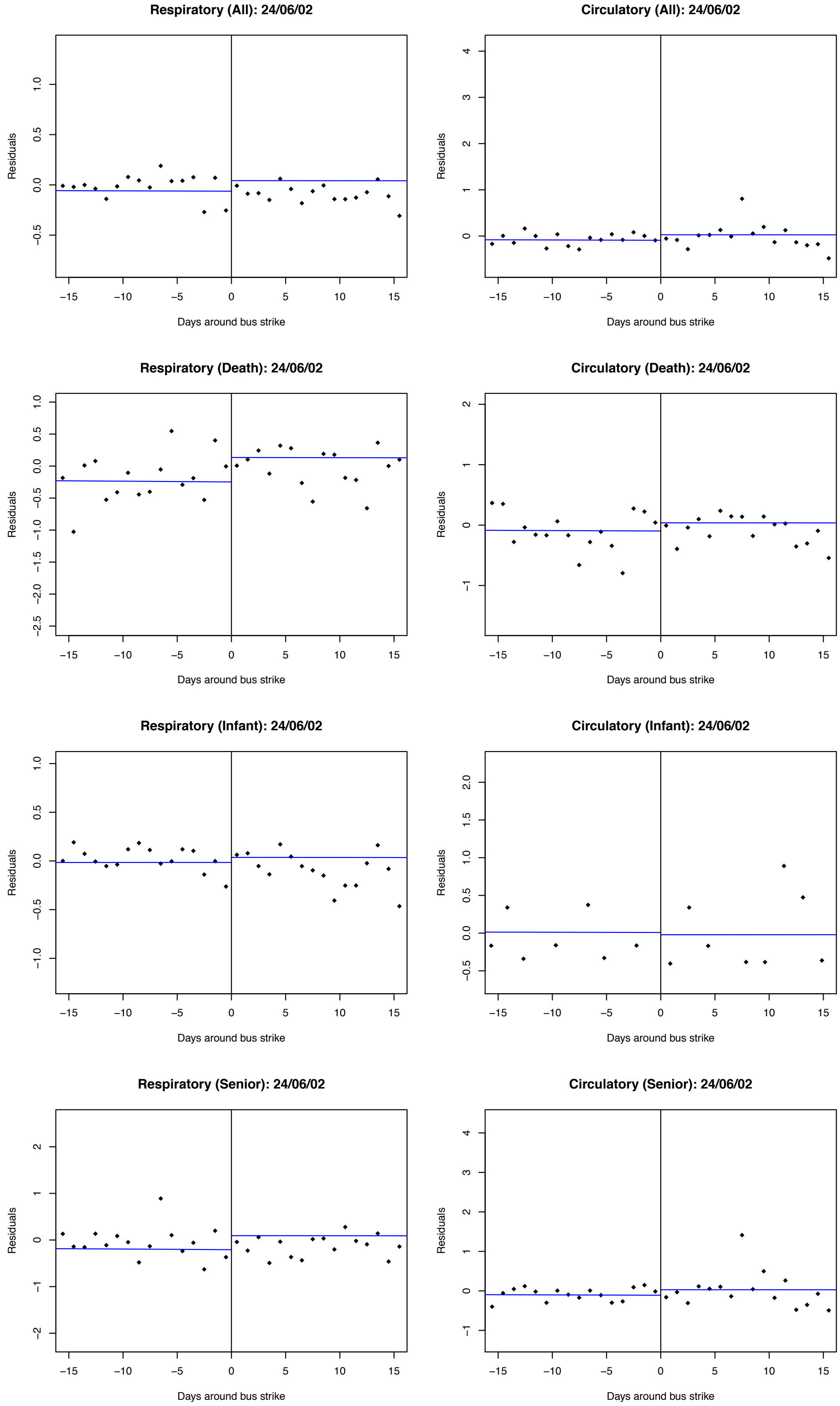
Figure 12 – Health outcomes around a metro strike event in the SPMA



Notes: The panels in the figure plot the residuals from the regression of daily hospital admission rates by respiratory and cardiovascular conditions, those that terminated in death, and age group (in logarithms) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected metro strike.

Source: Elaborated by the authors.

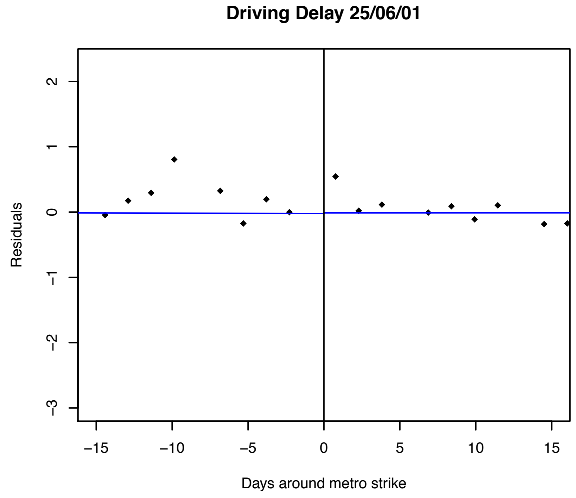
Figure 13 – Health outcomes around a bus strike event in the SPMA



Notes: The panels in the figure plot the residuals from the regression of daily hospital admission rates by respiratory and cardiovascular conditions, those that terminated in death, and age group (in logarithms) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected bus strike.

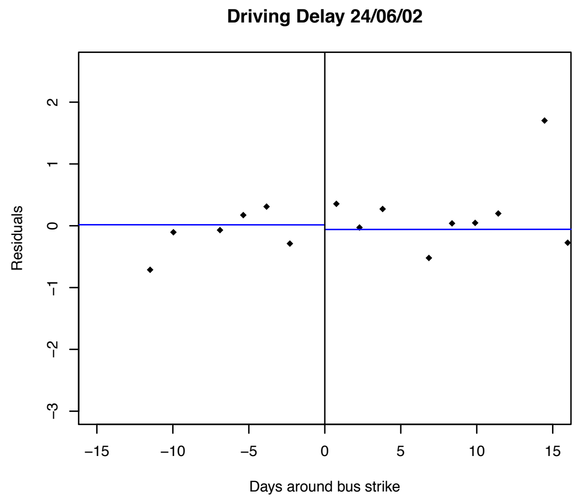
Source: Elaborated by the authors.

Figure 14 – Congestion outcomes around a metro strike event in the SPMA



Notes: The figure plots the residuals from the regression of driving delays' daily means (in logarithm) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected metro strike.
Source: Elaborated by the authors.

Figure 15 – Congestion outcomes around a bus strike event in the SPMA



Notes: The figure plots the residuals from the regression of driving delays' daily means (in logarithm) on the daily means of temperature, humidity, wind speed (including up to third-order polynomials and interactions), total daily rainfall, and dummies for driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions, and for holidays. It also incorporates dummies for a range of time controls (day-of-week, week-of-year, year). The black vertical line marks the start of the selected bus strike.
Source: Elaborated by the authors.

For the sake of brevity, we only present here in the main text a few examples of strike events. The event study graphs for all other metro and bus strike episodes can be found in the online Appendix[86].

[86] All event study graphs and R codes that generate them are available at: https://drive.google.com/open?id=1REw-ychmksGUILAaKPjI5FhrLP5BUA6c

5.4 Econometric Methods

The econometric methods that feature most prominently in quasi-experimental researches are instrumental variables, differences-in-difference models and regression discontinuity designs (ANGRIST; PISCHKE, 2010). What makes the instrument variable valid is a proper (institutional and data-driven) justification of the exclusion restrictions and assumptions of as-good-as-random treatment assignments. The difference-in-differences estimator compares the evolution of outcomes between groups affected differently by policy changes, assuming that any deviations from trends are related to changes in policy. The regression discontinuity estimates of causal effects are driven by the key assumption that individuals (or any other unit of observation) are otherwise similar on either side of the discontinuity cutoff (or that any difference can be controlled using a smooth function of the running variable).

In particular, the so-called regression discontinuity in time (RDiT), in which time is the running variable, has appeared frequently in environmental economics literature (e.g., AUFFHAMMER; KELLOGG, 2011; BENTO et al., 2014; CHEN; WHALLEY, 2012; DAVIS, 2008). It has been adapted from the more standard cross-sectional regression discontinuity design (RDD), but some particular features of the "typical RDiT" impose some additional challenges, which are not always explicitly addressed in current applications[87]. First, in general, there is no cross-sectional variation in policy implementation and, thus, estimates are based on observations far from the cutoff, in stark contrast to a cross-sectional RDD, in which bandwidth estimation tends towards zero even as the sample size increases. Thus, due to dependence on time-series variation for identification, there is a bias/precision trade-off in increasing sample size by increasing time rather than the number of cross-sectional observations. Second, in these applications, the data is usually available at an hourly or daily frequency over long periods of time, but estimates may be biased if they do not account for the time-series properties of the data, such as serial correlation. Third, there may be many time-varying confounders, but they must be assumed to change smoothly across the cutoff. However, there are recurring covariates that are discontinuous in time, such as days of the week or weather conditions, and thus, they need to be included as controls beyond the use for improving precision. Finally, the common density test to account for sorting or bunching near the cutoff (i.e. to test for manipulation of treatment) becomes irrelevant in these settings, in which the density of the running variable is uniform. In this sense, the regression discontinuity

[87] See HAUSMAN; RAPSON (2017) for a discussion on some of the caveats in the empirical applications of regression discontinuity in time (RDiT) framework.

in time is closer to an event study[88], as presented in the previous section, than to a standard cross-sectional RDD.

In the environmental-health literature, in contrast, the use of instrumental variables has been the main strategy to capture variation in air pollution levels that are unrelated to unobserved determinants of health (e.g., DESCHÊNES; GREENSTONE; SHAPIRO, 2017; KNITTEL; MILLER; SANDERS, 2016; MORETTI; NEIDELL, 2011; SCHLENKER; WALKER, 2016), freeing researchers from the need to control for a range of environmental, socioeconomic and other behavioral factors. Moreover, this strategy overcomes measurement error concerns that arise in trying to measure the individual's exposure to pollution[89] and subsequent avoidance behavior. However, in our case, it is hard to argue that transit strikes would be a good instrumental variable as they are most likely to be not only correlated with the causal variable of interest - air pollution outcomes, but also with other determinants of the dependent variable - cardio-respiratory health outcomes. Differently from other applications in the related literature (HE; GOUVEIA; SALVO, 2018; MORETTI; NEIDELL, 2011; SCHLENKER; WALKER, 2016), and even though the source of air pollution variation might be as good as randomly assigned, the arguably exogenous pollution shocks are likely to be known by the population and also might have an effect on health outcomes other than through the first-stage channel (i.e. it fails the exclusion restriction assumption).

Instead, in the spirit of BAUERNSCHUSTER; HENER; RAINER (2017) and RIVERS; SABERIAN; SCHAUFELE (2017), we exploited time series and cross-sectional variations in the panel data by comparing outcomes in affected and non-affected locations on strike and non-strike days in order to control for unobserved but fixed omitted variables. That is, we proceeded to estimate the effect of transit stoppage on air pollution and health using a difference-in-differences approach with location and time fixed effects, and including a vector of location- and time-varying weather covariates.

[88] Nevertheless, the "standard" event study, particularly in the finance literature as discussed in the previous section, differ in some marked ways from the regression discontinuity in time design. Event studies tend to focus on short windows around the event rather than using long time horizons. Moreover, they often use a large cross-sectional sample rather than a single cross-sectional unit. By contrast, they implicitly assume there are no unobservable variables correlated with time and, thus, do not use high-order polynomial controls in time (HAUSMAN; RAPSON, 2017).

[89] The most common approach for measuring an individuals' exposure is to assign pollutant concentration levels data from air quality monitors to the individuals' residential location. This somewhat crude approach is subject to measurement error, given distance to measurement points and non-uniform pollutant dispersion as well as the individuals' allocation in space and time throughout the day.

5.4.1 Difference-in-differences

It is worth remembering that the difference-in-differences approach is a version of the fixed effects model using aggregate data, and therefore, the source of the omitted-variable bias must be unobserved variables at a more aggregate or group level (ANGRIST; PISCHKE, 2009). The core in this setup is the additive structure for potential outcomes in the non-treatment location. In other words, in the absence of treatment, the outcomes of interest are determined by the sum of a time-invariant location effect and a time effect that is common across locations. The key underlying assumption is that outcome trends should be the same in both locations in the absence of treatment, and treatment should induce a deviation from this common trend. Nevertheless, the treatment and control groups may be different, as the differences among them should be captured by the location fixed effects, which parallel the unobserved individual effect in fixed effect models using disaggregated panel data.

In our data set we do not observe transit strikes in other metropolitan areas of the state of Sao Paulo other than the SPMA. In fact, the SPMA is the only metropolitan area in the state of Sao Paulo having both subway and rail networks[90]. However, there is no indication that any bus strikes occurred simultaneously in other metropolitan areas on the same strike days as in the SPMA. Moreover, due to differences in transit infrastructure provision, the SPMA has a much higher reliance on transit modals than the other four metropolitan areas considered in this study. The driving restriction policy has also been implemented exclusively in the SPMA.

Therefore, the metropolitan areas in the control are indeed different from the SPMA, but if we can maintain the assumption that they share common trends in the measurements of air pollution and health outcomes – as we can investigate them observing Figures 4–8 of the descriptive statistics section above –, the metropolitan areas, other than the SPMA, may provide a good estimate of counterfactual air pollution concentrations and health rates in the SPMA in the absence of a transit provision change. Hence, the identification strategy relied on the treatment effect (i.e. the transit strikes) inducing a sharp but transitory deviation from these common underlying trends.

The disparities among metropolitan areas are controlled for by adding dummies for each location. Additionally, as we have hourly air pollution data and daily health data for the entire twenty-year period of the study, we included in the estimated models, dummies for hour-of-

[90] There is a VLT under construction in the Baixada Santista Metropolitan Region (RMBS), which has been operating partially since 2016. The RMBS had a single rail line running from 1990 to 1999, when it was closed down. The Campinas Metropolitan Region (RMC) had a single VLT line operating from 1990 to 1995.

day, day-of-week, week-of-year and year. The full range of time fixed effects, which can also be flexibly allowed to vary by location, are intended to capture hour-of-day and day-of-week patterns, and seasonal and long-term time trends. Furthermore, we controlled for weather conditions as a source of omitted area-specific trends, by including a vector of location- and time-varying weather covariates.

5.4.2 Model Specification

We leveraged from the large dataset comprised of hourly air pollutant concentration measurements and daily hospital admission counts in more than one metropolitan area across multiple years to compare outcomes on strike days with outcomes on "similar" non-strike days in affected and non-affected locations, by estimating the following main model[91]:

$$(3) \qquad Y_{i,t} = \alpha_i + \gamma_t + \beta_1 STRIKE_{i,t} + \beta_2 X_{i,t} + \varepsilon_{i,t}$$

where $Y_{i,t}$ is hourly air pollutant concentration levels, observed in metropolitan area i[92] on time t. The model is estimated separately for CO, NOx, NO_2, MP_{10}, SO_2, O_3 in logarithms[93]. Time t is determined by a particular year, month, day and hour. $STRIKE_{i,t}$ is a dummy variable equal to one when a strike was in effect in metropolitan area i on time t, and zero otherwise[94]. Similar equations were estimated using dummy variables for multiple strikes of various transit modes, namely metro and bus strikes, total and partial network strikes, full-day and part-day strikes, and one-day and multiple-day strikes.

We also estimated a similar equation (3) for daily hospital admission rates, also in logarithms, separately to hospitalizations due to respiratory and circulatory diseases, those that terminated in death, and by age group for those under five years of age or over 64 years of age, in metropolitan area i[95] on time t. As the dependent health variables are measured daily, time t

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[91] All the estimated models can be replicated using the R codes available at: https://drive.google.com/open?id=1REw-ychmksGUILAaKPjI5FhrLP5BUA6c

[92] For the SPMA, where there are more than one monitoring station per selected air pollutant, we used the closest station to the city center (i.e. Cerqueira Cesar for all pollutants, expect O_3, for which we used the Ibirapuera station). RIVERS; SABERIAN; SCHAUFELE (2017) took a similar approach, whereas CHEN; WHALLEY (2012) took the average across monitoring stations, BAUERNSCHUSTER; HENER; RAINER (2017) considered outcome variables at the monitor-level when there was more than one station per city, weighting regressions by the inverse of the number of observations in each city, and LALIVE; LUECHINGER; SCHMUTZLER (2017) interpolated monitor data with inverse distance weighting to a one square kilometer grid and then computed county averages.

[93] Any zero-concentration value in the original dataset we deemed as missing information.

[94] In this case, only SPMA can have a value equal to one.

[95] We considered hospital admissions only for the main city of each metropolitan area (namely, Sao Paulo, Santos, Campinas, Sorocaba, and Sao Jose dos Campos), disregarding other municipalities in the metropolitan areas, as hospital coverage varies widely among smaller municipalities.

comprises year, month and day. We also assessed possible delayed impacts on health in the day following transit strike episodes, as the literature has long demonstrated the lag association of health outcomes following rises in air pollution levels.

The coefficient of interest, β_1, measures the changes in air pollution concentrations and cardio-respiratory hospitalizations as a result of transit strikes. Reduced public transit provision leads, presumably, to increases in automobile emissions, ambient air pollution, and air pollution-related illness. Hence, we should expect this coefficient to be positive. Therefore, β_1 is a reduced form coefficient that incorporates the technological and behavioral factors that rule the relationship between public transit, private vehicles, air pollution and health.

We controlled for temporal variation in pollution and health outcomes by including time fixed effects, γ_t, which incorporated dummies for hour-of-day, day-of-week, week-of-year, and year. In our preferred specification estimating the public transit impact on air pollution, we interacted hour-of-day with day-of-week fixed effects in order to account for hourly air pollution patterns that might differ between days. Additionally, we allowed for location-specific time fixed effects by including interactions of metropolitan area dummies, α_i, with hour-of-day (only for the air pollution model) and day-of-week (in both air pollution and health models)[96].

The control vector, $X_{i,t}$, includes hourly average weather covariates (temperature, humidity, and wind speed) and daily rainfall[97], as well as dummies for driving restriction suspension days (except those due to transit strikes), days of more restricted driving restrictions (CETESB "*rodizio*"), and national and local holidays (including holiday extension days). In our preferred specification, we included interactions between temperature and humidity, and quadratic and cubic functions of mean temperature, humidity and wind speed[98].

In the next section, we show how the results changed with the addition of weather and other controls, location, and time fixed effects. Furthermore, we ran falsification tests involving both placebo strikes as well as placebo outcomes and expected to find no effect in these placebo models. We replaced real transit strikes with false strikes placed roughly one calendar year later (except for the last strike, which is dated one calendar year earlier), on the same day of

[96] It would be possible also to include interactions of metropolitan areas dummies with week-of-year and year fixed effects to flexibly control for seasonal and long-run trends within each location.
[97] We took hourly means of temperature, humidity and wind speed measured by CETESB monitoring stations and total daily rainfall measured by DAEE stations across the respective metropolitan areas.
[98] It is usual in the related literature to flexibly control for weather, including polynomials, interactions and daily maximum and minimum, besides means, measurements (e.g., KNITTEL; MILLER; SANDERS, 2016; SCHLENKER; WALKER, 2016).

the week, and the same week of the year[99]. We also estimated models for air pollutants less related to auto vehicle emissions, and for diagnoses unrelated to pollution. Since these outcomes are supposedly unrelated to variations in public transit provision, they should not be significantly related to changes in the treatment. Thus, the falsification tests aimed to provide further support to the internal validity of the research design.

5.4.3 Results

We estimated the impact of public transit on hourly traffic-related air pollutant concentrations and daily cardio-respiratory hospital admission rates in the SPMA by comparing air pollution and hospitalization outcomes during transit strike days with outcomes in the same location, time, driving restrictions and weather conditions when no transit strike was in place, and also with outcomes on the same date in other metropolitan areas of the state of Sao Paulo with no relevant changes in transit availability. We interpreted the coefficients of the strike treatment as the effect of exogenous variation in public transit supply on SPMA air quality and on the health of its residents.

Table 6 presents the central estimated effects of public transit on air pollution, using total transit strikes, bus-only strikes, and metro strikes[100]. Transit strikes had a statistically significant and positive effect on major traffic-related atmospheric pollutant (CO, NOx, and NO_2) concentration levels. In particular, carbon monoxide concentrations increased by 8.8 percent and both nitrogen oxide and nitrogen dioxide concentrations increased by 13.2 and 8.9 percent respectively, on average during the hours of transit strike days in the SPMA[101]. As hypothesized, metro strikes had a higher impact on air pollution than bus strikes[102]. Metro strikes resulted in an increase of CO concentrations by 14.5 percent and of NOx and NO_2 by 17 and 13.5 percent respectively, whereas the coefficients for the bus strike treatment were not

[99] Alternatively, we could have geographically shifted the strikes from the affected SPMA to non-affected metropolitan areas on the same days of the real strikes.
[100] Some metro strikes overlapped with limited bus strikes with reduced coverage (partial network, part-time strikes).
[101] We kept all hours of the day in the sample. Alternatively, we could have restricted the sample to peak hours of the day, between 7am–10am and 5pm–8pm, in which driving restrictions are usually in place in the SPMA. As such, we would probably have found larger coefficient estimates.
[102] Supposedly, metro strikes induce commuters to change to either private motor vehicles, which is made easier by the suspension of driving restrictions, or buses, which usually have their operational fleet increased to meet the increased demand. By contrast, bus strikes most likely partially dislocate commuters to the metro system, which would have an effect in the opposite direction.

statistically different from zero. The magnitudes of the estimated public transit effects on key traffic-based air pollutants are also in line with findings in the economic literature[103].

Table 6 – Estimated impact of public transit on air pollutant hourly concentrations in the SPMA

	log (CO_{it})	*log* (NOx_{it})	*log* ($NO2_{it}$)	*log* ($MP10_{it}$)	*log* ($SO2_{it}$)	*log* ($O3_{it}$)
$Strike_{it}$	0.088^{*}	0.132^{*}	0.089^{*}	0.016	0.063	-0.189^{**}
	(0.048)	(0.068)	(0.046)	(0.059)	(0.082)	(0.073)
Bus $Strike_{it}$	0.055	0.121	0.051	-0.057	-0.042	-0.150^{***}
	(0.066)	(0.091)	(0.051)	(0.061)	(0.107)	(0.055)
Metro $Strike_{it}$	0.145^{**}	0.170^{*}	0.135^{*}	0.078	0.196^{*}	-0.289^{**}
	(0.068)	(0.102)	(0.077)	(0.094)	(0.109)	(0.126)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Metropolitan Area FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Hour-of-day FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	281,654	252,913	252,827	680,659	371,965	633,043
R2	0.644	0.609	0.577	0.31	0.455	0.583

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Number of transit strike days used in each treatment estimation: Strike = 34; Bus Strike = 16; Metro Strike = 17. Number of metropolitan areas included in the estimation for each dependent variable, including the SPMA (RMSP): CO =2 (RMSP, RMC), NOx = 3 (RMSP, RMC, RMBS), NO_2 = 3 (RMSP, RMC, RMBS), MP_{10} = 5 (RMSP, RMC, RMBS, RMVP, RMS), SO_2 = 3 (RMSP, RMC, RMVP), and O_3 = 5 (RMSP, RMC, RMBS, RMVP, RMS). The NOx, NO_2, SO_2 and O_3 measurements in metropolitan areas other than the SPMA are available only from 2000 onwards. All regressions were run at the metropolitan area-hour level and include metropolitan area fixed effects interacting with hour-of-day and day-of week fixed effects. The time fixed effects also include week-of-year and year dummies. Controls include dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). The weather controls include hourly temperature, humidity, and wind speed, and daily rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of hourly temperature, humidity and wind speed. Robust standard errors, clustered by day, are in parentheses.
Source: Authors own calculations.

One concern about statistical inference in our settings, was serial correlation in the time-series data[104]. Since hourly air pollution observations are most likely not independent, standard errors may be biased. Thus, we clustered the standard errors at the day level, considering the short persistence and rapid dispersion patterns of most local air pollutants[105]. The point estimates for

[103] BAUERNSCHUSTER; HENER; RAINER (2017) quantified a 4.3% rise in NO_2 in the morning peak hours due to public transit strikes in German cities, however they did not find statistically significant estimates for NO_2 during evening peak hours nor for CO in both peaks. CHEN; WHALLEY (2012) computed a reduction in CO pollution by more than 15% resulting from the opening of a new urban rail transit system in Taipei, but did not find point estimates statistically significant for variations in NOx concentration levels. GOEL; GUPTA (2017) suggested that one of the larger rail extensions of the New Delhi metro led to a decline of 34% in localized CO concentrations measured at a major city traffic intersection. LALIVE; LUECHINGER; SCHMUTZLER (2017) estimated that increases in rail service by 10% on all regional passenger railway lines in Germany lowered CO and NOx concentrations by around 1% and 2%, respectively.

[104] As we have chosen to work with just one air-monitoring station per metropolitan area, we did not have to address the other common concern in this literature of serial correlation across monitors, which would be solved by clustering along both time and monitor dimensions (as in, e.g., ANDERSON, 2014) following COLIN CAMERON; GELBACH; MILLER, (2011).

[105] We also computed the standard errors clustered both by day and by metropolitan area (not reported here, but easily replicated using the R codes made available). The statistical significance for the CO and NO_2 estimates did not change, but the NOx effect became not statistically different from zero. However, one problem with clustering at the metropolitan area level is the small number of groups available, for which the clustering procedure does not perform well (BERTRAND; DUFLO; MULLAINATHAN, 2004).

the three major tailpipe pollutants (three first columns of Table 6) are statistically significant at the 10 percent level after clustering standard errors by day in the regression with all transit strikes as treatment. In particular, the CO coefficient is statistically significant at the 5 percent level in the regression with the metro strike variable, even using a more conservative standard error for statistical inference[106].

We examined the sensitivity of our findings to different specifications and reported in Table 7, as an example, the estimated model with carbon monoxide as the dependent variable. Column 1 shows the estimated coefficient of interest using transit strikes as the only explanatory variable and no additional controls. It is easy to see why the estimator probably has an upward bias. However, it was especially interesting to note that by including weather covariates, which is typical when analyzing air pollution data in almost any context, and other obvious controls (columns 2–4 in Table 7), the point estimates did not change substantially. In fact, the transit strike coefficient was only considerably affected by adding location and time fixed effects in column 5. In our preferred specification, we interacted the metropolitan area dummies with hour-of-day and day-of-week, allowing for location-specific time fixed effects, as well as week-of-year and year dummies. Furthermore, in the last column of Table 7, the standard errors are substantively larger than the ones obtained without clustering by day, suggesting a more conservative confidence interval for statistical inference.

[106] Non-clustered standard errors yielded estimates at the highest level of significance, including for the coefficients in the regression with the placebo treatment (not reported here, but easily replicated using the R codes made available).

Table 7 – Estimated impact of public transit on hourly CO concentrations in the SPMA adding controls and fixed effects

	Dependent variable: $\log(CO_{it})$					
	1	2	3	4	5	6
$Strike_{it}$	0.407***	0.430***	0.443***	0.438***	0.088***	0.088*
	(0.024)	(0.023)	(0.023)	(0.022)	(0.015)	(0.048)
$Temperature_{it}$		0.028***	-0.089***	-0.098***	-0.030***	-0.030*
		(0.0003)	(0.008)	(0.008)	(0.005)	(0.016)
$Humidity_{it}$		-0.002***	0.045***	0.037***	0.004***	0.004
		(0.0001)	(0.002)	(0.002)	(0.001)	(0.003)
Wind $speed_{it}$		-0.132***	-0.725***	-0.726***	-0.847***	-0.847***
		(0.001)	(0.008)	(0.008)	(0.005)	(0.012)
$Temp_{it}$ x $Humid_{it}$			0.001***	0.001***	0.001***	0.001***
			(0.0001)	(0.0001)	(0.0001)	(0.0001)
Temperature^2_{it}			0.003***	0.003***	0.001***	0.001
			(0.0004)	(0.0004)	(0.0002)	(0.001)
Temperature^3_{it}			-0.0001***	-0.0005***	0.0001	0.0001
			(0.0001)	(0.0001)	(0.0001)	(0.0001)
Humidity^2_{it}			-0.001***	-0.001***	-0.0002***	-0.0002***
			(0.0001)	(0.0001)	(0.0001)	(0.0001)
Humidity^3_{it}			0.0001***	0.0001***	0.0001***	0.0001***
			(0.00001)	(0.00001)	(0.00001)	(0.00001)
Wind speed^2_{it}			0.180***	0.180***	0.215***	0.215***
			(0.003)	(0.003)	(0.002)	(0.004)
Wind speed^3_{it}			-0.012***	-0.012***	-0.016***	-0.016***
			(0.0004)	(0.0004)	(0.0003)	(0.0005)
$Rainfall_{it}$				-0.0002*	0.0005***	0.0005**
				(0.0001)	(0.0001)	(0.0001)
No $rodizio_{it}$				0.010*	-0.040***	-0.040***
				(0.006)	(0.005)	(0.014)
CETESB $rodizio_{it}$				0.841***	-0.038***	-0.038
				(0.013)	(0.010)	(0.037)
National $holiday_t$				-0.272***	-0.271***	-0.271***
				(0.005)	(0.004)	(0.012)
Local $holiday_{it}$				-0.301***	-0.184***	-0.184***
				(0.016)	(0.011)	(0.030)
Constant	-0.064***	-0.309***	0.697***	0.972***		
	(0.001)	(0.013)	(0.103)	(0.102)		
Weather controls		Yes	Yes	Yes	Yes	Yes
Weather polynomial controls			Yes	Yes	Yes	Yes
Other linear controls				Yes	Yes	Yes
Location and time FE					Yes	Yes
SE clustered by day						Yes
Observations	293,580	281,677	281,677	281,654	281,654	281,654
R^2	0.001	0.072	0.136	0.158	0.644	0.644

Signif. codes: *p<0.1; **p<0.05; ***p<0.01

Notes: Number of transit strike days in treatment = 34. Number of metropolitan areas included in the estimation =2 (RMSP, RMC). All regressions were run at the metropolitan area-hour level. The weather controls included hourly temperature, humidity, and wind speed, and daily rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of hourly temperature, humidity and wind speed. Other controls included dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). Location and time fixed effects interacted the metropolitan area dummies with hour-of-day and day-of-week, besides including week-of-year and year dummies. Reductions in the number of observations from the baseline specification were mainly due to missing data in the weather variables. Robust standard errors, clustered by day, are in parentheses.

Source: Authors own calculations.

Although we controlled for a range of weather conditions, location and time fixed effects, a key challenge to identification was whether air pollution on strike days was higher than usual for reasons other than the transit strikes themselves. In other words, there might be unobserved time-varying factors that were correlated both with transit strike occurrences and pollutant concentrations in ambient air, making it difficult to disentangle the public transit effect from other unobservable determinants of air pollution. Thus, as a robustness check, the related literature has been empirically assessing the relevance of these concerns by conducting falsification tests. In particular, it has investigated the impact of public transit on, arguably, non-road-transportation source air pollutants that nevertheless depend on environmental factors, such as sulfur dioxide (BAUERNSCHUSTER; HENER; RAINER, 2017; CHEN; WHALLEY, 2012; LALIVE; LUECHINGER; SCHMUTZLER, 2017) and ground level ozone (CHEN; WHALLEY, 2012; LALIVE; LUECHINGER; SCHMUTZLER, 2017), as well as using placebo strikes as treatment (BAUERNSCHUSTER; HENER; RAINER, 2017; RIVERS; SABERIAN; SCHAUFELE, 2017).

In many cities of the world, road transportation is a negligible source of SO_2 emissions, as modern fuels no longer contain significant amounts of sulfur. However, even though emission standards enforced in the SPMA have aimed at decreasing sulfur content in the running fuels of light-duty vehicles since the first phases of PROCONVE in the late 80's (as described in Section 2 of Chapter 3 above), only in 2012 was the use of reduced sulfur content fuel in diesel motors made mandatory (although old trucks and buses can still operate under the previous standards), which is mainly used by heavy-duty vehicles (as light-duty vehicles run largely on gasohol or hydrated ethanol) in the SPMA. Thus, in the SPMA, road transportation sources account for around 22% of sulfur oxide emissions[107] (CETESB, 2016) whereas, by contrast, in Germany they were responsible for just 3.1% of all SO_2 emissions back in 2000 (LALIVE; LUECHINGER; SCHMUTZLER, 2017).

In fact, SO_2 emissions were significantly affected by metro strikes in the SPMA. As shown in Table 6 above, the estimated impact suggests an increase of 19.6 percent in SO_2 concentrations during the stoppages in the metro system service, as these had the effect of increasing the diesel-run bus fleet in operation. By contrast, bus strikes may have had an effect in the opposite

[107] In fact, in Figure 4 of the descriptive statistics section of this chapter, we can see that, particularly in the SPMA, the hourly SO_2 concentration were correlated to traffic peak periods, especially in the morning, whereas it did not show such a clear pattern for other metropolitan areas, which rely much more on private motor vehicles rather than buses. Nevertheless, we can distinctly see reductions in SO_2 means for traffic peak hours in the SPMA over the years in Figure 19 of the Appendix.

direction, reducing SO_2 concentrations in ambient air when the bus system, at least partially, stopped service; but this point estimate is not statistically significant at the 10 percent level.

It is also disputable that ground level ozone can be considered a placebo air pollutant. Indeed, O_3 is not a primary direct tailpipe pollutant, but is instead created through a complicated non-linear process that is not completely understood where NOx and volatile organic compounds (VOC) react in the presence of sunlight and heat. Thus, even though NOx is an essential precursor of O_3, the effect of an increase in NOx depends on the initial concentrations of O_3 and many other chemical and meteorological conditions. In fact, O_3 formation can be also negatively correlated with increases in NOx emissions. Moreover, once O_3 is produced, it can further react with NO to form NO_2 and O_2. In the case of the SPMA, we find that O_3 concentrations were negatively affected by public transit strikes, especially bus strikes, as reported in Table 6 above.

Alternatively, many studies interested in estimating the effect of public transit on air pollution focus attention on variations in particulate matter (PM_{10}) concentrations in ambient air (BAUERNSCHUSTER; HENER; RAINER, 2017; GENDRON-CARRIER et al., 2018; SILVA et al., 2012). Besides being a traffic-related pollutant, data on this pollutant are often easier to obtain, as PM_{10} concentration levels have, in most places, been measured for a long period of time by numerous monitoring stations. Moreover, it is considered a proxy for $PM_{2.5}$ concentrations, which are currently one of the greatest air-pollution related health concerns along with ozone, but for which data is not readily available as monitoring has started much more recently. Nevertheless, we did not find any statistically significant effects of transit strikes on PM_{10} in the SPMA for the period of study[108].

Finally, we also tried to check for the robustness of our findings by estimating our most preferred model specification using false strikes placed around the same period of the real transit strikes, but one year later. As reported in Table 12 of the Appendix, it provided reassurance that for our main traffic-related pollutants (CO, NOx and NO_2), the placebo effects were all statistically insignificant and small in magnitude. Nevertheless, the statistically significant effects of the false strikes on the other three pollutants (MP_{10}, SO_2 and O_3), less important to our analysis, are somehow puzzling, as we expected no effect on them. We also estimated the model for different groupings of transit strike types, namely total and partial

[108] Note, however, that mobile sources in the SPMA account for around 97.5% of carbon monoxide (CO) emissions and for 67.5% of nitrogen oxides (NOx) emissions, but only for 40% of particulate matter (PM_{10}) emissions in the atmosphere (CETESB, [s.d.]).

network strikes, full-day and part-day strikes, and one-day and multiple-day strikes. In general, we did not find statistically significant coefficients, except for the part-day strike treatment dummy, but for which we only had observations for 3 strike days.

By contrast, we did not find evidence of a positive effect of public transit on cardio-respiratory daily hospital admissions[109]. As indicated in Table 8, all transit strike estimates for respiratory hospital admissions (J00–J99 ICD-10 codes) are statistically insignificant. The coefficients for hospitalizations due to circulatory diseases (I00–I99 ICD-10 codes) suggests a decrease in deaths following hospital admissions on transit strike days and hospital admissions for children under five years old[110]. The same effects are observed if we placed the treatment dummy in the day after the end of the transit strikes (i.e. lag strike treatment)[111].

Table 8 – Estimated impact of public transit on cardio-respiratory daily hospital admission rates in the SPMA

	log (J00–J99$_{it}$) All ages	*log* (I00–I99$_{it}$) All ages	*log* (J00–J99$_{it}$) death	*log* (I00–I99$_{it}$) death	*log* (J00–J99$_{it}$) 0-4 years old	*log* (I00–I99$_{it}$) 0-4 years old	*log* (J00–J99$_{it}$) 65+ years old	*log* (I00–I99$_{it}$) 65+ years old
Strike$_{it}$	-0.06	-0.059	-0.036	-0.121*	-0.072	-0.155*	-0.05	-0.035
	(0.071)	(0.069)	(0.071)	(0.066)	(0.089)	(0.080)	(0.087)	(0.086)
Lag Strike$_{it}$	-0.065	-0.093	0.023	-0.120**	-0.015	-0.196**	-0.091	-0.088
	(0.062)	(0.061)	(0.063)	(0.058)	(0.079)	(0.080)	(0.076)	(0.075)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Metropolitan Area FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	30,750	30,777	20,617	19,999	27,945	5,695	27,538	29,859
R2	0.3	0.304	0.519	0.445	0.304	0.868	0.256	0.168

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Dependent variables: hospital admission rates for the respiratory system diseases (J00–J99 ICD-10 codes); hospital admission rates for the circulatory system diseases (I00–I99 ICD-10 codes). Both diagnoses were also grouped by those hospitalizations that terminated in death, for inpatients under five years old or above 64 years old. Number of transit strike days in treatment = 34 (in which, Lag Strike = strike end date + 1day). Number of metropolitan areas included in the estimations = 5 (RMSP, RMC, RMBS, RMVP, RMS). All regressions were run at the metropolitan area-day level and include metropolitan area fixed effects interacting with day-of week fixed effects. The time fixed effects also included week-of-year and year dummies. Controls included dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). The weather controls included daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. Non-clustered standard errors are in parentheses.
Source: Authors own calculations.

These seemly counterintuitive results could be caused by the fact that transit strikes most likely affect health outcomes in ways[112] other than just through upsurges in air pollution. In fact, as reported in Table 13 of the Appendix, we find a statistically significant drop in hospital

[109] In contrast to our results, BAUERNSCHUSTER; HENER; RAINER (2017), estimating transit strike effects on hospital admissions as well, found an increase of 11 percent in hospital admissions for respiratory diseases on strike days for children under five years old in the largest German cities.
[110] When we clustered standard errors both by day and by metropolitan area levels (not reported here, but easily replicated using the R codes made available), the percentage level of statistical significance slightly increases.
[111] The lag strike explanatory variable is defined by Lag Strike$_{it}$ = strike$_i$ end date$_t$ + 1day.
[112] Transit strikes may affect mobility patterns, and thus, daily routines and also accessibility to hospital facilities. We also cannot control for avoidance behavior in response of perceptions of air pollution variations.

admissions for non-traffic related injuries of external causes (T20-T98 ICD-10 codes), for which we initially expected to find no significant impact. Nevertheless, it also may be the case that we had not assigned air pollution (through treatment) to individuals properly, assuming that it uniformly affected people across all locations within the metropolitan areas. Therefore, in the next section we exploit the potential spatially heterogeneous effects of transit strikes.

Additionally, as an example, Table 9 shows that estimates are statistically insignificant for hospital admissions due to respiratory system diseases in varying model specifications of the effect of transit strikes. Nevertheless, it highlights the role played by the inclusion of weather controls in the model, but also the difficult to specify the most correct functional form, in order to fully control for weather factors that directly affect health outcomes as well as air pollution concentration levels, as previously discussed in KNITTEL; MILLER; SANDERS (2016).

Table 9 – Estimated impact of public transit on daily respiratory hospital admission rates in the SPMA adding controls and fixed effects

	Dependent variable: $\log(J00–J99_{it})$					
	1	2	3	4	5	6
$Strike_{it}$	0.090	0.051	0.046	0.033	0.041	-0.060
	(0.086)	(0.082)	(0.082)	(0.081)	(0.076)	(0.071)
$Temperature_{it}$		-0.024^{***}	0.326^{***}	0.309^{***}	0.172^{***}	0.193^{***}
		(0.001)	(0.035)	(0.035)	(0.034)	(0.031)
$Humidity_{it}$		-0.004^{***}	-0.057^{***}	-0.047^{***}	-0.050^{***}	-0.048^{***}
		(0.001)	(0.014)	(0.014)	(0.013)	(0.012)
$Wind\ speed_{it}$		-0.087^{***}	-0.238^{***}	-0.229^{***}	-0.079^{***}	-0.070^{**}
		(0.004)	(0.030)	(0.031)	(0.029)	(0.028)
$Temp_{it}$ x $Humid_{it}$			0.0002^{***}	0.0002^{***}	0.0004^{***}	0.0005^{***}
			(0.0001)	(0.0001)	(0.0001)	(0.0001)
$Temperature\^2_{it}$			-0.017^{***}	-0.016^{***}	-0.010^{***}	-0.012^{***}
			(0.002)	(0.002)	(0.002)	(0.002)
$Temperature\^3_{it}$			0.0003^{***}	0.0002^{***}	0.0002^{***}	0.0002^{***}
			(0.00003)	(0.00003)	(0.00003)	(0.00002)
$Humidity\^2_{it}$			0.001^{***}	0.001^{***}	0.001^{***}	0.001^{***}
			(0.0002)	(0.0002)	(0.0002)	(0.0002)
$Humidity\^3_{it}$			-0.0001^{***}	-0.0001^{***}	-0.0001^{***}	-0.0001^{***}
			(0.00001)	(0.00001)	(0.00001)	(0.00001)
$Wind\ speed\^2_{it}$			0.049^{***}	0.049^{***}	0.020^{*}	0.008
			(0.013)	(0.013)	(0.012)	(0.011)
$Wind\ speed\^3_{it}$			-0.003^{**}	-0.003^{**}	-0.002	-0.0002
			(0.001)	(0.001)	(0.001)	(0.001)
$Rainfall_{it}$				-0.002^{***}	-0.0002	-0.0003
				(0.0003)	(0.0003)	(0.0002)
No $rodizio_{it}$				-0.243^{***}	0.045^{**}	-0.051^{**}
				(0.023)	(0.022)	(0.021)
CETESB $rodizio_{it}$				-0.162^{***}	0.084^{*}	-0.054
				(0.046)	(0.047)	(0.044)
National $holiday_t$				-0.123^{***}	-0.111^{***}	-0.166^{***}
				(0.013)	(0.013)	(0.012)
Local $holiday_{it}$				-0.234^{***}	-0.156^{***}	-0.138^{***}
				(0.038)	(0.036)	(0.033)
Constant	0.207^{***}	1.214^{***}	0.255	0.112		
	(0.003)	(0.030)	(0.423)	(0.425)		
Weather controls		Yes	Yes	Yes	Yes	Yes
Weather polynomial controls			Yes	Yes	Yes	Yes
Other linear controls				Yes	Yes	Yes
Time FE					Yes	Yes
Location FE						Yes
Observations	36,389	31,687	31,687	30,750	30,750	30,750
R^2	0.00003	0.043	0.049	0.058	0.182	0.300

Signif. codes: *p<0.1; **p<0.05; ***p<0.01

Notes: Dependent variable: hospital admission rates for respiratory system diseases (J00–J99). Number of transit strike days in treatment = 34. Number of metropolitan areas included in the estimation =5 (RMSP, RMC, RMBS, RMVP, RMS). All regressions were run at the metropolitan area-day level. The weather controls include daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. Other controls include dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). Location and time fixed effects interact the metropolitan area dummies with day-of-week dummies, besides including week-of-year and year dummies. Reductions in the number of observations from the baseline specification were mainly due to missing data in the weather variables. Non-clustered standard errors are in parentheses.
Source: Authors own calculations.

5.4.4 Spatially Heterogeneous Effects

There is considerable spatial heterogeneity in urban traffic-related air pollution. Thus, to be able to more precisely estimate the population dose-response function to variations in air pollution exposure, we should account for the locally heterogeneous effects of public transit supply. The main idea would be to allow the marginal effect of transit strikes to differ based on the relative distances of places of residence to more congested roads, and it would be expected the effects of transit strikes on health to decrease with distance. Therefore, in the empirical analysis, we may estimate how increases in traffic-related air pollution, due to public transit stoppages, disproportionately affected people closer to tailpipe emission sources than residents further away[113].

Since we have inpatients' zip code of residence, we can calculate the distance between zip code centroids and the nearest point on congested inner-city roads. As we do not have data on traffic volume by road segment, we proxy them with the key inner-city arterial roads, which, arguably, have the greater probability of presenting the highest upsurge in traffic volume during transit strike days. We limited our analysis to the city of Sao Paulo[114] and computed the shortest distance from city zip code centroids to any arterial road included in the city road network. Figure 16 plots all zip code centroids in the city of Sao Paulo[115] and arterial roads, which are classified into three levels[116] (including freeways, but not collector roads)[117].

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[113] Both HE; GOUVEIA; SALVO (2018); and SCHLENKER; WALKER (2016) adopted a similar strategy to investigate the effects of air pollution on hospital admissions and emergency visits, but in different settings, and using other identification strategies and econometric methods.

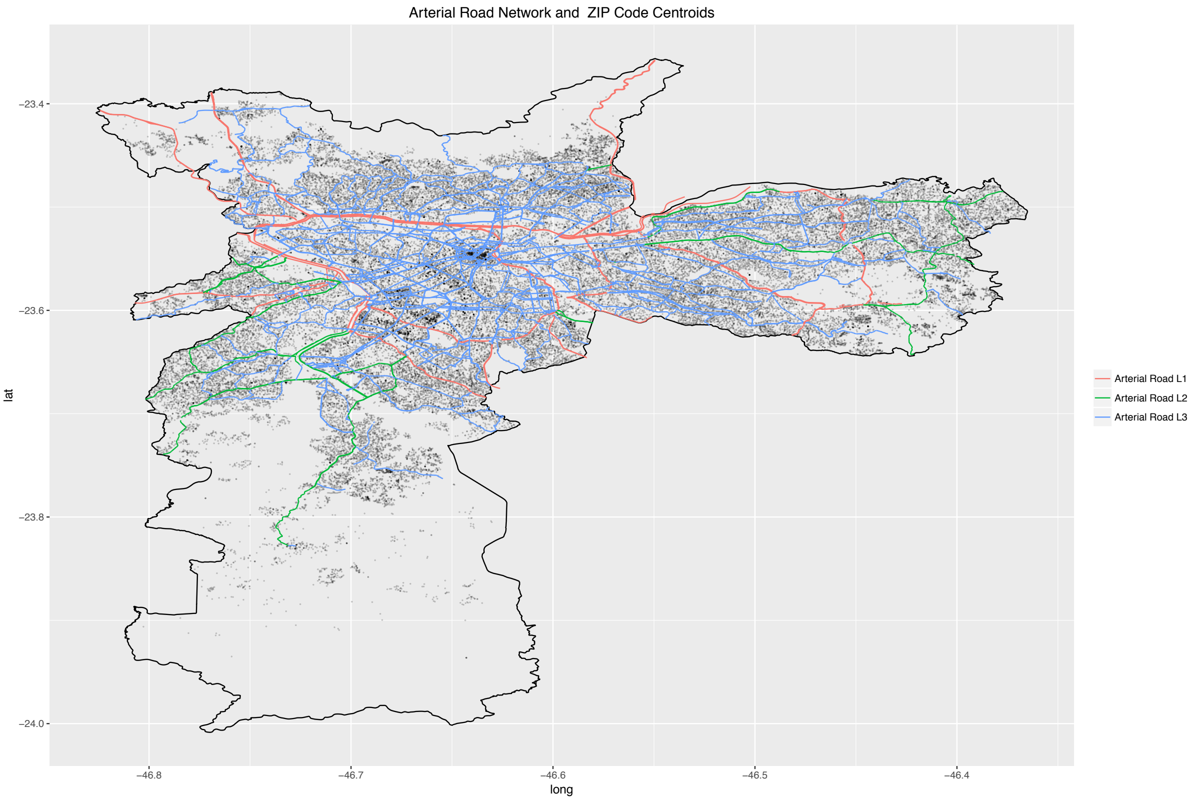
[114] Even though we have the longitude and latitude of zip code centroids for all of Brazil, we only have the geographical coordinates for the arterial road network for the city of Sao Paulo.

[115] In our zip code dataset, there are 56,445 zip code centroids within the territorial area of the city of Sao Paulo. We disregarded 2,470 zip code centroids for which the geographic coordinates were placed outside the city boundaries.

[116] The 2002 Sao Paulo Master Plan classifying the arterial road network into three levels, is available at: http://www.capital.sp.gov.br/cidadao/rua-e-bairro/gestao-urbana/plano-diretor-estrategico

[117] We also computed the distances to arterials roads including collector roads. The maximum distance from zip code centroid to the nearest point at any inner-city arterial road is 14.8 kilometers and the maximum distance to any collector road is 7.2 kilometers. The map that we generated plotting the arterial road network including collector roads is available at: https://drive.google.com/open?id=13lmqNGJzV1o1T-dUq5zPpF0RWK6Cgq8_

Figure 16 – The city of Sao Paulo arterial road network and zip code centroids



Notes: The map plots all zip code centroids of the city of Sao Paulo and city arterial roads classified into three levels, which include freeways, but not collector roads.
Source: Elaborated by the authors. Data by IBGE, SMDU/SP and CEM

Therefore, we leveraged on the spatial feature of our health data to alternatively estimate the reduced form relationship between public transit supply and health outcomes. In our specific case, given the small number of air-pollution monitoring stations relative to the geographical distribution of the population across the city, we believe this approach was more suitable than the more usual way of assigning air pollution variation to individuals by proximity to air pollution monitors. Moreover, the assumption that the arterial road network is the most affected by transit strikes may be plausible, as HE; GOUVEIA; SALVO (2018) present empirical evidence that the level of traffic congestion systematically falls with the distance from a key-inner city corridor in the SPMA.

5.4.5 Model Specification

In equation (3) of the previous section, we implicitly assumed a uniform effect of transit strikes on cardio-respiratory hospital admissions for all inpatients, regardless of their places of residence. We now exploit the hospital admission panel data in an individual fixed effects framework to estimate the spatially heterogeneous effects of public transit supply on health

outcomes by inpatient residence zip codes[118]. Thus, we added an interaction to the model with the distance of inpatient residence zip codes to key inner-city arterial roads, taken as proxies for city roads with a higher probability of becoming more congested on transit strike days. The underlying assumption is that the impact of transit service stoppage on the health of the population should be greater in residential areas closer to roads congested by automobiles substituting for public transit, and hence increasing the probability that their residents would be more exposed to higher air pollution levels (i.e. we estimate the effect of treatment on the population of compliers). However, the causal interpretation of this relationship requires strong assumptions, since the zip code fixed effects will control only for a certain type of omitted variable, the time-invariant unobserved determinants of health.

As in the difference-in-differences model specification of the previous section, the health outcomes of interest are daily hospital admissions due to respiratory or circulatory illness. For each type of diagnosis, we aggregated the number of hospitalizations by day of admission and inpatient residence zip codes per 100,000 inhabitants[119]. We estimated the following equation (4) for each hospital admission outcome variable, denoted by $Y_{i,t}$ (in logarithms) in residential zip code i on date t, by respiratory, circulatory and, as placebo, non-traffic related trauma diagnoses, including those that terminated in death, and by age group for those under five years of age or over 64 years old[120]:

$$(4) \qquad Y_{i,t} = \alpha_i + \gamma_t + \beta_1 STRIKE_t + \beta_2 D_i + \beta_3 STRIKE_t D_i + \beta_4 X_t + \varepsilon_{i,t}$$

As before, $STRIKE_t$ is equal to one when a transit strike is in effect on date t and zero otherwise. Similar equations are estimated using dummy variables for metro and bus strikes. We also assessed possible delayed impacts on health in the one day following the end of transit strike episodes. Once again, we ran a falsification test by replacing real transit strikes with fake ones that occur roughly one calendar year later. To capture the spatially differentiated transit strike effect, we interacted the strike dummy with D_i, which is the distance (in logarithms)

[118] We limited our analysis to city of Sao Paulo zip codes. However, it would also have been possible to adopt a difference-in-differences approach with the second difference corresponding to locations outside the affected area, which are hundreds of kilometers away from the city of Sao Paulo (local air pollution does travel to other locations, but in case of the state of Sao Paulo, metropolitan areas are scattered relatively far from each other).

[119] One caveat is that we did not scale zip code hospital admission counts by zip code total populations so that our dependent variables would be hospitalization rates per 100,000 zip code residents. The rates, instead, are obtained per 100,000 inhabitants based on the city of Sao Paulo annual estimated population (ranging from 9,927,868 in 1998 to 12,106,920 in 2017).

[120] Since age group density is not the same across zip codes, the most correct way would be to use hospital admission rates per 100,000 within age groups. For instance, GUIDETTI; PEREDA (2018) use population distribution by census tract from the demographic census and, inside a census tract, consider that people in this age group are uniformly distributed.

from the centroid of zip code i to the nearest point on the inner-city arterial road network[121]. Tables 14 and 15 in the Appendix show the summary statistics for daily respiratory and circulatory-related hospital admission counts by zip code distance to inner-city arterial roads. We allowed transit strike effects to vary by zip code, but, in the Tables, we present means and standard deviations across zip codes grouped according to distance to arterial roads.

The control vector X_t includes daily average weather controls (i.e. temperature, humidity, wind speed, and rainfall)[122], driving restriction suspension days (except suspensions due to transit strikes), days of more restricted driving restrictions (CETESB "*rodizio*"), and national and local holidays (including holiday extension days). To account for time-invariant unobserved determinants of health for any given zip code, the residential zip code fixed effects serve as intercepts, α_i. The time fixed effects, γ_t, incorporates a range of time control dummies (day-of-week, week-of-year, year), accounting for both daily systematic patterns and weekly and annual cycles and long-run trends. In our preferred specification, we included interactions between temperature and humidity, and quadratic and cubic functions of mean temperature, humidity and wind speed[123], as well as the interaction of zip code dummies with day-of-week, thus allowing for location-specific time fixed effects.

Equation (4) estimates how variations in transit supply might affect people's health by allowing the effects to differ by place of residence, based on their distance to inner-city arterial roads. We hypothesized that the substitution between transportation modals, from public transit to private vehicle travel, would lead to increases in respiratory and circulatory hospital admission rates, and expecting a greater impact at locations where residents might be more exposed to higher increases in tailpipe air pollution, particularly those in the vulnerable age groups (i.e. less than 5 years old and over 64 years of age). Hence, the two coefficients of interest in model (4) are β_1 and β_3. Hospital admission rates for respiratory and circulatory diseases should rise as transit service provision falls ($\beta_1 > 0$), but the effect of transit strikes should fade out with distance to inner-city arterial roads ($\beta_3 < 0$). Therefore, the marginal effects of transit strikes

[121] A few hospital admissions were recorded under a "generic" zip code (e.g., "01000000"), when we supposed, the inpatient place of residence was unknown. For these cases, the distance variable was not calculated and dropped from the model. Nevertheless, these missing observations appear only in the first few years of our sample.
[122] We take daily means of temperature, humidity and wind speed measured hourly by CETESB monitoring stations across the city and use DAEE daily total rainfall, averaging measurement points from the three stations that have observations for the entire period of study. Due to the high amount of missing weather data at each monitoring point, we were not able to allow weather factors to vary by location, as we did not assign weather conditions to people based on proximity of monitors to residence zip code.
[123] In the related literature, it is usual to also include polynomial functions of daily minimum and maximum temperature, humidity and wind speed observations measured by hour as well as data on thermal inversion. We could have included them also as controls to increase precision in our estimates.

were defined by $\beta_1 + \beta_3 D_i$. Moreover, we anticipated larger coefficients for vulnerable age groups; particularly in respiratory hospital admissions for children under 5 years old and on circulatory hospitalization for the elderly.

5.4.6 Results

The estimates of our preferred specification of model (4) are reported in Table 10 referring to the dependent variables of respiratory and circulatory hospital admission rates for inpatients of all ages, for those that terminated in death, and for the subgroups of inpatients under 5 years old and over 64 years of age. The interaction term of the transit strike dummy with distance to inner-city arterial roads shows how transit strike effects change linearly with distance. The findings corroborated our main hypotheses. Transit strikes did increase hospital admission rates due to respiratory illness, with a stronger effect on young children in the day following the end of transit strike episodes. Moreover, the estimated coefficients suggest that there is indeed significant spatial heterogeneity in the marginal effects of transit strikes, and this heterogeneity depends on the distance from inpatient residence zip codes to inner-city arterial roads.

Respiratory hospital admission rates increased on average by 2.2 percent on transit strikes days with the interaction distance coefficient linearly declining by 0.4 percent[124]. In particular, in-hospital deaths due to respiratory illness following hospitalization on transit strike days increased by 0.07 percent, although the linear decrease in distance from arterial roads is not statistically significant for this outcome. Children under 5 years of age were the most affected[125], with respiratory hospital admission rates for this subgroup increasing by 5 percent on the day following the end of the transit strike occurrences. Moreover, children living closer to arterial roads were more affected than those residing in more distant locations. In line with the findings of the effects of the transit strikes on air pollution, respiratory-related hospitalization rates for all ages were more affected by metro strikes, rather than bus strikes, as anticipated, with effects attenuating at an increasing distance from inner-city arterial roads.

[124] HE; GOUVEIA; SALVO (2018), exploiting the inauguration of a beltway that removed 20,000 cargo trucks which used to pass daily through inner-city arterial roads in Sao Paulo, examined the spatially differentiated policy impacts on health outcomes, and also found a positive and significant reduction of air pollution-related hospitalizations, with effects attenuating at increasing distances from the key inner-city corridor used by the trucks prior to the beltway opening.

[125] On the one hand, it is widely accepted that young children are the most susceptible population subgroup to the effects of air pollution. Moreover, they are more likely to be exposed to air pollution near their places of residence, as they travel less across the city compared to other age groups in the population. On the other hand, BAUERNSCHUSTER; HENER; RAINER (2017) pointed to a few potential confounding factors in similar settings. For instance, transit strikes might not only increase air pollution levels, but also increase the length of exposure to air pollution by affecting travel patterns; or transit strike episodes might lead to stress-related respiratory problems among children.

By contrast, hospitalization rates of the elderly due to circulatory diseases increased by 5.4 percent on bus strike days, however the effect did not decay by distance.

Table 10 – Estimated impact of public transit by distance to inner-city arterial roads on daily cardio-respiratory hospital admission rates in the city of Sao Paulo

	log (J00–J99$_{it}$) All ages	*log* (I00–I99$_{it}$) All ages	*log* (J00–J99$_{it}$) death	*log* (I00–I99$_{it}$) death	*log* (J00–J99$_{it}$) 0-4 years old	*log* (I00–I99$_{it}$) 0-4 years old	*log* (J00–J99$_{it}$) 65+ years old	*log* (I00–I99$_{it}$) 65+ years old
Strike$_t$	0.022**	0.010	0.070*	0.023	-0.014	0.083	0.008	0.008
	(0.011)	(0.012)	(0.037)	(0.030)	(0.017)	(0.405)	(0.025)	(0.021)
Strike$_t$ X D$_i$	-0.004*	-0.003	-0.011	-0.005	0.002	-0.022	-0.002	-0.0001
	(0.002)	(0.002)	(0.007)	(0.006)	(0.003)	(0.080)	(0.005)	(0.004)
Lag Strike$_t$	0.003	-0.005	-0.016	-0.010	0.050***	0.127	0.014	-0.002
	(0.010)	(0.012)	(0.034)	(0.029)	(0.016)	(0.325)	(0.025)	(0.019)
Lag Strike$_t$ X D$_i$	-0.0005	0.0004	0.003	0.0002	-0.007**	-0.034	-0.001	-0.001
	(0.002)	(0.002)	(0.006)	(0.006)	(0.003)	(0.061)	(0.005)	(0.004)
Metro Strike$_t$	0.031**	0.024	0.069	0.047	-0.018	0.352	0.013	-0.026
	(0.015)	(0.016)	(0.050)	(0.038)	(0.024)	(2.848)	(0.032)	(0.029)
Metro Strike$_t$ X D$_i$	-0.005*	-0.004	-0.009	-0.007	0.003	-0.078	-0.003	0.004
	(0.003)	(0.003)	(0.010)	(0.008)	(0.004)	(0.520)	(0.006)	(0.006)
Bus Strike$_t$	0.017	-0.002	0.065	-0.014	-0.002	-0.048	-0.002	0.054*
	(0.016)	(0.019)	(0.054)	(0.048)	(0.026)	(0.452)	(0.039)	(0.031)
Bus Strike$_t$ X D$_i$	-0.004	-0.001	-0.012	0.0003	-0.0002	0.016	0.002	-0.007
	(0.003)	(0.003)	(0.011)	(0.009)	(0.005)	(0.098)	(0.008)	(0.006)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Zip code FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	851,275	926,090	79,714	97,425	370,105	6,166	161,366	398,635
R2	0.473	0.417	0.422	0.489	0.419	0.732	0.458	0.403

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Dependent variables: hospital admission rates for the respiratory system diseases (J00–J99 ICD-10 codes); hospital admission rates for the circulatory system diseases (I00–I99 ICD-10 codes). Both diagnoses were also grouped by those hospitalizations that terminated in death, for inpatients under five years old or above 64 years old. Number of transit strike days in treatment = 34 (in which, Lag Strike = strike end date + 1day). All regressions were run at the zip code-day level and include zip code fixed effects interacting with day-of week fixed effects. The time fixed effects also included week-of-year and year dummies. Controls included dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). The weather controls included daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. Non-clustered standard errors are in parentheses.
Source: Authors own calculations.

The main results presented are statistically significant at least at the 10 percent level, but the statistical inferences were made not computing clustered standard errors. However, according to ABADIE et al. (2017), when fixed effects are included in the regression at the level of a cluster, one needs to adjust standard errors for clustering (only) if there is heterogeneity in the treatment effect. Moreover, as demonstrated by the authors, heterogeneity in the treatment effects is a requirement for clustering adjustments, irrespectively of whether the sampling process is clustered, or the assignment mechanism is clustered, or both.

SCHLENKER; WALKER (2016) in a setting similar to ours, adjusted standard errors for statistical inference clustering on both day and zip code dimensions. The idea is to account for both serial and spatial correlations, across observations within zip codes over time and across zip codes on a given day, respectively. HE; GOUVEIA; SALVO (2018), in contrast, tried

different ways to address standard error adjustment concerns, either by one-way clustering, by week-of-sample, or two-way clustering, by week-of-sample and road segment, which was their unit of observation, as they obtained road congestion data at the road segment level. Nevertheless, in both studies other levels of time and spatial clustering of standard errors were tested. The former also clustered standard errors at the airport level, which in that setting was the highest level of aggregation possible, whereas the latter clustered both by aggregated road segments up to 5 kilometers and by time spans up to 2 months-of-sample (i.e. they allowed for observations within 5km road segments to be spatially correlated over time and observations across road segments to be serially correlated with a two-month-sample period).

Nevertheless, we did not consider clustering standard errors by zip code level to be adequate in our case given our data set up, since we were using the maximum zip code 8 digit-disaggregation level, which yielded an excessive fragmentation of the spatial dimension[126]. Instead, we tested recalculate the standard errors for our estimations of model (4) clustering them by day and by neighborhood levels. That is, we allowed for hospital admission observations across neighborhoods to be serially correlated within the day and hospital admission observations within neighborhoods to be spatially correlated over time[127].

With standard errors clustered by day, our main results – respiratory hospital admission rates for inpatients of all ages and of those under 5 years of age – remain statistically significant at the 10 and 5 percent levels, respectively, but they lost the statistical significance for those respiratory hospital admissions that terminated in death and also to circulatory hospital admissions of the elderly. In contrast, the two-way clustering by day and neighborhood levels yields positive and statistically significant estimated coefficient, at 1% level, for respiratory hospital admission rates of young children on the day following the end of the transit strike episodes, lineally declining with distance, but respiratory hospital admissions for inpatients of all ages are statistically insignificant under this model specification[128].

[126] As mentioned before, in our dataset, there are 56,445 zip code centroids within the territorial area of the city of Sao Paulo.

[127] We are still not entirely convinced that clustering standard errors by neighborhood level is the most appropriative way to base our statistical inference of the results, since one should always cluster standard errors at the highest level of aggregation possible. It is easier to make the case for the standard errors clustering at the time dimension, giving the short persistence and rapid dispersion patterns of most local air pollutants that may affect health outcomes. However, it is similar to the approach generally adopted in the literature regarding clustering standard errors by air-quality monitor level when air pollution exposure is assigned to individuals based on proximity to the measurement points, which is a quite a crude way to form clustering groups.

[128] Results with day and neighborhood clustered standard errors are available for replication at: https://drive.google.com/open?id=1REw-ychmksGUILAaKPjI5FhrLP5BUA6c

As robustness check, we also estimated model (4) for both a placebo health outcome and a placebo set of transit strikes. The false transit strikes should not affect air-pollution related health outcomes, and hospital admissions unrelated to air pollution exposure should not vary significantly on transit strike days. Therefore, both tests are meant to provide internal validity to our research design. Results are shown in Table 16 of the Appendix. Estimated coefficients for false strikes were all statistically insignificant across all health dependent variables. By contrast, the estimated effect of the transit strikes on hospital admission rates for non-traffic related injuries of external causes (ICD-10 codes T20-T98) were found to be negative and statistically significant at the 10 percent level. However, with clustered standard errors by day and neighborhood, the point estimate became statistically insignificant for the placebo health outcome[129].

Finally, we report in Table 11, as an example, the estimated model (4) for the dependent variable respiratory hospital admission rates for all ages, in order to identify two features in the less controlled and less-flexible model specifications. First, the inclusion of location and time fixed effects effectively changed the magnitude of the point estimates and, without these, we would overestimate the effects of the transit strikes on air-pollution related hospital admission rates. Second, only by estimating the model without zip code fixed effects were we able to identify the coefficients to the distance term with no interaction with transit strikes, which were also declining with distance to the inner-city arterial roads. Nevertheless, all the central results reported in this section are based on our preferred specification, in which we interacted the zip code dummies with day-of-week dummies, allowing for location-specific time fixed effect, as well as added week-of-year and year fixed effects.

[129] Results with day and neighborhood clustered standard errors are available for replication at: https://drive.google.com/open?id=1REw-ychmksGUILAaKPjI5FhrLP5BUA6c

Table 11 – Estimated impact of public transit by distance to inner-city arterial roads on daily respiratory hospital admission rates in the city of Sao Paulo adding controls and fixed effects

	Dependent variable: log(J00–J99$_{it}$)				
	1	2	3	4	5
Strike$_t$	0.063***	0.065***	0.065***	0.063***	0.022**
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)
log(Dist.road$_i$)	-0.012***	-0.012***	-0.012***	-0.012***	
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	
Strike_t x log(Dist.road$_i$)	-0.005**	-0.005***	-0.005***	-0.005**	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Temperature$_t$		-0.002***	-0.040***	-0.038***	0.006*
		(0.0001)	(0.003)	(0.003)	(0.003)
Humidity$_t$		0.0003***	0.017***	0.012***	0.002
		(0.0001)	(0.001)	(0.001)	(0.001)
Wind speed$_t$		-0.047***	0.108***	0.125***	-0.012*
		(0.0001)	(0.006)	(0.006)	(0.006)
Temp$_t$ x Humid$_t$			-0.0001***	-0.0001***	-0.00001
			(0.000001)	(0.000001)	(0.00001)
Temperature^2$_t$			0.002***	0.002***	-0.0001
			(0.0002)	(0.0002)	(0.0001)
Temperature^3$_t$			-0.001***	-0.001***	-0.0001
			(0.0001)	(0.0001)	(0.0001)
Humidity^2$_t$			-0.001***	-0.001***	0.0001
			(0.0001)	(0.0001)	(0.0001)
Humidity^3$_t$			0.001***	0.001***	0.0001
			(0.0001)	(0.0001)	(0.0001)
Wind speed^2$_t$			-0.087***	-0.091***	0.006
			(0.004)	(0.004)	(0.004)
Wind speed^3$_t$			0.015***	0.015***	-0.001
			(0.001)	(0.001)	(0.001)
Rainfall$_t$				-0.0002***	-0.00002
				(0.0001)	(0.00002)
No rodizio$_t$				0.027***	-0.003**
				(0.001)	(0.001)
CETESB rodizio$_t$				0.099***	0.003
				(0.002)	(0.003)
National holiday$_t$				0.004***	-0.002**
				(0.001)	(0.001)
Local holiday$_t$				-0.013***	-0.010**
				(0.004)	(0.004)
Constant	-4.616***	-4.534***	-4.846***	-4.730***	
	(0.001)	(0.003)	(0.041)	(0.044)	
Weather controls		Yes	Yes	Yes	Yes
Weather polynomial controls			Yes	Yes	Yes
Other linear controls				Yes	Yes
Location and time FE					Yes
Observations	920,504	905,905	905,905	851,275	851,275
R^2	0.008	0.024	0.025	0.025	0.473

Signif. codes: *p<0.1; **p<0.05; ***p<0.01

Notes: Dependent variable: hospital admission rates for respiratory system diseases (J00–J99). Number of transit strike days in treatment = 34. All regressions were run at the zip code-day level. The weather controls include daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. Other controls include dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). Location and time fixed effects interact the zip code dummies with day-of-week dummies, as well as including week-of-year and year dummies. Reductions in the number of observations from the baseline specification were mainly due to missing data in the weather variables. Non-clustered standard errors are in parentheses.

Source: Authors own calculations.

6. Conclusion

This research exploited exogenous variations in public transit supply in the Sao Paulo Metropolitan Area (SPMA) to identify the effect of public transit accessibility on air quality and consequent health well-being. We found significant benefits for providing public transit, especially with regard to impacts on traffic-related air pollutants and respiratory illness in young children, with stronger effects for those residing near more congested arterial roads. Our central estimations suggested an increase of 8.8 percent in CO concentrations, 13.2 percent in NOx concentrations, and 8.9 in NO_2 concentrations stemming from stoppages in transit service provision, particularly by the metro system. Moreover, general respiratory hospital admission rates increased on average by 2.2 percent on transit strikes days. Children under 5 years of age were the most affected, with respiratory hospitalization for this subgroup increasing by 5 percent on the day following the end of the transit strike occurrences. The distance interaction term linearly decreases, respectively by 0.4 percent and 0.7 percent with distance to arterial roads. In particular, the transit impact on young children remains statistically significant in varying model specifications and standard error clustering levels.

Therefore, – given the policy-relevant question of whether public transit subsidies can be warranted for improving air quality and health outcomes – environment-health benefits alone may justify public transit subsidization, at least to some degree. However, a more thoughtful transportation policy recommendation would require further discussion beyond back-of-the-envelope calculations and the trivial conclusion that governments should invest and support public transit while disincentivizing the use of private vehicles through more efficient road pricing policies. The valuation of the environmental welfare impact of public transit could follow the general framework for evaluating existing fare subsidies and potential pricing reforms proposed by PARRY; SMALL (2009), complemented by a cost-benefit analysis along the lines of DESCHÊNES; GREENSTONE; SHAPIRO (2017). However, we leave this for future research.

Nevertheless, we add two notes on transportation policy. When buses are the main mode of transit (which is the case for most Latin American metropolitan areas), total emissions for some air pollutants might not be lower, even if the elasticity of substitution between modals is sufficiently large, as indicated by our results for bus strikes vis-à-vis metro strikes. Hence, a complementary policy, either in a first- or second-best scenario for pricing automobile externalities, would be to invest in heavy-duty clean-fuel vehicles when renovating the bus fleet (HE; GOUVEIA; SALVO, 2018; RIVERS; SABERIAN; SCHAUFELE, 2017).

Moreover, it has been theoretically argued that even if there is efficient substitutability between congestion pricing and transit subsidization – at least if the marginal cost of public funds and induced cost inefficiencies are not very large –, dedicated bus lanes are a superior policy, as they increase frequencies and decrease transit fares without the need for extra public funds, thus substantially reducing the marginal contribution of increased transit subsidies (BASSO; SILVA, 2014).

An additional note on the substantive importance of second-best policies – that low the cost of alternatives that mitigate externalities in circumstances where it is not politically or operationally feasible to internalize them – is whether the mitigating alternative is more attractive to those that generate greater externalities. Hence, exploiting self-selection into mitigating alternatives would provide more precise predictions instead of using population averages when assessing efficiency substitutability between transportation policies (ANDERSON, 2014). Once again, we leave this for future research, as well as discussions on the political economy of substituting and/or complementary transportation policies.

Lastly, a final note on the external validity of this research. The model estimates apply specifically to the Sao Paulo Metropolitan Area and, hence, the magnitudes of the estimated effects, most likely, cannot be generalized to other metropolitan areas in the developing world. Nevertheless, the suggested qualitative impact of public transit on air quality and health outcomes should translate to large metropolitan areas with a similar modal share, and geographic and climate conditions.

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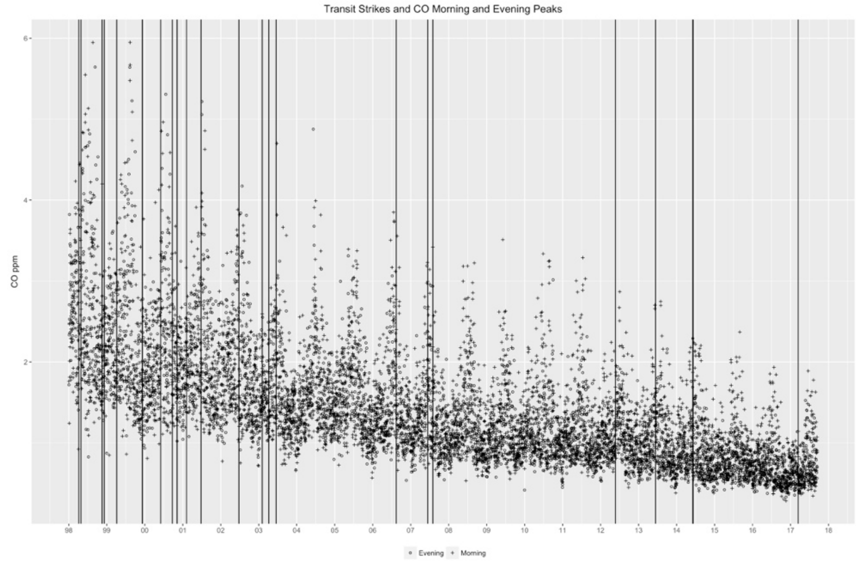
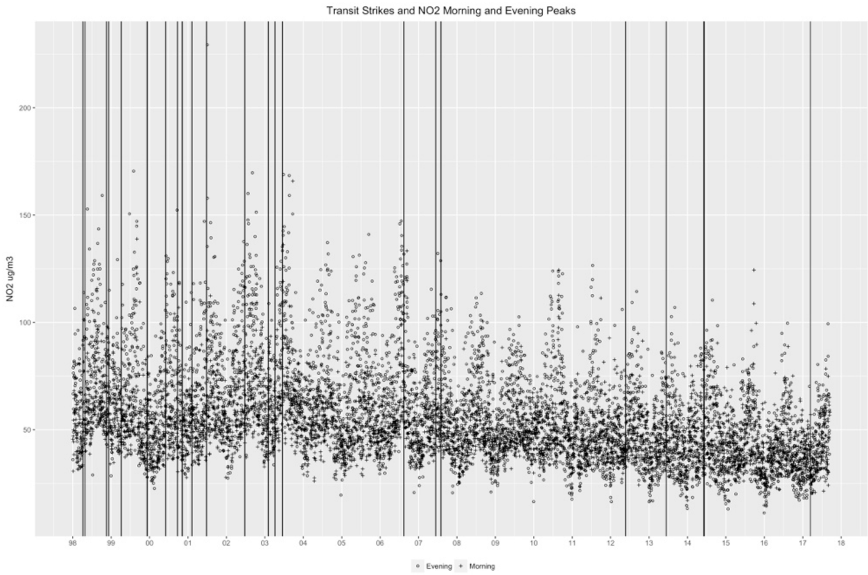
Appendix

Figure 17 – CO mean concentration levels for morning and evening peaks around transit strike events



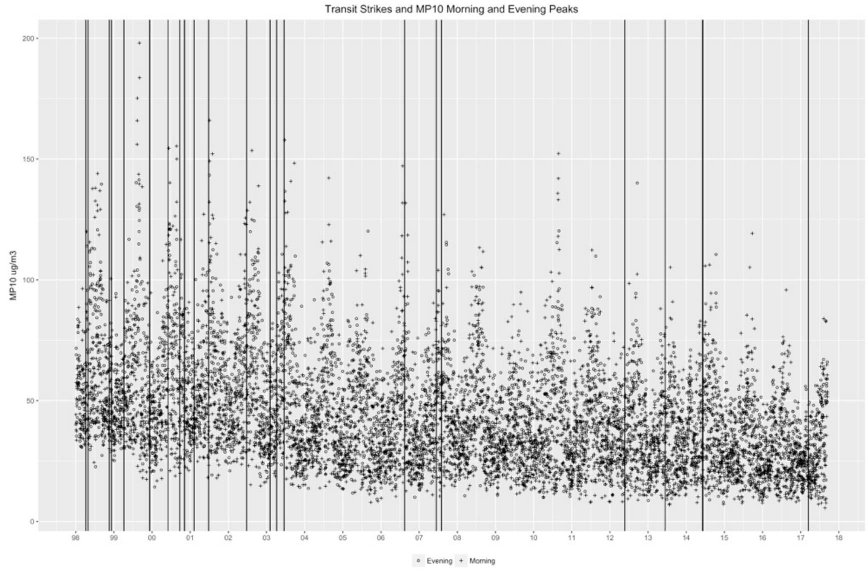
Notes: The graph plots CO mean concentration levels for morning and evening peaks, between 7am–10am and 5pm–8pm, by day for the entire period of study. The black vertical lines mark the transit strike days.
Source: Elaborated by the authors.

Figure 18 – NO_2 mean concentration levels for morning and evening peaks around transit strike events



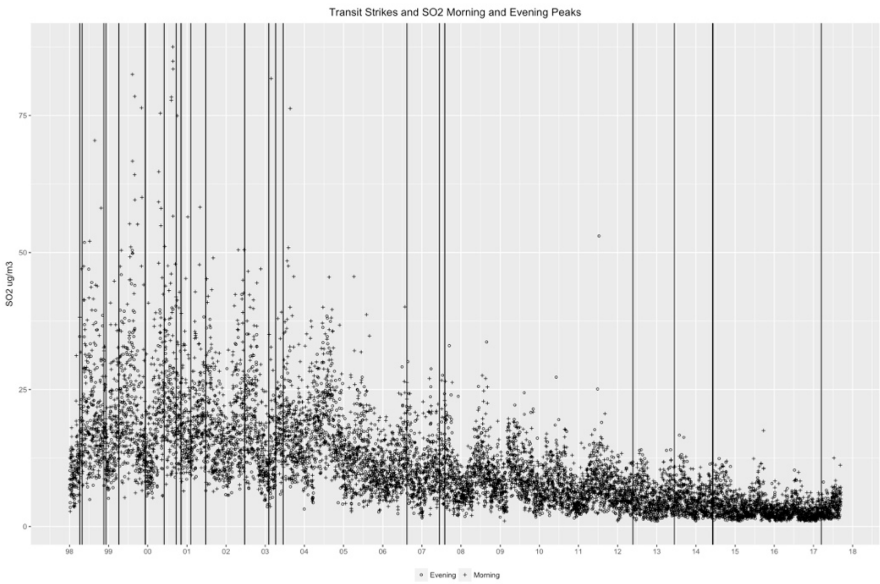
Notes: The graph plots NO_2 mean concentration levels for morning and evening peaks, between 7am–10am and 5pm–8pm, by day for the entire period of study. The black vertical lines mark the transit strike days.
Source: Elaborated by the authors.

Figure 19 – MP_{10} mean concentration levels for morning and evening peaks around transit strike events



Notes: The graph plots MP_{10} mean concentration levels for morning and evening peaks, between 7am–10am and 5pm–8pm, by day for the entire period of study. The black vertical lines mark the transit strike days.
Source: Elaborated by the authors.

Figure 20 – SO_2 mean concentration levels for morning and evening peaks around transit strike events



Notes: The graph plots SO_2 mean concentration levels for morning and evening peaks, between 7am–10am and 5pm–8pm, by day for the entire period of study. The black vertical lines mark the transit strike days.
Source: Elaborated by the authors.

Table 12 – Estimated impact of public transit on air pollutant hourly concentrations in the SPMA by transit strike type

	log (CO_{it})	*log* (NOx_{it})	*log* ($NO2_{it}$)	*log* ($MP10_{it}$)	*log* ($SO2_{it}$)	*log* ($O3_{it}$)
$Strike_{it}$	0.088*	0.132*	0.089*	0.016	0.063	-0.189**
	(0.048)	(0.068)	(0.046)	(0.059)	(0.082)	(0.073)
Bus $Strike_{it}$	0.055	0.121	0.051	-0.057	-0.042	-0.150***
	(0.066)	(0.091)	(0.051)	(0.061)	(0.107)	(0.055)
Metro $Strike_{it}$	0.145**	0.170*	0.135*	0.078	0.196*	-0.289**
	(0.068)	(0.102)	(0.077)	(0.094)	(0.109)	(0.126)
Total network $Strike_{it}$	0.092	0.108	0.130*	-0.011	0.012	-0.217***
	(0.060)	(0.093)	(0.069)	(0.090)	(0.079)	(0.073)
Partial network $Strike_{it}$	0.083	0.155	0.046	0.05	0.126	-0.153
	(0.078)	(0.098)	(0.058)	(0.070)	(0.152)	(0.134)
Full-day $Strike_{it}$	0.064	0.096	0.076	0.007	0.042	-0.163**
	(0.051)	(0.071)	(0.050)	(0.064)	(0.090)	(0.077)
Part-day $Strike_{it}$	0.301***	0.456***	0.198***	0.103	0.250***	-0.437**
	(0.079)	(0.089)	(0.067)	(0.114)	(0.091)	(0.176)
Multi-day Strike	0.038	0.129	0.109*	-0.014	0.029	-0.249***
	(0.058)	(0.085)	(0.064)	(0.079)	(0.107)	(0.080)
One-day Strike	0.187**	0.136	0.046	0.091	0.144	-0.043
	(0.080)	(0.112)	(0.044)	(0.060)	(0.103)	(0.149)
Placebo $Strike_{it}$	0.062	0.034	0.040	0.173***	0.157**	-0.163**
	(0.041)	(0.072)	(0.062)	(0.054)	(0.067)	(0.065)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Metropolitan Area FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Hour-of-day FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	281,654	252,913	252,827	680,659	371,965	633,043
R2	0.644	0.609	0.577	0.31	0.455	0.583

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Number of transit strike days used in each treatment estimation: Strike = 34; Bus Strike = 16; Metro Strike = 17; Total Network Strike =19; Partial Network Strike = 15; Full-day Strike = 31; Part-day Strike = 3; Multi-day Strike = 24; One-day Strike = 10; Placebo Strike = 34. Number of metropolitan areas included in the estimation for each dependent variable, including the SPMA (RMSP): CO =2 (RMSP, RMC), NOx = 3 (RMSP, RMC, RMBS), NO_2 = 3 (RMSP, RMC, RMBS), MP_{10} = 5 (RMSP, RMC, RMBS, RMVP, RMS), SO_2 = 3 (RMSP, RMC, RMVP), and O_3 = 5 (RMSP, RMC, RMBS, RMVP, RMS). The NOx, NO_2, SO_2 and O_3 measurements in the metropolitan areas other than the SPMA are available only from 2000 onwards. All regressions were run at the metropolitan area-hour level and include metropolitan area fixed effects interacting with hour-of-day and day-of week fixed effects. The time fixed effects also include week-of-year and year dummies. Controls include dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions. The weather controls include hourly temperature, humidity, and wind speed, and daily rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of hourly temperature, humidity and wind speed. Differences between numbers of observations were mainly due to the number of metropolitan areas included in the models for each air pollutant outcome. The reported R^2 refers to the first line estimation of all transit strikes treatment. Robust standard errors, clustered by day, are in parentheses

Source: Authors own calculations.

Table 13 – Estimated impact of public transit on cardio-respiratory daily hospital admission rates in the SPMA by transit strike type

	log (J00–J99$_{it}$) All ages	*log* (I00–I99$_{it}$) All ages	*log* (T20–T98$_{it}$) All ages	*log* (J00–J99$_{it}$) death	*log* (I00–I99$_{it}$) death	*log* (J00–J99$_{it}$) 0-4 years old	*log* (I00–I99$_{it}$) 0-4 years old	*log* (J00–J99$_{it}$) 65+ years old	*log* (I00–I99$_{it}$) 65+ years old
Strike$_{it}$	-0.06	-0.059	-0.179**	-0.036	-0.121*	-0.072	-0.155*	-0.05	-0.035
	(0.071)	(0.069)	(0.079)	(0.071)	(0.066)	(0.089)	(0.080)	(0.087)	(0.086)
Bus Strike$_{it}$	-0.082	-0.086	-0.245**	-0.123	-0.118	-0.123	-0.098	-0.153	-0.058
	(0.103)	(0.100)	(0.115)	(0.104)	(0.096)	(0.130)	(0.122)	(0.126)	(0.125)
Metro Strike$_{it}$	-0.043	-0.032	-0.126	0.046	-0.136	-0.033	-0.179*	0.045	-0.009
	(0.100)	(0.097)	(0.111)	(0.100)	(0.093)	(0.126)	(0.108)	(0.122)	(0.121)
Lag Strike$_{it}$	-0.065	-0.093	-0.120*	0.023	-0.120**	-0.015	-0.196**	-0.091	-0.088
	(0.062)	(0.061)	(0.070)	(0.063)	(0.058)	(0.079)	(0.080)	(0.076)	(0.075)
Total network Strike$_{it}$	-0.125	-0.096	-0.267**	-0.022	-0.200**	-0.141	-0.231**	-0.131	-0.080
	(0.094)	(0.092)	(0.106)	(0.095)	(0.088)	(0.119)	(0.113)	(0.116)	(0.115)
Partial network Strike$_{it}$	0.022	-0.011	-0.067	-0.054	-0.02	0.016	-0.079	0.053	0.021
	(0.106)	(0.103)	(0.119)	(0.107)	(0.099)	(0.134)	(0.113)	(0.130)	(0.129)
Full-day Strike$_{it}$	-0.05	-0.062	-0.185**	-0.055	-0.111	-0.055	-0.147*	-0.037	-0.044
	(0.074)	(0.072)	(0.083)	(0.075)	(0.069)	(0.093)	(0.081)	(0.091)	(0.090)
Part-day Strike$_{it}$	-0.168	-0.021	-0.115	0.151	-0.224	-0.239	-0.378	-0.183	0.048
	(0.236)	(0.230)	(0.265)	(0.239)	(0.222)	(0.299)	(0.419)	(0.291)	(0.287)
Multi-day Strike$_{it}$	-0.054	-0.071	-0.151	-0.073	-0.086	-0.050	-0.106	-0.056	-0.023
	(0.084)	(0.082)	(0.094)	(0.085)	(0.079)	(0.106)	(0.094)	(0.103)	(0.102)
One-day Strike$_{it}$	-0.074	-0.028	-0.244*	0.050	-0.203*	-0.123	-0.275*	-0.036	-0.064
	(0.130)	(0.126)	(0.145)	(0.131)	(0.122)	(0.164)	(0.148)	(0.159)	(0.157)
Placebo Strike$_{it}$	-0.111	-0.067	-0.103	-0.051	-0.042	-0.120	-0.135	-0.075	-0.053
	(0.071)	(0.069)	(0.079)	(0.071)	(0.066)	(0.089)	(0.084)	(0.087)	(0.086)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Metropolitan Area FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	30,750	30,777	22,705	20,617	19,999	27,945	5,695	27,538	29,859
R2	0.3	0.304	0.348	0.519	0.445	0.304	0.868	0.256	0.168

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Dependent variables: hospital admission rates for respiratory system diseases (J00–J99 ICD-10 codes); hospital admission rates for circulatory system diseases (I00–I99 ICD-10 codes); non-traffic related injuries of external causes (T20-T98 ICD-10 codes). Diagnoses of interest were also grouped by those hospitalizations that terminated in death, for inpatients under five years old or above 64 years old. Number of transit strike days used in each treatment estimation: Strike = 34; Bus Strike = 16; Metro Strike = 17; Lag Strike (strike end date + 1day) = 34; Total Network Strike =19; Partial Network Strike = 15; Full-day Strike = 31; Part-day Strike = 3; Multi-day Strike = 24; One-day Strike = 10; Placebo Strike = 34. Number of metropolitan areas included in the estimations = 5 (RMSP, RMC, RMBS, RMVP, RMS). All regressions were run at the metropolitan area-day level and include metropolitan area fixed effects interacting with day-of week fixed effects. The time fixed effects also include week-of-year and year dummies. Controls include dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). The weather controls include daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. The reported R^2 refers to the first line estimation of all transit strikes treatment. Non-clustered standard errors are in parentheses.

Source: Authors own calculations.

Table 14 – Summary statistics for daily respiratory-related hospital admission counts by zip code distance to arterial roads in the city of Sao Paulo, 1998–2017

				Daily Hospital Admission by Zip Code for Diseases of the Respiratory System							
				Total		Terminated in death		0-4 years old		65+ years old	
	N	#zip codes	#days	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Non-strike days	933625	48794	7270	1,08	0,77	0,10	0,30	0,46	0,64	0,20	0,45
Strike days	4561	3642	34	1,11	0,96	0,09	0,30	0,48	0,70	0,19	0,41
Only-bus	1792	1561	16	1,13	1,15	0,09	0,30	0,50	0,77	0,16	0,41
Only-metro	2570	2227	17	1,10	0,82	0,09	0,30	0,47	0,66	0,20	0,42
Only-rail	199	199	1	1,07	0,59	0,07	0,25	0,46	0,63	0,16	0,37
Total network	2441	2077	19	1,12	1,00	0,10	0,30	0,50	0,71	0,17	0,39
Partial network	2120	1876	15	1,10	0,90	0,09	0,29	0,46	0,70	0,20	0,44
Full-day	4207	3393	31	1,10	0,86	0,09	0,30	0,48	0,67	0,18	0,41
Part-time	354	349	3	1,18	1,70	0,11	0,31	0,46	1,01	0,20	0,42
Multiple-days	3197	2665	24	1,11	0,92	0,09	0,30	0,49	0,70	0,18	0,41
Single-day	1364	1261	10	1,10	1,04	0,09	0,29	0,46	0,71	0,19	0,41
Dist. < 1st Qu; Non-strike	228960	13130	7269	1,09	0,60	0,10	0,31	0,44	0,63	0,22	0,50
Dist. < 1st Qu; On strike	1170	895	34	1,13	0,70	0,12	0,34	0,45	0,64	0,22	0,43
Dist. > 1st Qu < Median; Non-strike	229076	11719	7268	1,08	0,59	0,11	0,31	0,43	0,60	0,22	0,45
Dist. > 1st Qu < Median; On Strike	1093	867	34	1,10	0,54	0,11	0,31	0,44	0,57	0,20	0,43
Dist. > Median < 3rd Qu; Non-strike	229060	9952	7269	1,07	0,51	0,10	0,30	0,46	0,57	0,20	0,42
Dist. > Median < 3rd Qu; On strike	1080	878	34	1,06	0,46	0,09	0,28	0,46	0,61	0,17	0,38
Dist. > 3rd Qu; Non-strike	229008	8891	7270	1,02	0,16	0,07	0,25	0,49	0,52	0,14	0,36
Dist. > 3rd Qu; On strike	1057	862	34	1,02	0,17	0,05	0,23	0,52	0,51	0,14	0,34

Notes: The counts of daily hospital admissions are not by rate (per 100,000 inhabitants) in these summary statistics for ease of reading the numbers in the table. The estimated models, however, use hospitalization variables in the rate form. The numbers are in Brazilian format, which uses comma rather than dot as decimal separator, and dot rather than comma as thousand separator.
Source: Elaborated by the authors.

Table 15 – Summary statistics for daily circulatory-related hospital admission counts by zip code distance to arterial roads in the city of Sao Paulo, 1998–2017

				Daily Hospital Admission by Zip Code for Diseases of the Circulatory System							
				Total		Terminated in death		0-4 years old		65+ years old	
	N	#zip codes	#days	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Non-strike days	1029289	52767	7270	1,11	4,45	0,11	0,32	0,01	0,12	0,47	2,47
Strike days	5014	4189	34	1,10	1,09	0,09	0,30	0,01	0,10	0,45	0,69
Only-bus	2147	1949		1,11	1,18	0,10	0,30	0,01	0,10	0,45	0,75
Only-metro	2685	2375	17	1,10	1,05	0,09	0,29	0,01	0,09	0,46	0,65
Only-rail	182	182	1	1,01	0,15	0,10	0,30	0,01	0,07	0,40	0,49
Total network	2640	2331	19	1,11	1,23	0,09	0,30	0,01	0,09	0,45	0,77
Partial network	2374	2150	15	1,09	0,90	0,09	0,29	0,01	0,10	0,46	0,59
Full-day	4614	3899	31	1,10	0,93	0,09	0,30	0,01	0,10	0,45	0,67
Part-time	400	392	3	1,16	2,21	0,09	0,29	0,00	0,05	0,49	0,93
Multiple-days	3525	3051	24	1,10	0,96	0,10	0,30	0,01	0,10	0,47	0,70
Single-day	1489	1398	10	1,10	1,35	0,08	0,28	0,01	0,08	0,43	0,67
Dist. < 1st Qu; Non-strike	252449	12829	7270	1,17	8,77	0,11	0,33	0,01	0,12	0,52	4,73
Dist. < 1st Qu; On strike	1296	1031	34	1,13	0,93	0,10	0,32	0,00	0,07	0,50	0,79
Dist. > 1st Qu < Median; Non-strike	252424	11317	7270	1,11	1,10	0,11	0,32	0,01	0,17	0,49	0,91
Dist. > 1st Qu < Median; On Strike	1257	1038	34	1,10	0,58	0,09	0,29	0,01	0,10	0,50	0,62
Dist. > Median < 3rd Qu; Non-strike	252604	10440	7269	1,07	0,96	0,10	0,31	0,01	0,08	0,46	0,91
Dist. > Median < 3rd Qu; On strike	1147	986	34	1,03	0,28	0,09	0,28	0,01	0,11	0,44	0,52
Dist. > 3rd Qu; Non-strike	252555	10628	7269	1,03	0,74	0,09	0,29	0,01	0,10	0,39	0,75
Dist. > 3rd Qu; On strike	1105	952	34	1,01	0,10	0,08	0,27	0,01	0,09	0,34	0,48

Notes: The counts of daily hospital admissions are not by rate (per 100,000 inhabitants) in these summary statistics for ease of reading the numbers in the table. The estimated models, however, use the hospitalization variables in the rate form. The numbers are in Brazilian format, which uses comma rather than dot as decimal separator, and dot rather than comma as thousand separator.
Source: Elaborated by the authors.

Table 16 – Estimated impact of public transit by distance to inner-city arterial roads on daily cardio-respiratory hospital admission rates in the city of Sao Paulo including placebos

	log ($J00–J99_{it}$) All ages	*log* ($I00–I99_{it}$) All ages	*log* ($T20–T98_{it}$) All ages	*log* ($J00–J99_{it}$) death	*log* ($I00–I99_{it}$) death	*log* ($J00–J99_{it}$) 0-4 years old	*log* ($I00–I99_{it}$) 0-4 years old	*log* ($J00–J99_{it}$) 65+ years old	*log* ($I00–I99_{it}$) 65+ years old
$Strike_t$	0.022**	0.010	-0.074*	0.070*	0.023	-0.014	0.083	0.008	0.008
	(0.011)	(0.012)	(0.044)	(0.037)	(0.030)	(0.017)	(0.405)	(0.025)	(0.021)
$Strike_t$ X D_i	-0.004*	-0.003	0.011	-0.011	-0.005	0.002	-0.022	-0.002	-0.0001
	(0.002)	(0.002)	(0.008)	(0.007)	(0.006)	(0.003)	(0.080)	(0.005)	(0.004)
Lag $Strike_t$	0.003	-0.005	0.019	-0.016	-0.010	0.050***	0.127	0.014	-0.002
	(0.010)	(0.012)	(0.046)	(0.034)	(0.029)	(0.016)	(0.325)	(0.025)	(0.019)
Lag $Strike_t$ X D_i	-0.0005	0.0004	-0.007	0.003	0.0002	-0.007**	-0.034	-0.001	-0.001
	(0.002)	(0.002)	(0.009)	(0.006)	(0.006)	(0.003)	(0.061)	(0.005)	(0.004)
Metro $Strike_t$	0.031**	0.024	-0.025	0.069	0.047	-0.018	0.352	0.013	-0.026
	(0.015)	(0.016)	(0.063)	(0.050)	(0.038)	(0.024)	(2.848)	(0.032)	(0.029)
Metro $Strike_t$ X D_i	-0.005*	-0.004	0.004	-0.009	-0.007	0.003	-0.078	-0.003	0.004
	(0.003)	(0.003)	(0.012)	(0.010)	(0.008)	(0.004)	(0.520)	(0.006)	(0.006)
Bus $Strike_t$	0.017	-0.002	-0.127**	0.065	-0.014	-0.002	-0.048	-0.002	0.054*
	(0.016)	(0.019)	(0.062)	(0.054)	(0.048)	(0.026)	(0.452)	(0.039)	(0.031)
Bus $Strike_t$ X D_i	-0.004	-0.001	0.019	-0.012	0.0003	-0.0002	0.016	0.002	-0.007
	(0.003)	(0.003)	(0.012)	(0.011)	(0.009)	(0.005)	(0.098)	(0.008)	(0.006)
Placebo $Strike_t$	0.011	-0.015	-0.021	0.007	0.012	0.001	0.094	-0.013	0.002
	(0.011)	(0.012)	(0.040)	(0.036)	(0.033)	(0.017)	(0.432)	(0.024)	(0.021)
Placebo $Strike_t$ X D_i	-0.002	0.002	0.001	-0.001	-0.003	-0.001	-0.029	0.002	0.001
	(0.002)	(0.002)	(0.008)	(0.007)	(0.006)	(0.003)	(0.085)	(0.005)	(0.004)
Weather polynomial controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Other linear controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Zip code FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Day-of-week FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Week-of-year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Observations	851,275	926,090	135,483	79,714	97,425	370,105	6,166	161,366	398,635
R2	0.473	0.417	0.538	0.422	0.489	0.419	0.732	0.458	0.403

*Signif. codes: *p<0.1; **p<0.05; ***p<0.01*

Notes: Dependent variables: hospital admission rates for the respiratory system diseases (J00–J99 ICD-10 codes); hospital admission rates for the circulatory system diseases (I00–I99 ICD-10 codes). Both diagnoses were also grouped by those hospitalizations that terminated in death, for inpatients under five years old or above 64 years old. Number of transit strike days used in each treatment estimation: Strike = 34; Bus Strike = 16; Metro Strike = 17; Lag Strike (strike end date + 1day) = 34; Placebo Strike = 34. All regressions were run at the zip code-day level and include zip code fixed effects interacting with day-of week fixed effects. The time fixed effects also included week-of-year and year dummies. Controls included dummies for national and local holidays (including holiday extension days), driving restriction suspension days (except those due to transit strikes), and days with more restricted driving restrictions (CETESB "rodizio"). The weather controls included daily mean temperature, humidity, and wind speed, and daily total rainfall as well as interaction between temperature and humidity, and quadratic and cubic functions of daily mean temperature, humidity and wind speed. Non-clustered standard errors are in parentheses.

Source: Authors own calculations.