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When Labor Disputes Bring Cities to a Standstill: The Impact of Public Transit Strikes on Traffic, Accidents, Air Pollution, and Health†

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Many governments have banned strikes in public transportation. Whether this can be justified depends on whether strikes endanger public safety or health. We use time-series and cross-sectional variation in powerful registry data to quantify the effects of public transit strikes on urban populations in Germany. Due to higher traffic volumes and longer travel times, total car hours operated increase by 11 to 13 percent during strikes. This effect is accompanied by a 14 percent increase in vehicle crashes, a 20 percent increase in accident-related injuries, a 14 percent increase in particle pollution, and an 11 percent increase in hospital admissions for respiratory diseases among young children. (*JEL* I12, J45, J52, L91, Q53, R41)

> *The right to strike may be restricted or prohibited* [...] *in essential services* [...] (*that is, services the interruption of which would endanger the life, personal safety or health of the whole or part of the population*). *The following do*[*es*] *not constitute* [*an*] *essential service*[...]*: transport generally.*
> — International Labour Organization (2006, para. 576 and para. 587)

> *Many public services are considered essential: police officers and firefighters, for example. Strikes are prohibited for this very reason. They are critical for the public on a day-to-day basis. The reliability of public transit should be no different.*
> — Robert S. Huff, California State Senate Republican Leader (2014)

In 1951, the International Labour Organization (ILO) set up the Committee on Freedom of Association (CFA). Shortly after its inception, the CFA declared strike action to be a fundamental right of organized labor (Gernigon, Odero, and Guido 1998; Gross 1999). Yet, where workers providing essential public services

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are concerned, the right to strike is often limited or even denied by national laws or regulations. The most common restriction is a ban on strikes by armed forces, policemen, and firefighters, for the legitimate reason that those walkouts would endanger the life, personal safety, or health of the whole or parts of the population.[1] But is that true of strikes by public transit workers? Two extreme positions shape answers to this question. According to the ILO, public transportation does not constitute an essential public service (ILO 2006, para. 587). Thus, some commentators argue that strikes by transit workers mainly pose an economic threat, which—being the very essence of industrial action—does not justify a strike ban (Swearengen 2011). Policymakers, by contrast, commonly regard mass transit as an essential public service, which segues into the wider concern that major cities and their inhabitants are highly vulnerable to transit strikes.[2] This is exemplified by attempts in numerous countries to also exclude transit workers from the right to strike.

New York City's *Taylor Law*, which was put into effect in response to a transit strike in 1966, represents an example of a particularly draconian measure. Under Section 210, the law prohibits any strike or other concerted stoppage of work or slowdown by public employees (Division of Local Government Services 2009). Instead, it prescribes binding arbitration by a state agency to resolve bargaining deadlocks between unions and employers. Violations against the prohibition on strikes are punishable with hefty penalties. The fine for an individual worker is twice the striking employee's salary for each day the strike lasts. In addition, union leaders face imprisonment. Since its inception in 1967, the *Taylor Law* has generated a lot of controversy. To proponents, it was successful in averting several potential transit strikes that would have imposed significant costs on the city and its inhabitants (OECD 2007). Indeed, New York City has only seen two transit strikes over the past four decades—in 1980 and in 2005. In both cases, harsh monetary penalties were imposed on workers and unions. The 2005 transit strike additionally led to the imprisonment of a union leader, and saw the Transport Workers Union (TWU) filing a formal complaint with the ILO. Since then, the ILO has urged the United States government to restore the right of transit workers to strike, arguing that they do not provide essential services justifying a strike ban (Committee on Freedom of Association 2011, 775). So far, the *Taylor Law* has not been amended in this direction.

This paper aims to answer two questions that are at the heart of the *Taylor Law* controversy and similar debates elsewhere: Do strikes in the public transportation sector cause disruptions that endanger the safety and health of urban populations? And how large are the costs of transit strikes to noninvolved third parties? To get at these questions, our analysis uses time series and cross-sectional variation in

[1] As the first quote illustrates, the ILO recognizes that strikes may be restricted or prohibited in essential services, which are defined to include: the hospital sector, electricity services, water supply services, the telephone service, the police and armed forces, the firefighting services, public and private prison services, and air traffic control (ILO 2006, para. 585).

[2] Thus, the second quote is from a California politician and was made following a strike by workers of the Bay Area Rapid Transit (BART) system in 2013. The position expressed in this statement received bipartisan support. Indeed, in the aftermath of the same strike, California State Senate Democratic candidate Steve Glazer expressed his "[…] support [for] state legislation to prohibit transportation workers from striking." For more, see http://calwatchdog.com/2014/01/24/democrats-crash-transit-strike-ban/ (accessed December 8, 2014).

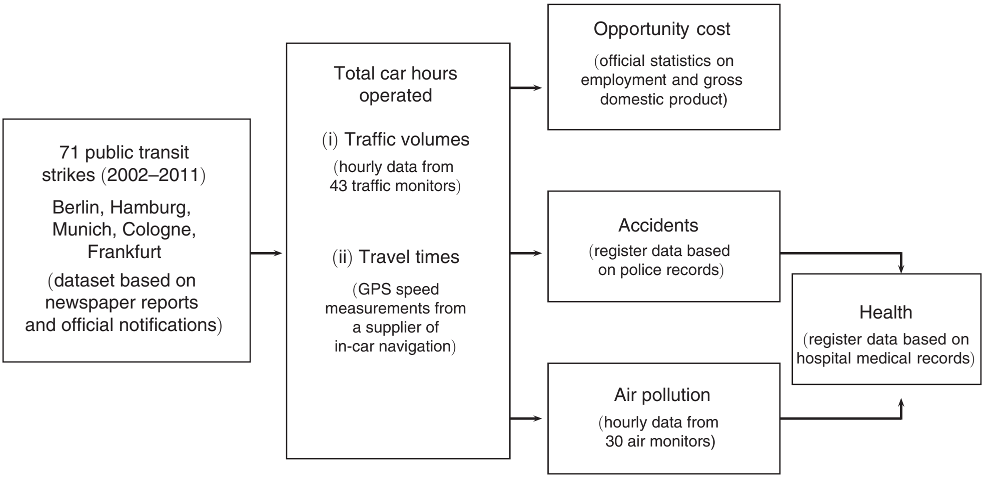

Figure 1. The Impact of Public Transit Strikes on Urban Populations

powerful registry data to quantify the effects of public transit strikes in five domains: traffic volumes, travel times, accident risk, pollution emissions, and health (see Figure 1). The context for our study are the five largest cities in Germany, which provides us with an ideal setting. In particular, in contrast to countries that have imposed de jure restrictions on public transit strikes, German courts de facto protect the right to strike in this sector. As a consequence, Germany regularly faces strikes by transit workers.

Our analysis exploits 71 one-day strikes in public transportation over the period from 2002 to 2011. We identify the daily effects of these strikes using both time series and cross-sectional variation in our data. In a first step, we estimate the impact on the total length of time that cars are in operation (henceforth, total car hours operated). To do so, we make use of two data sources. First, we use hourly information from official traffic monitors to estimate the effect of transit strikes on traffic volumes. Second, we use congestion data based on GPS speed measurements from *TomTom*, a global supplier of navigation and location products and services, to estimate the effect on travel times. Combining the two estimates allows us to compute the effect on total car hours operated. In a second step, we explore likely knock-on consequences by expanding the analysis in three directions. First, we assess the impact of strikes on the incidence and severity of car accidents using detailed register data, which includes all vehicle crashes recorded by the German police. Second, to investigate the effect on atmospheric pollution, we draw on hourly data from official air monitors. Third, we explore the effect on human health using register data, which includes information about all patients admitted to all German hospitals. Our identification strategy is based on a generalized difference-in-differences approach. It flexibly captures daytime and day-of-week patterns, seasonality effects, and long-run time trends, which are all allowed to vary by city.

What emerges is a picture of remarkable consistency. During the morning peak of a strike day, total car hours operated increase by 11 to 13 percent. This increase can be decomposed into two separate effects: a 2.5 to 4.3 percent increase in the number of cars on roads and a 8.4 percent increase in travel times. In addition, our results suggest that transit strikes pose a non-negligible threat to public safety and public health. We find a 14 percent increase in the number of vehicle crashes, which is accompanied by a 20 percent increase in accident-related personal injuries. Moreover, we observe that transit strikes have sizable effects on ambient air pollution. Emissions of particulate matter increase by 14 percent, while nitrogen dioxide concentrations in ambient air increase by 4 percent. Finally, analyzing health outcomes related to air pollution, we find that young children are subject to negative health effects. Among this subgroup, hospital admissions for respiratory diseases increase by 11 percent on strike days.

The costs of strikes—both to the parties directly involved in a dispute and to the public at large—have been the subject of extensive research since the mid-twentieth century. Until the 1990s, the main conclusion of the literature was that strikes impose significant financial costs on the workers and the firm directly involved in walkouts, but only negligible costs in most cases on noninvolved third parties (Kaufmann 1992). Our study firmly rejects this conclusion: based on our estimates, the increase in aggregate travel time caused by a single strike corresponds to 1,550 full-time equivalent work weeks. This translates into third-party congestion costs of €3.2 million per strike or €228.9 million for all 71 strikes in our sample.

Our work complements a small but impressive literature in economics analyzing the impact of strikes. Focusing on the hospital sector, Gruber and Kleiner (2012) investigate the effects of nurses' strikes on patient outcomes. After controlling for time and hospital specific heterogeneity, they observe increased mortality and readmission rates, and conclude that strikes in hospitals kill.[3] Examining walkouts in the education sector, Belot and Webbink (2010) and Baker (2013) find that teacher strikes had negative effects on student achievement in Belgium and Canada. Finally, there are a few interesting studies of strike impact in the private sector. Krueger and Mas (2004) show that strikes in tire production facilities decreased the quality of tires resulting in an increase of fatal accidents. In a similar vein, Mas (2008) finds that strikes at *Caterpillar* led to lower product quality.

In comparison to other strikes that have been studied in the literature, there is one specific aspect about urban public transport that makes it an intriguing case to study: the population at risk from strikes is potentially very large and likely to be affected along multiple dimensions. This is due to several interrelated facts: (i) in many advanced cities, the two major modes of transportation are private vehicles and public transit; (ii) urban public transport is typically provided under monopoly conditions—either by public sector companies or by operators working under licenses granted by public authorities; (iii) without the availability of a close substitute, public transit strikes are likely to significantly disrupt the normal travel of transit riders and disturb traffic patterns by increasing the use of private vehicles; (iv) two of the main externalities associated with an increase in the usage of private

[3] This result contradict earlier studies that did not as rigorously control for unobserved factors (see, e.g., Cunningham et al. 2008; Pantell and Irwin 1979). Another study by Mustard et al. (1995) highlights that there are fewer caesarian births during strike periods, which is suggestive of behavioral effects in hospitals.

cars are traffic accidents and air pollution, and entire city populations—not just transit users—may be adversely affected in each of these areas when public transport shuts down. Quantifying these potential impacts is not just interesting in itself, but also an important ingredient to meaningful discussions about the regulation of labor relations in sectors providing services regarded as public or essential.[4]

The remainder of the paper is organized as follows. Section I provides the institutional setting and discusses how transit strikes might affect cities and their inhabitants. Section II describes the data. Section III outlines the empirical strategy, followed by the results in Section IV. Section V discusses the size of the effects by monetizing the third party costs of transit strikes and comparing them to the private costs of struck employers.

I. Background

A. *The Role of Public Transit and the Regulation of Labor Relations*

The five largest German cities, home to roughly 8.2 million people, are characterized by an intensive use of public transportation. In 2013, Berlin, Hamburg, Munich, Cologne, and Frankfurt together accounted for a total number of 3.4 billion public transit users in their metropolitan areas.[5] This corresponds to an average 9.3 million passengers a day. In Berlin, the German capital, roughly 43 percent of commuters use public transit, while about 38 percent travel by car (Wingerter 2014). Public transportation networks are extensive in all sample cities. In Hamburg, for example, the transportation network comprises 91 subway stations, 68 suburban train stations (S-Bahn), more than 1,300 bus stops connecting a network of nearly 1,200 km in a city with less than 2 million inhabitants. The importance of public transportation in major German cities is comparable to the role it plays in the largest city in the United States. New York City has a population of roughly 8.4 million people. In 2014, its Metropolitan Transportation Authority moved about 9 million riders per day or 3.3 billion passengers a year on subways, buses, and railroads.[6] Approximately 56 percent of commuters in New York City use public transit, while about 27 percent travel by car.[7]

While the use of mass transit in New York City and major German cities is comparable, the regulation of labor relations in the public transportation sector differs markedly. As mentioned above, New York City's *Taylor Law* prohibits strikes by transit workers under the threat of harsh penalties. Other cities in the United States

[4] Other important questions arise in this context. As an example, it would be interesting to consider the flip side of the coin and ask: do strikes give employees of a monopoly provider of an essential service leverage to capture higher wages at the expense of the public. It is beyond the scope of this paper to consider that issue.

[5] 1,321 million passengers in Berlin (see http://www.vbb.de/de/article/verkehrsverbund/derverbund-in-zahlen/12552.html), 855 million passengers in Hamburg (see http://www.hvv.de/pdf/aktuelles/publikationen/hvv_zahlenspiegel_2013.pdf), 663 million passengers in Munich (see http://www.mvv-muenchen.de/de/der-mvv/mvv-in-zahlen/), 277 million passengers in Cologne (see http://www.kvb-koeln.de/newsfiles/310b-105c8ee08bf447f1df1f89cd3a87.pdf), and 203 million passengers in Frankfurt (see http://www.traffiq.de/1483.de.presse_informationen.html_pi=126798).

[6] For details see http://www.apta.com/resources/statistics/Documents/Ridership/2014-q2-ridership-APTA.pdf.

[7] See US Census Bureau, 2009–2013 American Community Survey 5-Year Estimates, Tables GCT0802, GCT0803, and GCT0804.

with no-transit-strike laws include Chicago, Boston, and Washington, DC. For a German, it must come as a surprise that many countries impose de jure restrictions on strikes in the public transportation sector. Indeed, in Germany, the right to strike is a fundamental right based on the Freedom of Association (*Koalitionsfreiheit*) as laid out in Article 9(3) of the constitution (*Grundgesetz*). Only civil servants, judges, and soldiers are excluded from the right to strike. Until the 1990s, the big infrastructure industries—i.e., telecommunications, postal, and public transportation services—were state monopolies. Workers in these industries had civil servant status and thus were not allowed to strike. However, when these industries were gradually privatized during the 1990s, newly hired workers were no longer given civil servant status and therefore gained the right to strike. Today, public transit workers, whether employed by Germany's rail operator *Deutsche Bahn* or local public transport providers, are allowed to engage in industrial action. The only de facto restriction on transit workers' right to strike is that the parties of an industrial conflict are responsible for the provision of a minimum service (Klaß et al. 2008). This is intended to act as a balance of their interests with those of non-involved third parties.[8]

In Germany, industrial action by transit workers is typically announced one day ahead of a strike. However, at that time, there is still substantial uncertainty as to exactly which services will be affected and to what degree. Thus, the actual extent of a strike cannot be clearly assessed prior to the start of a strike. The strikes we exploit in this study have the following feature in common: they do not shutdown the entire transportation system, but there are significant distortions in terms of service frequency. As a rule of thumb, at least one-third and up to two-thirds of all connections in affected cities are canceled or severely delayed on strike days. After the official end of a strike, it usually takes some hours until service is back to normal.

Having described the context and setting of our study, we now go on to discuss how urban populations might be affected by public transit strikes.

B. *Public Transit Strikes and Car Traffic*

Given the intensive use of public transportation in major German cities, we expect strikes by transit workers to have profound short-run effects on the mode of transport of commuters. Some might feel forced to use their private car or motorbike or a taxi on strike days. Others might switch to their bike or just walk. Again others might postpone their journey. Van Exel and Rietveld (2001) summarize the existing evidence as follows: public transit strikes induce most public transit users to switch to the car (either as driver or passenger) and as a result traffic density as well as road congestion increases. A similar conclusion is reached by Anderson (2014), who analyzes freeway traffic during a 35-day strike by transit workers in Los Angeles. His estimations reveal an increase in delays during peak periods by almost 50 percent

[8] Another restriction implicit in the German constitution is the so-called principle of *ultima ratio*. This principle represents the application of the general constitutional principle of proportionality (*Verhältnismäßigkeit*) in the field of labor law. According to this principle, a strike is only legal if it is necessary and the ultimate measure to solve an industrial conflict. Labor courts are empowered to assess the proportionality of industrial action and can, if necessary, sanction illegal strikes (Klaß et al. 2008).

due to increased car traffic.[9] Finally, Adler and van Ommeren (2015) exploit transit strikes in Rotterdam and also find positive effects of transit shutdowns on congestion. Based on these findings we formulate our first testable prediction.

PREDICTION 1: *Public transit strikes increase the number of cars on roads, especially during peak periods. Travel times increase due to rising traffic congestion.*

C. *Car Traffic and Accidents*

The frequency and severity of road accidents depends on several traffic characteristics that may be affected by public transit strikes. Examples we have in mind include the number of cars in road systems, driving skills, driver behavior, and speed. First, an often-used specification by transport economists suggests that the expected number of road accidents rises with the number of potential accidents which, in turn, is an increasing function of the number of cars in the system (Shefer and Rietveld 1997). Edlin and Karaca-Mandic (2006) confirm this prediction by showing that traffic density increases accident costs substantially. Second, the expected number of road accidents is a function of the behavior and skills of drivers. In this regard, we would expect that public transit strikes reduce average driving skills since marginal drivers with less experience appear on road systems. This channel works to increase the frequency of road accidents. In addition, it is well understood that driving in high-density traffic can contribute to stress and therefore lead to behavioral patterns—e.g., tailgating, aggressive driving, braking abruptly—that increase accident risk (Transport Research Center 2007). More accidents are likely to result in additional personal injuries (Shefer and Rietveld 1997). However, the same logic does not necessarily apply to accidents involving severe injuries or fatalities: with an increase in congestion stemming from more cars in the system, average travel speed decreases, thus potentially causing a reduction in the number of severe accidents. Evidence from the United States indeed suggests a substantial reduction in the number of fatal road accidents during morning peak hours, periods in which traffic density is the highest (Farmer and Williams 2005). But there is also evidence, emerging from the United Kingdom, that the picture is more differentiated. In particular, congestion as a mitigator of crash severity is less likely to occur in urban conditions, but may still be a factor on higher speed roads and highways (Noland and Quddus 2005). Our focus will be on accidents in urban conditions. Thus, it remains a priori unclear whether an increase in congestion stemming from public transit strikes affects the incidence of severe accidents, and if so in what direction. Against this background, our second testable prediction is:

PREDICTION 2: *Public transit strikes increase the frequency of car accidents which, in turn, leads to a rise in accident-related injuries. The effect on accidents involving severe injuries or fatalities is a priori unclear.*

[9] Lo and Hall (2006) analyze the same strike using a simple before and after comparison, which has some methodological shortcomings as noted by Anderson (2014).

D. *Car Traffic and Air Pollution*

Car traffic is associated with air pollution mainly due to engine exhaust. The chemical processes in fuel burning thus determine the expected effect of traffic on air pollution. Internal combustion engines powering the vast majority of cars in developed countries emit oxides of nitrogen, carbon monoxide, unburned or partially burned organic compounds, and particulate matter with the amounts depending amongst other things on operating conditions (Heywood 1988). In particular, it is well understood that congested stop-and-go traffic is associated with higher emissions than free-flow traffic. There are three reasons for this. First, the efficiency of internal combustion engines, which depends on revolutions per minute (rpm), is highest at medium speed (Davis and Diegel 2007). Acceleration and deceleration episodes decrease the time operated in the optimal rpm range, which in turn increases emissions per minute driven. Second, congestion increases travel times, and so leads to a rise in fuel consumption and emissions per distance driven. Third, particulate matter emissions not only stem from fuel burning process, but also from brake wear and tire wear on tarmac—both high in congested traffic. From an empirical viewpoint, several studies suggest that high traffic volumes and congestion are causes of ambient air pollution (see, e.g., Currie and Walker 2011; Knittel, Miller, and Sanders 2011). A pollutant that is not caused by car traffic, and therefore can be used for a placebo test, is sulfur dioxide (Lalive, Luechinger, and Schmutzler 2013). Indeed, sulfur dioxide emissions from cars are close to nonexistent since modern gasoline no longer contains significant amounts of sulfur. From these arguments our third testable prediction arises:

PREDICTION 3: *Public transit strikes increase road-traffic related air pollution. A pollutant expected to be unaffected is sulfur dioxide.*

E. *Air Pollution and Health*

The exact pathophysiological effects of most air pollutants are not yet fully understood. However, a large body of research across many different disciplines suggests that exposure to air pollution can impair human health, even at pollution levels well below the limits set in developed countries (Beelen et al. 2014). The identified effects range from respiratory symptoms and illness, impaired lung function, hospitalization for respiratory and cardiac disease, to increases in mortality. The most harmful of the air pollutants stemming from car traffic is thought to be particulate matter. It is also widely accepted that infants and children are the subgroup of the population most susceptible to the effects of air pollution. This is mainly due to their ongoing respiratory development, smaller average lung size, and higher activity levels (Beatty and Shimshack 2014). Furthermore, elderly people are at increased risk due to more frequent unfavorable health preconditions.[10]

[10] It is important to note that the relationship between car traffic and health might not only be mediated through air pollution. In particular, there is some evidence that traffic-related factors, such as stress and noise, may also trigger cardiovascular diseases (see, e.g., Peters et al. 2004).

Much of what we know about pollution-related health problems is based on annual frequency data (see, e.g., Chay and Greenstone 2003; Currie and Neidell 2005; Currie et al. 2009). In contrast, our empirical analysis explores the daily, contemporaneous effect of public transit strikes on pollution-related health outcomes. This reduced-form is based on the idea that public transit strikes cause daily pollution shocks due to increased car traffic and congestion. Should we expect a short-term effect of air pollution on health? The existing evidence, while still relatively scarce, points toward an affirmative answer. Schlenker and Walker (2016) show that daily variation in ground level airport congestion due to network delays significantly increases both carbon monoxide emissions as well as hospital admissions for respiratory problems and heart disease. Their findings also suggest that infants and the elderly have a higher sensitivity to pollution fluctuations. In a similar vein, Atkinson et al. (1999) show that there is a positive association between daily emissions of particulate matter and daily visits to accident and emergency departments in London for respiratory complaints.[11] Ransom and Pope (1992) exploit monthly variation in particulate matter emissions induced by the closure of a steel mill in Utah Valley, and find large effects on school absenteeism—a proxy for children's health. With this evidence in mind, we formulate our final testable prediction:

PREDICTION 4: *Public transit strikes increase pollution-related health problems, especially among young children and the elderly.*

II. The Data

Our main sample spans the period from 2002 to 2011 and covers the five largest cities in Germany: Berlin, Hamburg, Munich, Cologne, and Frankfurt on the Main. We exploit six sources of data to analyze the extent to which the inhabitants of these cities are affected by public transit strikes.[12]

A. *Strike Data*

Our data on public transit strikes is self-collected and comes from newspaper archives, press releases of unions, and official notifications of public transit operators. In order to ensure an accurate identification of strike activity, we employed a double-check procedure in the information-gathering process. In particular, we only coded a day as a strike day if congruent information from at least two independent sources indicated an episode of industrial action. During the sample period, from 2002 to 2011, unions calling strikes rarely resorted to lengthy campaigns of

[11] Relatedly, Schwartz and Dockery (1992) find that daily mortality in Philadelphia is positively associated with daily particulate matter pollution.

[12] The description of the data in the main body conveys core information only. In online Appendix Table A2, we present detailed summary statistics.

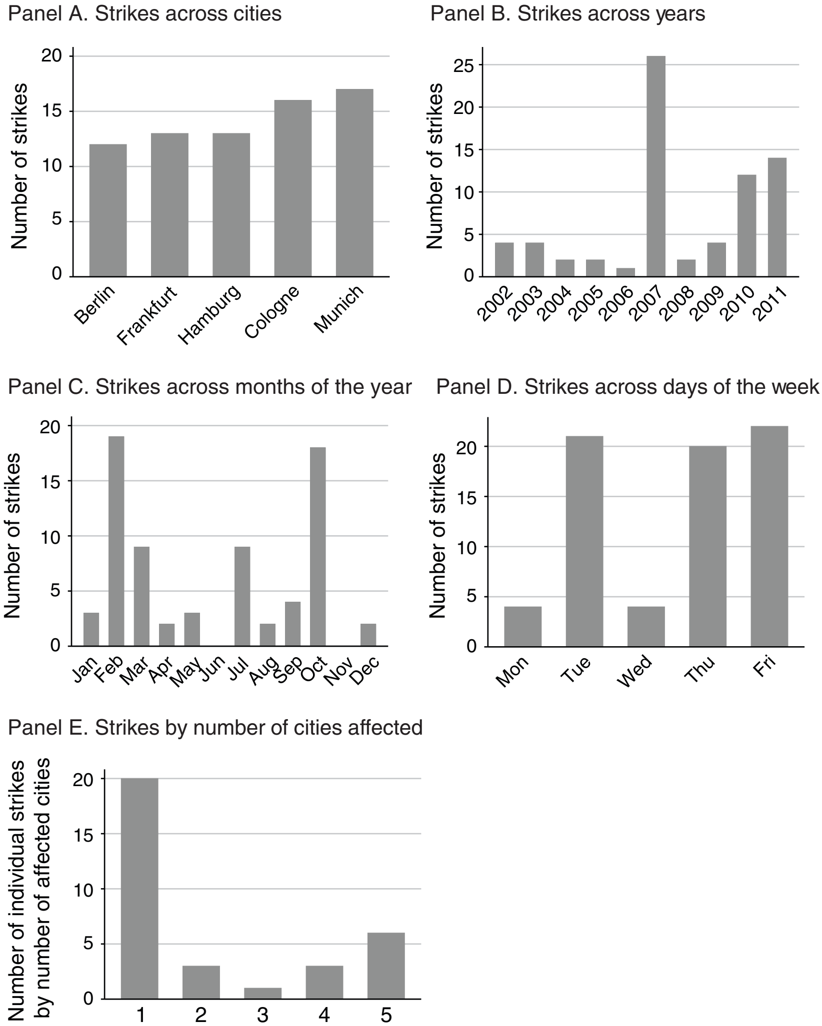

Figure 2. Distribution of Strikes across Time and Space

industrial action. Instead, the tactical use of one-day strikes was the norm.[13] We therefore only include public transit strikes of one day or less in our main sample, which leaves us with 71 incidences of strike activity across all cities.[14] The observed strikes either affect a city's local suburban train connections (*S-Bahn*) or its subway-tram-bus network.[15] Figure 2 illustrates the distribution of strike activity

[13] In the data collection process, one specific reason for this tactic became apparent: strikes by German transit workers typically cause a great deal of initial disruption, but within a day or two of lengthier strikes many transit providers manage to implement effective emergency schedules which considerably dampen the impact of strikes.

[14] We also identified 17 public transit strikes with a duration of more than a day. The days affected by these lengthier strikes—-amounting to a total of 74 city-day observations—-are dropped from our main sample. In Section IVG, we present regressions based on a sample that includes both one-day and multiday strikes.

[15] In German cities, suburban train connections are run by Germany's rail operator *Deutsche Bahn*, while subway-tram-bus networks are operated by local transit providers. Workers respectively employed by *Deutsche Bahn* and local transit providers are represented by different unions, who usually do not call strikes simultaneously.

across time and space. We observe 12 strikes in Berlin, 13 in Frankfurt on the Main and Hamburg, 16 in Cologne, and 17 in Munich. At least one strike occurred in each year of the study period, and there is a pronounced spike in strike activity in 2007. The reason for this spike is that, in 2007, the German Train Drivers' Union pushed for a collective agreement involving a 40 percent salary increase for its members; *Deutsche Bahn*, the main German rail operator, was not prepared to negotiate over this proposed increase, which led to pronounced strike activity. Going back to Figure 2, we see that all strikes took place on weekdays, while weekends were unaffected. We observe strikes in all months of the year except in June and November. Finally, in our period of observation, unions rarely called strikes affecting all five cities simultaneously. Quite to the contrary, 20 strikes only affected a single city. In addition, three strikes affected two cities, one strike affected three cities, three strikes affected four cities, and on six occasions all five cities were simultaneously hit by a strike. Thus, we are able to exploit both time series and cross-sectional variation in our data. Table A1 of the online Appendix provides detailed dates of all public transit strikes in the sample period.

Our empirical analysis focuses on workweek days since this is when congestion occurs. Furthermore, there was no strike activity on weekends during the study period, as described above. Thus, we exclude weekends and public holidays[16] from our dataset.

B. *Traffic Data*

We obtained data on traffic volumes from the Federal Highway Research Institute (*Bundesanstalt für Straßenwesen*, *BASt*). Automated monitors operated by *BASt* collect hourly data on the number of passing vehicles on all freeways (*Autobahnen*) and non-freeway federal roads (*Bundesstraßen*) across Germany. The monitors are technically equipped to distinguish between car and truck traffic. Thus, we are able to execute a clean empirical test of the prediction that public transit strikes lead to an increase in car traffic. We include a total of 43 traffic monitors in our sample, all selected based on their locations on commuter routes into the cities of interest and their proximity to the respective city centers. In Figures A1–A5 in the online Appendix, we use the geocodes of the monitors to display their exact locations on city maps. As can be seen from the figures, 27 monitors are located on freeways, while 16 monitors are located on federal roads.[17] The empirical analysis is based on hourly traffic data for the period January 1, 2002, to December 31, 2011. Due to maintenance work and upgrading, no data are available for Berlin from 2006 to 2010. Similarly, values are missing for Frankfurt on the Main in 2004 and 2005. Figure A6 in the online Appendix shows how passenger vehicle flows change over the course of 24 hours for an average workweek day. There are two peak periods for car traffic. The first is between 6 AM and 10 AM in the morning when car traffic is nearly 85 percent higher as compared to the average hour. The second peak is

[16] Public holidays also include the carnival days from Fat Thursday (*Weiberfastnacht*) to Shrove Tuesday (*Faschingsdienstag*), which can be regarded as de facto holidays.

[17] Note that our sample does not include any monitors located on federal roads in Cologne.

between 3 PM and 7 PM in the afternoon. Based on these patterns, we define the morning peak (respectively, evening peak) to last from 6 AM to 10 AM (respectively, from 3 PM to 7 PM).

C. *Congestion Data*

We obtained data on traffic congestion from *TomTom*, a global supplier of location and navigation products and services. Since 2008, *TomTom* has been collecting anonymous GPS speed measurements from navigation users across cities around the globe.[18] In a map-matching process, the GPS measurements are matched to digital city maps and assigned to road segments that vary in length between 2 meters and 2 kilometers (km), depending on the complexity of the road situation. For our sample cities, speed measurements exist for road segments that add up to 3,637 km in Berlin, 2,263 km in Hamburg, 2,041 km in Munich, 1,988 km in Cologne, and 662 km in Frankfurt. When the map-matching process is complete, an aggregated geographic database (geobase) of measured road speeds is produced. These geobases are updated regularly for each map of each city to take into account the growing GPS speed database as well as changes in the road network (map). Each digital city map with attached speed information can be used to compute an average congestion index (CI) at daily frequencies. This index is defined as

$$CI = \frac{T}{T_0}.$$

It compares actual travel times on all road segments in a city during the course of a day (T) to the free-flow travel times on these road segments when posted speed limits are adhered to (T_0). The difference is expressed as a percentage increase in travel time. Thus, a CI value of 1 implies that traffic was flowing freely throughout a day, while a value of 1.2 indicates that journeys took on average 20 percent longer than under non-congested conditions. In addition to the daily CI, we have access to daytime-specific CIs for the morning and the evening peak periods,[19] as well as separate CIs for freeways and city streets. The CI data we obtained cover each city in our sample and span the period from January 1, 2010 through to December 31, 2011. The average daily CI value is 1.31, which drops to 1.25 for highways and increases to 1.36 for city streets. As one would expect, the average CI values for the morning peak period, 1.47, and the evening peak period, 1.49, are higher than the average daily value.

[18] As of 2014, the GPS speed database contained 6 trillion measurements and grew by 6 billion measurements a day.

[19] The congestion indices from the data provider are predefined variables that are aggregated at the city-day level. Peak morning congestion times are 8 AM to 9 AM on workweek days. Peak evening congestion times are 5 PM to 6 PM from Monday to Thursday for Hamburg, Munich, Cologne, and Frankfurt, and 4 PM to 5 PM for Berlin. On Fridays they are 3 PM to 4 PM for Berlin, Hamburg, and Cologne, and 5 PM to 6 PM for Munich and Frankfurt.

D. *Accident Data*

Our information on accidents is based on register data, which includes *all* vehicle crashes recorded by the German police. The police records are collected and made available by the Reserch Data Centres of the Federal Statistical Office and the statistical offices of the Länder (*Statistik der Straßenverkehrsunfälle*). Note that the police do not forward records on minor accidents to the statistical offices, which are therefore not present in our database.[20] Each police record includes a wide variety of information about the accident (such as time, date, location) together with a description of the number and types of injuries sustained in the accident. For the five cities in our sample, the police records available for the period 2002–2011 cover just over 354,400 vehicle crashes. We aggregate the police records to the city-day level while distinguishing between the morning and the evening peak hours. This procedure leaves us with a dataset containing daily observations for the AM and PM peak period on (i) the number of vehicle crashes, (ii) the number of slightly injured persons, and (iii) the number of seriously or fatally injured persons.

E. *Pollution Data*

For the period 2002–2011, we obtained hourly data on atmospheric pollution from the Federal Environment Agency (*Umweltbundesamt*, *UBA*), which operates numerous air monitors across Germany. We include a total of 30 monitors in our sample, all selected based on their locations on streets within the five cities' boundaries.[21] Figures A1–A5 of the online Appendix show the locations of the monitors on city maps. We focus on two types of pollutants: inhalable coarse particles smaller than 10 micrometers in diameter (PM_{10}) and nitrogen dioxide (NO_2).[22] In addition, we use sulfur dioxide (SO_2) as a placebo pollutant in a falsification test. Figure A7 in the online Appendix shows how air pollution varies over the course of 24 hours for an average day of the workweek. For both PM_{10} and NO_2, there are emission peaks during the morning and evening hours, respectively. We create pollution measures for the morning peak period (respectively, evening peak period) by taking the averages of all hourly readings between 6 AM and 10 AM (respectively, between 3 PM and 7 PM).

F. *Hospitalizations: Diagnostic Data*

We use data from the Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder (Krakenhousstatistik Teil II) for the years

[20] Minor accidents are those in which (i) crashed vehicles remain in roadworthy condition and (ii) all persons involved remain uninjured. In addition, the statistical offices do not provide access to information on vehicle crashes in which drivers were under the influence of alcohol. Thus, alcohol-related accidents are not included in our database. Finally, our database does not include accidents in which the parties involved reached private agreements without involving the police.

[21] We exclude monitors that are situated around industrial areas, since these monitors capture air quality contaminant concentrations that relate to the industrial operators in the area.

[22] In an earlier version of this paper, we also examined carbon monoxide (CO) and found little evidence for a strike effect on this pollutant.

2002–2010. The dataset provides information about *all* inpatients in *all* German hospitals. In particular, the following characteristics are collected for each patient: main diagnosis (three-digit ICD-10 code),[23] day of admission and discharge (day, month, year), place of residence (county), and month and year of birth, as well as gender. In order to examine pollution-related health problems, we focus on hospital admissions for diseases of the respiratory system (ICD-10 codes J00–J99) and abnormalities of breathing (ICD-10 code R06). For each type of diagnosis, we aggregate the number of hospitalizations by day of admission and patients' city of residence. Hence, we obtain daily counts of hospitalizations, which we examine both for the entire population as well as for the population subgroups of those over 64 years of age and under 5 years of age.

G. *Weather and Holiday Data*

We obtained city-specific weather data at daily frequencies from the German Weather Agency (*Deutscher Wetterdienst*) for the years 2002–2011. In particular, we use daily measures of temperature, precipitation, wind speed, and a binary variable indicating snow cover to control for the direct effects of weather on the five outcomes of interest.[24] To control for the direct effects of school holidays, we construct city-day dummy variables equal to unity when school holidays are in effect and zero otherwise. Our holiday data comes from the Standing Conference of the Ministers of Education and Cultural Affairs of the German states (*Kultusministerkonferenz*).

III. Empirical Strategy

Our identification strategy is based on a generalized difference-in-differences (DID) model which essentially compares outcomes in affected and non-affected cities before, during, and after strike episodes. We now present our approach for regressions involving data at the monitor-hour level (car traffic). In this case, we estimate our basic specification as follows:

$$(1)\quad Y_{mchdwy} = \alpha + \beta(STRIKE_{cdwy}) + \gamma_h \times \delta_d + \eta_w + \theta_y + \vartheta_m + \mu X_{cdwy} + \epsilon_{mchdwy},$$

where Y_{mchdwy} is the number of cars passing monitor m in city c during hour h on day d in week w of year y. $STRIKE_{cdwy}$ is a binary variable equal to unity when a strike is in effect and zero otherwise. We control for a full set of day-of-week-specific hour fixed effects ($\gamma_h \times \delta_d$), week-of-year fixed effects (η_w), and year fixed effects (θ_y). The interactions between day-of-week and hour-of-day take into account that hourly traffic patterns might differ between days. Thus, we flexibly capture daytime and day-of-week patterns, seasonal effects, and long-run time trends. By additionally including fixed effects for all monitors (ϑ_m), we account for time-constant

[23] The ICD-10 classification ("International Statistical Classification of Diseases and Related Health Problems") categorizes diseases and other health problems recorded on many types of health and vital records.

[24] Few missing observations for wind speed cause our number of observations to drop slightly when including controls.

differences between monitoring stations. The vector X_{cdwy} includes holiday and weather controls. In our preferred specification, we additionally allow for city-specific time fixed effects by including interactions of city indicators with hour-of-day $\times$ day-of-week, week-of-year, and year. Moreover, our preferred specification also controls for city-specific weather effects by interacting city indicators with all weather variables. When outcome variables are observed at the monitor-level with more than one station per city, we weight regressions by the inverse of the number of observations in each city. This weighting procedure ensures that each city is given the same weight in the regressions. For regressions involving data aggregated to the monitor-day level (air pollution), we control for day-of-week fixed effects and drop the interactions between hour-of-day and day-of-week. For data aggregated to the city-day level (congestion, accidents, health), we additionally replace monitor fixed effects with city fixed effects.

In our setting, standard errors might be biased due to serial correlation. We therefore follow Bertrand, Duflo, and Mullainathan (2004) in clustering standard errors at the city level, the highest aggregation level where correlation may occur. In order to account for the small number of clusters, the Wald test uses a conservative $T(G-1)$ distribution to compute p-values, with G being the number of clusters. Since the ad-hoc corrections for few clusters might still understate the true size of the standard errors, we also check whether our results hold using wild cluster bootstrap t-procedures (Cameron, Gelbach, and Miller 2008). To do so, we create pseudo-samples applying cluster-specific Rademacher weights ($+1$ and -1 with equal probabilities) to the residuals of the original regression under the null hypothesis of no strike effect. We then estimate the strike effect on the pseudo-samples holding the vector of controls constant. Thus, we receive a distribution of t-values, which is finally used for statistical inference. In the results section, we will focus on models using clustered standard errors to draw statistical inference. However, virtually all findings are confirmed if we instead use wild cluster bootstrap t-procedures.

We assume that conditional on the covariates, the location and timing of strike activity is orthogonal to traffic volumes, travel times, accident risk, pollution emissions, and health. A potential threat to identification arises if public transit strikes are planned to cause maximum disruption. If this is the case, one might expect the timing of strikes to coincide with hours of the day and/or days of the week during which traffic density is the highest. Note, however, that we control for this type of confounding variation by including hour-of-day and day-of-week fixed effects as well as the interaction between them. Union leaders may also choose to initiate strikes at location-time combinations where they are likely to cause maximum disruption. In our most extensive specification, we account for this possibility by including a full set of city-specific time fixed effects in addition to the monitor or city fixed effects. There are other occasions where the impact of strikes is conceivably high: at the beginning of holidays or during periods of bad weather. Again, these candidate confounders are controlled for. In addition to suitable conditioning, we conduct a number of sensitivity checks to support our design and identifying assumption. In particular, we examine whether the estimated effects of interest are robust to the inclusion of additional city-specific time-varying covariates (e.g., mass

Table 1—The Effect of Strikes on Car Traffic

Dependent variable: Hourly passenger vehicle flows per monitor	(1)	(2)	(3)	(4)
Panel A. Freeways—morning peak				
Strike	160.4	136.8	128.3	131.6
[5,240]	(27.01)	(10.81)	(13.79)	(12.07)
Observations	212,896	212,896	212,896	212,896
R^2	0.899	0.903	0.921	0.922
Panel B. Federal roads—morning peak				
Strike	62.92	57.39	72.40	77.72
[1,790]	(13.05)	(15.33)	(8.00)	(9.49)
Observations	102,540	102,540	102,540	102,540
R^2	0.921	0.924	0.961	0.962
Panel C. Freeways—evening peak				
Strike	125.0	103.3	88.33	91.48
[5,786]	(28.56)	(21.22)	(19.70)	(17.87)
Observations	212,896	212,896	212,896	212,896
R^2	0.937	0.939	0.950	0.950
Panel D. Federal roads—evening peak				
Strike	21.43	13.73	26.29	37.89
[2,121]	(17.16)	(18.13)	(14.29)	(10.19)
Observations	102,540	102,540	102,540	102,540
R^2	0.961	0.962	0.972	0.973
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 64 for freeways, 45 for federal roads. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-hour level and include monitor fixed effects. Time FE include hour-of-day × day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

events). Moreover, we provide supportive evidence from regressions involving both placebo strikes as well as placebo outcomes.

IV. Results

A. *Car Traffic*

Table 1 reports the results for passenger vehicle flows. The first panel presents regression estimates involving only morning peak period data for freeways. The morning peak is defined to last from 6 AM to 10 AM. Column 1 estimates equation (1) conditioning only on monitor fixed effects and the full set of time fixed effects. In the morning peak hours of strike-free days, the average hourly traffic flow on freeways amounts to 5,240 passenger vehicles per monitor. During a strike, vehicle flows in the morning increase by 160 cars per hour and monitor an effect significant at the 1 percent level. Column 2 shows the result to be robust to including controls for local weather conditions and school holidays. In column 3, we interact the full set of time fixed effects with city indicators. Controlling for city-specific time effects in this way leaves the estimated strike effect largely unchanged. In column 4, we additionally interact the full set of weather controls with city indicators. The strike coefficient remains virtually unaffected and highly significant. The estimate from our preferred specification in column 4 suggests that public transit strikes lead to an increase in car traffic during the AM peak period by 2.5 percent. The second panel repeats the exercise for federal roads. The estimate from our preferred specification suggests a 4.3 percent increase in car traffic on federal roads during the AM peak of a strike day (column 4). The last two panels of Table 1 show the strike effects in the evening peak hours from 3 PM to 7 PM. Throughout all specifications, the strike effect turns out positive and significant for freeways. Moreover, in our preferred specification, the strike effect also gains statistical significance for federal roads. We observe that the estimates are somewhat smaller in size during the PM peak period than during the AM peak period, suggesting an increase in traffic flows by slightly less than 2 percent both on freeways and federal roads.[25]

Our data also allows us to provide a picture of strike impact over the course of a day. In Figure 3, we plot the results of a regression interacting our strike indicator with *all* hours of the day. For periods outside the morning and evening peak, strikes in public transportation leave traffic volumes virtually unaffected. For freeway traffic (panel A), significant hourly strike effects arise between 5 AM and 10 AM as well as between 1 PM and 7 PM. The most pronounced effect arises in the morning between 6 AM and 7 AM, when traffic volumes increase by 7.7 percent.[26] Compared to the AM peak effect of strikes, the PM peak effect is smaller but spreads out over a longer period. This might occur because commuters usually have more flexibility in decisions over departure time in the evening than in the morning commute. Supporting this argument, unreported regressions show that the strike effect on the total number of cars in the afternoon (i.e., after 12 AM noon) is just as large as the strike effect on the total number of cars in the morning (i.e., before 12 AM noon). For traffic on federal roads (panel B), the most pronounced strike effect also arises between 6 AM and 7 AM, when traffic volumes increase by 9.4 percent.[27] Moreover, the AM peak effects are again more pronounced than the PM peak effects.

Despite our flexible estimation approach, it is important to acknowledge the possibility that traffic is unusually high on strike days for reasons other than strikes being in effect. We now conduct a falsification test in order to rule out such confounding bias in our design. Recall that most strikes in our sample did not affect all five cities simultaneously. This allows us to geographically shift $STRIKE_{cdwy}$ from

[25] Since the monitors are positioned to capture traffic patterns on major commuter routes on normal days, the estimates might represent a lower-bound of the overall strike effect if drivers find alternative routes to their destinations on strike days.

[26] The effect size is 343 cars and the average number of cars during that hour is 4,478.

[27] The effect size is 140 cars and the average number of cars in during that hour is 1,483.

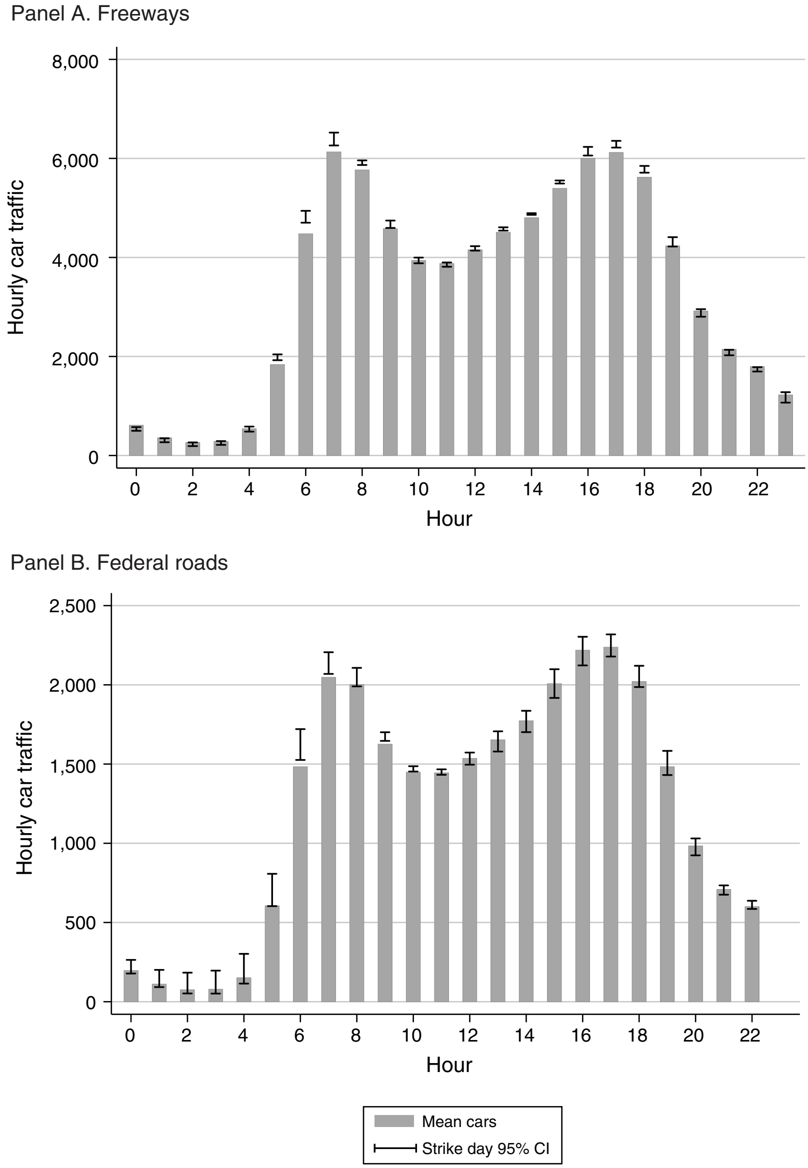

Figure 3. The Hourly Effect of Strikes on Car Traffic

Notes: The gray bars show the hourly mean of the number of cars passing monitoring stations from Monday to Friday. The black whiskers indicate the 95 percent confidence interval of the hourly strike effects that are added to the hourly mean numbers of cars. The strike effects are estimated in a regression controlling for monitor fixed effects and the full set of time fixed effects. Additional controls are the atmospheric temperature, amount of precipitation, wind speed, a snow cover dummy, and interactions of the full set of time fixed effects and the weather variables with city indicators. Standard errors are clustered at the city level.

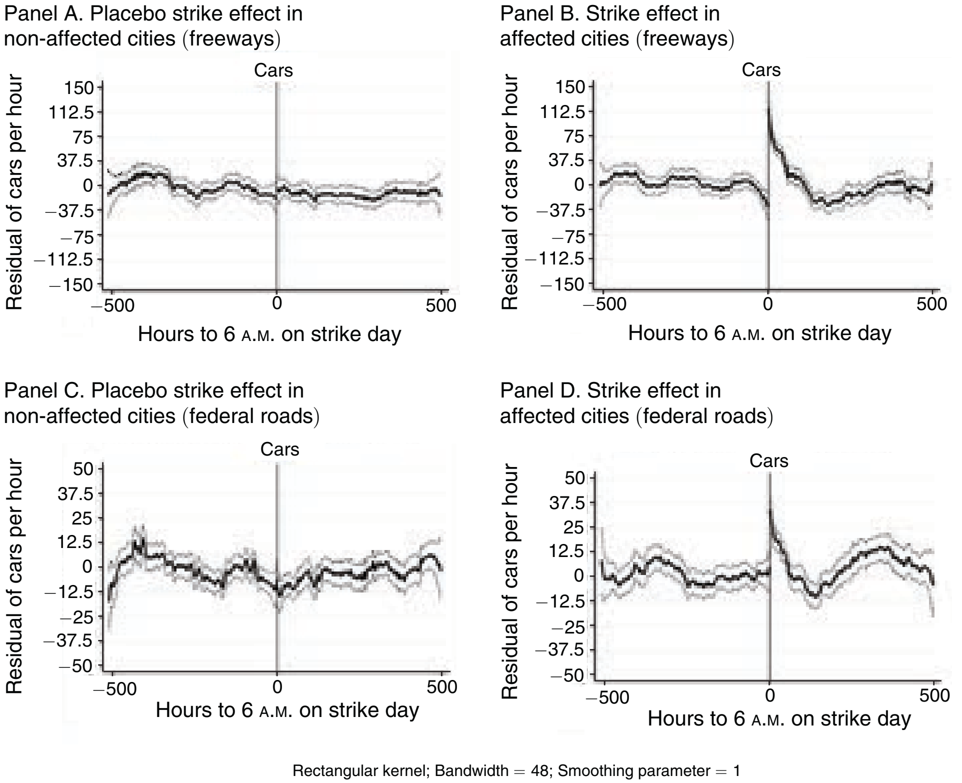

Figure 4. Placebo Strikes versus Actual Strikes

cities affected by strikes to non-affected cities. If our design is valid, then there should be no significant effects on car traffic in these non-affected cities.[28]

For graphical inspection, we first compute the residuals of a regression of the number of cars per hour on the most extensive set of control variables under the null hypothesis of no strike effect. We then plot the residuals of the number of cars per hour against hours, where 6 AM of a strike day is normalized to zero.[29] Thus, data points represent hourly averages of unexplained variations in vehicle flows. Based on these data points, we apply local polynomial smoothing techniques.[30] As is evident from the first panel of Figure 4, there is no jump in car traffic on freeways in non-affected cities when strikes begin elsewhere. Indeed, the unexplained variations in vehicle flows run absolutely smoothly across the placebo strike threshold. For affected cities, by contrast, there is a significant upward jump in car traffic when strikes begin, as can be seen in the second panel. Apart from this jump at the strike threshold, unexplained variations in vehicle flows are remarkably flat within a period of three weeks before and three after a strike episode. The last two panels

[28] Observations from struck cities are excluded from the placebo sample in order to exclude bias on the placebo control dates.

[29] For presentational reasons, we exclude data points left and right of the discontinuity that were also strike days.

[30] We follow Lee and Lemieux (2010) and use a rectangular kernel for the smoothing function with 1st-order polynomials and a bandwidth of 48 hours.

Table 2—The Effect of Placebo Strikes on Car Traffic

	Dependent variable: Hourly passenger vehicle flows per monitor			
	(1)	(2)	(3)	(4)
Panel A. Freeways—morning peak				
Placebo strike	−14.01	−28.37	−22.08	−16.17
[5,240]	(26.05)	(41.43)	(36.82)	(35.31)
Observations	211,572	211,572	211,572	211,572
R^2	0.899	0.903	0.921	0.922
Panel B. Federal roads—morning peak				
Placebo strike	16.85	7.179	−7.713	−6.834
[1,790]	(3.723)	(5.933)	(12.89)	(10.64)
Observations	101,960	101,960	101,960	101,960
R^2	0.921	0.924	0.961	0.962
Panel C. Freeways—evening peak				
Placebo strike	−14.48	−15.82	−11.82	−4.018
[5,786]	(13.16)	(18.23)	(22.22)	(20.03)
Observations	211,572	211,572	211,572	211,572
R^2	0.937	0.939	0.950	0.950
Panel D. Federal roads—evening peak				
Placebo strike	−6.320	−10.84	−21.50	−17.54
[2,121]	(7.643)	(11.27)	(18.74)	(16.13)
Observations	101,960	101,960	101,960	101,960
R^2	0.961	0.962	0.972	0.973
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day placebo strikes used in estimation sample: 74 for freeways, 61 for federal roads. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-hour level and include monitor fixed effects. Time FE include hour-of-day × day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

of Figure 4 repeat the exercise for federal roads and return qualitatively identical results. Table 2 presents the placebo analog of Table 1. Across all specifications, and for both morning and evening peak hours, the placebo effect of public transit strikes on vehicle flows on both freeways and federal roads is statistically insignificant[31] and small in magnitude, fluctuating around zero.

[31] One exception is the estimate in the minimum specification for federal roads during the morning peak.

B. *Travel Times*

Table 3 presents regressions estimating the effect of transit strikes on travel times. The dependent variables are congestion indices (CIs) based on *TomTom*'s GPS speed database. The first panel reports results using the CI for the morning peak period. The coefficient from our preferred specification indicates that the CI for the morning peak period increases by 0.123, an effect that is statistically significant at the 10 percent level ($p = 0.07$). Compared to the mean CI, this estimate implies that average morning travel times increase by 8.4 percent. As can be seen across columns 1 to 4, the sign and magnitude of the coefficient on our strike indicator is very robust across the four specifications. The second panel presents analogous estimates for the evening peak period, which are smaller in magnitude than the effects during the morning hours. Evaluated against the average evening CI of 1.49, the results suggest that average travel times in the evening increase between 3.8 and 4.4 percent, although the coefficients are not very precisely estimated. In the third panel, results for the average peak period hour are depicted. Our preferred specification in column 4 implies an increase of travel times by 6.3 percent, an estimate that just fails to reach conventional levels of statistical significance ($p = 0.06$). The fourth panel reports results of regressions using the CI averaged over the day as the dependent variable. The results suggest that strikes increase average travel times between 3.8 to 4.3 percent over the course of a day, and these estimates turn out to be statistically significant at the 1 percent level. In the last two panels, we use daily CIs for inner-city streets and highways, respectively. While the effects for city streets are more precisely estimated than for highways, the point estimates are almost identical and, depending on the specification, suggest increases in average travel times between 3.5 and 4.4 percent. Thus, strike-induced congestion spreads over all types of streets within cities and is not exclusive to freeways or inner city streets.

C. *Total Car Hours Operated*

In what follows, we will further investigate the effects of public transit strikes on accident risk and pollution emissions. Both outcomes are likely to depend on total car hours operated, which in turn are determined by the number of vehicles on roads and average travel time:

$$[\text{total car hours operated}] = [\#\text{ cars on roads}] \times [\varnothing \text{ travel time in hours}].$$

Our results so far suggest that strikes by transit workers affect both terms on the right-hand side of this equation, with the effects being strongest during the morning peak period. Indeed, during that period, strikes increase the number of passenger vehicles on roads by 2.5 percent (freeways) to 4.3 percent (federal roads) and raise travel times by 8.4 percent. Both effects combine according to

$$\begin{aligned}&\%\,\Delta[\text{total car hours operated}]\\ &\quad = \left[\left(1 + \frac{\%\,\Delta[\#\text{ cars on roads}]}{100}\right)\left(1 + \frac{\%\,\Delta[\varnothing\text{ travel time in hours}]}{100}\right) - 1\right] \times 100.\end{aligned}$$

Table 3—The Effect of Strikes on Travel Times

Dependent variable: Actual travel time divided by free-flow travel time (Congestion Index)	(1)	(2)	(3)	(4)
Panel A. Morning peak				
Strike	0.117	0.134	0.123	0.123
[1.47]	(0.052)	(0.042)	(0.049)	(0.051)
Observations	2,454	2,454	2,454	2,454
R^2	0.392	0.539	0.621	0.630
Panel B. Evening peak				
Strike	0.056	0.065	0.064	0.062
[1.49]	(0.020)	(0.024)	(0.027)	(0.030)
Observations	2,454	2,454	2,454	2,454
R^2	0.291	0.351	0.473	0.482
Panel C. All peaks				
Strike	0.086	0.099	0.094	0.093
[1.48]	(0.030)	(0.028)	(0.033)	(0.036)
Observations	2,454	2,454	2,454	2,454
R^2	0.329	0.474	0.583	0.594
Panel D. All day				
Strike	0.050	0.056	0.051	0.050
[1.30]	(0.013)	(0.013)	(0.016)	(0.017)
Observations	2,454	2,454	2,454	2,454
R^2	0.306	0.397	0.533	0.547
Panel E. City streets—all day				
Strike	0.047	0.054	0.048	0.048
[1.36]	(0.007)	(0.011)	(0.009)	(0.010)
Observations	2,454	2,454	2,454	2,454
R^2	0.350	0.478	0.591	0.601
Panel F. Freeways—all day				
Strike	0.050	0.055	0.051	0.049
[1.25]	(0.020)	(0.019)	(0.022)	(0.024)
Observations	2,454	2,454	2,454	2,454
R^2	0.252	0.308	0.503	0.522
City FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 26. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the city-day level and include city fixed effects. Time FE include day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Cluster-robust standard errors are in parentheses.

During the AM peak period, public transit strikes therefore lead to a 11 to 13 percent increase in total car hours operated. This is the benchmark against which we will evaluate subsequent results on accident risk and pollution emissions.

D. *Vehicle Crashes and Accident-Related Injuries*

Table 4 reports the effects of public transit strikes on vehicle crashes and accident-related injuries. The first panel uses the number of vehicle crashes during the AM peak period as the dependent variable. In the morning peak hours of strike-free days, there are on average 4.28 vehicle crashes per city. During a strike, the number of vehicle crashes in the morning hours increases by 0.607, or 14.2 percent of the strike-free level (column 4). This increase is statistically significant at the 10 percent level ($p = 0.07$) and remains very stable regardless of the specification. The second panel reports the results for the number of persons sustaining slight injuries in vehicle crashes. Focusing on our preferred specification (column 4), we find that strikes increase the number of slightly injured persons by 0.790, an effect significant at the 5 percent level. Compared to the 3.94 personal injuries we observe during the morning hours of strike-fee days, this corresponds to a 20.1 percent increase. Finally, there is no significant effect on the number of seriously or fatally injured persons, as is evident from the results reported in the third panel. In the last three panels of Table 4, we repeat the exercise using accident data for the PM peak period. All evening estimates on vehicles crashes and accident-related injuries are statistically insignificant.

E. *Air Pollution*

Table 5 contains two central results on air pollution. The estimates in the first panel indicate that public transit strikes have a statistically significant and positive effect on morning peak emissions from particulate matter, a major traffic-related pollutant. In particular, the results in columns 1 to 4 imply that particle pollution significantly increases by 13.3 to 14.8 percent during the AM peak hours of a strike day. The results in the second panel suggest that public transit strikes also have positive effects on morning peak emissions of nitrogen dioxide, although the coefficients are significant only at the 10 percent level. For example, our preferred specification (column 4) yields an increase of NO_2 by 3.31 $\mu g/m^3$ ($p = 0.08$), or 4.3 percent of the strike-free level. In the last two panels of Table 5, we repeat the exercise using pollution data for the PM peak period. All evening estimates on air pollution are statistically insignificant.

A potential threat to identification is that air pollution on strike days might be higher than usual for reasons other than strikes being in effect. Although we control for an extensive set of time fixed effects and local weather conditions, there may be unobserved time-varying factors that are correlated with strikes and at the same time determine the occurrence and durability of pollutants in ambient air. To empirically analyze the relevance of these concerns, we now conduct another falsification test. In particular, we investigate the effect of public transit strikes on SO_2. As mentioned above, sulfur dioxide is no longer a major tailpipe pollutant. However, it nevertheless depends on environmental conditions like many other pollutants. Table 6 reports the results. Across all specifications, and for both morning and evening peak hours, there are no statistically significant effects of public transit strikes on SO_2 pollution. That said, it is important to note that the SO_2

Table 4—The Effect of Strikes on Vehicle Crashes and Accident-Related Injuries

Dependent variables: Number of vehicle crashes and accident-related injuries				
	(1)	(2)	(3)	(4)
Panel A. Vehicle crashes—morning peak				
Strike	0.617	0.618	0.616	0.607
[4.280]	(0.255)	(0.273)	(0.259)	(0.250)
Observations	12,238	12,238	12,238	12,238
R^2	0.405	0.428	0.472	0.478
Panel B. Slightly injured persons—morning peak				
Strike	0.762	0.765	0.793	0.790
[3.940]	(0.151)	(0.181)	(0.201)	(0.192)
Observations	12,238	12,238	12,238	12,238
R^2	0.388	0.408	0.449	0.455
Panel C. Seriously or fatally injured persons—morning peak				
Strike	−0.011	−0.014	−0.012	−0.013
[0.354]	(0.060)	(0.057)	(0.055)	(0.055)
Observations	12,238	12,238	12,238	12,238
R^2	0.096	0.101	0.124	0.127
Panel D. Vehicle crashes—evening peak				
Strike	0.271	0.284	0.080	0.084
[6.963]	(0.420)	(0.481)	(0.476)	(0.475)
Observations	12,238	12,238	12,238	12,238
R^2	0.539	0.561	0.599	0.607
Panel E. Slightly injured persons—evening peak				
Strike	0.547	0.561	0.357	0.388
[6.786]	(0.464)	(0.517)	(0.533)	(0.527)
Observations	12,238	12,238	12,238	12,238
R^2	0.497	0.514	0.552	0.558
Panel F. Seriously or fatally injured persons—evening peak				
Strike	−0.107	−0.101	−0.090	−0.093
[0.648]	(0.055)	(0.055)	(0.053)	(0.055)
Observations	12,238	12,238	12,238	12,238
R^2	0.155	0.162	0.187	0.192
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 71. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the city-day level and include city fixed effects. Time FE include day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Cluster-robust standard errors are in parentheses.

regressions do not yield precisely estimated effects of zero, with implied percentage changes that fluctuate between +2.9 percent in the morning and −3.6 percent in the evening peak period.

Table 5—The Effect of Strikes on Particle Pollution and Nitrogen Dioxide Emissions

Dependent variable: Mean hourly pollution emissions in micrograms per cubic meter of air				
	(1)	(2)	(3)	(4)
Panel A. PM_{10}—morning peak				
Strike	5.101	5.016	5.569	5.336
[37.63]	(1.336)	(1.601)	(1.608)	(1.654)
Observations	33,007	33,007	33,007	33,007
R^2	0.184	0.313	0.342	0.351
Panel B. NO_2—morning peak				
Strike	2.719	2.839	3.276	3.313
[76.82]	(1.429)	(1.459)	(1.416)	(1.426)
Observations	38,525	38,525	38,525	38,525
R^2	0.399	0.490	0.510	0.519
Panel C. PM_{10}—evening peak				
Strike	1.057	0.551	0.468	0.297
[35.29]	(2.389)	(2.677)	(2.942)	(2.940)
Observations	33,737	33,737	33,737	33,737
R^2	0.196	0.305	0.338	0.350
Panel D. NO_2—evening peak				
Strike	−0.516	−0.458	−0.967	−1.056
[77.18]	(2.471)	(3.074)	(3.300)	(3.437)
Observations	39,468	39,468	39,468	39,468
R^2	0.348	0.436	0.463	0.478
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 68. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-day level and include monitor fixed effects. Time FE include day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

F. *Hospitalizations*

Table 7 reports the results for pollution-related health problems. The first panel presents regression estimates involving data on hospitalizations for diseases of the respiratory system. On an average strike-free day, we observe 61 hospital admissions for respiratory illnesses per city, roughly 8 of which occur among children under 5 years of age. On a strike day, the number of children diagnosed with respiratory illnesses increases by 0.879, or 11 percent of the strike-free level (column 2). The estimate is statistically significant. At the same time, there is no evidence for an increase in respiratory illnesses in the total population or in the subgroup of the elderly (columns 1 and 3). The second panel uses hospitalizations for abnormalities

Table 6—The Effect of Strikes on Placebo Air Pollution

Dependent variable: Mean hourly pollution emissions in micrograms per cubic meter of air	(1)	(2)	(3)	(4)
Panel A. SO_2—morning peak				
Strike	0.362	0.233	0.089	0.186
[6.46]	(0.384)	(0.380)	(0.255)	(0.190)
Observations	14,038	14,038	14,038	14,038
R^2	0.187	0.227	0.272	0.297
Panel B. SO_2—evening peak				
Strike	−0.042	−0.275	−0.237	−0.183
[5.02]	(0.221)	(0.297)	(0.222)	(0.235)
Observations	14,349	14,349	14,349	14,349
R^2	0.258	0.300	0.361	0.371
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 45. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-day level and include monitor fixed effects. Time FE include day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

of breathing as the dependent variable. On an average strike day, the total number of patients admitted to hospitals due to breathing problems increases by 13 percent (column 1), an estimate significant at the 5 percent level. As before, the effect appears to be driven by the subgroup of young children for whom we find a precisely estimated 34 percent increase in hospital admissions for abnormalities of breathing (column 2). The strike dummy variable for the elderly patient subgroup has a positive but not statistically significant coefficient. We also examined hospital admissions for diseases of the circulatory system (ICD-10 codes I00–I99). We found no evidence for a strike effect on circulatory illnesses.

Compared to the estimated strike effect on air pollution (P10: 14 percent; NO_2: 4 percent), the increase in breathing-related hospitalizations among young children (34 percent) is large. This suggests that there might be channels other than air pollution through which hospitalizations are affected. Three examples spring to mind. First, transit strikes not only lead to more air pollution through increased traffic, but also increase the *length of exposure* to air pollution by affecting travel times and modes of travel. Second, it is conceivable that transit strikes might lead to *stress-related* respiratory problems among children. Indeed, there is evidence suggesting that stressful events in the course of day-to-day childcare (e.g., changes in drop-off or pick-up routines) may cause respiratory illnesses among psychobiologically reactive children (Boyce et al. 1995). Third, our results indicate that transit

Table 7—The Effect of Strikes on Hospitalizations

Dependent variable: Number of hospitalized patients per day	Full sample (1)	Ages below 5 (2)	Ages 65 and above (3)
Panel A. Respiratory diseases (ICD-10 codes J00–J99)			
Strike	0.963	0.879	0.145
	(1.746)	(0.208)	(0.829)
Observations	11,000	11,000	11,000
R^2	0.924	0.692	0.861
[Mean]	[61.08]	[7.82]	[22.09]
Panel B. Abnormalities of breathing (ICD-10 code R06)			
Strike	0.160	0.074	0.049
	(0.048)	(0.018)	(0.096)
Observations	11,000	11,000	11,000
R^2	0.182	0.098	0.089
[Mean]	[1.27]	[0.22]	[0.39]
City FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
City × Time	Yes	Yes	Yes
City × Weather	Yes	Yes	Yes

Notes: Number of 1-day strikes used in estimation sample: 57. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the city-day level and include city fixed effects. Time FE include day-of-week, week-of-year, and year. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Cluster-robust standard errors are in parentheses.

strikes simultaneously increase PM_{10} and NO_2 concentrations, and this simultaneous increase might exacerbate health problems.[32]

G. *Robustness*

Mass Events.—Our estimates in the previous section would be biased if there were omitted variables that are correlated with the occurrence of strikes and the outcomes of interest. For example, suppose that strikes by transit workers tend to coincide with mass events (e.g., trade fairs, sporting events, festivals). If mass events result in an increase (respectively, decrease) in traffic volumes, then omitting controls for such events results in an upward (respectively, downward) biased estimate of the true effect of public transit strikes. To mitigate this omitted variable bias, we now extensively control for mass events at the city-day level. In particular, we add the binary variable ($MassEvent_{cdwy}$) to equation (1), which equals unity for events such as the Beer Festival (*Oktoberfest*) and Security Conference in Munich,

[32] It is also possible that parents stay home on strike days and use the available time to seek medical care for marginally sick children. Thus, hospitalizations might increase without an increase in medical problems per se. If this is the case, we would expect an increase in hospitalizations for both pollution-related and non-pollution-related illnesses. In unreported regressions, we examined hospital admissions for infectious and parasitic diseases (ICD-10 codes A00–B99), and found no evidence for a strike effect on these types of illnesses.

the Harbor Festival (*Hafenfest*) in Hamburg, the Museum Embankment Festival (*Museumsuferfest*) in Frankfurt on the Main, the Christopher Street Day Parade in Cologne, or the Carnival of Cultures (*Karneval der Kulturen*), the Fan Park during the 2006 Soccer World Championship in Berlin, and a number of trade fairs.[33] The results reported in Table 8 show that the mass event coefficients turn out to be small throughout all specifications and mostly insignificant. More importantly, the coefficients on our strike dummy variable remain virtually unchanged compared to the benchmark estimates in Table 1. Unreported regressions confirm this result for all other outcomes.

Multiday Strikes.—We have so far exploited 71 1-day strikes in public transportation over the period 2002 to 2011. During that period, there were also 17 strikes with a duration of more than one day across the 5 cities. We now add all workweek days affected by these multiday strikes to our sample. Then, we reestimate equation (1) using both a one-day strike dummy and a multiday strike dummy as independent variables. Table 9 presents the results for passenger vehicle flows. For freeway traffic, we find that the average effect of multiday strikes is only marginally smaller than the one-day effect. For traffic on federal roads, the differences between the one-day and multiday estimates is somewhat more pronounced, though not statistically distinguishable from zero.

Estimates on a Common Sample.—To exploit as much information as possible, we have constructed samples that differ for the five outcomes of interest. In unreported regressions, we have repeated the traffic, accident, pollution, and hospitalization analyses on a common sample. The results, available on request, do not change the substantive conclusions reported here. However, there are four points worth mentioning. First, in the common sample, we observe a precisely estimated 30 percent increase (instead of a weakly significant 14 percent increase) in the number of vehicle crashes during the AM peak, which is accompanied by a 32 percent increase (instead of a 20 percent increase) in slightly injured persons. Second, the negative evening peak estimate for seriously or fatally injured persons turns statistically significant. Third, the estimated morning peak effects for nitrogen dioxide emissions turns statistically insignificant due to larger standard errors. Fourth, while our health result for respiratory diseases among children continues to hold, the estimates for abnormalities of breathing decrease in size and turn statistically insignificant.

Standard Errors.—Since reliable inference is a concern when there are few clusters, we checked whether our results also hold using wild cluster bootstrap t-procedures instead of clustering standard errors. As mentioned above, our findings were robust to using the standard 2-point wild cluster bootstrap suggested by Cameron, Gelbach, and Miller (2008). However, Webb (2014) argues that this procedure may be noisy with a small number of clusters because the estimated p-values

[33] The extended model controls for a total of 55 mass events across the 5 cities, attracting crowds of more than 150,000 people per day, on average. The days affected by these events amount to a total of 1,094 city-day observations.

Table 8—The Effect of Public Transit Strikes on Car Traffic—Controlling for Mass Events

Dependent variable: Hourly passenger vehicle flows per monitor	(1)	(2)	(3)	(4)
Panel A. Freeways—morning peak				
Strike	160.2	136.5	128.4	131.6
[5,240]	(26.67)	(10.76)	(13.79)	(12.09)
Mass event	−29.88	−40.48	−6.287	−0.239
	(26.50)	(15.40)	(11.72)	(10.09)
Observations	212,896	212,896	212,896	212,896
R^2	0.899	0.903	0.921	0.922
Panel B. Federal roads—morning peak				
Strike	63.03	57.30	72.43	77.67
[1,790]	(13.01)	(15.27)	(8.024)	(9.387)
Mass event	4.546	−3.427	2.696	−4.463
	(15.11)	(4.688)	(3.764)	(6.446)
Observations	102,540	102,540	102,540	102,540
R^2	0.921	0.924	0.961	0.962
Panel C. Freeways—evening peak				
Strike	124.9	103.1	88.37	91.49
[5,786]	(28.49)	(21.35)	(19.77)	(17.89)
Mass event	−20.25	−25.82	−10.23	−3.993
	(25.01)	(17.88)	(7.039)	(7.193)
Observations	212,896	212,896	212,896	212,896
R^2	0.937	0.939	0.950	0.950
Panel D. Federal roads—evening peak				
Strike	21.78	13.97	26.29	37.83
[2,121]	(17.00)	(18.03)	(14.32)	(10.28)
Mass event	14.35	9.487	−0.911	−5.526
	(19.95)	(12.20)	(3.855)	(5.106)
Observations	102,540	102,540	102,540	102,540
R^2	0.961	0.962	0.972	0.973
Monitor FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
City × Time			Yes	Yes
City × Weather				Yes

Notes: Number of 1-day strikes used in estimation sample: 64 for freeways, 45 for federal roads. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-hour level and include monitor fixed effects. Time FE include hour-of-day, day-of-week, hour-of-day × day-of-week, week-of-year, year, and holiday fixed effects. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

are intervals rather than point estimates. In order to receive more precise p-values, he suggests expanding the standard 2-point wild cluster bootstrap to a multi-point wild cluster bootstrap. We substituted the Rademacher weights (+1 and −1 with equal probabilities) by randomly drawing the weights from a normal distribution with a

Table 9—The Effect of One-Day and Multiday Strikes on Car Traffic

Dependent variable: Hourly passenger vehicle flows per monitor	Freeways (morning peak) (1)	Federal roads (morning peak) (2)	Freeways (evening peak) (3)	Federal roads (evening peak) (4)
One-day strike	131.6	77.77	91.34	37.83
	(12.12)	(9.532)	(17.96)	(10.15)
Multiday strike	113.7	54.23	94.34	−0.615
	(33.57)	(12.57)	(23.63)	(27.35)
Observations	213,892	103,128	213,892	103,128
R^2	0.922	0.962	0.950	0.973
[Mean]	[5,240]	[1,790]	[5,786]	[2,121]
Time FE	Yes	Yes	Yes	Yes
Weather	Yes	Yes	Yes	Yes
City × Time	Yes	Yes	Yes	Yes
City × Weather	Yes	Yes	Yes	Yes

Notes: Number of 1-day strikes used in estimation sample: 64 for freeways, 45 for federal roads. Multiday strikes used in estimation sample include 12 events covering 41 city-day observations for freeways and 10 events covering 37 city-day observations for federal roads. Mean of the dependent variable on strike-free days reported in square brackets. All regressions are run at the monitor-hour level and include monitor fixed effects. Time FE include hour-of-day, day-of-week, hour-of-day × day-of-week, week-of-year, year, and holiday fixed effects. Controls include a dummy for school holidays and the following weather variables: atmospheric temperature, amount of precipitation, wind speed, and a snow cover dummy. City × Time are interactions of city indicators with all time FE. City × Weather are interactions of city indicators with all weather variables. Weights are the number of observations per station over the number of observations per city. Cluster-robust standard errors are in parentheses.

mean of zero and a standard deviation of one. The *p*-values obtained from this alternative bootstrap procedure also support our reported levels of statistical significance.

Measurement Error.—Our strike indicator is based on self-collected data and might therefore be prone to measurement error. Indeed, we cannot entirely rule out that we (i) missed days that were affected by strikes or (ii) erroneously coded a day as a strike day even though no strike took place. Note, however, that both types of measurement error would result in a downward bias in the estimated effects of public transit strikes. If we missed days that were affected by strikes and hence erroneously coded them as non-strike days, then car traffic on non-strike days would be higher, which in turn reduces the estimated effect of strikes. If we erroneously coded a day as a strike day, car traffic on strike days would be lower, which again reduces the estimated effect of strikes.

V. Discussion

Third-Party Congestion Costs.—How large are the costs of transit strikes to noninvolved third parties? The lion's share of third party costs stems from the increase in travel time due to congestion. From the 2003 wave of the German Socio-Economic Panel (SOEP),[34] we obtain information on commuter incidence,

[34] Socio-Economic Panel (SOEP), data from 1984–2012, DOI: 10.5684/soep.v29.1. We use the SOEPremote version to identify the cities of our sample.

modes of transport, and travel times. In the five cities of our sample, 47 percent of the working population commute to their work place using a car, while 43 percent rely on public transit. Combining this information with local employment data,[35] the average number of car commuters per city amounts to 486,000, while there are on average 445,000 commuters using mass transit. According to the SOEP, average one-way travel-to-work time is 27 minutes for car commuters and 37 minutes for commuters relying on public transit. The estimates in Table 3 imply that travel times for car commuters increase by 6.3 percent during the peak periods. We assume that mass-transit commuters experience the same percentage increase in travel times as car commuters, irrespective of whether they switch to the car or continue to use public transport on strike days. In the average city, a single one-day strike therefore implies an increase in aggregate travel time by roughly 62,000 hours, or 1,550 full-time equivalent work weeks. Valuing time at average GDP per hour worked, €52,[36] we estimate congestion costs of €3.2 million per strike or €228.9 million for all 71 strikes in our main sample.

If these costs are not internalized in the collective bargaining process, the level of strike activity resulting from failed negotiations will be inefficiently high. In this regard, it might be interesting to set the third-party congestion costs in relation to the costs of struck employers. For transit providers, the withdrawal of striking workers means a partial shutdown of services, and with it a loss of revenues from ticket sales. In the average city, transit providers generate revenues from ticket sales of €445.8 million annually. Assuming that struck transit providers are unable to raise any revenue from their users, this corresponds to a revenue loss of €1.2 million per strike day, or roughly one-third of the daily congestion costs to non-involved third parties.

Labor Relations Policy.—Our most interesting and novel finding is that strikes in public transportation not only cause congestion costs, but also pose a non-negligible threat for public safety and public health. We have shown that public transit strikes cause daily pollution shocks accompanied by an increase in pollution-related health problems. For children under five years of age, hospital admissions for respiratory diseases and abnormalities of breathing increase by 11 and 34 percent, respectively. With 71 transit strikes in our sample, 68 more young children had to be admitted to hospitals than would have been if there had been no strikes. Moreover, our estimates suggest that transit strikes increase the risk of being injured in a motor vehicle crash by 20 percent. According to the International Labour Organization (ILO), governments can ban strikes in "essential services," defined as a service whose stoppage poses a clear and imminent threat to the life, personal safety, or health of the whole or part of the population. Public transportation does not fall under the ILO's definition of an essential service. Taken at face value, our results seem to provide

[35] Average number of employed individuals per city is 1,043,000. See Federal Statistical Office and the statistical offices of the Länder (2011), working population, http://aketr.de/tl_files/aketr/DATA/Tabellen/KR_ET.pdf, as of March 26, 2014.

[36] See Federal Statistical Office and the statistical offices of the Länder (2011), http://www.vgrdl.de/VGRdL/Arbeitskreis_VGR/tbls/R2B1.zip and http://aketr.de/tl_files/aketr/DATA/Tabellen/KR_AV.pdf, as of March 26, 2014.

evidence in support of the opposite position: that mass transit—just as the police or firefighters—is critical to public safety and health on a day-to-day basis. That said, it is also important to point out that the economic costs associated with the strike-induced increases in accidents, accident-related injuries, and hospitalizations are small, coming to less than 0.5 percent of the above calculated congestion costs.[37]

Some strikes in our sample only span a few hours, while others last all day. We are therefore able to examine whether longer strikes are more disruptive than shorter strikes. In unreported regressions, we have replaced our binary strike indicator with a continuous variable capturing the duration of strikes in hours. Using a simple linear specification, we find that a reduction by one hour in strike duration reduces the negative effects of an average strike during the AM peak period by (i) 7.6 percent for freeway traffic, (ii) 7.8 percent for travel times, (iii) 16 percent for vehicle crashes, (iv) 13 percent for accident injuries, (v) 9.8 percent for particle pollution, and (vi) 7.3 percent for respiratory-related hospitalizations among young children. From a policy perspective, this suggests that restrictions on strike duration might undo some of the negative effects unearthed in this study.

It is important to keep a few caveats in mind. Our analysis leaves open the question of whether laws banning public transportation strikes are welfare-enhancing. Tracing the total welfare consequences of strikes is complex. Our analysis shows that strike-induced disruptions of mass transit services have adverse effects on urban populations in the short run. However, it misses any longer run impacts of public transit strikes. For example, it stands to reason that these strikes may provide offsetting long-term benefits for urban populations if they result in agreements that improve organizational performance in urban mass transit.[38] Further research is therefore warranted to develop a comprehensive approach for establishing a measure of the welfare effects of strikes in public transportation.

External Validity.—The size of the impact of transit strikes on the studied outcomes is likely to depend on several mediating factors. The following examples spring to mind: the capacity of road networks to absorb additional drivers; laws regulating vehicle emissions; posted speed limits and vehicle safety standards; or prominent weather features that affect the accumulation of pollution. These mediating factors are likely to vary across jurisdictions. Thus, the size of the estimated strike effects in German cities might be different from similar strikes in, say, US cities, which we have cited as points of comparison.

In order to get some idea of the generalizability of our results, it may be useful to briefly consider the extent to which the German context offers similarities and dissimilarities to the United States:

- *TomTom*'s annual Traffic Index explores traffic congestion in over 200 cities around the world and may thus serve as a reasonable proxy for the capacity

[37] This calculation is based on information available from *BASt*. See http://www.bast.de/DE/Publikationen/Foko\\/Downloads/2010-17.html (accessed November 20, 2015).

[38] There is also evidence suggesting that public transit strikes have the unintended positive consequence of reducing the spread of viral diseases (Adda 2016).

of roads to absorb additional drivers. The most recent ranking indicates that several major US cities (e.g., New York, Miami, Chicago, Washington) have congestion ratings similar to those of the five largest German cities, while others have higher (e.g., Seattle, Los Angeles, San Francisco) or lower (e.g., Philadelphia, Denver, Pittsburgh) ratings.[39]

- German and US vehicle emissions standards are administered by the European Commission (EC) and the US Environmental Protection Agency (EPA), respectively. A comparison of current standards for selected pollutants indicates (i) comparable limits for particulate matter emissions; (ii) stricter EPA emission standards for nitrogen oxides and non-methane organic gases; and (iii) stricter EC standards for carbon monoxide and greenhouse gas emissions (Canis and Lattanzio 2014).
- In Germany, there is a standard 50 kph (31 mph) limit on major roads in urban areas. In addition, the so-called "30 zone" is very common in residential areas, limiting speed to 30 kph (18 mph) for noise reduction and child safety. In the United States, most speed limits are set by state or local statute, and typically range from 40 to 72 kph (25 to 45 mph) on major roads in urban and suburban areas and from 40 to 56 kph (25 to 35 mph) on residential streets.[40] In terms of vehicle safety, there is some evidence that cars meeting EU standards offer reduced risk of injury in frontal or side crashes and have better driver-side mirrors that reduce the risk of lane-change crashes, while cars meeting US standards provide a lower risk of injury in rollovers and have headlamps that make pedestrians more conspicuous (Flannagan et al. 2015).
- Certain weather and geographic conditions may exacerbate acute episodes of air pollution. Temperature inversions prevent air masses from moving in their natural vertical direction, thereby trapping pollutants at ground level. Cities surrounded by hills or mountains, which block horizontal air motion, are particularly susceptible to the effects of inversions. Temperature inversions occur frequently in major cities such as Los Angeles, Mexico City, or Tehran. The five German cities in our sample are not subject to major, regular inversion effects due to comparatively favorable climatic and geographic circumstances.

Given these stylized facts, it seems not unreasonable to argue that there is nothing inherently specific about the German case to suggest a systematic bias toward positive effects. Indeed, Germany and the United States appear to be quite similar in terms of urban congestion, posted speed limits, car safety standards, and vehicle emission limits for particulate matter. However, two notable differences lie in the emission standards for nitrogen oxides (stricter in the United States) and carbon monoxide (stricter in Germany). Both pollutants are emitted by cars, but it is

[39] The congestion ratings provided by *TomTom* capture the increase in overall travel times when compared to a free flow situation. The most recent congestion ratings for the cities mentioned above are: Berlin (AM peak: 42 percent, PM peak: 51 percent), Hamburg (48, 55), Munich (50, 49), Cologne (47, 59), Frankfurt (53, 49), New York (48, 56), Miami (46, 56), Chicago (43, 59), Washington (44, 58), Seattle (51, 74), Los Angeles (60, 80), San Francisco (53, 68), Philadelphia (38, 49), Denver (35, 50), Pittsburgh (31, 41). For more details, see https://www.tomtom.com/en_gb/trafficindex/ (accessed February 17, 2016).

[40] See http://www.ems.gov/pdf/HS810826.pdf (accessed October 14, 2015).

mainly high levels of nitrogen dioxide that have been linked with increased hospital admissions due to respiratory problems. Thus, one could speculate that the health effects identified in this paper might look different for the United States. That said, future research should attempt to document how the impact of public transit strikes varies along mediating factors, thus more firmly assessing the external applicability of our findings.

Mass Transit Externalities.—Our paper also makes contact with a growing literature in economics that examines the role of mass transit in mitigating agglomeration diseconomies, such as traffic congestion, accident risk, and pollution emissions.[41] Anderson (2014) evaluates the congestion relief benefit of the Los Angeles public transportation system by exploiting a strike by transit workers in 2003 during which the entire system shut down for 35 days. The author uses the abrupt and complete cessation of service to quantify the *effects of transit provision* on congestion. His regression discontinuity estimates suggest that the total congestion relief benefit of operating the Los Angeles transit system lie between $1.2 billion to $4.1 billion per year.[42] The strike episodes we exploit in this study differ from the 2003 Los Angeles transit strike in one important aspect: they did not lead to a complete shutdown of the entire system, but to substantial disruptions in terms of service frequency. While our empirical results can therefore not be thought of as capturing the effects of transit provision, they might to a certain degree reveal the *effects of marginal changes in the transit network*, such as an increase in service quality or connection frequency. However, one caveat in this respect is that the impact of long-term changes in service quality is likely different than the short-run effect resulting from strikes. One simple reason is that long-run adaptations in commuter behavior might differ from those in the short-run. Thus, our main contribution to the literature on mass transit externalities lies in showing that even short-term disruptions of public transport networks can have far reaching consequences for urban populations in terms of time lost to travel, accident risk, air pollution, and health.

[41] Much of this literature touches upon an influential study by Duranton and Turner (2011), who coined the notion of the "fundamental law of road congestion." The idea is that the provision of public transit is unlikely to relieve the overall level of congestion in a city since it only results in additional traffic that continues to rise until peak congestion returns to its natural level.

[42] Relatedly, Nelson et al. (2007) provide structural estimates suggesting that the rail transit system in Washington, DC generates congestion-reduction benefits that exceed rail subsidies. Chen and Whalley (2012) quantify the pollution relief of urban rail transit using the sharp discontinuity in ridership on the opening day of a new rail transit system in Taipei. Their findings suggest that the opening of the rail transit system caused a 5 to 15 percent reduction in carbon monoxide emissions. Lalive, Luechinger, and Schmutzler (2013) analyze a railway reform in Germany which substantially increased the frequency of regional passenger services. Their results suggest that the reform reduced the number of severe road traffic accidents, carbon monoxide, nitrogen monoxide, nitrogen dioxide pollution, and infant mortality.

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