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Subways and Road Congestion†

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We study whether subways alleviate road congestion by examining 45 subway line launches in China and by using detailed data on road speed. Our difference-in-differences estimation finds that in the first year after a subway line is launched, rush hour speed on nearby roads increases by about 4 percent. The effect is most prominent in initially congested roads and declines over distance to the new subway line. Evidence on road speed is corroborated with substitution patterns among modes of transportation. Using auxiliary data from Beijing, we calculate that the time savings for each automobile or bus commute from faster speed is worth US$0.10. (*JEL* O18, P25, R41)

Traffic congestion is a major challenge facing many cities around the world. The problem is particularly acute in large cities in the developing world that have recently experienced rapid increases in population and car ownership. Recent studies have found that many cities in developing countries are among the world's most congested (e.g., Reed and Kidd 2019, TomTom 2019).

Various policies have been implemented aiming to reduce traffic congestion. Demand-side policies include congestion pricing (Small and Verghoef 2007) and restrictions on automobile ownership and usage (e.g., Li 2018; Davis 2008; Gu, Deakin, and Long 2017). On the supply side, urban rail transit systems (henceforth "subways") are considered an effective way to reduce congestion, because they have large capacity and do not require much surface land. By 2014, 171 cities worldwide had a subway system in operation (Gendron-Carrier et al. 2018). Cities in developing countries account for a large share of recent subway construction (Gonzalez-Navarro and Turner 2018). In China alone, total subway length increased from less than 400 kilometers in 4 cities in 2001 to about 5,000 kilometers in 30 cities by 2017 (Cui et al. 2018).

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Due to data limitations, empirical evidence on the congestion-reducing effect of subways is scarce. First, subway line openings are infrequent. An analysis covering multiple subway lines often requires data from a number of cities over many years, which are hard to come by. Second, standard datasets in socioeconomic studies lack good measures of road speed. Speed can be backed out from travel diaries available in many household transportation surveys (e.g., Couture, Duranton, and Turner 2018). However, these surveys are infrequent, have limited sample sizes, and suffer from substantial measurement errors. Traffic data can also be derived from traffic cameras, wired loop detectors, or GPS-enabled bus and taxi fleets (e.g., Anderson 2014). But municipal governments usually administer these data separately, making it difficult to compare across cities.

This paper takes advantage of the recent massive construction of subways across Chinese cities. We focus on the 45 new subway lines (including 7 extensions to existing lines) launched between August 2016 and December 2017 across 25 cities. We use a novel source of traffic speed data from China's leading provider of digital map services, which collects and processes real-time traffic information from user-generated data. The company offers a free digital map application on smartphones and other mobile devices. With the location service turned on, mobile devices remit bits of location data every few seconds to the company's database. With these data the company generates real-time speed information at the road segment level. The speed information is then displayed on the digital map and can be used to generate real-time optimal travel routes.

We obtain hourly speed data between August 1, 2016 and January 31, 2018 for a sample of road segments near subway lines. Road segments in the sample are those inside 2.5 kilometer buffer zones of selected 5 kilometer segments of existing, new, and planned subway lines. After applying several restrictions, our baseline sample includes more than 20,000 road segments that cover all 42 cities in Mainland China that had an existing or planned subway system by the end of 2017. The spatial and temporal dimensions of our data are a significant improvement on existing studies that typically look at a shorter period of time for a much smaller number of subway lines.

Big data at the granular level allow us to adopt a saturated empirical model. First, log hourly speed is regressed on a set of fully interacted indicators for road segment, day of the week, and hour of the day. The residual from this regression captures the deviation from the "usual" traffic speed. It is then averaged at the weekly level and used as the outcome variable in a difference-in-differences (DID) estimation: road segments in cities that had existing or planned subway lines but did not have a new line launched during the sample period are randomly assigned to each treated line to serve as the control group. The control road segments are given a fake opening date that is the same as that of the corresponding treated line. All 45 treated lines and their controls are then stacked according to the week relative to opening. This stacked DID approach helps control for seasonality and macro trends common to all cities.

Perhaps the biggest challenge to identification is that most of the openings are at a time of the year, Chinese New Year, when traffic eases up anyway in major cities, combined with the fact that our control cities are systematically smaller. We address this by allowing for different seasonality for larger cities. We find a similar effect when we restrict the analysis to the small number of openings that occur at other times of the year. The results are also similar from a two-way fixed effects model using only treated cities.

We first investigate whether subways have *any* effect on road speed by focusing on weekday rush hours and road segments that are likely to be directly affected. Directly affected road segments are those that a person needs to travel through if one chooses to drive instead of taking the subway. They are identified by using the route-planning function of the digital map platform.

Inspection of the coefficients in the pre-treatment period suggests that we cannot reject the parallel trend assumption. The launch of a new subway line has an immediate positive effect on the speed in directly affected road segments. In the first week after a line opens, speed in those road segments increases by about 2.5 percent relative to that in the control segments. The effect increases to about 5 percent in the sixth week before it declines and stabilizes between 2 percent and 3 percent. We track up to 48 weeks after the line opening; the effect remains stable and statistically significant by then. The average effect in the post-treatment period is between 3.6 percent and 4.4 percent.

We conduct a host of additional robustness checks. Evidence from a placebo test with fake opening dates suggests that the result is unlikely to be driven by confounding trends. Results are similar with various cuts of the sample or time period. A difference-in-discontinuity design also suggests an immediate jump in road speed around the time of the subway line opening.

We then investigate the effects of a new subway line on different types of road segments. For 23 out of the 25 treated cities, we are able to extract road segments near an existing or planned subway line. We include all sampled road segments in these treated cities to study how the effect spreads through the urban transportation network. We find substantial heterogeneity in the congestion-relieving effect of a new subway line. The effect is larger for directly affected road segments and for initially more congested roads, suggesting that marginal switchers from road vehicles to subways are more likely to be those who experienced high levels of road congestion (Anderson 2014). The effect declines quickly with the distance to the new subway line. There is suggestive evidence of a network effect of the subway system: road segments that are far away from the new subway line but close to an existing subway line also experience substantial increases in speed. We also expand the analysis sample to include nonrush hours and find that the effect is larger in rush hours, especially for road segments that have the same direction as the flow of the traffic.

To corroborate our finding on road speed, we study the substitution patterns among different modes of transportation using micro data from household transportation surveys in Beijing. We find that improved access to the subway is associated with increases in subway trips and decreases in bus trips, car trips, and annual vehicle kilometers traveled of private cars.

We build a conceptual framework of transportation mode choices. The framework leads to a formula for the welfare impact of subways that relies only on observable quantities and prices. The welfare impact can be decomposed into three components: (i) welfare gains from savings in travel time by road vehicles (including automobiles and buses), (ii) welfare gains for those who switch to the subway from road vehicles, and (iii) changes in government spending on public transit.

The main empirical result of the paper, the effect of subways on road speed, helps us estimate the first component of the welfare expression. We do so by focusing on commutes in Beijing. Our estimate suggests that Beijing's subway increases rush hour average road speed by 3 percent. Using additional data on volumes of ridership, average length of commutes, and average wages, we calculate that the time saved for each automobile or bus commute from reduced congestion is worth US$0.10. We need to rely on stronger assumptions and correlational evidence to estimate the other two terms. We find that each of these terms is large in magnitude, but they offset each other. If accurate, this would suggest that the benefit of Beijing's subway system exceeds its cost. Note that other potential benefits of the subway, such as the reduction in air pollution (Chen and Whalley 2012, Gendron-Carrier et al. 2018) and car accidents, are left out of our framework and calculation.

Congestion reduction is one of the most cited reasons by proponents of subways. Using a panel of US cities, Winston and Langer (2006) find that longer rail transit mileage is associated with lower congestion costs. However, empirical evidence on the causal effect is limited. Yang et al. (2018) find that the city-level "congestion index" drops sharply following six subway line openings in Beijing. Yet the event study design does not purge out potential time trends. Other studies find significant reductions in air pollution upon the launch of the subway (Chen and Whalley 2012, Gendron-Carrier et al. 2018, Li et al. 2019), which *implies* reduced traffic. Our paper provides direct evidence of this relationship.

There has been a long debate in the literature of urban economics on the value of public transit, mostly in the setting of developed countries (Voith 1991, Baum-Snow and Kahn 2005, Winston and Maheshri 2007, Parry and Small 2009). Some studies have used temporary interruptions to the public transit system, such as strikes, to evaluate the benefits (e.g., Anderson 2014, Adler and van Ommeren 2016). However, halting existing systems is likely to induce very different responses compared with the introduction or expansion of services. In addition, because public transits in developing countries face very different constraints, evidence from developed countries may not immediately apply.

Although building additional road capacity is a natural response to traffic congestion and is widely adopted by policymakers, there is debate on whether the supply-side approach is effective. The "fundamental law" of highway congestion (Downs 1962, 2000; Hsu and Zhang 2014) suggests that the elasticity of vehicle kilometers traveled (VKT) with respect to lane kilometers is one; thus, adding road capacity does not reduce congestion. Using a panel of US cities, Duranton and Turner (2011) find empirical evidence that the fundamental law also applies to urban roads; the supply of public transit does not reduce VKT and is unlikely to alleviate traffic congestion. In contrast, our finding suggests that building subways—an alternative way to add road capacity—does reduce congestion in Chinese cities, at least in the short run.

This paper also contributes to a new and growing strand of literature that takes advantage of user-generated big data in urban transportation. Some recent studies have collected data on millions of simulated trips using the "usual" traffic speed provided by Google Maps (Akbar and Duranton 2017, Akbar et al. 2018). Kreindler (2018) designed a new smartphone application that collected precise GPS coordinates over 100,000 commuter trips in an experimental setting in Bangalore. Our collaboration with a large digital map provider allows us to analyze real-time speed data from a large set of road segments over a relatively long period of time.

The remainder of the paper is structured as follows. Section I describes the institutional background, data sources, and sample constructions. Section II presents the main empirical analyses. Section III documents supporting evidence from mode substitution using household travel data. Section IV briefly discusses the welfare impact. Section V concludes.

I. Background, Data, and Sample

A. *Subways in Chinese Cities*

There has been a large boom in subway construction across Chinese cities in the past two decades. Figure 1 shows the growth of subway length and ridership in China. In 2001, only four cities in Mainland China—Beijing, Shanghai, Guangzhou, and Tianjin—had a subway system. The combined length of all subway lines was below 400 kilometers. By the end of 2017, 30 cities had a total of 4,476 kilometers of subway lines, and 12 other cities had their first subway lines under construction. Subway ridership increased accordingly, from just under 1 billion in 2001 to about 16 billion in 2017.

Massive subway construction is a response to the rapid growth in population and car ownership in China's major cities. The overall urbanization rate increased from 35 percent in 2000 to 58 percent in 2017. Much of the increased urban population concentrated in large cities. Rapid increase in car ownership has made major Chinese cities among the most congested and polluted in the world. Many city governments regard subways as the essential infrastructure to reduce congestion and pollution. Billions of dollars have been invested in building and expanding subway systems. Despite the large amounts of investment, there has been little empirical evidence on whether these subway lines achieved the stated goal of reducing congestion.

B. *Data on Road Speed*

The company we work with, Baidu Maps, is China's leading provider of digital maps and online navigation services. It runs a smartphone application that is similar to the one provided by Google Maps or Apple Maps. With the application installed and location service turned on, mobile devices remit bits of location information every few seconds to the company's data center. Mobile devices also transmit information on acceleration, rotation, and angle, which helps distinguish devices in vehicles and those carried by pedestrians or cyclists. The location of the device is matched with a digital map of roads. Speed can be calculated with multiple location records from the same device and the time lapse in between. The

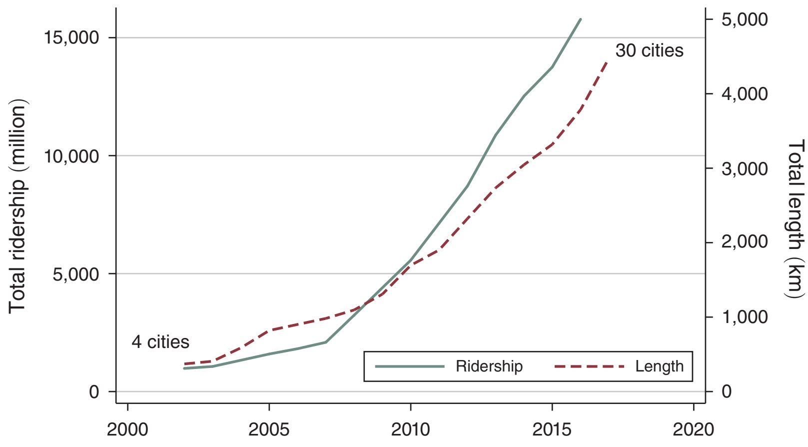

Figure 1. Subway Construction in China

Source: Data from *Statistical Yearbooks of Chinese Cities*, published annually by China's National Bureau of Statistics (NBS 2010–2017)

application has 280 million monthly active users in China. With large amounts of data flowing into the company's server every second, it is able to compute real-time speed at very fine geographic levels. Real-time speed is displayed on the digital map and can be used to calculate optimal routes for different modes of transportation.[1]

We obtain hourly average speed at the road segment level between August 1, 2016 and January 31, 2018.[2] A road segment is a short stretch of road. In our baseline sample, the median length of a road segment is about 50 meters. For each road segment we know the name of the road, direction of the segment (the two directions of a stretch of a two-way road are two separate road segments), and a set of coordinates indicating its location. From these coordinates, we calculate the length of the segment and its geographic relation to the subway and other urban features. Road segments are classified into five hierarchical categories: highways, urban expressways, arterial streets, subarterial streets, and local streets.

The speed data also come with a "congestion index," which is defined as the time needed to travel through the road segment under the current speed relative to the time needed under traffic-free speed. The traffic-free speed is the average speed on the same road segment between midnight and 5 am. For example, half of the traffic-free speed corresponds to a congestion index of 2.

[1] Online Appendix Figure A.1 shows a screenshot of the digital map with color-coded road speed. Online Appendix A.1 presents additional facts about the data.

[2] The exceptions are for the periods between September 1 and September 10, and between October 10 and November 30, 2016, for which the original data were no longer available. For each day, we have hourly speed between 7 am and 7 pm.

C. *Data Sample*

The ideal data would be a random sample of road segments in the city combined with an oversampling of those near new subway lines. Such a sample could give us both a big enough sample size in regions that are most likely to be affected and an overall representative sample. However, resources allocated to help us extract and prepare the data were too limited for such a sampling procedure.

Instead, we extract road segments in the neighborhoods of selected subway lines. We first select segments of new, existing, and planned subway lines across all 42 cities with an existing or planned subway system and then extract all road segments in the neighborhood of these subway line segments. This approach comes at a cost. We do not have data on segments further from the subway, so we cannot assess effects on the city's overall traffic during rush hour.

D. *Subway Lines and Road Segments in the Sample*

Our data include subway lines that were launched between August 1, 2016 and December 31, 2017. There were 45 such lines (including 7 extensions to existing lines) across 25 cities. Table 1, panel A lists these "treated lines." The 25 cities (henceforth the "treated cities") include China's largest metropolises and provincial capitals. The table lists the official opening date for each new subway line. It is worth noting that a disproportionately large share of the lines opened toward the end of the calendar year. Twenty-five out of the 45 subway lines were launched in December. There were seven new line openings on December 28, 2016 alone, and six on December 28, 2017. There are reasons to believe that the official opening dates are not randomly determined. The clustering of opening dates can be correlated with potential confounding factors. Indeed, the end of the calendar year is also the start of China's holiday season. Chinese New Year (CNY), the nation's most important holiday, follows a lunar calendar and usually falls between mid-January and mid-February. With economic activity at a slower pace, it is possible that the roads will be less congested even without the launch of new subway lines. Thus, it is important to purge out seasonality in traffic patterns.

For 23 out of the 25 treated cities, our data also include an existing or planned subway line. Changes in speed on road segments near these existing or planned subway lines shed light on how the effect spreads along the road and subway network, which we study in Section IIC. These lines are chosen such that they are at least 3 kilometers away from the treated subway line and have a comparable distance to the downtown. Table 1, panel B lists these lines. Among the 25 such lines there are 22 existing and 3 planned. The opening dates of these lines are, in general, at least one year apart from those of the treated lines in the same city. So they should not generate confounding effects.

In addition to the 25 treated cities, there are 17 "control cities" that have existing or planned subway systems but did not have a new line launched during the sample period. Road segments from these cities serve as the control sample. In each of these control cities, we pick the latest line completed; if the city does not

Table 1—Subway Lines in the Sample

City	Line(s)	Opening date	City	Line(s)	Opening date
Panel A. New subway lines opened between August 1, 2016 and December 31, 2017					
Beijing	16	12/31/16	Nanchang	2	8/18/17
Beijing	Xijiao	12/30/17	Nanjing	4	1/8/17
Changchun	1	6/30/17	Nanjing	S3	12/6/17
Chengdu	4 (second phase east), 4 (second phase west)	6/2/17	Nanning	1	12/28/16
Chengdu	10	9/6/17	Nanning	2	12/28/17
Chengdu	7	12/6/17	Qingdao	3 (second phase)	12/18/16
Chongqing	Airport	12/28/16	Qingdao	2	12/10/17
Chongqing	5, 10	12/28/17	Shanghai	9	12/30/17
Dalian	1	6/8/17	Shenzhen	7, 9	10/28/16
Foshan	Guang-Fo	12/28/16	Suzhou	2 (second phase)	9/24/16
Fuzhou	1 (second phase north)	1/6/17	Suzhou	4	4/15/17
Guangzhou	6 (second phase), 7 (first phase)	12/28/16	Tianjin	6	8/6/16
Guangzhou	9, 13	12/28/17	Wuhan	6, Airport	12/28/16
Guiyang	1	12/28/17	Wuhan	8, Yangluo	12/26/17
Hangzhou	2 (first phase northwest)	7/3/17	Xi'an	3	11/8/16
Harbin	3	1/26/17	Xiamen	1 (first phase)	12/31/17
Hefei	1	12/26/16	Zhengzhou	2	8/19/16
Hefei	2	12/26/17	Zhengzhou	1 (second phase), Suburban	1/12/17
Kunming	3	8/29/17			
Panel B. Exisiting or planned subway lines in treated cities					
Beijing	15	12/30/10	Nanjing	1	9/3/05
Changchun	3	6/30/11	Qingdao	3	12/16/15
Changsha	2	4/29/14	Shanghai	10	2010
Chengdu	1	9/27/10	Shanghai	11 extensions	4/26/16
Chongqing	1	3/18/11	Shenzhen	5	6/22/11
Dalian	2	5/22/15	Shenzhen	11	6/28/16
Fuzhou	5 (first phase)	2021	Suzhou	1	4/28/12
Guangzhou	6	12/28/13	Tianjin	1	12/28/84
Guiyang	1 (old town)	2018	Wuhan	3	12/28/15
Hangzhou	4 (first phase)	2/2/15	Xi'an	1	9/15/13
Harbin	1	9/26/13	Xiamen	2	2019
Kunming	6	6/28/12	Zhengzhou	1 (first phase)	12/28/13
Nanchang	1	12/26/15			
Panel C. Exisiting or planned subway lines in control cities					
Changsha	2	4/29/14	Shaoxing	1	2022
Changzhou	1	2019	Shenyang	1	9/27/10
Dongguan	1	2022	Shijiazhuang	3	2022
Hohhot	1 (first phase)	2020	Taiyuan	2	2020
Jinan	R2	9/30/21	Urumqi	3	2021
Lanzhou	1	2018	Wuhu	2	2020
Luoyang	2	2022	Wuxi	1	7/1/14
Nantong	1	2021	Xuzhou	1	2019
Ningbo	2	9/26/15			

Sources: Opening dates of subway lines are from pages on Baidu Baike, Wikipedia, and various news sources.

have a completed line, we choose the first line to be built. Table 1, panel C lists these lines.

We choose a 5 kilometer stretch of each subway line listed in Table 1. We then create a 2.5 kilometer buffer zone on both sides of the segment, and we extract all road segments that lie within the buffer zone.[3] Because most subways are designed

[3] We picked these buffer zones before seeing the speed data.

to alleviate congestion in urban centers, in general, we pick subway segments close to the downtown. In cities with multiple subway lines included in the sample, we adjust the positions of the segments such that their buffer zones do not overlap so that we can maximize the sample size. These buffer zones include 1.3 million unique road segments. Most of these segments are tiny streets and do not have speed information. Our raw data have about 1.8 billion hourly speed observations from more than 350,000 unique road segments.

E. *Baseline Sample*

Road Segments Directly Affected by the New Subway Lines.—Our sample includes nonrandom patches of a city's roads. The average effect on sample road segments does not have an intuitive interpretation. Because subway lines are built in places with heavy traffic and aim to alleviate congestion on nearby roads, as a first pass, it is interesting to investigate whether subways have *any* effect on road congestion. Distance to the subway is arguably related to how much a road segment is affected, but it is an imperfect measure. Whether the subway is effective in diverting traffic from certain roads depends on the substitutability between subway trips and traffic through these roads.

The trip-planning feature of the digital map application can be used to identify road segments that are close substitutes to, and are directly affected by, the new subway line. These segments are those en route if one chooses to drive instead of taking the subway. Specifically, we first divide the buffer zone around each new subway line into 1km-by-1km grids. Between any pair of grids, we find the best public transit route (or routes, as the digital map service sometimes recommends several alternative routes). We save the collection of pairs between which the best public transit routes involve the newly built subway line. Then for each pair of grids in this collection, we find the best route(s) for driving. Road segments in these optimal driving routes are regarded as those directly affected by the new subway line. We repeat the same process under the typical traffic conditions for weekday morning and afternoon rush hours. This procedure yields 8,275 road segments that are most likely to be directly affected by the new subway lines. Online Appendix A.1 provides additional illustrations on how directly affected roads are selected.

Assigning Control Segments to Treated Lines.—In the baseline sample, there are 45 groups of treated road segments. Each group represents road segments directly affected by a new subway line. Our identification strategy is a stacked DID specification, where road segments from the 17 control cities serve as controls. We randomly divide control segments into 45 equal-sized subsamples and assign them to each treated group. For control segments, we include all road segments in the buffer zone, not only those that are directly affected.

We create a variable for each treated line that indicates the time relative to the opening date. The opening date of the treated line is assigned to the corresponding control road segments as their "fake" opening date. Road segments near each treated subway line and the corresponding control segments form a "group" of comparison. These groups are then stacked according to the time relative to the opening date.

We control the group-by-time-elative-to-opening fixed effects, which restricts the comparison between the treated and the control to be within the same *calendar* time. The inclusion of road segments from control cities and the use of the stacked DID approach eliminates macro trends and seasonality that are common to all cities.

Further Restrictions on the Sample.—We impose several additional restrictions on the sample. We first drop weekends and national holidays. In the baseline, we keep only morning rush hours between 7 AM and 9 AM and evening rush hours between 5 PM and 7 PM.[4]

We drop local streets due to concerns over the quality of speed information on those narrow, less-traveled streets. We notice some local streets are actually in restricted access areas such as paved paths in a park or driveways in a gated residential community. Many local streets have a substantial portion of missing values, presumably because no devices have passed through during the period of time. Although we could assign traffic-free speed to these missing values, we choose to be on the conservative side.[5]

For each group of comparison we include observations that are up to 48 weeks post opening. Although our sample expands over 18 months, the longest time we can track a new subway is about 1 year after its opening. This is due to two reasons. First, the first large batch of subway openings took place in December 2016, about a year before the end of our sample period. Second, 12 of the 25 treated cities have multiple line openings during the sample period; the gap between two openings is typically about one year. Tracking the effect up to 48 weeks guarantees that we do not include effects from further expansions of the subway system.

We also restrict the baseline sample up to six weeks prior to the opening. Subway construction itself could affect traffic conditions on nearby roads, thus making pre-trends uninformative about the nature of traffic conditions on these roads. We take advantage of an engineering fact of subway construction. Once the construction is mostly finished, a new subway line requires some time testing the hardware and software systems. For the subway lines we study here, the testing period usually runs two to three months before opening to the public. So traffic during the six weeks prior to opening can be assumed to be unaffected by the construction.[6] This relatively

[4] Baidu Maps uses the same definition of rush hours. Using the data we have, online Appendix Figure A.4 shows that peak traffic takes place between 7 and 10 AM in the morning and between 5 and 7 PM in the evening. The discrepancy between our data and the Baidu Maps definition is probably due to the fact that our data have a much smaller scope and are not representative of the cities in our sample. As a robustness check, we also estimate the effect on road speed in nonrush hours in Section IIC.

[5] About 76 percent of our road segments are local streets. Local streets may play an important role in alleviating congestion on the main roads. Akbar and Duranton (2017) find that even at times when main roads are very congested, many local streets are still relatively smooth. They suggest that the existence of small local streets essentially puts an upper bound on traffic congestion on the main roads. We leave the substitution patterns between main and minor roads to future research.

[6] The construction of a subway takes several major steps. The first step involves digging tunnels. Modern shield tunneling technology can advance tunnels underneath the roads. Yet it still requires operations on the ground, such as lifting the tunnel boring machine up and down, trucking rocks and pebbles, and pumping water from the underground. These operations are likely to affect traffic. Once tunnels are dug, tracks are laid and electrified. Usually one year prior to the line opening, scale test cars are run on the electrified tracks. In the meantime, subway stations are under construction. All of this construction is likely to affect traffic on nearby roads. Usually, major constructions are completed six months before opening. Then subway trains not carrying passengers are tested. In the last few months before opening, final touches on the system may still be in progress. The final approval for

short pre-treatment period is also chosen because in many cases the pre-treatment is in the post-treatment period of another new subway line in the same city. We perform robustness checks to validate this assumption. In particular, the inclusion of flexible time trends and a longer pre-treatment period also yields quantitatively similar results.

An additional 10 percent of the road segments are dropped from the sample due to missing values during the sample period. The baseline sample is a perfectly balanced panel of road segments.

The official opening of a subway line usually involves a ceremony with the presence of government officials and media. In order to make sure that the system is ready for carrying real passengers, in some cases there were "test rides" before the line officially opens to the public. The existence of those test ride periods may generate a spurious positive trend on road speed in the pre-treatment period and bias our DID estimate downward. We find detailed project progresses for each subway line from various sources.[7] We treat these test ride periods differently depending on whether they are contiguous to the official opening date. If the official opening date immediately follows the test ride period, we redefine the opening date as the first day of the test ride. If the test ride ends several days prior to the official opening date, we drop the test ride period from the sample.

Summary Statistics.—Table 2 reports the summary statistics of the sample. Panel A compares city-level characteristics between treated and control cities. The average treated city is more than twice as large (by population) and about 10 percent richer (by GDP per capita) than the average control city.

Panel B presents the speed and congestion index by the type of road and treatment status in the baseline sample. Arterial and subarterial streets account for the majority of road segments in the sample (local streets are dropped). Treated and control cities have similar compositions of road types. This is reassuring because it shows that although the control cities are in general much smaller and less prosperous than the treated cities, the road networks of the studied areas are not substantially different. The average speed is slightly above 30 kilometers per hour during rush hours. Except for highways, which we have a small number of segments, the average congestion indices are surprisingly similar across all types of roads, at around 1.7. This suggests that in equilibrium there is little room for arbitrage by taking a detour on smaller roads. Average speed and congestion index for each road type are also similar between the treated and control road segments.

Weekly Average Residual log Speed.—In order to reduce dimensionality and computational burden, we first partial out fixed effects in road speed and group the

public operation comes after a panel of specialists inspects the system, which usually takes place weeks ahead of the official opening. Online Appendix A.4 shows examples of the detailed administrative and engineering processes from two subway lines in the sample.

[7] Online Appendix Table A.1 lists the periods for test rides of the treated subway lines. Links to sources are also provided.

Table 2—Summary Statistics of the Baseline Sample

	Treated cities ($N = 25$)				Control cities ($N = 17$)			
	Mean	p25	Median	p75	Mean	p25	Median	p75
Panel A. City-level characteristics								
Population (million)	8.68	3.98	5.54	11.04	3.81	2.66	3.54	4.11
GDP per capita (yuan)	105,597	82,082	105,417	126,364	95,674	71,120	94,402	109,106
	Road segments in treated cities				Road segments in control cities			
	Observations	Number of unique segments	Average speed (km/h)	Average congest. index	Observations	Number of unique segments	Average speed (km/h)	Average congest. index
Panel B. Road speed and congestion								
All road segments	284,301	8,342	31.76	1.69	358,152	12,088	31.48	1.7
Highways	1,353	36	82.9	1.18	1,685	56	60.83	1.61
Urban expressways	5,773	219	50.09	1.77	7,921	269	53.18	1.59
Arterial streets	117,321	3,287	33.14	1.76	135,256	4,715	34.21	1.71
Subarterial streets	159,854	4,800	29.66	1.65	213,290	7,048	28.71	1.69

Notes: Data in panel A are from the *2017 Statistical Yearbook of Chinese Cities*. In panel B, each observation is a road segment-by-week-to-opening. Segments in the baseline regression sample are included. Treated road segments are those directly affected by the new subway lines. Week-to-opening is between 6 weeks before and 48 weeks after line opening.

residuals at the weekly level before embarking on the baseline model.[8] We first run the following regression:

$$(1) \qquad \ln speed_{lt} = \lambda_{l,dow,h} + \varepsilon_{lt},$$

where $\lambda_{l,dow,h}$ is the complete set of fixed effects, each of which indicates a specific road segment (l) in a given hour of the day (h) on a given day of the week (dow). The residual from this regression, $\widetilde{\ln speed}_{lt} = \hat{\varepsilon}_{lt}$, measures the log point deviation of hourly speed from the "usual" speed—the average speed on the same road segment, in the same hour of the day, on the same day of the week. Such a saturated model is not possible without the high-frequency data at the granular level. The adjusted R^2 of this regression is 0.67, suggesting that a large portion of the variation in speed can be explained by the "usual" traffic pattern. Note that $\widetilde{\ln speed}_{lt}$ may still include time trends and seasonality. The standard deviation of the residual is 0.24, indicating that seasonality or trends in speed remains nonnegligible.

Hourly residual log speed is then averaged at the weekly level:

$$\widetilde{\ln speed}_{lw} = \frac{1}{N_w} \sum_{t \in w} \widetilde{\ln speed}_{lt},$$

where w is the week *relative* to opening. Week 0 starts from the day the subway line was launched; N_w is the number of hours sampled in the week; $\widetilde{\ln speed}_{lw}$ should

[8] The results are almost identical when we run the regressions in one step. Online Appendix Table B.5 replicates the baseline with one-step regressions. The small differences are due to different sizes across weeks: due to days without observations and national holidays, some weeks may have a smaller number of hourly observations, so the weights for some observations are different between the one-step and the two-step estimations.

be interpreted as the weekly average log point deviation from the road segment's "usual" speed.

III. Effects of Subway on Road Speed

A. *Baseline Specifications*

We estimate the effect of subway on nearby road speed first using a stacked DID model with 45 subway line openings. Each of the 45 components of the DID model compares the change in residual log speed in treated road segments before and after the launch of a new subway line with the contemporaneous change in speed in control road segments. The empirical model can be written as

$$(2)\qquad \widetilde{\ln speed}_{lgw} = \sum_{w=\underline{w},\, w\neq -1}^{\bar{w}} \beta_w \cdot T_{lg} \cdot d_{gw} + \lambda_l + \lambda_{gw} + \gamma_t \cdot d_t \cdot \mathbf{X}_c + \varepsilon_{lgw},$$

where g indicates a group of road segments that includes the directly-affected road segments near a treated subway line and their randomly assigned road segments from control cities. The term T_{lg} is a binary variable indicating treatment status of the road segment. The term d_{gw} is a binary variable that takes value 1 when an observation in group g is w weeks away from the launch of the new subway line, with $w = 0$ indicating the week of launch. The range $[\underline{w}, \bar{w}]$ is the extent of time periods included in the sample, where $\underline{w} = -6$ and $\bar{w} = 47$. The variable λ_l is the road segment fixed effect; λ_{gw} is the fixed effect of group g in the wth week relative to the opening; d_t is a binary variable that indicates *calendar* week t; $\mathbf{X}_c$ is a vector of city-level characteristics. Note that d_t is colinear with λ_{gw}, $\mathbf{X}_c$ is colinear with λ_l, so d_t and $\mathbf{X}_c$ are not separately included in the regression.[9]

We choose the week prior to the launch, $w = -1$, as the base for comparison and impose β_{-1} to be 0. All β_w with $w \geq 0$ should be interpreted as the percent increase in speed in week w as a result of a new subway line opening, relative to the week before opening. Because the outcome variable is the logarithm of speed relative to the usual level in the same road segment, the coefficient can also be interpreted as the percent of time saved to travel through the road segment. We expect $\beta_w > 0$ for $w \geq 0$ if subways are effective in alleviating road congestion. Standard errors are clustered at the group level.

The fairly saturated model of equation (2) is robust to many common concerns over identification. Differences in levels of speed between the treated and control groups do not matter because road segment fixed effects purge out any level differences. Because in each case, time is realigned to indicate the week relative to opening, λ_{gw} captures the macro trend and seasonality common to both treated and control

[9] There are 45 groups, which is not a small number. We also experiment with the 95 percent confidence intervals from the block wild bootstrap procedure (Cameron, Gelbach, and Miller 2008). Results are almost identical. Randomly assigning the control segments into different groups could generate spatial and temporal correlations as nearby segments will appear as controls in different weeks for different cities. Clustering the standard errors at the city level could address this problem. Standard errors are also almost identical if they are two-way clustered at the city and the group levels.

road segments. Key to identification is the usual parallel trend assumption, which we can assess by testing whether $\beta_w = 0$ for $w < 0$, both individually and jointly.

Probably the biggest challenge to identification is the *differential seasonality* in traffic speed between treated and control cities. Differential seasonality means that traffic speed in some particular months is systematically different between the treated and control cities, even though there may not be a difference in overall trends. Three facts make this a valid concern. First, Table 1 shows that most (25 out of 45) of our treated lines opened in December. Second, January is China's holiday season and economic activity slows down. Online Appendix Figure A.5 shows that road speed in control cities increased by about 20 percent relative to the usual level in weeks around the 2017 Chinese New Year (CNY), which landed on January 28. Thus, the launching dates of many subway lines coincide with a period when road speed starts to increase. Third, panel A of Table 2 shows that treated and control cities differ substantially in size and economic performance. Larger and richer cities attract more migrant workers and thus are likely to experience a larger hike in traffic speed around the CNY as migrant workers return to their hometowns. With many lines launched right before the CNY, differential seasonality may confound our result, and it cannot be ruled out by a parallel pre-trend.

To account for potential differential seasonality, the term $\gamma_t \cdot d_t \cdot \mathbf{X}_c$ allows the residual speed in each *calendar* week to differ linearly with observable city-level characteristics, $\mathbf{X}_c$. In the regression, $\mathbf{X}_c$ includes log population and log GDP per capita. Admittedly, this approach is imperfect because (i) we are only able to control for a limited number of city-level characteristics and (ii) the relationship may not be linear. Online Appendix B.1 presents additional analyses to support this approach. In particular, online Appendix Figure B.2 shows that results are robust to the inclusion of higher-order polynomials of city-level characteristics. Online Appendix Figure B.3 plots the estimates of γ_t, which captures how road speed systematically differs by city characteristics in each calendar week. It shows that the role of differential seasonality is limited; it is only relevant in the weeks around the CNYs. Results are robust when these weeks are excluded.

The issue of differential seasonality arises because the treated and control cities are significantly different along some dimensions. As an alternative approach to address this issue, we estimate a standard two-way fixed effects model using observations only from treated units:

$$(3) \qquad \widetilde{\ln speed}_{lt} = \sum_{w=\underline{w},\, w \neq -1}^{\bar{w}} \beta_w \cdot T_l \cdot d_{lt}^{w} + \lambda_l + \tau_t + \gamma_t \cdot d_t \cdot \mathbf{X}_c + \varepsilon_{lt},$$

where the binary variable d_{lt}^{w} takes value 1 when segment l in calendar week t is w weeks away from the launch. The variable τ_t is the calendar week fixed effect and captures the average seasonality or time trend in treated cities. The term $\gamma_t \cdot d_t \cdot \mathbf{X}_c$ is controlled to guard against the possibility that cities with larger seasonal fluctuations are more likely to have new lines launched around the time when seasonality is most pronounced (e.g., December). Because every unit is eventually treated, τ_t and β_w are separately estimated from the variation in the timing of launch. Pre-treatment periods of the units that are treated later serve as controls for the post-treatment periods of the units that are treated earlier. With most of the treated lines launched in

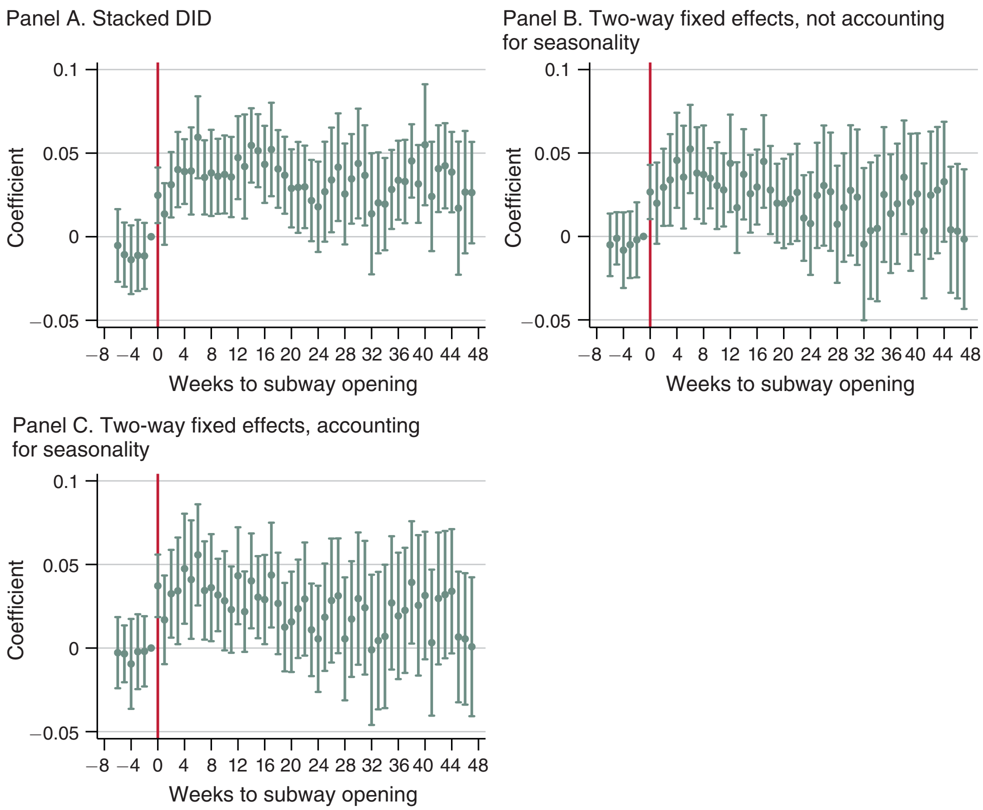

Figure 2. Dynamic Effects of Subway Launches

Notes: Panel A is estimated using equation (2). Panels B and C are estimated using equation (3), using only observations from treated cities. Panels A and C control for differential seasonality by allowing week-specific effects to differ by city characteristics. See Section IIA for details.

December, it is a concern that our sample may not have enough variation to credibly identify the two time-varying parameters (Abraham and Sun 2020, Goodman-Bacon 2018). We estimate equation (3) to corroborate the results from the stacked DID model.

Panel A of Figure 2 shows $\hat{\beta}_w$s and the corresponding 95 percent confidence intervals from the stacked DID model in equation (2). There is no evidence of differential pre-treatment trends. Road speed increases by about 2.5 percent in the week of opening and keeps rising in the first month. The effect reaches its maximum in the sixth week at around 5 percent before declining and stabilizing at between 3 percent and 4 percent. We still see a sizable effect 48 weeks after the launch. The remaining panels report results from estimating different versions of the two-way fixed effects model in equation (3). Panel B includes only the treated lines and does not adjust for seasonality. Overall, we still see a positive and significant effect throughout the sample period, although the magnitude of the effect is somewhat smaller and the coefficients associated with later weeks are less precisely estimated. Panel C adds the adjustment for differential seasonality to account for the possibility that cities

Table 3—Baseline Estimates

	(1)	(2)	(3)
Treated × post	0.044	0.036	0.036
	(0.008)	(0.009)	(0.010)
Model	Stack DID	Two-way fixed effects	Two-way fixed effects
Group-by-week-to-open fixed effects	✓		
Road segment fixed effects	✓	✓	✓
Calendar week fixed effects		✓	✓
Adjusted for differential seasonality	✓		✓
Including segments from control cities	✓		
Observations	642,453	284,301	284,301

Notes: The dependent variable is weekly average residual speed. Standard errors are in parentheses, clustered at the group level in column 1, clustered at the subway line level in columns 2 and 3.

with larger seasonality are more likely to have lines launched in December; results are almost identical.

Week-by-week estimates in equations (2) and (3) can be seen as "dynamic" specifications. We estimate the average effect in "static" specifications by replacing weekly dummies with a post-treatment indicator. In equation (2), $\sum_{w=\underline{w}, w\neq -1}^{\overline{w}} \beta_w \cdot T_{lg} \cdot d_{gw}$ is replaced by $\beta \cdot T_{lg} \cdot Post_{gw}$; in equation (3), $\sum_{w=\underline{w}, w\neq -1}^{\overline{w}} \beta_w \cdot T_l \cdot d_{lt}^{w}$ is replaced by $\beta \cdot T_l \cdot Post_{lt}$. Table 3 reports results from specifications corresponding to the three panels in Figure 2. In the stacked DID specification, the estimated average effect is 4.4 percent. The two-way fixed effects models, whose results are reported in columns 2 and 3, lead to a somewhat smaller effect at 3.6 percent. We use the stacked DID specification for the remaining analyses in the paper.

A few remarks are in order about endogeneity. Subway lines are likely to be built in areas with congested traffic. But this should not be a threat to the internal validity as long as these areas do not exhibit different *trends* in traffic congestion. Depending on the initial levels of road congestion, the magnitude of the subway's effect on alleviating congestion may be different. To the extent that subway lines are likely to be built in otherwise congested areas, the congestion-relieving effect we find here is expected to be greater than the hypothetical case where subway lines are randomly placed. We believe that the effect of randomly-placed subway lines is a parameter that bears little policy relevance. We do, however, present evidence in Section IIC on how the effects differ by the road segment's initial level of congestion and its geographical location relative to the subway.

Because a subway takes many years to build, its opening is widely reported and largely anticipated. However, urban traveling is not something one can easily arbitrage over time—one needs to get to work every workday. It could be a concern though, with the expectation of a subway line opening, people who have a higher idiosyncratic value for public transit are more likely to move to places near the line before it opens. This may have mixed impacts on our estimate. On the one hand, one may imagine that these people are more likely to ride the subway, so the estimation out of this selected population is likely to be larger than the effect on the whole population. On the other hand, these people are arguably less likely to own a car and more likely to travel by bus. To the extent that the marginal negative effect of a bus ride on road congestion is smaller than a car trip, diverting bus rides to subway trips is not as effective in reducing traffic congestion as diverting the same number of car rides. So our result may underestimate the average effect on the whole population. Nevertheless, using individual-level data from household travel surveys, Section III shows that restricting the sample to households that had not moved in the past five years does not affect the substitution patterns between modes of transportation, suggesting that the endogenous selection of residents is unlikely to be a major concern here.

Duranton and Turner (2011) find that over a period of 10 years, VKT increases proportionately to road lane kilometers, which is consistent with the prediction of the fundamental law of road congestion—building more roads does not reduce traffic congestion. They also find that the provision of public transportation does not affect VKT. Our results suggest otherwise, at least in the short run. Increases in aggregate VKT may come from increases in driving by current residents or from increased migration. Existing residents likely respond to the change in transportation infrastructure quickly. The fact that we find a persistent positive effect one year after a subway line opens basically rules out that demand for driving from existing residents explains the fundamental law. We cannot rule out the possibility that in the longer run, an improved public transit system will attract migration, increasing VKT and congestion on nearby roads. However, in a recent study, Gendron-Carrier et al. (2018) find that the opening of a subway system has a persistent and stable effect on reducing air pollution for up to three years. Using a panel dataset that tracks expansions of subway and road systems in US cities from 1991 to 2014, Pang and Shen (2019) find that a 10 percent expansion in subway length reduces traffic on interstate highways by 0.8 percent. These findings imply that the effect on road congestion may persist for a relatively long period of time.

B. *Robustness Checks*

Confounding Trends.—To alleviate the concern that our results are driven by some confounding time trends, we test whether the timing of the effect coincides with the timing of subway line openings. We expand the sample to up to 48 weeks before and after the subway line opening and repeat the same specification as in column 1 of Table 3, each with a placebo subway opening week between 48 weeks prior and 48 weeks posterior to the actual opening week. The Wald statistics associated with the treat $\times$ post variable from these regressions are then plotted against the placebo opening week relative to the actual week of opening. If the results are truly due to the subway line opening instead of some confounding trends, the largest Wald statistic should be found at around the actual week of the line opening. Figure 3 confirms that this is the case.

Some other events may take place at the same time of the launch of a subway line. Road blocks due to construction may be removed; shops may open near the subway station; road patterns may change; buses may be rerouted. Admittedly, we cannot partial out impacts from all these possible events. However, as long as the timing of these events does not synchronize with the subway opening, the null effect

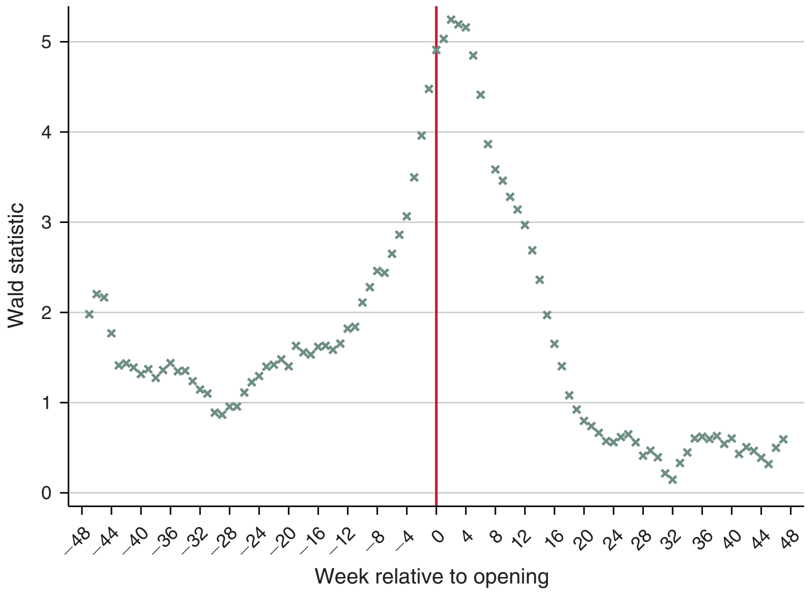

Figure 3. Wald Statistics from Estimates with Placebo Opening Dates

Notes: Each cross is a Wald statistic from the *t*-test of the key coefficient in the stacked DID specification where a placebo week of subway opening is used. The *x*-axis indicates the placebo week of opening relative to the actual week of opening.

in the pre-period and the placebo results in Figure 3 suggest that they are unlikely to drive our result. The spatial pattern of these confounding effects are likely to be different from the effects of the new subway line. Therefore, we can also devise ways to separately distinguish these effects. We discuss more evidence on this front in Section IIC.[10]

Longer Pre-treatment Periods.—We restrict the sample to be within 6 weeks prior to a subway line opening due to the concern that subway construction may directly affect road congestion. Within the 6-week window of the pre-treatment period, all major constructions should have concluded and traffic on the ground should be back to "normal." Nevertheless, 6 weeks is a short period of time, so we experiment with including 12, 24, and 48 weeks prior to the subway launch. Columns 1 to 3 of Table 4 show that the estimates are similar across different lengths in the pre-treatment periods. Figure 4 presents the week-by-week estimates up to 48 weeks pre-treatment. The pre-treatment effects all hover around 0 and there is an immediate and significant effect right at the time of the subway line opening.

[10] For example, online Appendix B.4 shows that conditional on the distance to the subway line, road segments that are closer to a subway station do not seem to have a slower traffic speed after the launch. This could rule out the effects due to shop openings and bus rerouting, provided that such events bring additional traffic to road segments near subway stations. In general, Section IIC shows various pieces of evidence that the effect is indeed larger in segments that we would expect to be more affected. Confounding factors are unlikely to be consistent with all these patterns.

TABLE 4—VARYING THE LENGTH OF THE PRE-PERIOD

	Length of pre-periods Number of weeks before launch			Discontinuity around the launch Treatment-specific time trend polynomial		
	12 (1)	24 (2)	48 (3)	Linear (4)	Up to third (5)	Up to fifth (6)
Treated × post	0.043	0.039	0.039	0.042	0.028	0.023
	(0.007)	(0.008)	(0.008)	(0.009)	(0.012)	(0.013)
Range of weeks relative to opening	[−12, 47]	[−24, 47]	[−48, 47]	[−48, 47]	[−48, 47]	[−48, 47]
Polynomial of time trends	None	None	None	Linear	Up to third	Up to fifth
Observations	726,727	915,041	1,174,944	1,174,944	1,174,944	1,174,944

Notes: The dependent variable is a segment's weekly average log residual speed. All models are estimated using the stacked DID model and include all 45 treated lines. Polynomial of flexible time trends indicate the degree of the polynomials for treatment-specific "pre" and "post" time trends. Standard errors are in parentheses, clustered at the group level.

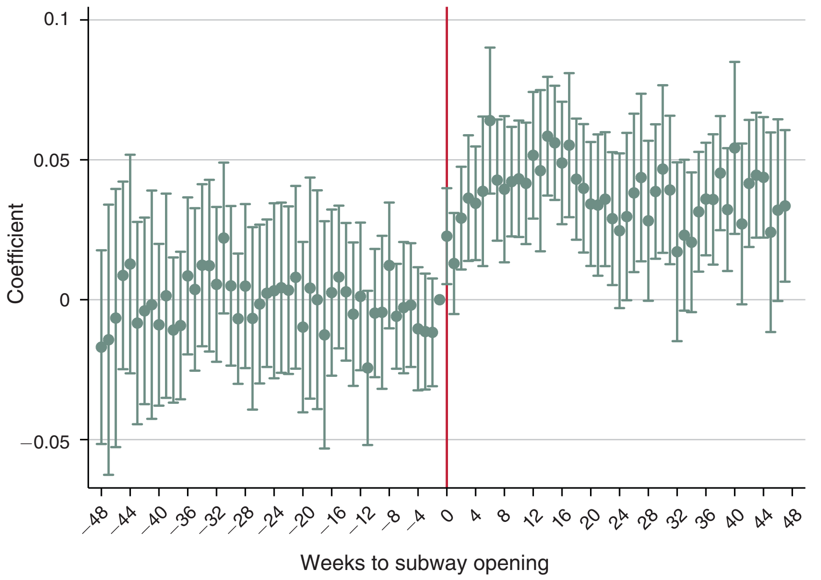

FIGURE 4. LONGER PRE-TREATMENT PERIODS

Notes: The model is estimated using the stacked DID specification. Up to 48 weeks in the pre-treatment period and 48 weeks in the post-treatment periods are included.

The issue of pre-treatment trends can be circumvented when we focus on the effect in a narrow neighborhood around the date of launch. We estimate difference-in-discontinuity models by including a flexible time trend with up to the fifth polynomial, separately for the treated and control segments, and separately for the pre- and post-periods. The coefficient associated with the treated × post variable thus indicates the discontinuous change in road speed at the time of subway launch relative to that in the control segments. Columns 4 to 6 of Table 4 report that the

Table 5—Subsamples by Time of Launch

	All lines (1)	Before 1/31/17 (2)	2/1/17–11/30/17 (3)	After 12/1/17 (4)
Treated × post	0.036	0.050	0.039	0.022
	(0.009)	[0.030, 0.069]	[0.021, 0.055]	[−0.008, 0.051]
Range of weeks relative to opening	[−6, 3]	[−6, 47]	[−20, 20]	[−48, 3]
Number of treated lines	45	21	9	15
Observations	179,741	408,546	143,567	445,626

Notes: The dependent variable is a segment's weekly average log residual speed. All models are estimated using the stacked DID model. In column 1, standard errors are in parentheses, clustered at the group level. In columns 2 to 4, numbers in brackets are 95 percent confidence intervals from 499 repetitions of wild block bootstrapping.

discontinuous effect is salient across different specifications. The most saturated model yields an effect of around 2.3 percent, which is close to the first-week effect as shown in Figure 4 and various panels of Figure 2.

Subgroups of Subway Lines by Time of Opening.—Our data cover 18 months between August 2016 and January 2018. The opening dates of the treated lines spread across this period. Therefore, although we restrict the observations from each treated line to be within 6 weeks before and 48 weeks after the opening, the number of post-opening periods in the sample differs across treated lines. In the dynamic specification in equation (2), each β_w is estimated from a different mix of treated lines. In the static specification the treated × post variable is identified from a mix of treated lines and periods. Different composition complicates the interpretation of our results.

We can circumvent this problem by restricting the sample period such that all β_ws are estimated from all subway lines, although this would substantially reduce the sample size. For lines launched in December 2017, we are able to track up to four weeks since launch. Most new subway line openings in 2016 were in December. Although our data start in August 2016, there is a gap from early October through November. In column 1 of Table 5, we narrow the sample period to six weeks prior to and three weeks posterior to the subway line opening so that the coefficient of treated × post is estimated from the same set of weeks relative to line opening for most subway lines; this results in a significant effect of 3.6 percent.

To avoid the problem of differential composition yet still be able to include a reasonably long sample period, we divide the treated lines into three subsamples: 21 lines launched before January 31, 2017; 9 lines launched between February 1 and November 30, 2017; and 15 lines launched after December 1, 2017. We include the maximum length of period that all the lines in a specific subsample can cover.[11] We run separate estimations for each subsample. Because there are small numbers

[11] For the earlier group, we include 6 pre-treatment weeks and 48 post-treatment weeks. Note that not every line in this subgroup has an observable pre-period up to six weeks. For the middle group, we include 20 weeks in the pre-period and 20 weeks in the post-period. For the later group, we include 48 weeks in the pre-period and 3 weeks in the post-period including the week of opening.

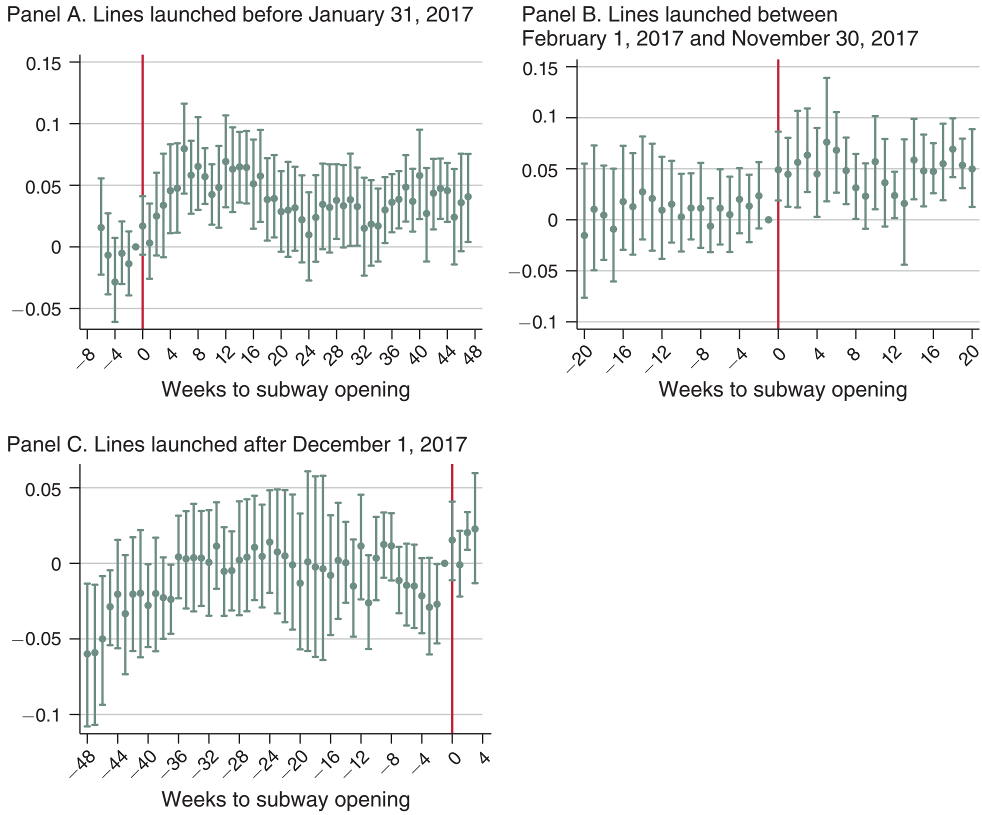

Figure 5. Lines Launched in Different Periods

Note: Ninety-five percent confidence intervals are from 499 repetitions of wild block bootstrapping (Cameron, Gelbach, and Miller 2008).

of lines in each group, we report the 95 percent confidence intervals from the block wild bootstrap (Cameron, Gelbach, and Miller 2008).

The dynamic effects of the subway line opening for each group are shown in Figure 5. For all three subsamples, we see a statistically significant increase in road speed right after the opening of a subway line. For the first and second subsamples, for which there are several months in the post-period, the effect hovers between 4 percent and 5 percent. For the last subsample, the effect is about 2.5 percent in the first four weeks. In general, the coefficients in the pre-treatment period are around zero, except for the earliest few weeks in the estimation for the last subsample, where the coefficients are estimated less precisely. We suspect that the small sample size and the small number of cities in each subsample make it hard to separately identify β_w and γ_t.[12] Results from the static model are shown in

[12] Online Appendix B.7 shows results from an alternative specification where the $\gamma_t \cdot d_t \cdot \mathbf{X}_c$ terms are not controlled. We find that the pre-period coefficients are more precisely estimated and are closer to zero.

columns 2 to 4 of Table 5. The average effect is about 5.0 percent, 3.9 percent, and 2.2 percent, respectively, for the three subsamples.

C. *Heterogeneous Effects by Road Segments Characteristics*

The treated road segments in the baseline are those directly affected by the new subway lines. These are the likely group of road segments for which the congestion-relieving effect of a subway is the largest. However, an arguably more interesting question is how much a subway reduces the *overall* road congestion and how the effect differs across different road segments in the urban transportation network. There is at least one constraint and one caveat to properly answer this question. The constraint is that we do not have speed data on all, or a random sample of, the road segments in a city. The caveat is that the effect of a subway line on the overall congestion in the city depends on the size of the city, location of the subway, and the layout of the existing road network. A couple of subway lines may be sufficient for a medium-sized city, while China's largest cities, such as Beijing and Shanghai, already have over a dozen subway lines. The marginal effect of a subway line on the overall road congestion may be different by line, but this difference is less informative without knowing the existing traffic patterns and transit infrastructure in the city.

In light of these two observations, we rephrase the question to how the effects differ on road segments with different characteristics. These characteristics include the segment's geographic location relative to the subway line, its initial congestion level, and its position in the broader urban transportation network. These heterogeneities could speak to the effect of a typical subway line in a hypothetical road network. We expand our treated sample to include all road segments in treated cities, and run the following regression:[13]

$$(4) \qquad \widetilde{\ln speed}_{lgw} = \sum_{k \in K} \beta_k \cdot T_{lg} \cdot Post_{gw} \cdot z_{lk} + \lambda_l + \lambda_{kgw} + \gamma_t \cdot d_t \cdot \mathbf{X}_c + \varepsilon_{lgw}.$$

Here, K is a set of complete partition of road segment characteristics. Binary variable z_{lk} is equal to one if segment l has characteristic k. The variable λ_{kgw} is the segment-characteristic-by-group-by-week-to-opening fixed effects. This is a set of more saturated fixed effects than those in the baseline. It allows time trends in traffic speed to differ by road characteristics. Each β_k indicates the speed-enhancing effect of the subway on road segments in treated cities with a certain characteristic, compared with road segments with the same characteristic in control cities.[14] For

[13] We include all road segments in cities with treated subway lines, except for local streets, in the treated sample. Because in some cities there were multiple subway line openings during the sample period, in order to avoid double counting, we group subway lines that opened within the same city-month and consolidate comparing groups. In each consolidated group, the treatment time is assigned as the first opening date of the treated lines. We consolidate 45 line openings into 35 groups.

[14] In some cases, we are unable to produce the exact same characteristics in the control sample. Because some subway lines in control cities are still under construction, we cannot identify "directly affected" road segments using the trip planning function on the online digital map. In these cases, we define directly affected segments as those within 1 kilometer away from the proposed subway line. In each control city, we only extract one buffer

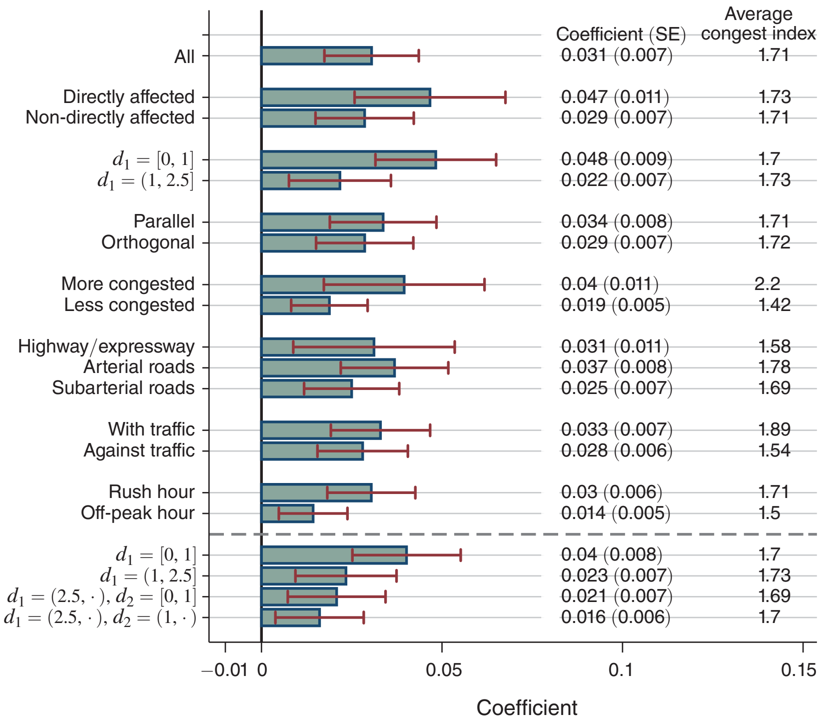

Figure 6. Heterogeneous Effects by Road Segment Characteristics

Notes: Each group of bars report $\hat{\beta}_k$s from estimating equation (4). Except for the last block, the treated road segments include those within 2.5 kilometers from the treated subway line. The length of the bar indicates the magnitude of the coefficient. The range bar indicates the 95 percent confidence interval. Values of $\hat{\beta}_k$ and its corresponding standard error, and average congestion index for the group of road segments are also reported next to each bar. In the third-to-last block, each observation is split into weekly average in morning rush hours and weekly average in evening rush hours. Road segment-by-morning/evening indicator fixed effects are also controlled for. In the second-to-last block, weekly average in non-rush-hour (9 AM–5 PM) speed is also included, road segment-by-rush hour indicator fixed effects are also controlled for. In the last group, treated road segments also include those within 2.5 kilometers from an existing or planned subway line. See Table 1, panel B for these lines. The last group is estimated from a specification that replaces λ_{kgw} with λ_{gw}. The variable d_1 represents the road segment's distance to the treated subway line and d_2 represents the road segment's distance to the nearest existing subway line.

each set of segment characteristics, each block of Figure 6 plots β_ks and their associated 95 percent confidence intervals from estimating equation (4). The figure also reports the magnitude of the coefficient and its standard error, as well as the average congestion index for road segments with the corresponding characteristic.

We first focus on nearby roads and include road segments that are within the 2.5 kilometer buffer zones from treated subway lines. The first estimate in Figure 6 shows that on average the launch of a new subway line increases nearby road speed by 3.1 percent. The second block of coefficients is from a regression where we divide the nearby road segments into those directly affected by the treated subway

zone. So in the set of results where we investigate how the effect spreads across the road network, we control for λ_{gw} instead of λ_{kgw}.

lines (those in the baseline regressions) and those not directly affected. The effect for the former group is 1.8 percentage points larger than the latter group, a statistically significant difference. The third block of coefficients show that the speed-enhancing effect of a subway declines quickly with distance. Speed on road segments that are within 1 kilometer of the treated subway lines increases by 4.8 percent, while the effect on those between 1 and 2.5 kilometers is 2.2 percent. The next regression divides the nearby road segments into those largely parallel to the new subway and those largely orthogonal to it. The effect is somewhat stronger in parallel roads.

Next, we divide road segments into halves based on whether the average of pre-treatment congestion index is below or above the sample median (1.7). The congestion-relieving effect is much larger in initially more congested road segments. Speed on these road segments increases by 4 percent upon a subway line opening, while the effect on less congested roads is less than 2 percent. Next, we divide the sample by road type. Because there are not many highways and urban expressways, we combine these two. The effect is strongest in arterial roads. It is intuitive because most of the subway lines are parallel with, and sometimes directly underneath, arterial urban roads.

Recall in the baseline sample that road speed is derived from four hours in a day, two in the morning (7 AM–9 AM) and two in the evening (5 PM–7 PM). We divide the observations into morning and evening rush hours, and categorize road segments-by-rush hour into those "with" or "against" the traffic. A segment is "with" traffic in morning rush hours if its average congestion index in morning rush hours is larger than that in evening rush hours. If a segment is "with" traffic in the morning, it is considered "against" traffic in the evening, and vice versa. Average congestion index is 1.89 for road segments that are with traffic and 1.54 for those against traffic. The effect is larger in road segments that are with traffic. In the next regression, we also include observations from off-peak hours. Traffic is 20 percent worse in rush hours and the effect is twice as large.

Two conclusions are immediately evident from these heterogeneous regressions. First, the congestion-relieving effect is larger in more congested road segments. This is consistent with the hypothesis that travelers who experience high congestion levels are more likely to switch to public transit (Anderson 2014). Second, the effect declines quickly with distance to the treated subway line, which suggests that access is an important constraint to using public transit.

Proponents of subways argue that subway lines exhibit a network effect. The addition of a new subway line could increase ridership in existing subway lines, reducing traffic congestion on road segments that are close to existing lines even if they are far away from the new subway line. The final block in Figure 6 examines this hypothesis. In the regression, we include road segments in buffer zones of existing subway lines in treated cities (listed in Table 1, panel B). Road segments in treated cities are divided into four mutually exclusive groups. The first group includes those within 1 kilometer of the new subway line ($d_1 \in [0,1]$). The second group includes those between 1 and 2.5 kilometers from the new subway line ($d_1 \in (1,2.5]$). The remaining two groups are more than 2.5 kilometers from the new subway line. While those in the third group are within 1 kilometer of an existing subway line ($d_1 \in (2.5,\infty)$, $d_2 \in [0,1]$), those in the fourth group are

TABLE 6—INDIVIDUAL TRANSPORTATION MODE CHOICES AND HOUSEHOLD VKT

	Subway (1)	Bus (2)	Car (3)	log VKT (4)	VKT (5)
Panel A. All households					
log distance to subway station	−0.209	0.085	0.062	0.686	620
	(0.026)	(0.017)	(0.014)	(0.191)	(326)
Mean dependent variable	0.472	0.499	0.558	2.964	4,302
Observations	63,710	63,710	63,710	49,500	49,500
Panel B. Households not moved between 2009 and 2014					
log distance to subway station	−0.203	0.072	0.064	0.696	632
	(0.025)	(0.016)	(0.015)	(0.194)	(333)
Mean dependent variable	0.463	0.504	0.561	2.948	4,289
Observations	60,591	60,591	60,591	47,305	47,305
Panel C. Households not moved and had a car since 2009					
log distance to subway station	−0.467	0.156	0.043	−0.050	4.412
	(0.121)	(0.067)	(0.064)	(0.088)	(940)
Mean dependent variable	0.577	0.519	0.458	9.337	14,075
Observations	12,482	12,482	12,482	10,195	10,195
Household characteristics	✓	✓	✓	✓	✓
Individual characteristics	✓	✓	✓		
TAZ fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓

Notes: The key explanatory variable is TAZ's log average distance to the nearest subway station. See footnote 16 for details about how this average distance is calculated. Data are from the Beijing's Household Travel Surveys in 2010 and 2015 (BTI 2010, 2015). Each observation is an individual in columns 1–3. The outcome variable is the number of trips in the corresponding mode of transportation on the day of the survey, taken from the "One-Day Travel Diaries" in the survey. Individuals in the sample include those who have trip records on the day of survey. Trips that use only walking are excluded. An observation is a household in columns 4 and 5. The outcome variable in column 4 is the log total kilometers driven in the past year from all the cars owned by the household. Total vehicle-kilometer traveled (VKT) is added by one before taking logs, so households that do not own a car are also included in the sample. The outcome variable in column 5 is the level of VKT. Household characteristics include indicators for household income brackets, home ownership, house type (commercial apartment, work unit dormitory, low-income housing, etc.), whether having children under age five, and household size. Individual characteristics include gender, age, indicators for educational levels, industry and occupation. All models include TAZ and year fixed effects. Standard errors are clustered at the TAZ level.

farther away $(d_1 \in (2.5, \infty),\ d_2 \in (1, \infty))$. If there is a network effect in the subway system, we expect to find a meaningful speed increase in the latter two groups, and the effect on the third group to be larger than that on the fourth group. Our findings confirm these hypotheses. The effect is 2.1 percent for the third group and 1.6 percent for the fourth group.

III. Substitution Patterns among Modes of Transportation

Higher speed suggests less traffic on the road. In this section, we use individual-level travel data from Beijing to show substitution patterns between the subway and other modes of transportation. Due to data limitations, the results presented in this section are largely descriptive and should be interpreted as correlations rather than causalities.

Individual travel records are from one-day travel diaries, which are part of the Household Travel Surveys in Beijing (BTI 2010, 2015). We obtain two rounds of surveys in 2010 and 2015. During this period, 16 new subway lines and extensions were launched in Beijing. The smallest identifiable geographic level is a Transportation Analysis Zone (TAZ).[15] For each sample year, we measure a TAZ's access to the subway by its average distance to the nearest subway station.[16] The travel diaries record detailed modes of transportation used in each trip. We drop respondents who did not have a trip record or only had walking trips. We count the number of trips that use bus, subway, and car. Notice that an individual can use multiple modes of transportation in a day's trip.

Table 6 reports the correlations between a TAZ's distance to a subway and residents' choice of transportation modes. All models control for TAZ and year fixed effects, so the coefficient should be interpreted as the correlation between the log change in distance to a subway and the change in the number of trips that use a certain mode of transportation. The surveys are repeated cross-sectional and do not track the same households across different rounds, so we control for detailed household and individual characteristics to account for changes in the demographic composition in a TAZ.

Panel A shows that on average, each commuter has 0.47 subway trips, 0.5 bus trips, and 0.56 car trips per day. Improved access to subway is positively associated with subway trips and negatively with bus and car trips. Estimates in columns 1–3 suggest that when the TAZ's distance to the nearest subway station declines by 70 percent (log distance declines by 1), the number of subway trips per commuter increases by 0.21, and the number of bus and car trips per commuter decreases by 0.085 and 0.062, respectively. The coefficients imply that about 1 additional subway trip is associated with 0.4 fewer bus trips and 0.3 fewer car trips.

The BTI survey also has detailed information on car ownership and usage; in particular, it records the mileage driven on cars in the past year. Although the mileage variable is based on recollection and likely has plenty of measurement error, we use it to check whether car usage drops in association with improved access to a subway. We sum over VKT from all the cars a household owns, plus one to that sum, and take the natural logarithm, so households that do not own a car are also included in the sample. Column 4 shows that, in contrast to what the fundamental law of road congestion predicts, improved access to a subway is indeed associated with less driving. The elasticity of VKT on distance to subway is about 0.7. Column 5 estimates the model with the level of VKT as the outcome variable. As the log distance to the nearest subway station decreases by 1, average VKT per household declines by 632 km, which is about 15 percent of the sample average.

Households that value subway trips more may move to neighborhoods that are expected to have improved access to a subway. Although it is hard to credibly identify causal effects using the current data, we can alleviate concerns over false

[15] TAZs are small geographic areas. In 2010, Beijing is divided into about 2,000 TAZs. Within the fourth ring road, where the density of subway lines is the highest, the median TAZ covers an area of 1.3 square kilometers and contains about 10,000 residents.

[16] We calculate a TAZ's average distance to the nearest subway station in the following steps. First, we divide the city into 500m $\times$ 500m grids. Second, we calculate the distance between the center of each grid and the nearest subway station. Third, we calculate the TAZ's average distance to the nearest subway station as the weighted average of grids' distances to their nearest subway stations, where the intersected area between the TAZ and the grid is used as weight. We repeat the same procedure separately for subway systems in 2010 and 2015.

correlation by focusing on the nonmovers. Panel B includes only households that had not moved between 2009 and 2014. The results are essentially unchanged.

To investigate the driving behavior in more detail, panel C further restricts the sample to households that had a car in 2010.[17] For this smaller sample, access to a subway is associated with increased subway trips and reduced bus trips, some evidence of reduced car trips, but no evidence of smaller mileage on cars. This result suggests that for households that already own a car, driving behavior does not change significantly in association with improved access to a subway. Therefore, the reduction in overall driving may come from reduced car purchases.

IV. Welfare Analysis

A. *Conceptual Framework*

In this section, we build a conceptual framework of transportation mode choices, which helps link our empirical finding to welfare implications. Consider a city with N commuters, each inelastically demanding one trip. Indirect utility of a commute for individual i using transportation mode m is written as

$$V_{im} = b + A_m - c_m + \xi_{im}. \tag{5}$$

The individual can choose between two modes of transportation, road vehicle (denoted as a, including cars and buses) or subway (denoted as s), $m \in \{a, s\}$. The variable b is the benefit from the trip, which is assumed to be a constant. The term A_m is the amenity value of mode m. Amenities include things like comfort and privacy. The term c_m is the *private* cost of each commute in mode m. Specifically,

$$c_m = \begin{cases} c(N_a), & \text{if } m = a; \\ r, & \text{if } m = s. \end{cases} \tag{6}$$

The private cost of a commute by road vehicle has two components. The first component is the fixed cost of taking such a trip. For commutes by car, this includes costs of fuel, parking, and depreciation of the vehicle; for commutes by bus, it is the fare. The second component is the time cost, which is an increasing function of the number of total road vehicle commutes, $c'(\cdot) > 0$. Thus, $N_a \cdot c'(\cdot)$ captures the negative externality one additional trip imposes on all other trips on the road. The variable r is the cost of one subway commute, which is the sum of the subway fare and the time cost. We assume there is no congestion in the subway system, so the cost of a subway trip does not depend on the number of riders.

[17] The 2015 BTI survey asks the age of each vehicle. We include all households in the 2010 survey that owned a car and households in the 2015 survey that owned a car made in or before 2010. This imputation is not perfect. Households that owned a car in 2010 but replaced it with a new car between 2010 and 2015 will be mistakenly excluded from the sample. First-time car owners after 2010 who bought a secondhand car made before 2010 will be mistakenly included in the sample. The lower share of car trips in this sample compared with those in panel A and panel B is probably due to the fact that the sample here is more skewed toward respondents in the 2010 survey.

In addition to the private cost, the government spends τ_s on each subway trip, which includes construction costs as well as subsidies to cover operating costs. Assuming the subway fare plus government subsidies equal total cost, the *social* cost of each subway trip is $r + \tau_s$. Similarly, the government spends τ_a on an average commute by road vehicle, which includes construction and maintenance costs of the road infrastructure, as well as subsidies to bus trips.

We denote $u_m = b + A_m - c_m$, which is the same for all commutes given the mode of transportation. The term ξ_{im} captures commuter i's idiosyncratic preference for mode m. We assume ξ_{im} follows an i.i.d. distribution across individuals. The difference $\xi_{ia} - \xi_{is}$ captures individual i's preference for road vehicle relative to subway.

Without subway, r can be seen as infinitely large and everyone uses road vehicle for commute, $N_a = N$. The aggregate social welfare is

$$W_{ns} = N \cdot \big(b + A_a - c(N)\big) + \sum_i \xi_{ia} - \tau_a \cdot N, \tag{7}$$

where the subscript *ns* indicates the case with "no subway." When a subway is available, we assume a nonempty subset of individuals will switch to the subway. The person who is indifferent between the two modes of transportation has preference such that $\xi_{ia} - \xi_{is} = c(N_a) - r + A_s - A_a = \overline{\Delta\xi}$. Commuters who have $\xi_{ia} - \xi_{is} > \overline{\Delta\xi}$ will remain traveling by road vehicle. We denote this group of commuters as Ω_a. Commuters who have $\xi_{ia} - \xi_{is} \leq \overline{\Delta\xi}$, denoted as Ω_s, switch to the subway. The aggregate social welfare is

$$\begin{aligned} W_s = {} & \Big[N_a\big(b + A_a - c(N_a)\big) + \sum_{i \in \Omega_a} \xi_{ia}\Big] + \Big[N_s(b + A_s - r) + \sum_{i \in \Omega_s} \xi_{is}\Big] \\ & - \big(\tau_s \cdot N_s + \tau_a \cdot N_a\big). \end{aligned} \tag{8}$$

Change in social welfare due to the introduction of a subway is therefore

$$\begin{aligned} \Delta W = {} & \underbrace{\big[c(N) - c(N_a)\big] \cdot N_a}_{\text{stayers' gain}} \\ & + \underbrace{\Big[\big(c(N) - r\big) \cdot N_s - (A_a - A_s) \cdot N_s - \sum_{i \in \Omega_{is}} (\xi_{ia} - \xi_{is})\Big]}_{\text{switchers' gain}} + \underbrace{[\tau_a - \tau_s] \cdot N_s}_{\text{change in govt spending}}. \end{aligned} \tag{9}$$

The first term is the welfare gain accrued to those who remain commuting by road vehicle (the "stayers"). The introduction of a subway benefits this group by increasing road speed and reducing travel time. The second term is the welfare gain for those who switch to the subway (the "switchers"); this is the sum of the difference in trip cost, the difference in amenities, and the difference in idiosyncratic preferences for the two modes. The third term is the change in government spending.

Figure 7 illustrates the model. Commuters are ranked according to their idiosyncratic preference along the horizontal axis. From left to right, people have increasing preferences for road vehicle over subway. When there is no subway, everyone travels by road vehicle; their utility is represented by $u_a + \xi_{ia}$. When a subway

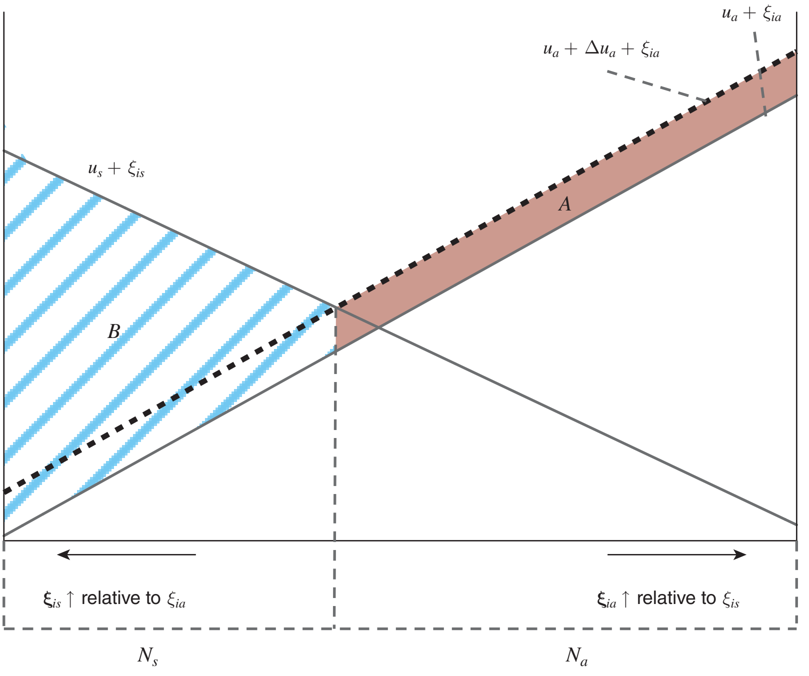

Figure 7. Illustration of the Welfare Impact

Notes: $\Delta u_a = c(N) - c(N_a)$ is the gain for a commute by road vehicle from reduced congestion. The red shaded area A indicates the welfare gain to those who continue to commute by road vehicle. The blue hashed area B indicates the welfare gain to those who switch to subway. See Section IVA for details.

becomes available, some are diverted and road speed increases. The utility from a commute by road vehicle shifts up by $\Delta u_a = c(N) - c(N_a)$, shown in the graph by the dashed curve. Utility from a subway trip is represented by the curve $u_s + \xi_{is}$.[18]

The empirical result found in this paper speaks directly to the first term of equation (9).[19] The welfare gain for the stayers is represented by the shaded area A in Figure 7. It is the product of $c(N) - c(N_a)$—the value of time saved in each commute by road vehicle due to faster road speed (Δu_a)—and the number of remaining commutes by road vehicle.

[18] Several assumptions are made to keep the model tractable. Demand for trips is assumed to be inelastic. In fact, additional trips could be induced, generating additional welfare gains or deadweight loss if the private gains from those marginal trips are smaller than government subsidies. Utility from completing a trip is assumed to be the same, while in reality different trips have different purposes and values. Relaxing these assumptions complicates the analysis and could change our conclusion substantively.

[19] Note that $c(N) - c(N_a)$ is related to the concept of "road technology"—how road speed changes as a function of traffic volume. Suppose the substitution pattern between subway and car trips follows the results in Table 6, panel B: 1 subway trip replaces 0.315 car trips. Also assume the supply of bus services does not change. Beijing had 1.4 billion car commutes and 1.1 billion subway trips in 2016. Without the subway system, there would have been 1.7 billion car commutes. We show later that our empirical result suggests that Beijing's subway system increases citywide average speed by 3 percent. This implies an elasticity of road speed with regard to the number of car trips of −0.14.

B. *Quantifying the Welfare Gain from Beijing's Subway*

We quantify the first term of equation (9) using data from Beijing. We choose Beijing as the example not only because it provides the best data available but also because it has one of the most extensive and complete subway networks among Chinese cities. Most additional data used for this exercise are from the 2016 *Beijing Transportation Annual Report* (*BTAR*) published by the municipal government (BTI 2016). Online Appendix C.1 lists detailed sources of data used in the back-of-the-envelope calculations. In line with our empirical setting, we focus on commuting trips and assume they all take place during rush hours.

The monetized value of time saved in each commute by road vehicle, $c(N) - c(N_a)$, can be calculated with the time cost of each commute, the increase in road speed (our main empirical result), and the monetized value of time. In 2016, the population weighted average distance to the subway is about 1 kilometer in Beijing. At this distance, we estimate that the road speed increases by 3 percent.[20,21] The average one-way commute costs 56 minutes (BTI 2016). A 3 percent increase in speed implies a 1.68-minute decrease in commuting time. Average annual wage in Beijing was 92,456 yuan. Assuming 2,000 hours a year for a full-time job, this translates into a wage of 0.77 yuan per minute. The monetary value of commute time is typically assumed to be half of the wage rate (Parry and Small 2009, Anderson 2014), so the time savings for each commute from reduced congestion is worth 0.65 yuan (US$0.10). In 2016, there were 1.4 billion car commutes and 1.1 billion bus commutes in Beijing (BTI 2016). This translates into a welfare increase of 1.6 billion yuan per year (US$249 million) due to faster commutes.[22]

The other two terms in equation (9) are not directly related to our empirical results. Quantifying them requires additional data, stronger assumptions, and correlational evidence. Additional cautions are called for when interpreting these calculations. We delegate details to online Appendix C.2. We find that each of these terms is much larger than the value of time saved from faster road speed, but according to our calculations they largely offset each other. If accurate, this would suggest that, when only considering the cost and benefit of commuting trips, the benefit of Beijing's subway system seems to exceed its cost.

[20] The population-weighted average distance to the nearest subway line is calculated as the average distance of TAZ to the nearest subway line weighted by population. We run the following regression: $\ln speed_{lgw} = \beta_0 \cdot T_{lg} \cdot Post_{gw} + \beta_1 \cdot T_{lg} \cdot Post_{gw} \cdot \ln dist_l + \lambda_l + \lambda_{gw} + \iota \cdot \lambda_{gw} \cdot \ln dist_l + \gamma_t \cdot d_t \cdot \mathbf{X}_c + \varepsilon_{lgw}$. The differences in this specification from the baseline are the inclusion of terms (i) $\ln dist_l$ and (ii) $Post_{gw} \cdot \ln dist_l$. The term $\ln dist_l$ is the log distance to the treated (for segments in treated cities) or the control (for segments in control cities) subway line. The term $Post_{gw} \cdot \ln dist_l$ accounts for potential heterogeneous trends for road segments with different distances to the subway line. The parameter β_0 indicates the effect at 1 kilometer away from the subway, which is estimated to be 0.03 with a standard error of 0.0068.

[21] We assume the estimated effect on average road segment applies to the average commute. In exploiting the heterogeneous effects, we show that the effect is larger in more congested roads. To the extent that more congested roads are also more traveled, our estimate of the speed increase on the average road segment is an underestimate of the gains to an average commute.

[22] $0.77/2$ (unit time value) $\times$ 1.68 (time saved each commute) $\times$ $(1.4 + 1.1)$ billion (number of commutes).

V. Conclusion

This paper studies the effect of subways on road congestion in the setting of the rapid expansions of subway networks across Chinese cities. We use user-generated, real-time big data to measure road speed, which is new to the literature. We find that the launch of a new subway line immediately and significantly increases speed on nearby roads. On average the speed on road segments that are close substitutes to the new subway line increases by about 4 percent over the first year following the line opening. The effect is concentrated in road segments that were initially congested and declines quickly with distance to the new subway line. There is also suggestive evidence of spillover effects along the subway network.

We present a conceptual framework of transportation mode choices. Through the lens of the model, we show how our empirical result is related to the welfare calculation. Using data from Beijing, we estimate that the time savings for each commute by road vehicle from faster speed is worth US$0.10.

Many other potential benefits, such as reduction in air and noise pollution and traffic accidents, are not considered in this paper. In fact, existing studies suggest that health benefits from reduced air pollution account for a substantial share of the construction and operation costs of the subway (Chen and Whalley 2012, Gendron-Carrier et al. 2018, Li et al. 2019). Our calculations are also based on short-run results. In the long run, the existence of an extensive subway system could change location choices of residents and firms, encourage migration, and facilitate economic agglomeration (e.g., Tsivanidis 2019; Heblich, Redding, and Sturm 2020; Gonzalez-Navarro and Turner 2018).

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