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New metro and housing price and rent premiums: A natural experiment study in China

Abstract: This paper used a stringent natural experiment, leveraging longitudinal housing price and rent data before-and-after two newly operated metro lines in Shenzhen, China, to provide robust causal inference on the effects of new transport infrastructure on housing premiums. Local planning knowledge was used to enhance the natural experiment research design. We assigned control groups from housing surrounding alternative line alignments that were planned but not implemented, and treatment groups were from housing in the new stations' catchment areas. In addition, how the metro route was planned, particularly the pursuit of land leasing revenue and engineering feasibility, helped us achieve as-if-random treatment-control group assignment. We applied hedonic difference-in-difference (DID) models to assess the average treatment effects. The results showed that for one metro line, housing prices and rents increased significantly after the metro went into service, while only the housing rent premium was observed for the other line. In addition, housing rents of treatment groups showed a price gradient over distance to metro stations in both metro lines, which might suggest that housing rents would be a more reliable measure to estimate the housing premiums from the metro interventions. This study offers a better understanding of the causal links between metro access and housing premiums in developing countries. The findings also provide insights into designing property taxation to offset metro development costs in China.

1. Introduction

Many metro lines (mass railway transit) have been built in the world's rapidly expanding cities in the past two decades (Sun et al., 2020; Xu et al., 2016). More than 40 cities operate metro systems in China, with a total track length of 6,730 km by 2019 (Lin et al., 2021). These metros are expected to improve accessibility, tackle traffic congestion, and stimulate local economic growth (Billings, 2011; Loo et al., 2010; Mohammad et al., 2013). However, the massive metro investments create heavy fiscal burdens on local governments (Chang, 2014), urging them to seek financial tools for the metro projects. Metro access could be capitalised in housing prices, while it was typically harvested by developers and property owners as windfalls due to the public goods nature. The government has therefore devised various approaches to capturing betterment value to offset the costs associated with metro development (Yang et al., 2020). For example, China widely used land leasing as the instrument, while recently started considering value-added tax reform as more sustainable approach, and a typical example is to collect capital gain taxes associated with property value increase (McIntosh et al., 2017). For the targeted value capture mechanisms, justification and specific design (e.g., tax rate and spatial extent of collection area around a station) need stronger evidence of the housing premiums arising from metro access, and causality is important in enhancing the credibility (Billings, 2011).

Previous research mainly relied on observational studies to evaluate proximity to rail transit stations on housing premiums (Bartholomew & Ewing, 2011). Most studies found a positive association between transit accessibility and housing prices (Mohammad et al., 2013). They applied statistical techniques to create the hypothetical treatment and control comparison to estimate the premium effects (Xiao, 2017). However, unobservable confounders (e.g., self-selection of transit access, attitude toward property investment) may distort the observed associational effects on housing premiums (Cao & Lou, 2018). There is still a lack of research design to examine the before-and-after impact of a new metro infrastructure with a robust treatment-control comparison(Diao et al., 2017), especially for rapid developing countries. Without a rigorous control group, we cannot ensure that some of the measured access premium is not due to confounding factors such as metros being located in places where prices are rising. The main difficulty is that metro interventions are intrinsically difficult to manipulate experimentally, and their design and opening are beyond the researchers' control.

Several longitudinal studies have attempted to estimate housing premiums from new urban rail transit (Ibraeva et al., 2020), with longitudinal data analysis being one way to uncover the effect. These studies compared the price changes before and after new lines and stations using the hedonic difference-in-difference (DID) method (Billings, 2011; Cao & Lou, 2018; Tian et al., 2020). Generally, they assigned the housing units to treatment and control groups based on cut-off points in the distance (Melser, 2020). For example, areas far from the stations are considered control groups, whereas housing in station catchment areas is considered as treated (Diao et al., 2017). However, there may exist significant difference between treatment and control groups before the construction of rail transit lines, which might even be the reasons (e.g., density and land use pattern) for the government to develop the line. It is difficult to exclude such confounding factors between the two groups in analysis, thus the causality inferred in those studies might not hold (Billings, 2011; Wagner et al., 2017).

Researchers are seeking more rigorous quasi-experiment approaches to improve the validity of the evidence, and recent review papers include a broad range of interventions such as rapid bus lines and light rail transit to estimate housing premiums (Murray & Bardaka, 2021; Yen et al., 2018). However, these reviews rarely make a distinction between natural experiments and those with quasi-experiment designs, since the latter might be difficult to be structured as approaching the power of a randomised controlled experiment for causal inference (Dunning, 2012). Principally, a natural experiment should use the "naturally occurred" variations in exposure to the new interventions to investigate the treatment effects (Craig et al., 2017). A less powerful design like quasi-experiment approaches might compromise experimental principles and make it difficult to obviate confounders in practice.

Context specific knowledge has the potential to verify random treatment-control group assignment to create an as-if random distribution of confounders between the groups to exclude confounding explanations (Sun et al., 2022). Leveraging planning knowledge, this paper aims to present a natural experiment to estimate housing premiums of metro access using two newly developed metro lines in Shenzhen, China. Based on DID approach, we will compare the longitudinal housing prices and rents (construction to post-operation stage) between two groups before and after the new metro lines to infer causal effects on housing premiums. It will contribute to current literature in threefold. First, we used local metro planning knowledge, resting on strategies that pursue land finance and engineering efficiency, to verify the as-if randomness of the treatment-control group assignment. Compared with quasi-experiment research design, it will reinforce the credibility of the research findings. Second, we used different indicators to assess housing premium level, which will provide different insights into metro access capitalization. Third, although there is rapid rail transit intervention and booming housing market in developing countries, limited causal evidence was provided for metro-triggered housing premium. It will extend the evidence in rapid growing Chinese megacities.

2. Related work

2.1 Housing premiums from transit proximity

Transport infrastructure access is important for urban development, and different facilities (e.g., rail transit, expressway) have experienced wide expansion globally (Donaldson, 2018; Hurst & West, 2014). Since a metro can provide alternative travel options for users, reduce travel costs, and encourage active living, urban residents may be willing to live close to stations and pay more for access (Mohammad et al., 2013). The extra money paid was embedded into the cost of purchasing or renting housing in the property market (Webster, 2010). or received as a windfall if they owned or rented the housing before the new metro line went into operation. Researchers and policymakers often want to know how and to what extent these premiums will be, when designing value capture schemes to recover metro investment costs.

Numerous studies have investigated associations between urban rail transit proximity and housing premiums (Debrezion et al., 2011; Wu et al., 2020). Many studies found positive associations, assuming that everything else is statistically controlled, despite the fact that these results vary with transit types, neighbourhood characteristics, and city contexts (Debrezion et al., 2007). In addition, some studies used sophisticated statistical modelling to account for spatial autocorrelation and price gradients to transit stations (Xiao, 2017). However, since most of these studies measure associational relationships, they are unable to infer causality in the relationship between rail transit and housing premiums.

2.2 Causal inference of transit intervention and housing premiums

Natural experiment or quasi-experiments are more suited than multivariable statistical modelling for testing the effect of transport infrastructure on housing premiums (Wagner et al., 2017). For quasi-experiment research design, a hedonic difference-in-difference (DID) model can estimate the average treatment effect by comparing housing outcome variables between treatment and control groups before and after the intervention (Melser, 2020). Housing around new metro stations that will receive improved transit accessibility is included in the treatment group (Im & Hong, 2018). The control group is comprised of housing without exposure to the new stations that are assumed to have otherwise exhibited a similar premium pattern (Murray & Bardaka, 2021). Most studies identify control groups based on a cut-off point in the distance to the station, assuming that housing beyond is unaffected by the new metro. In relevant studies, mixed housing premium effects are observed (Dubé et al., 2014; Wagner et al., 2017; Yen et al., 2018).

Control group assignment for quasi-experiment studies could be problematic. The validity of causal inference rests on whether the treatment-control groups are comparable and the assignment process is randomized (de Vocht et al., 2021; Dunning, 2012). First, treatment and control group delineation was based on a distance threshold to transit stations, which cannot ensure control group is unaffected by the intervention (Billings, 2011). In this regard, the observed treatment effects may be underestimated. Second, the inconclusive findings from previous studies are likely due to difficulties in controlling unobserved confounders (Mohammad et al., 2013; Tian et al., 2020). The metro investment is often an endogenous decision made by local governments, in principle based on the population density (e.g., ridership potentials) and land development of the station area. In China, numerous new stations are located on empty land to promote land leasing uplifting (Yang et al., 2016). The line alignment and station location may be chosen intentionally, leading to biased estimates (Murray & Bardaka, 2021). Third, the time-variant but unobservable confounders among groups are difficult to be excluded by statistical models. For example, whether exposed to new metro stations may be subject to exogenous shocks, and different locations and housing sub-markets were affected differently.

2.3 As-if randomness and natural experiment

Increasing studies noticed that non-randomized assignment may hinder the validity of causal inferences, and natural experiment has been advocated recently. Natural experiment tended to be narrowly defined as the situations that the treatment-control assignment process is random or as-if random (Craig et al., 2017). Since truly random assignment is rare in practice (Angrist et al., 2002), stepping towards as-if randomness is more feasible and crucial for enhancing credibility of the causal inference. Regardless of the unclear definition, different methods identifying as-if random processes were proposed, including carefully justifying the plausibility of the determination and checking the balance of covariates between treatment and control groups (Sun et al., 2022).

A qualitative method was useful for understanding the random or as-if random nature of assignment process (Dunning, 2012). The common approach is to leverage unique background knowledge to verify the randomness of assignment process (Phan & Airoldi, 2015). For instance, several studies argued that in some unique cases, decisionmakers can achieve as-if random assignment of the treatment group unconsciously.

2.4 Integrating metro planning knowledge into natural experiment design

Deep-diving into planning knowledge of the new metro lines may help achieve more rigorous natural experiment (Dunning, 2012; Sun et al., 2022). For metro-related topic, recent studies have sought alternative methods to assign control groups (Cao & Lou, 2018; Wagner et al., 2017). For example, control groups could be the line alignments originally planned but abandoned eventually, or comparable lines completed at a later stage (Billings, 2011). However, relevant studies have not stepped further to pursue as-if randomness of the treatment group assignment and natural experiment research design.

Optional line alignments and decision-making processes by government officials in China provide possibilities for discovering as-if randomness. Since as-if-random assignment in natural experiments can work well when assignment rules are 'discovered' ex-post facto and are somewhat obscured in history and process, deep domain knowledge detected by qualitative research can be vital in shaping a good experimental research design (Sun et al., 2022). During the planning process, metro companies and planning institutes usually formulate several line choices for consideration by government officials (Yang et al., 2016). All those line choices would satisfy planning criteria such as ridership potential, population density, and master plan integration. However, the city government needs to fund the metro projects with either general city revenue or bank loans (Song et al., 2021). Tax revenue cannot help much, as 75% needs to be returned to the central government. In contrast, land transaction revenues remain at the discretion of local governments and can be used to support their infrastructure provisions and other local expenditures (Sun et al., 2020). As a result, land transactions constitute the financial pillar of Chinese municipal financing. Government officials would use different priorities to finalise metro line alignment in the decision-making process. Metro lines and stations are often routed to bypass dense and well-developed areas, locating in relatively undeveloped areas instead, or linking new towns to extend land finance opportunities (Shen & Wu, 2020).

Another characteristic of the process is the dominating role of engineering considerations in infrastructure construction. Metro lines in China are often constructed rapidly, which is partly due to the political needs of local officials. It would be an achievement if the new metro could operate during the mayor's governing term (e.g., within 5 years). Consequently, key policymakers are forced to find a relatively cost-effective way to locate the line alignment when there are different options, reduce demolition of existing housing, design straight route, and reduce engineering difficulties (Lai & Schonfeld, 2016). Therefore, engineering constraints could also determine the final route.

Neither engineering constraints nor land exploitation potential were related to the initial housing price or subsequent potential premiums near the planned metro stations. This disjuncture in goals and objectives provides an opportunity for designing a natural experiment with a degree of as-if-randomness in group assignment by comparing housing premiums around the built and unbuilt routes.

2.5 Housing price vs rent: which one better measures accessibility premium?

Previous studies generally use housing prices to measure the accessibility premium, partly because data can be conveniently extracted from transaction records (Sun et al., 2015). However, recent studies challenged the reliability and validity of this measure (Melser, 2020). The aggregated housing price changes before-and-after the intervention can theoretically be decomposed into speculation effects, anticipation effects, and effects from transit services after metro operation (Yen et al., 2018). In this situation, housing prices might have started to increase from the time the metro project was publicly announced (Yiu & Wong, 2005). Therefore, it is difficult to separate the effects at different stages. Longitudinal housing price datasets are required for such studies, since a metro project needs several years to accomplish (Melser, 2020), and to capture the full effect, the data series should ideally extend between the announcement and opening of a new line. This is a challenging requirement for data availability for rapidly developing cities in China.

By contrast, housing rents may better reflect a metro's accessibility premium. Rental contracts are signed for a fixed term, usually one year or shorter in China. Tenants will estimate housing utility over the given period when they sign a new or renewed lease, allowing the market signal to be more responsive to changes in accessibility in the short term (Melser, 2020). Theoretically, rent will not change until the metro starts to operate, and it can capture the treatment effect of the metro intervention using relatively short-term data (Melser, 2020). It may also reflect short-term negative externalities of construction in the period before opening. Housing rents thus could better reflect utility changes due to metro interventions. Moreover, housing rents may be a more accurate indicator of end-user preferences than those of investors (Hu et al., 2022). In China's booming housing market, housing is largely unaffordable, being driven by owner and investor speculation (Cui et al., 2021) and many residents living in megacities must rent their homes. This holds in other developed cities as well. For example, Melsers (2020) compared housing prices with rents before and after a new rail transit line in Sydney, Australia. They found that rent prices performed better in estimating transit-induced housing premium than housing prices.

In summary, previous studies called for a stringent natural experiment enhanced by planning knowledge and leveraging longitudinal housing price and rent data before-and-after metro operation to provide robust causal inference. This is particularly necessary in developing countries like China where experienced soaring housing market and transport infrastructure expansion recently.

3. Method

3.1 Intervention: two metro lines

We implement the ideas discussed above by investigating metro lines No. 7 and 11 in Shenzhen, both of which have planned optional line alignments but were abandoned in decision-making process. This enables us to compare housing price and rent changes between the built and unbuilt routes. Shenzhen is located in southern China and had a population of 13.4 million in 2019 (Shenzhen Statistic Bureau, 2020). Ten metro lines were in operation by 2019, with a total length of 411 km. The city has been expanding its metro system rapidly over past two decades, and one guideline proposed in 2008 advocated speeding up the planning of the new lines, which included line 7 and 11 (Shenzhen Municipal Government, 2008). Meanwhile, Shenzhen has experienced a booming housing market in the past decade, due to internal reasons, such as rapid economic development, population growth and the constraint of limited construction land. In addition, the cross-border effect of housing demand from Hong Kong and over-spilled capital further stimulated housing market in Shenzhen (Zhou & Guo, 2015).

The local government and state council approved the two lines between 2010 and 2012. Line 7 connected the western and eastern parts of the city with a length of 30.2 km (see Appendix S1) and covers major residential and employment areas. The construction started in 2012, and the line went into service in November 2016. Similarly, line 11 started construction in 2012 and operation in June 2016, with a total length of 51.9 km. This line connected the western periphery (e.g., the new airport) and the high-speed rail station in the urban centre. Lines 7 and 11 cost about 25 billion and 20 billion CNY, respectively.

3.2 Treatment and control group assignment

Local planning institutes in Shenzhen considered optional line alignments in certain segments of Line 7 and Line 11 before final plan were finally approved. After the consideration of key transport and urban planners, as well as government officers, metro route selection among alternative line alignments were finished in 2011. Relevant information and figure were documented later in *Shenzhen immediate rail transit construction plan (2011-2016)*. The selected routes were moved to construction stage shortly, while abandoned line alignments were not implemented in subsequent practice. In addition, nearby residents were not informed about the detailed plan and route until the final metro plan completed publicly.

In this study, the treatment groups were housing in the catchment areas of the built metro route, and control groups were those in the route planned but not selected by decisionmakers. To strengthen credibility of causal inference, we used several criteria suggested by Dunning (2012) to verify as-if randomness in the treatment and control group assignment. First, decision-makers have no information, incentives, or capacity to intentionally manipulate housing units in the treatment-control group assignment. In other words, we could verify if policymakers intentionally select specific treatment or control groups and relevant housing units. Second, if treatment assignments are random, the driving forces should be independent of potential outcomes, referring to housing prices and rents, and of pre-treatment attributes related to those potential outcomes.

Local planning knowledge of each metro line helped us understand the planning process. Specifically, line 7 had two optional alignments near *antuo* mountain area (Fig. 1). One option crossed with existing line 2 at *antuo* mountain station (red line, Fig. 1), while the other plan would intersect it at *qiaochengbei* station (blue line). To comprehend the underlying decision-making process, we conducted interview with the staffs in Shenzhen Metro who participated these projects (see appendix). After getting the written materials and explanations, we understood that selection between the plans was driven by engineering consideration. In the overall plan, the metro depot and carriage parking space were pre-set in this area because there are several small patches of greenfield land, which meant lower land acquisition, relocation and compensation costs for metro company. In the detailed plan, the metro depot was placed in a narrow valley. A two-floor parking lot was on the hillside of *antuo* mountain. Rolling stock would need to ascend from underground to surface for parking when they are out of service. In the alternative plan (the blue line), the high slope of the track posed some engineering difficulties, given that the elevation to distance ratio between metro line and parking lot was high. This plan was thus abandoned. By contrast, the finally selected alignment (red line) was built from the underground route to the above-ground parking space with a longer track. The government officials did not anticipate the engineering constraints until sub-system coordination was needed between the metro depot, parking sites and slope tracks design tasks. From this investigation, we concluded that policymakers had little consideration for placing the metro line in a specific area for better housing premiums (Fig. 1).

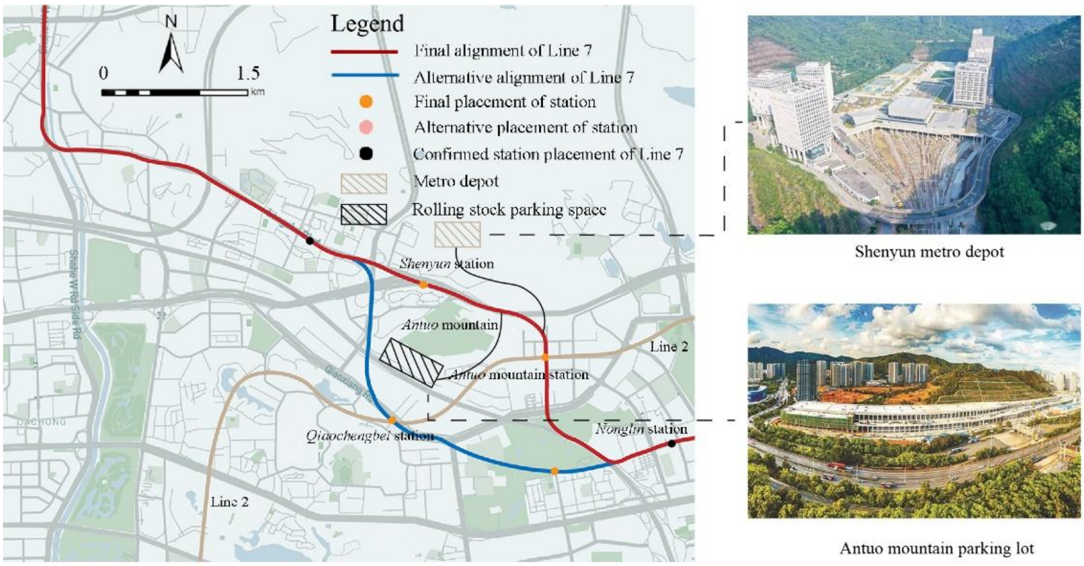

Fig 1. The initial line alignment and station placement of line 7

Line 11 also had two optional line alignments in the central area of *baoan* district (Fig. 2). One plan was along the *baoyuan* road (red line, Fig. 2) with two proposed stations, covering the *baoan* new town area and *bihai* area. The other plan (in blue) was along the No. 107 national highway with three stations mainly serving the old town area of *baoan*. The old town area experienced rapid industrialisation. The blue route should have prevailed because it could serve a large population with heavy traffic. By contrast, the other (red route) option for Line 11, overlapped with existing Line 1, which has been serving adjacent areas since 2011, and would not therefore increase transit accessibility as much as the blue route. However, the red route plan was built eventually. Similarly, after discussing with the staffs from Shenzhen Metro, we understood the reasons drove the final decision-making. Similar to other metro lines in Shenzhen (Yang et al., 2016), pursuing land-leasing fees was non-negligible for line alignments. Since there was no other approach (e.g., value-added tax) for the local government to cover the betterment value of existing housing, land leasing fee was crucial to support metro projects. The land-leasing data provided further evidence of this goal. We could see that numerous land transactions were concentrated around the red line alignment (see Fig. 3), and the land-leasing fees of those parcels exceeded 28 billion RMB from 2012 to 2020. By contrast, in the past decade, few lands were transacted along the blue line (the abandoned route). The logic is that the highly built area along the 107 national highway constrained the land leasing potential, while the vacant lots along *baoyuan* road created land leasing opportunities. Therefore, land-leasing fees were the motivation for the final route choice. This line planning process was independent of pre-treatment housing prices.

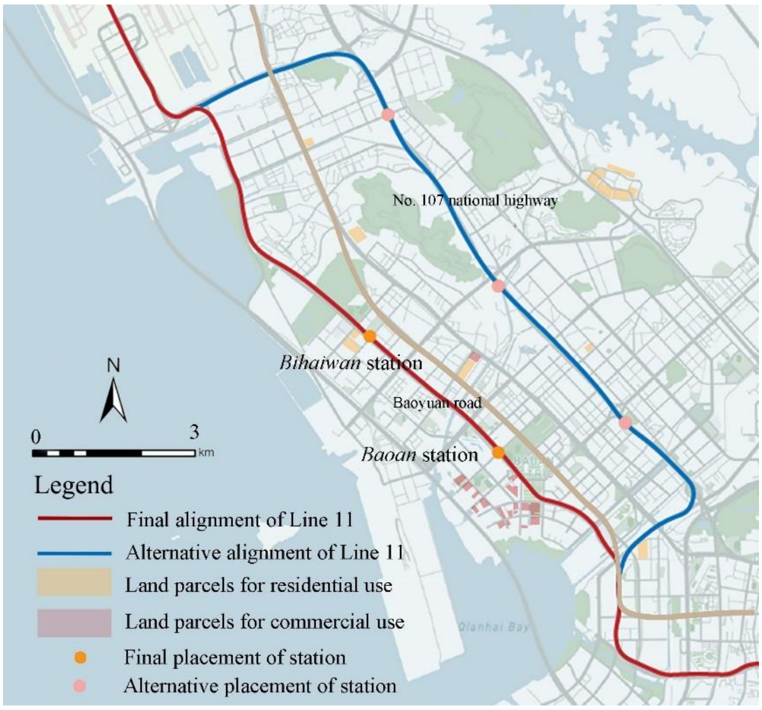

Fig 2. The initial line alignment of line 11 and transaction of land parcels in this area (data source: Shenzhen Land and Real Estate Exchange Centre)

3.3 Study design

Fig. 3 (a) shows our analysis framework. The as-if randomness in the line planning choice assigns the housing units into treatment groups exposed to the new metro (red lines in Fig. 1 and 2) and control groups without the new metro (blue lines in Fig. 1 and 2). We applied a hedonic difference-in-difference (DID) approach to analyse the longitudinal data of housing prices and rents to tease out the housing premiums of the metro interventions. We assumed a potential housing fluctuation trend based on the time span. Our analysis thus covered periods from construction to completion stages (indicated in the red dush line in Fig. 3 (b)).

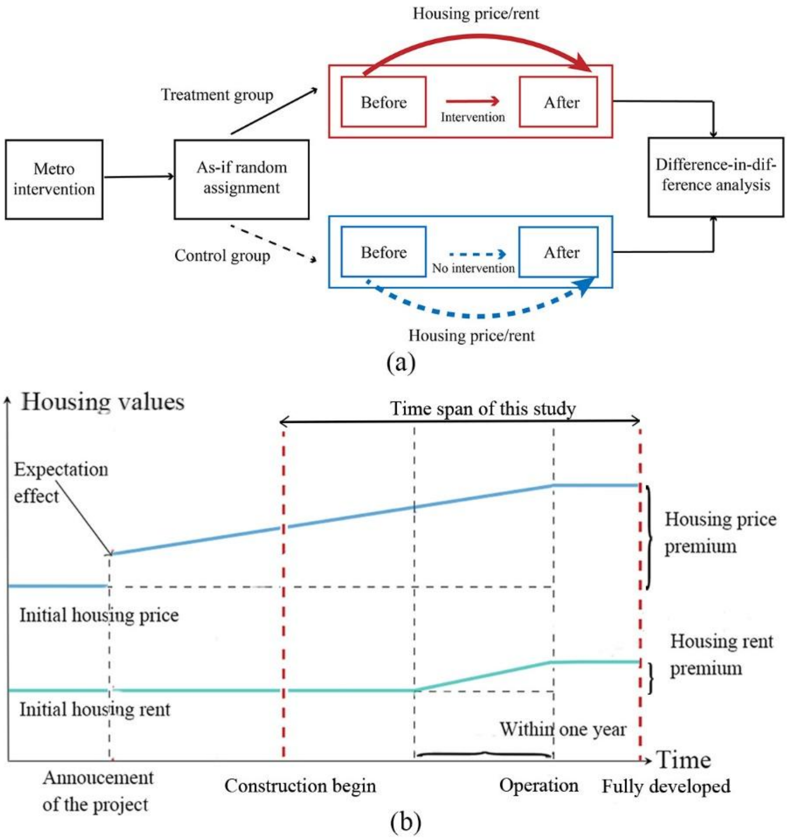

Fig 3. Analysis framework of this study

3.4 Measurement

3.4.1 Repeat cross-sectional housing data

We collected housing price and rent data to estimate housing premiums from the new metro line. The housing price data were based on transaction records collected from *fang* (https://www1.fang.com/), *lianjia* (https://www.lianjia.com), and *zhongyuan* (https://sz.centanet.com/). Rent data came from *fang* and *leju* (https://www.leju.com). These are mainstream housing agencies in China, and the data sources have been validated in previous studies (Su et al., 2021). We excluded duplicate transaction records collected from different agents. Our valid dataset ranged from January 2013 to December 2020, covering a reasonable period before and after the metro intervention.

Based on previous studies and planning practices in China, we used samples within a 1,000 m walking distance of stations (Sun et al., 2016; Xu & Zhang, 2016). Authority in China also noted that 800-1000 m walking distance was the covered station areas for different TOD projects. Housing data were geocoded in the Geographic Information System (GIS). In addition, we have accounted for changes in the value of the Chinese currency (CNY) by converting prices and rents into 2013 real values using the annual Consumer Price Index (CPI) from the National Bureau of Statistics.

3.4.2 Covariates

Covariates included commonly used unit-level attributes for each housing unit, including floor, bedrooms, housing age, orientation, and total number of building floors in the residential estate. We also controlled for locational and amenities attributes, including estate land area, floor area ratio, and number of buildings in the estate, and covered school district (number of primary and middle schools). In addition, we controlled for walking distance to the nearest existing metro station, distance to nearest metro transfer station, and distance to the CBD, all measured for each transacted housing unit. Detailed information was shown in Appendix S2.

3.5 Statistical modelling

3.5.1 Hedonic difference-in-difference analysis

We applied hedonic DID models to estimate the average treatment effect by comparing housing premiums at different stages based on repeat cross-sectional observations. DID models can overcome omitted variable bias and time-invariant effects that often plague traditional hedonic models. For example, without DID, a hedonic model will be biased if metro stations were located in more (or less) expensive housing areas (Wagner et al., 2017).

There are 32 quarters in the study period (2013-2020), and the quarters in which the metro lines were opened (2016 Q2 for Line 7, and 2016 Q3 for Line 11) were used to distinguish before- and after- intervention periods. The baseline model estimated average treatment effect (Model 1-4), with the hedonic DID model specified as:

$$Ln(HP_{it}) = \beta_0 + \beta_1\ (Treat_i * Post_t)\ + \beta_2\ Post_t + \beta_3\ Treat_i + \beta_4 Individual_i + \gamma_t + \lambda \quad (1)$$

where $Ln(HP_{it})$ is the log value of the transaction price (sale or rent) of property *i* in time *t*; $Treat_i$ is an indicator equalling one if the estate is within the treatment group and zero otherwise; $Post_t$ is a time indicator that equals one for the quarters after the operation of metro station and zero otherwise; $Individual_i$ is a set of covariates (see section 3.4.2) that control time-invariant effects. Coefficient β_1 on the interaction term captures the treatment effect of the opening of metro lines on per price of housing units. γ_t is a year fixed effect controlling the overall time influence on treatment and control groups, and λ is an error term.

We fitted several DID models to explore different walking distances to metro stations. Following previous studies (Diao et al., 2017), three interaction items were created (Treatment (0—200m) ∗ Post), (Treatment (200—600m) ∗ Post), and (Treatment (600—1000m) ∗ Post), after dividing each treatment group into three categories: within 200 meters, 200—600 meters, and 600—1000 meters (Models 5—8). The variance inflation factors (VIF) of the independent variables were less than 2 in all models, showing no multicollinearity. All analyses were conducted in STATA 17.0.

3.5.2 Parallel trend analysis

We conducted a parallel trend analysis to check the internal validity of the DID model. We explored the plausibility of the common trends assumption over the study period. A revision of equation (1) was used to test the parallel trend:

$$Ln(HP_{it}) = \beta_0 + \beta_1\,(Treat_i * Post_t) + W_1\,Before_t + W_2\,Post_t + \beta_2\,Treat_i + \beta_3 Individual_i + \gamma_t + \lambda \qquad (2)$$

where W_1 reflects the differences between the treatment and control groups before the intervention, and W_2 refers to the differences after the intervention. Due to the anticipation effect, housing prices may increase ahead of the operation of the new metro. The time trend diagram can depict the overall trend, and the W_1 should be insignificant if the parallel trend holds.

4. Results

4.1 Descriptive statistics

Table 1 summarises the descriptive analysis of observations for Line 7, with 818 sales and 615 rental records. The average sale price was 66,432.2 CNY per m^2, and the average monthly rent price was 84.0 CNY per m^2. The average price was higher in the treatment than in the control groups. Most covariates do not show significant differences between treatment and control groups for Line 7, except for age of housing and whether oriented to south.

Table 2 shows the observations for Line 11, with 4,424 sales and 1,514 rental records. The average sales price was 48,444.2 CNY per m^2, and the average monthly rental was 52.5 CNY per m^2. The average price was also higher in the treatment than control group. Most covariates do not show significant difference between treatment and control groups for Line 11, except for walking distance to the nearest metro station.

Table 1. Descriptive results of transaction records in study area (Line 7)

	Full sample (sales)	Full sample (rental)	Treatment group of Line 7 (sales)	Control group of Line 7 (sales)	Treatment group of Line 7 (rental)	Control group of Line 7 (rental)
	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage
Outcome						
Per price for sales (CNY/m^2)	66432.2 (23641.8)		68190.1 (24544.0)	63710. 4 (21933.2)		
Per price for rental (CNY/m^2/month		84.1 (27.5)			88.0 (1.0)	80.4 (1.1)
Ln per price	11.0 (0.3)	4.4 (0.3)	11.1 (0.3)	11.0 (0.3)	4.4 (0.3)	4.4 (0.3)
Covariates						
Number of bedrooms	2.8 (0.8)	2.9 (1.1)	2.9 (0.7)	2.6 (0.9)	3.0 (1.0)	2.7 (1.1)
Floor area (m^2)	98.0 (43.3)	110.4 (56.4)	97.7 (34.4)	98.4 (54.3)	115.1 (57.6)	106.0 (55.1)
Whether oriented to south (Yes=1)	67.2	75.8	59.6	79.1	69.9	81.4
Age of the housing (in years)	12.2 (5.9)	12.7 (6.1)	9.3 (2.9)	11.1 (8.7)	9.7 (3.5)	16.6 (6.6)
Number of total floors	25.0 (10.5)	25.0 (10.9)	26.1 (9.5)	23.4 (11.6)	26.3 (10.4)	23.8 (11.3)
Number of buildings in the estate	9.89 (6.2)	9.04 (6.3)	9.14 (3.7)	11.07 (8.7)	8.7 (4.0)	9.4 (7.9)
Floor area ratio	3.1 (1.1)	3.1 (1.2)	3.2 (0.9)	2.8 (1.4)	3.0 (1.0)	3.1 (1.3)
Number of covered schools	2.6 (0.6)	2.5 (0.6)	2.9 (0.5)	2.2 (0.4)	2.8 (0.6)	2.2 (0.4)
Walking distance to nearest metro station (km)	0.7 (0.3)	0.6 (0.3)	0.6 (0.3)	0.7 (0.2)	0.7 (0.5)	0.6 (0.3)
Walking distance to nearest transfer station	4.4 (2.2)	3.7 (1.9)	5.7 (1.9)	2.6 (1.1)	4.6 (2.1)	2.8 (1.1)
Distance to CBD (km)	8.1 (1.5)	9.7 (4.4)	8.7 (1.4)	7.1 (0.9)	11.0 (5.2)	8.5 (0.3)
Observations	818	616	497	321	298	318

Table 2. Descriptive results of transaction records in study area (Line 11)

	Full sample (sales)	Full sample (rental)	Treatment group of Line 11 (sales)	Control group of Line 11 (sales)	Treatment group of Line 11 (rental)	Control group of Line 11 (rental)
	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage	Mean (SD) /Percentage
Outcome						
Per price for sales (CNY/m^2)	48460.0 (15865.5)		59952.0 (15445.4)	44056.5 (13668.0)		
Per price for rental (CNY/m^2/month)		52.5 (14.1)			58.7 (13.7)	47.8 (12.6)
Ln per price	10.7 (0.4)	3.9 (0.3)	11.1 (0.3)	10.6 (0.4)	4.0 (0.2)	3.8 (0.3)
Covariates						
Number of bedrooms	2.5 (1.0)	2.7 (1.2)	2.8 (1.1)	2.4 (0.8)	3.1 (1.2)	2.4 (1.1)
Floor area (m^2)	78.3 (32.0)	88.5 (40.9)	92.9 (36.0)	72.6 (28.4)	109.1 (39.0)	72.8 (35.0)

Whether oriented to south (Yes=1)	64.6	74.0	72.9	61.4	81.9	68.0
Age of the housing (in years)	12.9 (6.0)	12.0 (5.9)	10.0 (4.1)	14.0 (6.2)	10.3 (3.7)	13.3 (6.8)
Number of total floor	20.6 (10.0)	21.3 (10.0)	23.5 (7.3)	19.6 (10.7)	24.5 (7.3)	18.9 (11.0)
Number of buildings in the estate	8.4 (7.7)	8.8 (8.1)	9.9 (9.1)	7.9 (7.1)	11.0 (9.4)	7.1 (6.5)
Floor area ratio	3.3 (1.2)	3.1 (1.3)	3.1 (1.1)	3.3 (1.3)	2.8 (1.0)	3.3 (1.4)
Number of covered schools	4.6 (1.9)	4.5 (1.9)	5.7 (2.0)	4.1 (1.7)	4.9 (2.1)	4.2 (1.6)
Walking distance to nearest metro station (km)	1.3 (0.9)	1.1 (0.9)	0.5 (0.3)	1.6 (0.8)	0.4 (0.3)	1.7 (0.9)
Walking distance to nearest transfer station	4.3 (2.6)	3.9 (2.5)	3.2 （1.4）	4.7 （2.8）	2.6 (1.5)	5.0 (2.7)
Distance to CBD (km)	22.1 (5.0)	22.5 (4.3)	22.3 (5.1)	22.0 (5.0)	22.0 （4.6）	22.9 (4.0)
Observations	4393	1500	1217	3176	648	852

4.2 Housing premiums of new metro lines

Table 3 shows the results of the hedonic DID model for housing premiums in treatment and control groups of Line 7 and 11. Note coefficient (β) differs from the estimated impact for interpreting dummy variable coefficients in a logarithmic equation, and the actual effect is calculated by $(e^{\beta} - 1)$. The key variable is the interaction term, which captures the housing value change of the treatment group (built route) relative to that of the control group (unbuilt route) due to intervention. Model 1 estimated the effect of line 7 on housing premiums in price. The coefficient of the interaction item was 0.084 after controlling for the covariates, indicating significant housing price premiums comparing treatment and control groups. Model 2 estimated the effect of metro line 7 on housing rent changes, and the coefficient indicated that the estimated rent premium is 20.0%, comparing treatment and control groups.

For Line 11, Model 3 shows that the effect of metro intervention on housing price premium of treatment groups were insignificant, relative to control groups. Model 4 shows treatment effects on housing rents for Line 11, and the coefficient indicates that rent premium is 18.4% comparing treatment and control groups. The findings show that only housing rents experienced a premium for treatment group in Line 11.

Table 3. Results of average treatment effects on housing premiums

	Model 1 DV: Housing price of Line 7	Model 2 DV: Housing rent of Line 7	Model 3 DV: Housing price of Line 11	Model 4 DV: Housing rent of Line 11

Variables	Coefficient (standard error)	Coefficient (standard error)	Coefficient (standard error)	Coefficient (standard error)
Treatment	-0.148*** (0.027)	-0.012 (0.035)	0.172*** (0.014)	0.014 (0.024)
Treatment x Post	0.057* (0.028)	0.090** (0.035)	-0.008 (0.013)	0.152 *** (0.021)
Post operation	0.363 (0.025)	-0.059 (0.069)	0.036* (0.014)	-0.069 * (0.030)
Constant	11.803*** (0.079)	-14.063 *** (4.635)	-26.839*** (1.337)	-30.270*** (2.313)
Covariates	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Observations	818	616	4393	1500
R-square	0.779	0.617	0.837	0.587
Adj. R-square	0.774	0.603	0.836	0.582

Note: (1) #p<0.10, *p<0.05, **p<0.01, ***p<0.001
(2) unit-level, locational, and amenities attributes were incorporated in the models

4.3 Spatial variations in treatment effects

Table 4 shows spatial variations in the treatment effects for Line 7 and 11. The key variables are three interaction terms, capturing the housing value premium of different bands in treatment groups to that of control ones. For Line 7, coefficients illustrate that the two outer rings (housing units between 200—600m and 600—1000m) show significant higher housing price premiums than the control group, while the inner ring (housing units within 200m) show no significant effects (model 5). Model 6 finds that rents in the three rings were 19.3% (0—200m), 21.7% (200—600), and 7.3% (600—1000m) higher than those in the control groups, respectively.

Regarding Line 11, the treatment effects show that housing prices closer to the station have lower housing premiums than those in the outer ring (Model 7). We discuss the possible reason in the next section. Model 8 illustrates that treatment effects of rental records in different bands were 25.9% (0—200m), 13.6% (200—600m), and 17.8% (600—1000m) higher than those in the control groups, respectively. Although the trend varied some with specific lines, we found an overall price gradient of housing rent premiums for both lines, while not on housing price.

Table 4. Spatial diffusion effects of housing premium

	Model 5 DV: Housing price of Line 7	Model 6 DV: Housing rent of Line 7	Model 7 DV: Housing price of Line 11	Model 8 DV: Housing rent of Line 11
Variables	Coefficient (standard error)	Coefficient (standard error)	Coefficient (standard error)	Coefficient (standard error)
Treatment (0—200m)	0.085 (0.090)	-0.115 (0.092)	0.250*** (0.040)	-0.033 (0.042)
Treatment (200—600m)	0.108* (0.046)	-0.003 (0.048)	0.279*** (0.022)	0.040 (0.035)
Treatment (600—1000m)	0.235 *** (0.036)	0.030 (0.040)	0.114 *** (0.015)	0.032 (0.027)
Treatment (0—200m) x Post	-0.069 (0.088)	0.137# (0.097)	-0.138*** (0.039)	0.218*** (0.038)
Treatment (200—600m) x Post	0.095** (0.084)	0.188*** (0.051)	-0.117*** (0.022)	0.086* (0.036)
Treatment (600—1000m) x Post	0.049# (0.031)	0.047 (0.040)	0.061*** (0.016)	0.153*** (0.027)
Post operation	-0.112*** (0.029)	-0.064 (0.068)	0.042** (0.014)	-0.069* (0.030)
Constant	-32.796*** (4.447)	-15.320 (4.690)	-27.572*** (1.341)	-30.974*** (2.347)
Covariates	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Observations	818	616	4393	1500
R-square	0.782	0.626	0.840	0.591
Adj. R-square	0.776	0.611	0.840	0.584

Note: (1) #p<0.10, *p<0.05, **p<0.01, ***p<0.001
(2) unit-level, locational, and amenities attributes were incorporated in the models

4.4 Parallel trend test

Treatment and control groups had a similar trend before 2016, especially one year before operation (see Appendix S3). The statistical results also confirmed that the hypothesised parallel trend holds. The coefficients on the interaction term were insignificant in all models, suggesting that housing units in the treatment and control groups had similar trends in per housing price between 2013 and 2016 (in the absence of the metro service). Therefore, the parallel trends assumption holds true in this study.

5. Conclusion and discussion

The importance of transport infrastructure, particularly rail transit system, have been highlighted in numerous contexts (Hurst & West, 2014). Meanwhile, policymakers were increasingly prudent to increasing costs for funding the projects. In the past a few decades, increasing studies investigated the economic, environmental, and social effects of transport investment (Ihlanfeldt, 2003; Kahn, 2007), while debates remain unsolved varying from the methodology to the adopted dataset. Therefore, providing causal inference of newly launched infrastructure was indispensable for financing, governing, and modifying relevant projects. In this paper, we assessed causal effects of new metro intervention on housing premiums in Shenzhen, China. Our research design took account of the decision-making process of line alignment to achieve as-if random treatment-control group assignment. Validation of as-if randomness is therefore by *a priori* logic as well as *ex-post facto* comparison of groups. The models showed varied causal effects of metro interventions. For Line 7, we found housing prices and rents increased significantly after the operation of the new metro, while we only observed increased housing rents for Line 11. In addition, housing rent premiums showed a price gradient to metro stations in both lines. Our parallel trend analysis confirmed that prices and rents in the treatment and control groups followed a similar trend before the metro lines were opened.

Our study showed that the design of natural experiments can be improved if we integrate planning knowledge about the intervention at the research design stage. The knowledge of engineering constraints and land exploitation potentials helped to assign treatment and control groups in an as-if random manner and reinforced the research design to present a natural experiment. As-if randomness of the treatment assignment meant that housing premiums measured through DID models are less likely to be distorted by confounders because this procedure should have created a balanced distribution of (un-)observable confounders between the treatment and control groups (Sun et al., 2022). Our parallel trend test also strengthened this argument. Natural experiments differed from other quasi-experimental research designs that rely on post-design statistical modelling approaches, which cannot exclude alternative explanations of housing premiums (Acton et al., 2022). We thus suggested future studies to improve research design and credibility of the evidence by incorporating local planning knowledge.

We provided causal evidence of a new metro line on housing sales and rental premiums. Housing prices and rents in treatment groups achieved higher premiums than in control groups along Line 7. The positive results were consistent with most longitudinal studies that highlight housing premium effects of urban rail transit (Diao et al., 2017). We noted that the premiums were higher than several light rail transit projects in North America and Australia, whose estimates ranged from 3.8%-11.3% (Billings, 2011; Melser, 2020; Pilgram & West, 2018). It may be attributed to the higher operation capacity and efficiency of metro lines in a rapidly developing megacity like Shenzhen, where we would anticipate a single line or station to create greater positive externalities. Put differently, our study is consistent with the assumption that a new metro line yields higher marginal, average, and total benefits in a high-density megacity. Likewise, we found a significantly higher premium in housing rents around Line 11 than in previous studies. However, housing prices in this line did not show a significantly higher premium compared to the control group along the unbuilt route, even though housing price rises were detected along Line 11 during the study period (but not significantly). A possible explanation was that the adjacent Line 1 has been in service since 2011, which may have created speculation and anticipation effects before the construction of Line 11 in 2012. In addition, the high housing supply resulting from numerous recently leased land parcels has made this area (built along the route of Lines 11) a hot spot for housing investment and has suppressed the premiums that metro access brought. This shows that as a complex intervention, the specific treatment effect of metro lines on local real estate development is context dependent.

We further validated housing rents as a measure of metro-induced accessibility benefits (Melser, 2020), a potentially more accurate metric than sale prices in rapidly growing cities like Shenzhen. The observed housing price premium is likely to be confounded by other factors during the entire metro intervention period (last about one decade), especially in Chinese cities with soaring housing prices and high housing ownership. On the contrary, housing rents were arguably a more reliable measure of accessibility premium based on a short span of dataset immediately before and after the new metro operation. Specifically, we found that housing rents experienced a higher premium than sales prices in the treatment group compared to controls, along both metro lines. Since renting housing is much more affordable than purchasing and the tenancy is flexible, housing rent premium is more related to actual housing utility. In Shenzhen, metro intervention and subsequent utility changes attract consumers in a tightly competitive rental housing market. In the dynamic process, tenants with lower bids will likely be replaced by others with stronger preferences for metro access. Eventually, consumers with higher budgets occupy rental housing with greater metro access. By contrast, people consider a wider range of comprehensive factors (e.g., education, environment) when purchasing housing, metro access being just one. Prices of speculative property may be less influenced by short-term utility improvements from metro access. In summary, our analysis of housing rents indicated that it should be considered in further studies to infer transport infrastructure's property value benefits.

The study has limitations. First, we did not have access to repeated sales or rents, which track the transactions of the same housing unit. Previous studies applied a propensity score matching to create a statistical pair in the treatment-control groups. However, it would have required complicated statistical hypotheses about the similarity of the pairs (Acton et al., 2022). Instead, our DID models accounted for the variance of the unit and estate covariates and time fixed effects, and the compositions (e.g., floor area, age of the housing) of housing transactions in different years showed similar patterns. Second, we only estimated average treatment effects from the construction to operation stages. We could analyse the anticipation effects between project announcement and construction beginning (before 2013) if having a better-established dataset in the future.

Our study also has notable strengths. We advanced causal inference of the complex effects of new metro lines on property prices in a rapidly growing city. Currently, there are no institutional schemes in place in China that can capture unearned externality benefits created by metro investment. China started considering a property tax law in 2021, which may provide a platform for metro value capture in the future. However, mechanisms for capturing premium created by metro infrastructure have not been specified in the tax calculation yet. Levying property tax to capture these windfalls could recover more of the costs of metro investment. Whether and when this happens will become crucial, and we recommend our method to those planning for such a mechanism. Meanwhile, a tax focusing on rent revenue has been implemented loosely in the rental market in China (Tang et al., 2011). Some rent premiums could be captured if housing is not put in the sales market in the long run. Our findings can support better institutional design of metro infrastructure financing.

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