

The Impact of Road Rationing on Housing Demand and Sorting

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April 3, 2023

Abstract

Canonical urban models postulate transportation cost as a key element in determining urban spatial structure. This paper examines how road rationing policies impact the spatial distribution of households around transit centers using rich micro data on housing transactions and resident demographics in Beijing. We find that Beijing's road rationing policy significantly increased the demand for housing near subway stations. The premium for proximity is stable in the periods prior to the driving restriction, but shifts significantly in the aftermath of the policy. The composition of households living close to subway stations shifts toward slightly wealthier households. Our findings suggest that city-wide road rationing policies can have the unintended consequence of limiting access to public transit for lower income individuals.

Keywords: road rationing, housing, sorting, urban structure

JEL Classification Codes: R21, R41

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1 Introduction

In an effort to combat congestion and air pollution, some of the world’s largest cities have adopted road rationing policies that restrict private vehicle travel in urban centers during peak hours.¹ Since Athens, Greece implemented the first of these travel restrictions in 1982, permanent adoption of road rationing policies have increased steadily over the past three decades. Several Latin American cities, including Mexico City, Bogotá, Colombia; São Paulo, Brazil; and Santiago, Chile implemented center-city driving bans in the late 1980s and early 1990s. More recently Oslo, Paris, and London have converted major city center streets to car-free zones. Today, at least 137 million people around the world live in cities with driving restrictions.

Previous research on urban driving restrictions has primarily focused on their first-order impacts on traffic congestion and air pollution with mixed findings.² We know significantly less about the effects of these policies on residential housing markets. Cities are a physical manifestation of people’s desire to minimize transportation costs. Thus, driving bans will likely affect not only congestion and pollution, but residential transportation costs and, therefore, location decisions. In this paper, we provide the first empirical analysis of how a city-wide driving restriction affects how people sort in space.

Our focus is on the price and sorting behavior of households near subway stations following a road rationing in Beijing, China. In anticipation of the 2008 Olympic Games, Beijing central planners implemented road rationing along with large investments in the subway network and transit fare subsidies in a layered approach to address air pollution and congestion. A major goal of these policies was to motivate travelers to choose transit over roadway vehicles and to provide a low-cost travel option for all classes of workers. While these initiatives did increase transit relative to car usage, our findings suggest that road rationing policies can have potentially regressive consequences on access to public transit stations through the housing market.

Our empirical approach is motivated by the robust finding in urban economics literature that higher transportation costs lead to greater competition for areas accessible to labor markets.³ In the context of Beijing’s 2008 road rationing policy, however, predicting how the policy might impact hous-

¹Over 80% of the world’s major cities fail to meet World Health Organization guidelines on air quality ([World Health Organization 2019](#)). Emissions from vehicles is a major contributor to urban air pollution.

²See for example [Davis \(2008\)](#), [Sun et al. \(2014\)](#), [Viard and Fu \(2015\)](#), [Carrillo et al. \(2016\)](#), [Kreindler \(2016\)](#), [Zhang et al. \(2017\)](#), and [Zhong et al. \(2017\)](#) for studies of driving restriction policies effects on air pollution and congestion.

³Early models of urban land use developed by [Alonso \(1964\)](#), [Muth \(1967\)](#), and [Mills \(1967\)](#) were the first to formalize connections between land prices and transport costs. However, more recent theoretical and empirical work that abstracts from monocentricity assumptions also demonstrate that transportation costs are a dominant factor in explaining urban land use patterns ([Glaeser et al., 2008](#); [Gaigné et al., 2022](#); [Tsivanidis, 2022](#)).

ing markets or income sorting is unclear for at least three reasons. First, Beijing is not a monocentric city, but has several major business districts, some of which overlap in their industrial and commercial composition. A polycentric structure could depress price effects that would theoretically arise from a driving restriction within a monocentric city. Second, while driving is the dominant mode choice for higher income groups, busing, walking and biking, as well as subway transit, are common travel mode choices across the income distribution. The degree to which road rationing creates competition for subway proximity depends, in part, upon the substitution behavior of car drivers. Third, at the same time that Beijing’s government implemented road rationing, they invested heavily in expanding the city’s subway network. This expansion could have offset some of the rationing policy’s effects by increasing transit accessibility throughout the city (Gin and Sonstelie, 1992; Ahlfeldt and Wendland, 2011). These nuanced aspects of Beijing’s urban environment invite an empirical approach to understand the impact of road rationing on the demand for proximity to transit centers.

We employ a difference-in-differences (DID) research design to examine how Beijing’s road rationing policy (hereinafter the “RRP”) impacted the price premium for proximity to subway stations as well as the income of residents occupying those proximate housing units. Our empirical approach proceeds in two steps. First, we estimate an hedonic regression where price is a function of distance to a subway station. We interpret the change in this “bid-rent” gradient before relative to after the RRP as a measure of the change in people’s demand for residential proximity (Rosen, 1974). Having examined changes to the bid-rent gradient, we then estimate the causal effect of the RRP on the propensity of high income groups to live closer to subway stations relative to low income groups who are less likely to own cars. Our unique data allow us to examine whether income-based sorting near stations is a mechanism that drives housing price appreciation in these areas.

A critical component of our empirical design is that we test for post-RRP changes in bid-rent gradients and income sorting using variation within neighborhoods as well as within building complexes of Beijing. These fixed effects control for time-invariant differences across locations that could otherwise confound spatial variation in prices and household income (such as proximity to natural amenities or high-performing schools). In addition, we include a comprehensive set of controls for housing characteristics so that our estimates are identified from similar properties that differ, observationally, only with respect to their distance to subway stations.

Despite the use of fine-scale fixed effects and detailed housing-attribute controls, our difference-in-differences approach may be confounded by at least two factors that occurred during our period

of study. As previously stated, Beijing’s subway network expanded considerably between 2000 and 2015.⁴ Additionally, Beijing’s housing values experienced substantial growth at a rate of 13.6% per year during this time period (U.S. Census 2019; Fang et al. 2016). If Beijing’s subway expansion and housing boom explain most of the change in housing prices near stations before relative to after the RRP, our estimates of the policy effect will be spurious and will likely over-estimate the true effect.

We address these identification threats in a variety of ways including an out-of-sample validation test, a placebo test, a triple-difference design, and an instrumental variable design. First, we rely on dynamic effects specifications of the hedonic and sorting response in order to test for the existence of pre-RRP trends. Our event study shows that the price premium for proximity to subway stations changed only after the RRP, suggesting that secular price appreciation in the housing market does not explain our results. We find some evidence of a sorting response in anticipation of the RRP that we believe is largely explained by an early announcement of the policy. Second, we limit our study sample to neighborhoods proximate only to older stations that pre-date the expansion, and show that our results hold even for these neighborhoods.

Third, we show that the neighborhoods where new stations were built during our study sample did not exhibit differential income growth trends in the two years leading up to the station opening date. We bolster this finding with an out-of-sample validation test and examine neighborhoods where subway stations were built several years after our study period. We find no differential trends in the bid-rent gradient directly following the RRP for houses in those “future subway” neighborhoods. We interpret these findings as evidence that the post-RRP shift in demand for subway proximity observed in our main sample were unlikely to be driven by secular investment or gentrification trends in areas that received new subway stations. Fourth, we conduct a placebo test using neighborhoods near major pollution sites and find no evidence that the RRP affected house prices or income sorting within these undesirable locations.

Fifth, we employ a triple-difference design that exploits subway station openings before and after the RRP. We show that only after the RRP does the opening of stations significantly increase house prices. Lastly, we instrument for the location of subway stations using their historic planned locations to address potential concern of non-random placement of subway stations. Our IV estimates are similar to those from our benchmark results.

Our analysis provides two key findings. First, the RRP increased the premium for proximity

⁴See Appendix Figure A1 for year-on-year expansions of Beijing’s subway network.

to subway stations. Our preferred estimates suggest that road rationing increased the price-distance gradient (i.e., the price premium for a housing unit one kilometer closer to a subway station) by about 6.5 percentage point (or ¥36,306 for the average housing unit). The baseline premium for proximity is stable in the periods prior to the RRP, but shifts significantly in the aftermath of the policy. By exploiting the most comprehensive data set yet compiled on the housing market and home-owner demographics in Beijing, our results imply that the Beijing bid-rent gradient for subway proximity was at least twice as responsive to the RRP as prior work by [Xu et al. \(2015\)](#) suggests. Our study further extends their work by showing that underlying household sorting and changes in the spatial distribution of residential wealth contributed to these price increases.

Second, we find modest but statistically significant evidence that the demographic composition of households near subway stations shifted toward higher earning households. Following the RRP, a household one kilometer from a subway earned ¥120 to 168 more per year relative to a household two kilometers from a station. This effect may appear to be small but constitutes a 19% increase from the pre-RRP income disparity across households that differ by one kilometer in their distance to a station. Data patterns suggest that these sorting results are driven by increased housing development and inflows of high income households in these new developments as opposed to the displacement of low income residents. Taken together, we interpret these findings as evidence that after the RRP, high income households were more likely than lower income households to move to transit accessible areas.

We contribute to prior literature in the following ways. First, we provide new causal evidence that higher travel costs lead to steeper bid-rent gradients and greater income sorting near transit-accessible areas when travel modes are income-stratified. Our findings suggest that workhorse urban land use models developed by [Alonso \(1964\)](#); [Muth \(1967\)](#); [Mills \(1967\)](#), and [LeRoy and Sonstelie \(1983\)](#) can provide relevant predictions on land use even when monocentricity assumptions are impractical. In general, our results underscore the importance of transit costs and transit regulation as determinants of urban spatial structure.

Second, our study imparts somewhat distinct findings on a large body of literature concerned with the local effects of public transit ([Kahn, 2007](#); [Billings, 2011](#); [Heilmann, 2018](#); [Tyndall, 2021](#); [Severen, 2021](#); [Ubeda, 2021](#); [Gagné et al., 2022](#)). Much of this work uses variation in the expansion of rail systems to show that access to light rail or subways capitalizes positively into house prices. We find similar patterns in the context of Beijing, wherein housing closer to stations command a higher price per square meter on average. However, our analysis on *new* station openings suggests that positive

capitalization effects from subway access occurred only *after* Beijing’s RRP. In the years prior to the RRP, we do not find strong evidence that gaining proximity to new subway stations increased house prices. This finding suggest that price appreciation following the RRP was largely a result of the increased demand for transit access as opposed to demand for amenities or commercial development in Beijing (Billings, 2011; Heilmann, 2018; Tyndall, 2021).

One possible reason for the difference in the price response to new stations is that, with the exception of Gaigné et al. (2022), the aforementioned studies are focused on US cities where rates of transit use are substantially lower than that of Beijing. Slightly less than 6% of US workers in metro areas of the US commuted by public transit as of 2015, whereas 17.2% of workers in Beijing used either bus or subway to travel as of 2014 according to the 2014 Beijing Household Travel Survey. Thus, expansion of transit in US cities may constitute a large, discrete change with unique impacts on housing markets and sorting compared to transit investments in Chinese cities (Sieg et al., 2004).

Prior findings on the income sorting effects of rail transit are more mixed. Gaigné et al. (2022) find urban areas in the Netherlands with greater access to transit had higher income residents. Heilmann (2018) finds that light rail expansion in Dallas-Fort Worth had a polarizing effect, increasing incomes in already-high income areas and depressing income in high-poverty areas. Glaeser et al. (2008) provide suggestive evidence that expanding public transit increased poverty rates locally. However, their study also finds a positive correlation between income and proximity to subway stations in transit-oriented “subway cities.” Tyndall (2021) does not directly explore income, but finds a positive impact of light rail expansion on local labor force participation.

While our analysis does not speak to the direct effects of rail transit on income sorting, we find evidence of income sorting near stations as a consequence of a city-wide road rationing policy. In light of the prior literature, our findings underscore that the degree of income stratification across travel modes matters for predicting income sorting outcomes of transit policies. A defining feature of Beijing’s RRP was that it increased the propensity of car driving households to utilize travel modes formerly dominated by lower-income households. This marginal homogenizing of travel mode choice helped accelerate price appreciation and competition for housing near subway stations. In absence of such a unifying force, it is unclear how income sorting may respond to expansion of subway or light rail systems.

Our study focuses on proximity to subway stations in order to understand how changes in travel costs impact demand for travel accessibility. However, a growing body of work uses more

generalized forms of travel time or measures of job market access to measure changes in demand for travel accessibility (Severen, 2021; Tsivanidis, 2022; Barwick et al., 2022). The advantage of focusing on subway station proximity is that our estimates measure the overall effect of a driving restriction, inclusive of impacts on both transit and non-transit users, and does not require detailed data on commute flows or employment concentration throughout a city, which can be difficult to obtain for developing or middle-income countries. The disadvantage is that our approach cannot account for general equilibrium effects nor disentangle effects driven by transit versus non-transit users. To provide supporting evidence on the findings in the housing market, we use aggregate as well as travel diary data from Beijing in Section 2.3 to explore changes in travel mode choices during our period of study. We find evidence that subway usage increased relative to car usage following the RRP, suggesting that commuting margins explain at least some of our price and sorting results.

This article proceeds as follows. In the next Section, we provide institutional context to Beijing’s driving restriction policy and we discuss our data sources. Section 3 provides a simple theoretical framework to motivate our empirical approach and explores patterns in the data. We explain our empirical approach as well as threats to identification in Section 4 before reporting results in Section 5. In the results section, we conduct several robustness checks and placebo analyses, and explore mechanisms behind our sorting results. Section 6 concludes.

2 Background and Data Description

In this section, we provide background information on Beijing’s road rationing and describe patterns in travel mode choices over time and across income classes. The data show that use of subway and other forms of transit increased overtime in Beijing relative to cars, despite the fact that the total number of registered cars within Beijing grew substantially over our study period. In addition, our analysis on travel model choices based on detailed travel dairy data illustrates that the road rationing policy led to travel modal shift from using cars to public transit. These facts help motivate our empirical findings that the RRP increased competition for housing near subway stations. Lastly, we discuss our data sources and sampling restrictions.

2.1 Road Rationing in Beijing

Beijing’s RRP first went into effect in the summer of 2008 and is still enforced today with some changes. Initially, the policy allowed drivers use of their car only every other day based on whether the last number on their license plate was even or odd, and the restriction extended through the

entire city of Beijing including the suburbs. In Fall of 2008, the government relaxed some of these restrictions, most notably by allowing individuals to freely drive their car four out of five weekdays per week. Buses, taxis, and public-use vehicles for the police and military were not affected by the restriction. Appendix B provides more detail on the policy and its evolution over time.

Because July 20, 2008 was the start of Beijing’s perpetual restriction on driving, we assign this date as the start of the RRP. However, behavioral responses to the policy could have started in different points in time, for example, as early as the Fall 2007 trial period, later in Fall 2008 once the policy was renewed following the Olympic Games, or even later if people did not initially expect the policy to be permanent. For all empirical analyses, we rely on event studies in order to be agnostic as to the effective start date of the RRP.

In Beijing, it is difficult to violate one’s restricted day without penalty because cameras throughout the city monitor the plates on vehicles. If an individual violates the restriction, they are fined ¥300 (or about \$40) per violation. These fines repeat every three hours, allowing some grace period for drivers to leave the roadway if they drive on a restricted day. Additionally, Beijing restricts people’s ability to purchase a second car, and purchase of a first car is regulated by a lottery system. Consequently, during our period of study there was limited scope for noncompliance or substituting to a second car, as Davis (2008) found for the case of Mexico City’s driving restriction.

2.2 Trends in Travel Mode Choices by Income in Beijing

Transit mode choice is stratified by income in Beijing. Appendix Figure A2 depicts the distribution of mode choice by income group based on travel data from the 2010 Beijing Household Travel Survey (Beijing Transport Institute 2010). Nearly 40% of trips among survey respondents in the top income bracket relied on a car, while less than 10% of all trips among respondents in the bottom income brackets relied on a car. In contrast, for public transit, just 14% of trips among respondents in the top income bracket relied on public transit (subway or bus), whereas 20-21% of trips among people in the bottom income bracket involved a subway or bus. Walking is a prominent mode of travel across all income groups, particularly the lowest earning group. The findings suggest that the direct effect of the RRP on travel mode choices would be mainly on wealthier households.

2.3 Impact of RRP on Travel Mode Choices

Is there evidence that the RRP reduced the use of cars and increased the use of public transit? Some studies have documented improvements to Beijing’s air quality following the RRP (Viard and Fu,

2015; Chen et al., 2013; Lu, 2016; Li et al., 2019), however, direct evidence on the policy’s impacts on travel mode choice is limited. We do not have access to trip-level data prior to the RRP that could be used as a comparison for post-RRP commute patterns. However, we combine aggregate data spanning 2001 through 2015, as well as trip-level micro data from 2010 to better understand how the RRP impacted travel behavior.

City-wide aggregate data from the Beijing Transport Annual Reports shows that the share of trips on subways grew faster than all other trip mode shares between 2005 and 2013. These trends are summarized in Panel A of Figure 1. The absolute number of subway trips increased considerably starting in 2008 relative to the pre-RRP years while the average daily use of cars per household actually fell after 2007 (Panel B). Average daily car trips increased after 2010, but did not ever recover to pre-RRP levels as of the end of the data series in 2015. We also find that, even though overall ownership of cars increased year-on-year within Beijing, the intensity of car use—measured as the share of all trips in each year—stayed relatively flat over this time period (Panel C). Collectively, these figures demonstrate that subway use increased after the RRP, while car use stayed relatively steady despite a substantial increase in car ownership.

To supplement analysis of these aggregate trends, we use dis-aggregated trip diary data from the 2010 Beijing Household Travel Survey to test whether car owners were more likely to use public transit during restricted driving hours. The data were collected between September and October of 2010 and the trip diary data include a complete record of all trips conducted by over 162,000 people over a 24-hour period prior to the time of the survey (approximately 253,600 trips). Our analysis focuses on 86,989 trips by travelers from car-owning households. The restriction applies to vehicles (instead of individuals or households) and a vehicle is restricted from driving during the restricted hour one day per week during workdays. The last digit of the licence plate number is used to determine the workday when the vehicle is restricted. A vehicle’s restricted day rotates every four weeks according to a pre-set schedule (Yang et al., 2020). We exploit the fine temporal scale of these data to compare travelers’ mode choices during restricted hours (7am-8pm) relative to non-restricted hours on restricted days relative to non-restricted days.

Following a DID design, Appendix Table A1 presents the shares of trips using cars in Panel A, and the shares of trips using bus or subway in Panel B among different groups. For a given trip, the restricted day is defined as a day when a vehicle that a household owns is restricted following the rotation schedule. The restriction would limit the ability of the traveler to use that particular car

being restricted, but the trip can still be carried out using a car (e.g., another car in the household or carpooling) and hence registered in the survey data as a car trip. The comparison in Panel A shows that when a car is restricted from driving (during 7am-8pm) on a given day, the trips taken by all the members in the household that owns that car are 7.3 percentage points less likely to be a car trip. This represents a 20% reduction in the share of car trips among all trips by car owners.⁵ The comparison in Panel B shows that the trips using public transit (bus or subway) increased 6.6 percentage points, suggesting that over 90% of the “reduced” car trips were shifted to public transit.

Appendix Figure A3 presents similar findings from a multinomial logit model where the choice set includes five modes: using a car, riding a bus, riding a subway, walking, and others (such as biking, taxi, motorcycle, school bus, and company van). The results show that during restricted hours on restricted days, car owners reduced the car trips by 7 percentage points, but increased the usage of bus, subway, and other modes by 4.6, 1.5, and 1.2 percentage points, respectively, compared to restricted hours on their non-restricted days. There does not appear to be switching from car trips to walking. This is intuitive as the average distance of a car trip is 13km.

While a large share of trips were conducted using other modes than riding a car or subway, the analysis based on the household travel survey does suggest that road rationing policy has impacted the travel mode choices: car trips were reduced, and most of those trips were carried out by public transit instead. The modal shift induced by the policy could in turn imply changes in locational preferences for housing, thus providing a micro-foundation for our housing analysis below.

2.4 Housing Data & Description

Our empirical analysis requires information on housing prices, residential locations, and household income. We assembled this information from two separate data sets. The first data set consists of individual real estate transactions, including sales of new as well as resale properties. Records on the sale of new properties are compiled from home registration records from the Beijing Municipal Commission of Housing and Urban-Rural Development. Records on the resale properties are sourced from Lianjia, the largest real estate brokerage company in Beijing. Together, the resale and new sale transactions account for approximately 45% of all housing purchases during our period of study.⁶

⁵The reduction of car trips is not 100% due to at least the following three reasons: (1) some of the trips could be carried out with another car among multi-vehicle owners; (2) some of the trips could be carried out with car pooling or a borrowed car; and (3) non-compliance. The survey does not ask which car was used for a given car trip so we cannot discern the frequency of these three cases. However, Viard and Fu (2015) find a high compliance of the RRP policy based on parking garage entrance records.

⁶Total volume of transactions in Beijing from 2006 through 2011 was 1,286,269. Before sampling restrictions, our real estate data covers 590,599 transactions over the same time period.

These real estate transactions data contain the latitude and longitude, the sale price, descriptive information on the housing unit (including number of bedrooms, floor level, decoration level, types of appliances, etc.), and information on the housing complex (including geographic location, total size, parking availability, green space, proximity to key schools, etc.). The final data set includes 252,426 transactions from 2005 through 2011.⁷

The second data set consists of mortgage contracts sourced from an anonymous government-backed loan program.⁸ The mortgage data cover the universe of all home purchases in Beijing that made use of this government-backed program from 2005 to 2011. These data provide detailed information on the loan applicant’s demographic characteristics, including their income, education level, and place of employment, as well as the mortgaged property’s sale price. Unlike the first data set, our mortgage data allow us to examine the impact of the policy on location sorting by income groups. Mortgage refinancing is uncommon in China, and there are no refinancing observations in the sample. As a result, each mortgage contract represents a unique home purchase. The mortgage data exclude real estate purchases made by non-local investors because only those who live and work in Beijing are eligible for the government-backed program. The final sample includes 46,471 mortgage contracts.⁹ While our mortgage dataset encompasses the majority of mortgage loans made in Beijing during the study period, the precise market share is difficult to quantify because data on the total number of mortgage loans made to individuals not participating in the government-backed loan program (e.g., individuals at non-salaried jobs, individuals working part-time or unemployed, or the very wealthy who do not need a mortgage to purchase a home) are not publicly available.

Table 1 provides summary statistics on a subset of the variables available within each of our data sets. The mortgage data housing units are further from the city center and further from subway stations, on average. The house purchase price varies considerably more in the real estate data than in the mortgage data set. Smaller price variation in the mortgage data set is due to the selection of individuals represented by the mortgage data, which over-represent the middle and upper class

⁷We remove transactions with missing or zero reported price, missing unit size, price per square meter below the 5th and above the 95th percentile, addresses outside the fifth ring road, missing complex ID or jiedao information, or unit size above the 99th percentile.

⁸The anonymous mortgage program operates as a government-backed credit provider to encourage home ownership. At participating firms, each employee and his/her employer have to contribute a specific percentage of his/her monthly salary to the government-backed mortgage account. The employees can then obtain a mortgage loan with a subsidized interest rate for home purchase, which is about 1.5 percentage points, or nearly 30%, lower than the commercial mortgage rate. Virtually all eligible home buyers apply for this mortgage first before going to other sources of funding (Tang and Coulson, 2017).

⁹We remove mortgage contracts where applicants report working but also report zero income, missing work years, commute distances above the 99th percentile or below the 1st percentile, and household income above the 99th percentile.

demographic; home buyers able to utilize the government-backed mortgage loan system are less likely to be entrepreneurs or extremely wealthy households.

Spatial Variation—Our sample of housing units are concentrated near the city center and near subway lines of Beijing, as shown in Panel A of Figure 2. The city is divided into 18 districts, each of which contains several smaller neighborhoods. The real estate transaction data identifies the “jiedao” of a housing unit, whereas the mortgage loan application data identifies the zip code of a housing unit. Jiedao are administrative units similar to a census tract, whereas zip codes are used for mailing addresses. They are similar in size: each district of Beijing contains an average of 35 jiedao and 32 zip codes. The median area of a jiedao in our sample is approximately 1.65 square miles. Due to the confidential nature of our data, we were unable to obtain a zip code shapefile, and thus were unable to assign each zip code to a jiedao. To reduce any measurement error in assigning a housing unit to its appropriate neighborhood, we conduct our analyses using the administrative unit provided by each data set. Throughout this paper, we refer to jiedao and zip codes collectively as “neighborhoods,” however our estimation strategies define unique fixed effects for each level of geographic organization.

Our identification strategy exploits variation in the house price and income gradient before and after the RRP within a neighborhood of Beijing. Panel B of Figure 2 demonstrates our within-neighborhood variation. The jiedao are outlined by the thick black lines. Among our data sample, an average of 802 transactions occurred within each jiedao while an average of 194 mortgage loan applications occurred within each zip code. We exclude housing units outside of the fifth ring road because these areas were not subject to the RRP after September of 2008. This restriction skews the sample toward those in smaller neighborhoods in size. Our sample covers roughly two-thirds of the 300 jiedao within Beijing and over 90% of the 200 zip codes. This restriction skews the sample toward those in smaller neighborhoods in size. Our sample covers roughly two-thirds of the 300 jiedao within Beijing and over 90% of the 200 zip codes. This restriction skews the sample toward those in smaller neighborhoods in size. Our sample covers roughly two-thirds of the 300 jiedao within Beijing and over 90% of the 200 zip codes.

Data Suitability & Representativeness—We assess the representativeness of our data by comparing our sample of real estate prices, transaction volume, and household income to Beijing-wide averages. While our transaction data account for less than half of all transactions made within Beijing over our period of study - approximately 45% between 2006 through 2011 - these data track closely with Beijing-wide house price trends. Appendix Figure A4 compares our real estate transaction data set

with un-adjusted as well as quality-adjusted population averages of house price per square meter over time within Beijing. Price trends from our data sample follow the population average, although our unadjusted price per square meter grows significantly more than the market average after 2010. After controlling for housing attributes like floor level and size, our data tracks closely with the population average.

The average household in our data set has income in the top 40% to 20% of Beijing’s household earnings distribution and the average income in our data set grows over time (as shown in Panel C of Appendix Figure A4). These trends are indicative of the fact that the average property owner in Beijing is significantly more wealthy than the average Beijing resident; and that wealthier households transact real estate more frequently than poorer households. While our purpose in using these mortgage data is to test income-based sorting responses to Beijing’s driving restriction, we replicate our bid-rent analysis using this data set’s property sale price information and find results that are similar to those estimated from the larger real estate transaction data set.

The year-on-year frequency of transactions in our real estate sample correlates closely with Beijing’s aggregate housing market, as shown in Appendix Figure A5. The housing market contracted during the 2008 recession, but rebounded in 2009 following a series of government-backed policies that lowered mortgage interest rates and down payment requirements. In April of 2010, Beijing’s local government implemented housing market cooling measures to avoid a housing bubble. These anti-inflationary policies remained in place through 2014, which is why the transaction volume never again reached its 2009 level. The Beijing government enacted several city-wide policies after 2011 that likely impacted the housing market and demand for automobiles. For example, in January 2012 the government implemented a lottery system for purchases of automobile license plates in order to limit the total vehicle fleet on Beijing’s roadways, while in April of 2011, the government enacted an anti-speculative policy that restricted home purchases for natives, and prohibited home purchases for non-natives of Beijing.¹⁰ For this reason, we focus our analysis on the years between 2005 and 2011 to mitigate the impacts of confounding policies.

3 A Framework of Urban Land Use & Preliminary Evidence

Our empirical work examines how house prices and household income evolve in proximity to subway stations following a city-wide road rationing policy. A straightforward examination of bid-rent

¹⁰See Lu (2018) for an analysis of the license plate restriction on Beijing’s housing market. See Sun et al. (2017) for analysis of the home purchase restriction on Beijing’s housing market.

gradients offers several insights about the empirical patterns that we document below.

Stylized models of urban land use characterize locations within a city as distinguished only by their access to employment and commercial centers.¹¹ In this simplified setting, a spatial equilibrium is achieved when all city residents have chosen their optimal house location by trading off travel costs with housing costs. As a result, house prices per unit decrease with distance from an employment or commercial center. If residents are able to access employment and commercial centers quickly by transit, then a similar bid-rent gradient can emerge proximate to transit centers themselves (Severen, 2021).

Groups with different preferences for housing consumption and time costs will have distinct bid-rent gradients. Figure 3 shows land use equilibrium with heterogeneous income groups. For exposition purposes, we follow LeRoy and Sonstelie (1983) and show a case where a high-income group possessing greater opportunity cost of time commute via car. Cars are a more expensive, but faster transit mode relative to public transit. The poorer group will be willing to pay more per unit of housing closer to subway stations because cars are difficult to afford, so they prefer to commute via transit. Consequently, they have a steeper bid-rent gradient than the rich and live, on average, closer to stations in smaller housing units.

Beijing’s RRP would hypothetically change this equilibrium by increasing travel time costs for the rich car-users. Because driving a car would become costlier during restricted days (violators must pay a fine), the optimal trade-off between travel costs and housing costs changes among car users. Consequently, higher-income individuals will find housing close to subway stations more attractive, and will outbid some of the poorer households for proximate locations. Figure 3 shows this change graphically: the gradient of richer households becomes steeper, represented by movement from *Rich*⁰ to *Rich*¹. This, in turn, causes an increase in the per-unit house price and a decrease in the average distance to the subway station among wealthier households.

Notably, several dimensions of Beijing’s RRP complicate the highly stylized predictions of this model. As discussed previously, commuters—particularly wealthy commuters—may have several options to circumvent a driving restriction other than moving closer to subway transit, including walking, busing, carpooling or asking for more flexible work-from-home arrangements from their employers. Furthermore, the overall intensity of Beijing’s subway expansion may create non-marginal changes in transit access that depress any price impacts predicted by the stylized model. Alternatively, subway

¹¹See McMillen (2006) for a literature review of the monocentric city model of urban land use.

stations, themselves, may carry significant dis-amenities like congestion or crime that repel marginal buyers. These ambiguities in potential outcomes of the RRP highlight the importance of using an empirical approach and adequately controlling for local characteristics and city-wide trends that might otherwise impact housing demand.

We show preliminary evidence from our raw data that the bid-rent and income gradient for subway proximity shifted following the RRP. Panel A in Figure 4 shows that the price premium for subway access clearly increased following the RRP. Panel B shows a similar pattern for the income-distance to subway gradient: the relationship between income and proximity became stronger after the RRP as lower-income households were less likely to live close to stations relative to high-income households. The level differences across pre- relative to post-restriction gradients is a result of house price appreciation and income growth of home-buying households over our period of study. Averaging all locations, house prices were approximately 80% higher after 2008 relative to before 2008, consistent with the price and income growth trends discussed in Section 2.

4 Empirical Approach

The goal of our empirical approach is to estimate how Beijing’s RRP affected, first, the premium for proximity to subway stations and, second, the spatial distribution of households by income. That is, we are interested in the *change* in the price gradient and the income gradient with respect to subway proximity as a result of the RRP policy. We use a difference-in-differences estimator to establish the causal effects of the RRP on the gradient changes. We present results, both, as event studies to test for evidence of pre-trends as well as mean estimates. The event studies cover seven years from 2005 through the end of 2011, while our mean estimates focus on transactions two years before and two years after the RRP, from July 2006 to July 2010. We choose this time window to strike a balance between having enough power to identify price and sorting effects through a seasonally cyclical housing market, while mitigating spurious correlations from other confounding policies that occurred after 2010.

4.1 Hedonic Analysis

We first estimate an event study model that relates house prices to proximity to subway stations over time. Our baseline econometric model is the following:

$$\ln(p_{ijt}) = \sum_{q=1}^{28} \kappa_q (Km_{it} \times D_q) + \alpha Km_{it} + \mathbf{x}_{ijt}'\theta + \gamma_j + \tau_t + \varepsilon_{ijt} \quad (1)$$

where p_{ijt} is the price of housing per square meter for housing unit i in jiedao j transacted on date t . Km_{it} measures the distance of housing unit i to the nearest subway station on date t and D_q is

an indicator variable for each of the 28 quarters from 2005 through 2011. The empirical quantity of our interest is the change (in %) in the price gradient with respect to subway proximity before vs. after the policy, and this can be defined as: $\phi = 100 * E(\frac{\partial \ln p}{\partial \text{km}})|_{\text{post-RRP}} - 100 * E(\frac{\partial \ln p}{\partial \text{km}})|_{\text{pre-RRP}}$. The event study coefficient κ_q , captures difference in the housing price gradient in quarter q relative to the quarter prior to the RRP (April - June of 2008, or quarter 14).¹² \mathbf{x}_{ijt} is a vector of controls for housing unit attributes including size, floor-to-area ratio, floor level, number of bedrooms, decoration level, whether on the top floor; and building complex attributes including size, green space, parking and maintenance fees, total floors, and number of buildings and units within the complex. We also include fixed effects for the subway line of the nearest station to control for connectivity differences across lines.

γ_j is jiedao time fixed effects to control for confounding unobserved differences in amenities across neighborhoods, such as the existence of high quality shops in the Xidan district or proximity to premiere universities in the Haidian district. In some specifications, we include more comprehensive building complex fixed effects. While building fixed effects reduce the amount of variation available for identification, their inclusion best approximates a “repeat-sales” approach (McMillen, 2003), and removes any time invariant differences across housing locations that may be correlated with subway proximity. Finally, τ_t is year and month fixed effects. Our preferred specification further includes district-specific linear time trends to control for differences in housing market growth over time across districts due to changes in amenities, such as the development of the Olympic Park in the Chaoyang District.

The parameters κ_q are identified off of cross-sectional as well as temporal variation in subway proximity across housing units. Because Beijing’s subway system expanded during our time period of interest, some housing units became closer to their nearest subway station over time. In Section 5.3, we conduct several robustness checks in order to account for potential endogeneity of subway development, including excluding housing units that change in their distance to the nearest station, an instrumental variable approach, as well as a triple-differences design.

¹²The effect of the policy on the price premium of subway proximity ϕ can be consistently estimated using $100 * \left(\exp[\hat{\kappa} - \widehat{\text{var}}(\kappa)/2] - 1 \right)$. The second component in the bracket reduces the finite-sample bias (Kennedy, 1981). We have used this formula to translate κ into the estimate of the policy impact throughout the paper. When κ is small (e.g., less than 0.1), it provides a good approximation of ϕ .

4.2 Income Sorting Estimation Framework

We next explore how the RRP affected household sorting manifested through the changes in the average income of households living near subway stations. To carry out this analysis, we employ data on household income, demographics, and location from the mortgage application data. Our event study specification is as follows:

$$y_{izt} = \sum_{q=1}^{28} \alpha_q (Km_{izt} \times D_q) + \psi Km_{it} + \mathbf{w}'_{izt} \theta + \zeta_z + \tau_t + \mu_{izt} \quad (2)$$

where y_{izt} is the monthly income for the primary and secondary earners in household i located in zip code z at time t . As with Equation (1), Km_{it} measures kilometers to the nearest subway station and D_q is an indicator variable for each of the 28 quarters from 2005 through 2011. Parameters α_q capture the change in the income gradient with respect to subway proximity in quarter q relative to the quarter prior to the RRP, quarter 14.

We use similar controls in Equation (2) to those in Equation (1) with a few exceptions. First, instead of housing unit characteristics \mathbf{x} , we include controls for demographics of the primary and secondary earners of the household denoted by vector \mathbf{w} , including age, education level, work tenure, employer industry type, and employee title. These demographic controls remove potential sorting responses due to unobserved changes to labor demand or changes to the location of employers. Second, we utilize zip code fixed effects ζ_z instead of jiedao fixed effects. As discussed in Section 2, data limitations prevented us from creating a spatial correspondence between zip codes and jiedao, however, they both delineate neighborhoods of Beijing. We include building complex fixed effects in some specifications to more rigorously control for unobserved differences across housing locations. As before, Equation (2) includes subway line fixed effects.

4.3 Identification Challenges

The underlying identification assumption in our empirical framework above is that prices (and income) would have trended in parallel for observationally similar housing units within the same neighborhood in the absence of the RRP. There are two main threats to identification. First, unobservables could have changed at the time of the policy and also affected the outcomes of interest (prices or income). For instance, if dining, shopping, and entertainment establishments grew over time in the same areas as subway stations, then bid-rent gradients could have become steeper over time due to growth in these amenities, as opposed to the RRP. Other possibilities are that the price premium for subway proximity could have changed as a consequence of other city-wide policies, like improvements in the overall

subway network, improvements in air quality (Viard and Fu 2015; Li et al. 2019), or improvements in traffic congestion during our period of study.¹³ If any of these correlated time-varying trends capitalized positively into housing prices, an ordinary least squares (OLS) estimate of κ_q in Equation (1) would over-estimate the effect of the RRP. Similarly, if any of these time-varying trends disproportionately attracted higher income individuals, then the composition of income near subways would have increased regardless of the RRP. Thus, OLS would not provide a consistent estimate for α_q in Equation (2).

We explore whether pre-existing growth dynamics near subway stations might influence our estimates by examining trends in the house price bid-rent gradients and income sorting in quarters leading up to the policy. If the difference in price or income between proximate and far housing units was stable in periods leading up to the RRP, it is likely that such differences would have remained stable in the absence of the RRP.

We further address this identification threat by adding more granular controls to our main specification (including district-specific linear time trends and building complex fixed effects) and through several robustness checks including a triple-difference design that relaxes the parallel trends assumption in the DID design discuss above.

The second main threat to identification is reverse causality. That is, proximity to subways could have changed *as a consequence of* house price or income growth over our time period. If subway stations were sited in areas where house prices were expected to increase, or areas where the average income of residents was expected to increase, we would not be able to interpret our results as consequences of the RRP. Li et al. (2016) argue that transportation planning in Beijing is largely disjoint from development and land-use planning. Nonetheless, we empirically test for possible siting endogeneity in several ways, including testing whether subway stations opened in gentrifying neighborhoods, re-estimating our hedonic analyses excluding areas of the city where new subway stations opened, and testing for price growth trends in areas that received subway stations outside of our study period. Each of these robustness checks are described in greater detail in Section 5.3.

¹³To the extent that changes to congestion or air quality are location specific, we believe that these changes unlikely present serious concerns for our empirical design. First, the city-wide traffic condition did not improve after the RRP: morning rush hour traffic speed within the fifth ring Road increased slightly by 1.5% between 2008 and 2009 (Beijing Transport Institute 2010). In fact, the Beijing Municipal government began to cap new vehicle purchases in 2011 as an additional strategy to address serious traffic congestion. Any improvements in traffic congestion in some neighborhoods if exist following the RRP would unlikely be salient enough to impact house prices or sorting. Second, it is unlikely that air quality changes would have factored into housing purchasing decisions during our time period of study when information on and awareness of air pollution was quite limited among the general public. Both Barwick et al. (2019) and Rao (2022) find no relationship between house prices and air quality in Beijing prior to 2013, after which the government began to systematically monitor and disclose pollution information.

5 Empirical Results

We now present evidence on the connection between housing demand and road rationing in Beijing. Our results generally follow predictions of our stylized model: the change in travel costs caused by the RRP shifted the house price bid-rent gradients as well as income gradients with respect to subway stations. Specifically, price premiums for subway proximity increased following the RRP and average household income marginally increased at locations closer to stations. Throughout this section, we display estimates in terms of the house price or income “discount” for distance. The absolute value of this discount can be equivalently interpreted as the house price or income “premium” for proximity. We end this section with several robustness checks and discuss mechanisms behind the sorting results.

5.1 Road Rationing and the Price Premium for Subway Proximity

Panel A of Figure 5 shows estimation results of Equation (1). Each dot shows the difference in the price discount per kilometer to the nearest subway station in a given quarter relative to the quarter prior to the RRP (April-June of 2008). Prior to the policy, the price discount for distance from a station (or price premium for subway proximity) is not significantly different from the omitted quarter. However, starting in the first quarter following the RRP, the discount for subway distance (premium for subway proximity) decreased (increased) by approximately 6 percentage point per kilometer relative to the quarter prior to the policy. Around the second quarter of 2011, there is an additional uptick on the price premium for subway proximity corresponding to vehicle purchase restrictions unexpectedly started in 2011 .

The DID estimates in Table 2 reaffirm that the price gradient with respect to subway distance became steeper following the RRP. Each column adds successively more demanding controls: column (2) includes jiedao and subway line fixed effects to control for unobserved neighborhood amenities and subway network expansion; column (3) includes characteristics of the housing unit; columns (4) include district-specific time trends to control for differential trends in housing prices across areas in Beijing; columns (5) further adds building complex fixed effects to control for amenities at a very granular level. Building complex fixed effects absorb jiedao fixed effects and subway line fixed effects.

Across the five specifications, the interacted term between the policy dummy and subway distance shows that after the RRP, housing units one kilometer further from a subway station sold for 4.6 to 7.8 percentage points (or ¥24,791 to ¥43,006) more than a comparable housing unit one kilometer further away from a subway station. The effect of subway distance on price itself before the RRP policy is not robust across specifications and imprecisely estimated after including more

controls. In particular, because all the housing units in the same building complex have the same distance to subway, the identification of the distance coefficient in column (5) with building complex fixed effects relies only on limited time-series variation in the distance to subway when the subway network expands. All specifications suggest a robust finding that the price gradient with respect to subway proximity became steeper, implying increased demand for subway proximity post-RRP.

As discussed above, the focus of this paper is to identify the *change* in the price gradient but not the level of the gradient. The DID strategy lends itself well in estimating the gradient change (captured by the interaction term) even if the level of the gradient is not well identified as in our case. The coefficient estimates on the distance variable itself in Table 2 capture the level of the price gradient pre-policy. While the coefficient estimates are intuitively signed and statistically significant in columns (1) and (2), the estimates become statistically insignificant and even counter-intuitive after introducing detailed controls and finer spatial fixed effects.

It is important to note that the identification of the level of the price gradient, and the *change* in the price gradient relies on different identifying assumptions. The identification of the level of the gradient would need to rely on the variation in subway distance that is exogenous to local unobservables (e.g., neighborhood amenities). The identification of the *change* in the gradient, captured by the coefficient on DID terms, would need to rely on the exogenous variation in the policy: the key identification assumption is that there are no other contemporaneous shocks that affect the price gradient (i.e, the slope of the price-distance relationship). In another word, even if the subway distance variable is endogenous, we can still identify the change in the gradient under the aforementioned assumption. Intuitively, if we were to separately estimate the price gradient using data before and after the treatment, the endogeneity (e.g., due to unobserved amenities) in subway distance would lead to inconsistency in both estimates. However, if the nature of the endogeneity is not affected by the RRP policy, the inconsistency in the slope estimates could cancel out, leaving the *change* in the gradient consistently estimated from OLS.¹⁴

The previous analysis assumes a linear price-distance relationship and the same impact of the policy on the price gradient across distance bins. Several prior studies have found non-linearities in the demand for transit access by distance (Kahn, 2007; Bowes and Ihlanfeldt, 2001; Severen, 2021). Panel A of Figure 6 shows the non-parametric estimates of the change (before vs. after the policy) in the price gradient by half-kilometer bins. The omitted bin is housing units beyond five kilometers from

¹⁴A formal proof of this argument is presented Barwick et al. (2019). The quantify of interest in the paper is the *change* in health-pollution gradient due to a policy shock.

subway stations. Similar to previous studies, our results show that the RRP increased the premium for subway access only for stations within 2.5 to three kilometers. The premium becomes statistically insignificant after 2.5 kilometers and falls to zero after three kilometers.

We assess the plausibility of our DID estimates using a back-of-envelope calculation to obtain the implied value of time (VOT). Based on our calculation, the VOT is about 57-86% of the (pre-tax) hourly wage assuming a housing tenure of 20-30 years.¹⁵ An important caveat of our VOT calculation is that it only applies to car owners who are directly affected by the RRP policy in their travel mode choices. According to the 2013 Beijing Transportation Report, a vehicle is used to undertake 2.7 trips per day during workdays by households within the fifth ring road. We assume that two car trips were replaced with subway trips during the restricted hours (7am-8pm) on a restricted day. The implied VOT would be larger (smaller) if there are fewer (more) car trips being replaced by subway trips. The VOT would be smaller when factoring in multiple passengers in some of the car trips. With these caveats, the magnitude of our estimated VOT is within the range of previous estimates: [Wolff \(2014\)](#) finds a value of time between 45-63% of hourly wage, while [Small et al. \(2005\)](#) find a value of time closer to 93% drivers' hourly wage in Los Angeles. Some more recent studies estimate VOT based on the trade-off between wait time and price among users on ride-hailing platforms. [Buchholz et al. \(2020\)](#) find the average VOT in Prague to be roughly 100% of users' wage during work hours while [Goldszmidt et al. \(2020\)](#) find an average (median) VOT of 75% (100%) of the hourly (after-tax) wage in 13 US cities.

5.2 Road Rationing and Income Sorting near Subway Stations

We now explore whether income sorting is a potential mechanism driving changes in the house price gradient found in the prior section. This is an empirical question, and will ultimately depend upon how different income groups value time saved relative to housing consumed. If the RRP policy had affected different income groups the same in terms of their willingness to pay for subway proximity, the relative locations of higher versus lower income households would not change. However, if the RRP affects

¹⁵By moving closer to a subway station by 1km, the total hours saved post-RRP (relative to pre-RRP) can be calculated as follows: 50 working weeks per year \times 1 driving-restricted day per week \times 2 subway trips per day \times 2 walks to/from subway per trip \times walking speed of 15 min/km \times a 30-year housing tenure \div 60 min/hour = 1,500 total hours saved per household. The regression results in column (5) of Table 2 suggest the average increase in purchase price for a housing unit 1 km closer to the subway was ¥36306 post-RRP. Dividing this premium by the 1,500 hours saved gives an implied VOT of ¥24 per hour. In 2007, the average resident in our sample living within 5km of a subway station with non-zero income earned approximately ¥7,555 per month, or about ¥42 per hour (assuming they work 180 hours per month). Consequently, the estimated price premium for subway proximity implies a VOT of about 57% of the hourly wage in our sample. Assuming a 30-year housing tenure, the implied VOT would be 86% of the the hourly wage.

wealthy households more than lower income households (due to vehicle ownership) by increasing the value of subway access more for higher income households, the average income of households proximate to subway stations will increase.

Panel B of Figure 5 shows estimates of α_q from Equation (2). Each dot shows the income gradient with respect to subway proximity (measured as distance to the nearest station) in a particular quarter relative to the quarter just prior to the RRP. The income gradient is generally stable and not statistically different from the omitted quarter in periods prior to July of 2008. However, the late 2007 period exhibits some pre-trends, suggesting that higher income individuals were more likely to move to locations closer to stations relative to lower income groups before the policy went into effect. The Beijing government enacted a trial period during August 17th-20th, 2007 and Beijing news media covered stories on the coming road rationing during the first quarter of 2008. Consequently, it is likely that these pre-trends reflect adjustment to new information on the RRP. Indeed, the search intensity from Baidu (the dominant search engine in China) in Appendix Figure A6 suggests individuals anticipated the policy in late 2007 and early 2008. We do not think these anticipation effects reflect gentrification trends or amenity development near stations because we do not see pre-trends in the price response, and because results of several robustness checks in Section 5.3 do not support a gentrification or amenity growth story near newly built or existing stations. Ultimately, these anticipation effects will work against finding an impact of the RRP. Consequently, we are likely to underestimate the true income sorting response to the RRP rather than overestimate the effect.

After the RRP, the composition of wealth increased (fell) in areas proximate to (far from) the nearest subway station. The gradient shift estimates are small and statistically insignificant in the quarters immediately following the RRP, but become significant and larger in magnitude after the fourth quarter of 2008. The sorting response may have become stronger at this time because the Beijing government announced a continuation of the RRP in late September of 2008.

Table 3 shows the average DID estimate of the RRP effect on the income sorting gradient with respect to subway distance. As in Table 2, each column adds successive controls to capture unobservables in both spatial and temporal dimensions. The coefficient estimates of the interaction term between subway distance and the RRP policy attenuate after introducing more controls. In general, our sorting results are less precisely estimated than the hedonic results in Table 2 likely due to the smaller sample size and more limited variation. However the sign of the DID coefficient estimate remains negative across specifications. These results indicate that the difference in income

for households with a one kilometer difference in their distance to a station increased by ¥10 to 47 per month (or ¥120 to 168 per year) after the RRP relative to before the policy. These estimates imply that households within versus outside the relevant distance of a station of 2.5 kilometers (based on Figure 6) in their annual income by approximately ¥1,400 per year.

Our hedonic analysis and sorting analysis are based on distinct data sets. However, we replicate our hedonic analysis using data on the sale price of mortgaged homes from the mortgage loan data set in Appendix Figure A7. The mortgage data set is a smaller sample relative to the real estate transaction data set, thus individual point estimates of the event study are often statistically insignificant. Using the mortgage dataset to estimate price gradient effects, we find estimates that are slightly attenuated, but within range of our main event study results: the premium for proximity to subway stations increased by an average of 2 percentage points (standard error of 0.612) per kilometer in the two years following the RRP. Both data sets reflect similar behavior in the marginal mover’s response to Beijing’s city-wide driving restriction.

Like the hedonic results, we find non-linearities in the income sorting effects. Panel B of Figure 6 shows non-parametric estimates of the change in the income gradient by one-kilometer bins.¹⁶ The omitted bin is housing units beyond nine kilometers from a station. The income response to the RRP dissipates after three kilometers. The fact that the income response attenuates at a similar distance as the price response is consistent with higher income households demanding residential locations closer to stations following the RRP.

While these results on demographic shifts appear small, the income effect constitutes a 19% increase from the baseline disparity in income across households that differ by one kilometer in their distance to a station.¹⁷ Our estimate on the RRP effect also explains approximately 39% of the overall increase during our study period in the relationship between proximity and household income.¹⁸ This suggests that city-wide policies aimed at reducing traffic and air pollution can be potentially regressive because they increase competition for housing near public transit, the mode choice disproportionately utilized by lower income groups.

¹⁶Because the mortgage dataset is significantly smaller than the real estate dataset, non-parametric estimates are noisy using half-kilometer bins.

¹⁷The baseline difference in income across households differing in their location from a station by 1km is ¥73 per month (the household 1km closer earns ¥73 per month more), and is derived from a regression of monthly household income (’000) as a function of distance to a subway station with year-by-month fixed effects. A change of ¥14 per month constitutes a 19% change from pre-RRP levels.

¹⁸The rate at which monthly income fell with distance to a station was ¥69 in 2005 and ¥125 as of 2011. The mid-point of income sorting estimates in Table 3 of ¥22 per month is approximately 39% of this difference. We estimate mean gradients using regressions of $\text{Income}_{it} = \delta \text{Km to Subway}_{it} + \epsilon_{it}$ in years 2005 and 2011, respectively.

5.3 Robustness Checks

Potential Endogeneity of Subway Locations.—Our empirical analysis so far has ignored possible reverse causality in the relationship between house prices and new subway station locations. The government of Beijing has invested heavily in expanding its subway network over the past two decades. As of 2000, Beijing had two subway lines with 31 stations while today the city has 21 lines with over 370 stations (as shown in Appendix Figure A1). The placement of these new lines and stations is unlikely to be random. If the location of new subway stations is determined by pre-existing trends in real estate development or gentrification, our results may be spurious. We address these concerns in several ways.

First, we test whether subway stations are more likely to open in gentrifying neighborhoods relative to stagnant or declining neighborhoods. In Appendix Figure A8, we find the population weighted mean of household income by jiedao in each quarter-of-year and compare changes in mean income across quarters leading up to and following the opening of a subway station in that jiedao. There is no evidence of a significant upward trend in household income preceding the opening date of a station, suggesting that across Beijing, siting of new stations is not directly correlated with income growth, consistent with Li et al. (2016).

Second, we exclude housing units in areas of the city that received a new subway station. For this sensitivity check, our sample includes only housing units that maintained the same distance to their nearest subway station from 2005 through 2016. Approximately 1,200 building complexes in our sample met this criteria, leaving about 50% of all transaction observations. Results of the bid-rent gradient shift for subway proximity are consistent with our main results after relying purely on pre- versus post- RRP variation within a jiedao, rather than spatial variation in subway expansion. In Appendix Figure A9, we compare our main hedonic DID point estimate—indicated by “Main”—with this alternative empirical specification. The point estimate “Excl. New Stations” uses the sub-sample of housing units with no change in their distance to the nearest subway station. The gradient change of 7.6 percentage points is very close to our main point estimate.

Third, we consider whether demand for subway station proximity may have increased over time as a result of the improved subway network in a way that is not fully captured by existing controls in our baseline specification. To explore this possibility, we add controls for a housing unit’s subway network density as well as an interaction of density with distance to station to our baseline specifications.¹⁹ Appendix Figure A9 shows that the additional subway network density controls do

¹⁹We construct a measure of a location-specific network density over time as follows: we divide our study area within the

not change our main conclusion.

Lastly, we address the concern of non-random placement of subway stations by instrumenting for their actual locations using their historic planned locations in the spirit of [Baum-Snow \(2007\)](#), which uses historical highway plans in the U.S. to instrument for observed highway routes. Specifically, we follow [Li et al. \(2019\)](#) and use Beijing’s 2003 subway plan. The 2003 plan closely mimics earlier plans from 1957, 1983, and 1999, but provides the most complete information.²⁰ [Li et al. \(2019\)](#) argue that the location of these stations were selected to facilitate national defense mobilization several decades ago when the population of Beijing was a fraction of its current size. City planners’ location choices for these stations could not have been influenced by travel demand or growth trends sixty years later. Appendix Figure [A9](#) shows that the RRP effect under the instrumental variable approach (“Subway Plan IV”) is not statistically different from our main effect.

Out-of-Sample Test for Pre-Trends—We conduct an out-of-sample analysis and test whether areas that received *future* subway development after our study period experienced differential price trends over time. In Appendix Figure [A10](#), we estimate the effect of subway proximity among a sample of housing units that were more than three kilometers from a subway station up through 2013, but came to be within three kilometers after new stations were built after 2013. This sample of housing units should not be affected by the RRP during our study period because they were not within the relevant “treatment” distance of subway stations until five years after the policy. Any price effects from the policy would raise concern that correlated shocks stemming from unobserved economic investment or growth, as opposed to increased demand for the subway itself, caused the shift in the price gradient. Figure [A10](#) is suggestive that the RRP did not significantly increase the price premium for subway proximity among this group of housing units. The point estimates are imprecisely estimated due to smaller sample sizes in the post-RRP period, however, the quarterly estimates do not show a clear downward trend, as in Figure [5](#).

Placebo Analysis using Pollution Sites—We conduct a placebo analysis to test whether the RRP adjusted either prices or the spatial distribution of wealth in areas close to *undesirable* locations of the city. This test informs whether we are justified in interpreting our main results as a consequence

fifth ring road into 516 transit zones following the sampling unit used by the Beijing Transportation Institute for their commuting surveys. For each zone, we measure it’s subway density as the inverse distance-weighted sum of stations from the zone’s centroid. The zone-specific density measure increases over time as the subway system expands. Zones closer to stations have a higher network density measure than zones further from stations.

²⁰We assign the station’s actual opening date in absence of the planned opening date.

of people’s desire to reduce commuting costs. A restriction on traveling by car during the week should not affect the (un)desirability of proximity to major pollution sites.

We obtained data on major sources of pollution throughout Beijing as of 2006 and 2007 from the emissions monitoring program of the Ministry of the Environmental Protection. These data provide the location and emissions level of industrial pollution sources. We isolated firms in the top 10th percentile of total air discharge, and geocoded their locations. We then estimate the same hedonic and sorting specifications based on Equations (1) and (2). Some major pollution sources are located close to subway stations. To ensure that we do not conflate commuting effects with the dis-amenity effects of pollution sites, we restrict our sample of pollution sites to those that are at least 2.5 kilometers away from a subway station, following the results of Figure 6. This leaves a sample of 133 pollution sites throughout Beijing, out of a total of 449 sites. Appendix Figure A11 compares our main estimates on the price gradient shifts for subway proximity to that of proximity to major pollution locations. “M1” through “M4” denote various specifications of the hedonic regression. The placebo estimates are generally smaller in magnitude than our main hedonic estimates, and statistically insignificant. Appendix Figure A12 shows results of a similar exercise for income sorting. Again, the placebo estimates (shown in gray) of the effect of the RRP on income sorting with respect to pollution sites are statistically indistinguishable from zero. This placebo analysis lends support that our empirical design isolates gradient shifts caused by a change in the desirability for transit access, as opposed to secular changes in price or sorting patterns throughout Beijing.

Triple-Difference Design—As a final robustness check, we relax the parallel trends assumption required for our DID estimator and employ a “triple-difference” design to estimate the impact of the RRP on the price gradient. Our approach leverages the various subway station openings across Beijing between January of 2005 through December of 2011. Whereas our main DID estimate captures the average impact of the RRP on the premium for subway proximity, the triple-difference estimate captures the effect of the RRP on house price responses to new station openings. The advantage of the triple-difference approach is that it allows for the existence of pre-trends prior to subway station openings. As long as pre-trends are similar in the pre-RRP and post-RRP periods, the triple-difference estimator still identifies a causal impact of the RRP. The main disadvantage of the triple-difference estimator in our setting is that it is data-intensive, and does not capture the more general impact of the RRP on subway proximity among housing units that do not gain a new station. Because the sample for triple-difference analysis is substantially smaller and there are limited instances of multiple

mortgage transactions treated by station openings, the results on income sorting are imprecise. Thus, we focus this robustness check on the effect of RRP on the price gradient.

To carry out the triple-difference analysis, we first identify the set of houses “treated” by subway station openings during our sample period. We focus on housing units for which their distance to the nearest station changes from over one kilometer to within one kilometer of a station as a consequence of a station opening at some point between 2005 and the end of 2011.²¹ We compare these “treated” units to “control” units whose nearest station is always over one kilometer for the entire study period. Thus, the three “differences” in house prices that we compare in our triple-difference design include: (i) treated versus control units, (ii) before versus after subway station openings, and (iii) before versus after the RRP. We focus on a distance threshold of one kilometer in order to strike a balance between identifying plausible proximity effects from a station opening and excluding spillover situations where housing units are within range of multiple station openings during our period of study. However, we find similar results using a two-kilometer threshold.

Between January 1st, 2005 and December 31st, 2011 new subway stations opened on several distinct dates.²² We exclude housing units treated by station openings during two of these opening dates, 19th of July 2008 and 10th of October 2008, because these dates overlap with the start of the RRP and we cannot meaningfully distinguish station opening effects from RRP effects among these housing units using a triple-difference design. For a similar reason, we restrict our data to transactions that occurred within nine months before and after a station opening date so that none of our event time windows overlap with the RRP, itself.²³ Some housing units were exposed to multiple station openings during this period. We focus on the first time a housing unit “moves” from over to under one kilometer of a station as the effective station opening date.

Our estimation equation is as follows:

$$\ln(p_{ibt}) = \beta(D_i \times Open_{bt} \times RRP_t) + \sigma(D_i \times Open_{bt}) + \mathbf{x}'_{ibt}\alpha + \gamma_b + \tau_t + \nu_{ibt} \quad (3)$$

where p_{imt} is the price per square meter for housing unit i in building complex b on date t . D_i is an indicator equal to one if a housing unit is ever “treated” by a station opening within one kilometer during our study period. D_i is zero if a housing unit is always over one kilometer from a station.

²¹We treat the addition of new subway lines to existing stops the same as new subway station openings.

²²The dates include: 7th of October 2007, 19th of July 2008, 10th of October 2008, 28th of September 2009, 25th of December 2010, 30th of December 2010, and 31st of December 2011.

²³For instance, nine months after the 7th of October 2007 opening is June of 2008, prior to the RRP. Nine months before the 19th of September 2008 opening is January of 2008, after the RRP.

$Open_{bt}$ is an indicator equal to one after a subway station opens within one kilometer of a complex b . We assume that all housing units within the same building complex have the same distance to a station. This variable is zero in periods prior to the station opening among treated complexes as well as for housing units that are always over one kilometer from a station. RRP_t is an indicator equal to one for transactions that occurred after July of 2008. The parameter of interest β gives the RRP-induced change in the effect of station openings on house prices. \mathbf{x}'_{it} includes all the same controls as in Equation (1). The building complex fixed effects, γ_b , ensure that β is identified off of temporal variation in proximity across housing units in the same building complex. Lastly, τ_t is temporal fixed effects such as year and month fixed effects and district-specific linear time trends.

We first show results of an event study specification. Panel A of Appendix Figure A13 shows the pre-RRP effect of station openings on house prices nine months before and nine months after a station opening. Panel B shows the post-RRP counterpart. Neither panel exhibits a significant pre-trend in prices prior to a station opening. There is a striking difference in the post-station opening effect on house prices across the two panels, however. Panel A shows that prior to the RRP, there is a short-term increase in prices following a station opening that subsequently becomes slightly negative. Panel B, in contrast, shows a clear increase in price following a station opening. Appendix Figure A13 implies that station openings generated a slight price discount for proximate housing units prior to the RRP; but created a significant price premium after the RRP.

Appendix Table A2 shows results of Equation (3) across various specifications and under one and two-kilometer treatment thresholds. The statistically significant results imply that the RRP increased the premium for station access by between 8.5 and 14.0 percentage points. These magnitudes are broadly in line with those of the DID design. Notably, new station access capitalized positively into house prices only *after* the RRP. This pattern contrasts with prior work, which finds a positive impact of new light rail or transit access on home prices (Tyndall, 2021; Heilmann, 2018; Kahn, 2007; Tsivanidis, 2022; Billings, 2011). The difference in findings may be because much of this prior work focuses on US cities where rates of light rail expansion were generally slower and rates of transit use are low relative to Beijing. We interpret the un-interacted subway opening effect in Equation (3) with caution, since the location of subway openings is non-random. Nevertheless, our triple-difference results underscore that the RRP price appreciation effect for housing units near subway stations was largely a result of the increased demand for transit access as opposed to growth in demand for amenities or commercial development near stations.

5.4 Sorting Mechanisms

While the prior analysis documented how the RRP increased the price and average household income of housing units close to economically important areas of Beijing, it is not clear whether these sorting effects are a result of displacement of poorer households, or gentrification and development of new housing. Evidence of displacement raises equity concerns for the incumbent households. We explore which mechanism explains the sorting results by testing first, whether total housing stock increased and second, whether the number of lower income households fell in absolute terms in areas proximate to subway stations over time.

We delineate “proximity” at 5 kilometers because this is the mean distance to the nearest CBD in our sample and because demand for subway proximity is indistinguishable from zero at this distance. By neighborhood, we count the number of newly built housing by month in locations within versus over 5 kilometers of subways. We then take the mean of new builds across neighborhoods, and plot these values by month in Figure 7. Panel A shows that new development increased after the RRP in areas within 5 kilometers of subway stations more so than areas further away. This figure is suggestive that housing supply adjusted to the increased demand for subway access.

We repeat this non-parametric approach in Panels B of Figure 7 with a focus on count of high relative to low income households. We first look only at households living within 5 kilometers of subway stations. We then divide this sub-sample into households above median versus below median income, where the median is calculated using the pre-RRP distribution from January 2005 through July 2008. We count the number of households in each income category by neighborhood and plot the count changes over time. Panel B shows a clear upward trend in the number of higher income households in proximate locations, whereas the count of lower income households stays relatively flat. Based on the data patterns in this figure, it does not appear that lower income households were displaced from proximate locations. Instead, these patterns are suggestive that newly-built housing near subway stations were more likely to be occupied by higher income households after the RRP.

6 Conclusion

Road rationing policies are an increasingly common policy instrument used among major cities around the world to reduce traffic congestion and air pollution. While prior work has investigated the effectiveness of these policies at improving air quality and congestion, less is known about the ramifications of these policies on residential location decisions. Urban land use theory provides predictions on how such policies could impact the housing market and the sorting of demographic groups.

Leveraging a city-wide road rationing policy and detailed micro-level data on home purchases and buyer demographics in Beijing, we find the predictions of urban land use theory are consistent with our empirical results. Specifically, we find that Beijing’s road rationing policy (RRP) increased the price premium for properties closer to subway stations, thus steepening the slope of the price gradient with respect to subway proximity. We additionally utilize novel micro data on household income and housing locations to explore how the RRP impacted the spatial distribution of households with different income. Our analysis shows that the composition of households living close to subway stations shifted toward marginally wealthier households after the RRP policy. The sorting effect appears to be driven by wealthier households moving into newly-built properties near subway stations after the policy.

These results underscore the relevance of the transit-based explanation championed by [LeRoy and Sonstelie \(1983\)](#) to explain patterns of income sorting in cities. From a policy perspective, our study provides evidence that city-wide road rationing policies could have the unintended consequence of limiting access to public transit for lower income individuals. These effects are likely to be strongest in regions where car ownership is cost-prohibitive to the poor as in many countries in South and Southeast Asia and Latin America.

Our findings point to several important avenues for future research. First, the welfare implications of road rationing policies are not clear and will depend upon how commute times and exposure to pollution change among the wealthy relative to the poor. Such analysis can inform whether rent stabilization or welfare transfers may be necessary to offset impacts of road rationing policies on housing affordability. Second, how might a market-based instrument such as congestion pricing affect household location decisions and urban spatial structure relative to a road rationing policy? Understanding the efficiency and equity consequences of market-based versus command-and-control approaches warrants future research.

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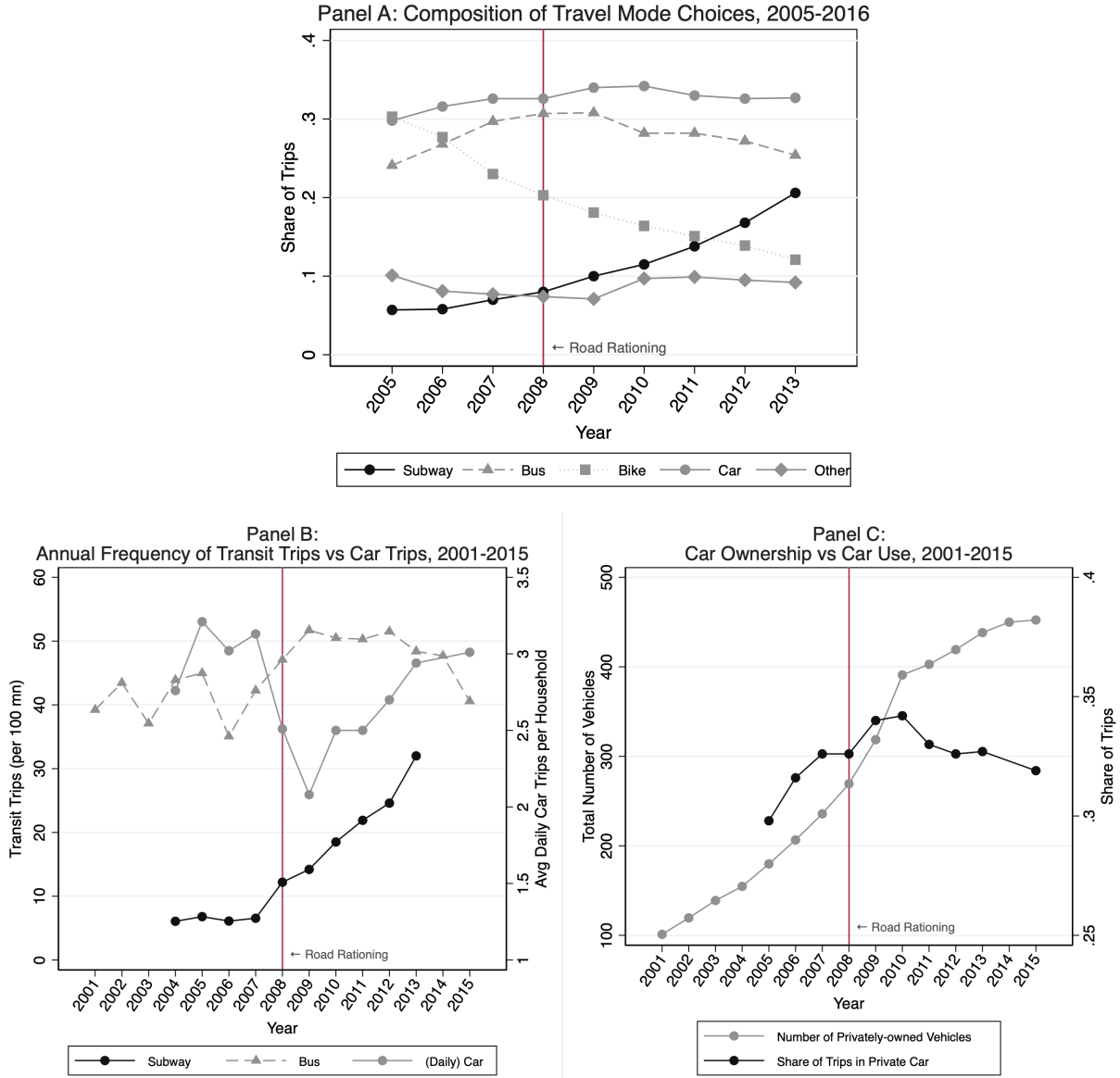
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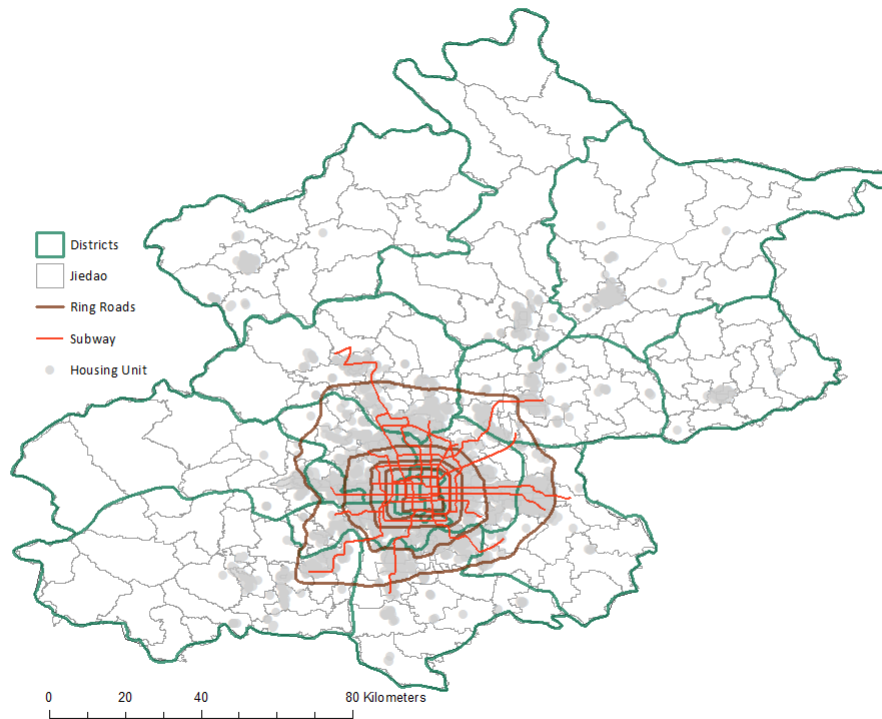
Figure 1: Trends in Travel Mode Choice Among Beijing Residents



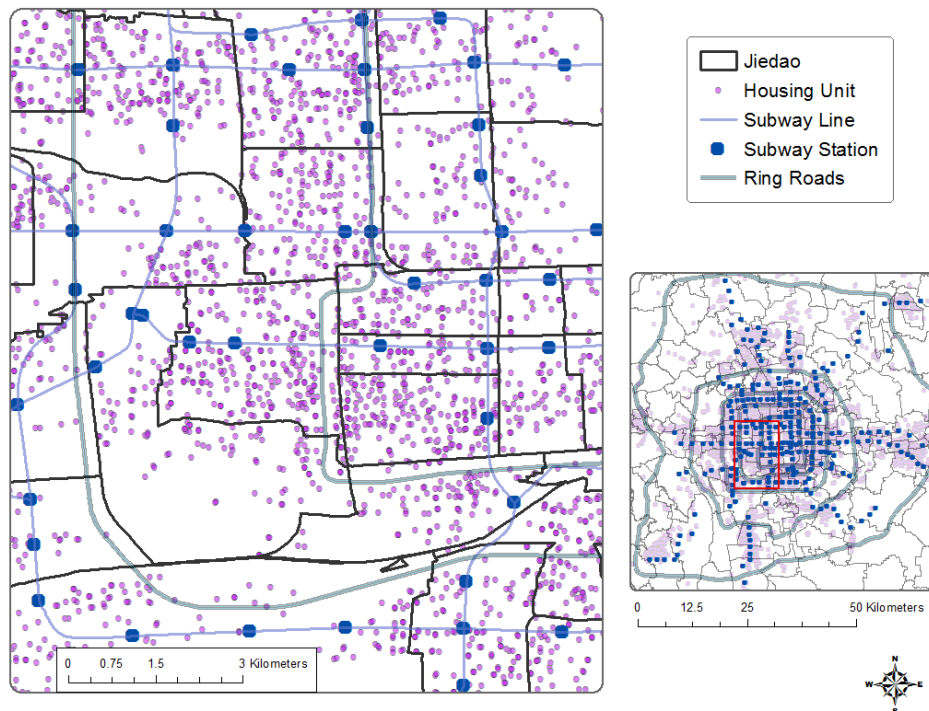
Note: **Panel A** shows the share of trips that used either the subway, bus, bicycle, a private car, or other in each year from 2005 through 2013. Data sourced from the 2005 through 2013 Beijing Transport Annual Reports. The Annual Reports do not record walking trips, thus walking is excluded from the figure. **Panel B** Figure plots total annual usage of subway and bus (left axis) against the daily average number of car trips per household (right axis). Car trips based only on trips in privately-owned vehicles. Data sourced from the 2001 through 2015 Beijing Transport Annual Reports. **Panel C** Figure shows total number of privately-owned vehicles in Beijing relative to the share of commutes that use private cars in each year 2001 through 2015. Number of privately-owned cars based on vehicle registration data for the city of Beijing. All data sourced from the 2005 through 2015 Beijing Transport Annual Reports.

Figure 2: Spatial Variation in Housing, Stations, and Neighborhoods of Beijing

A. Housing Units & Subway Stations in Beijing

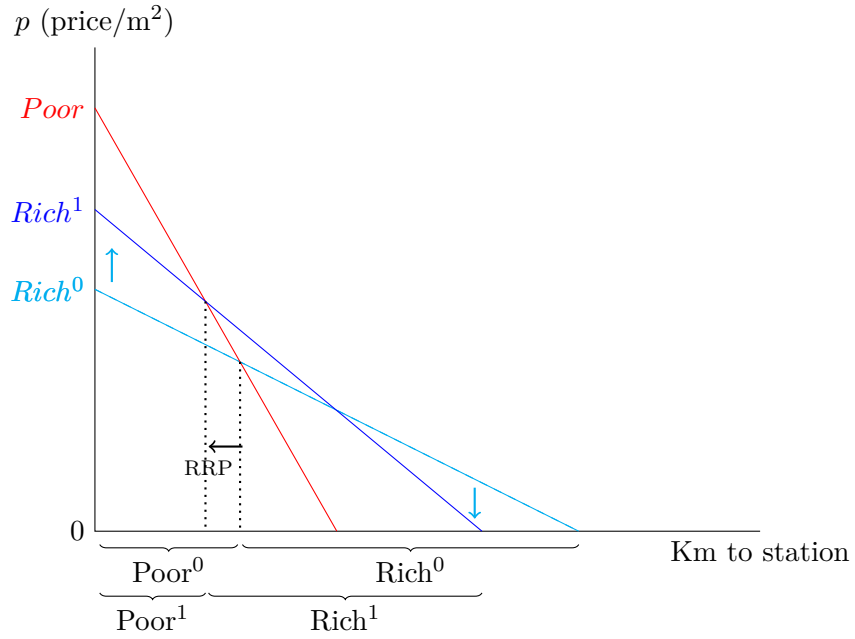


B. Neighborhood Variation



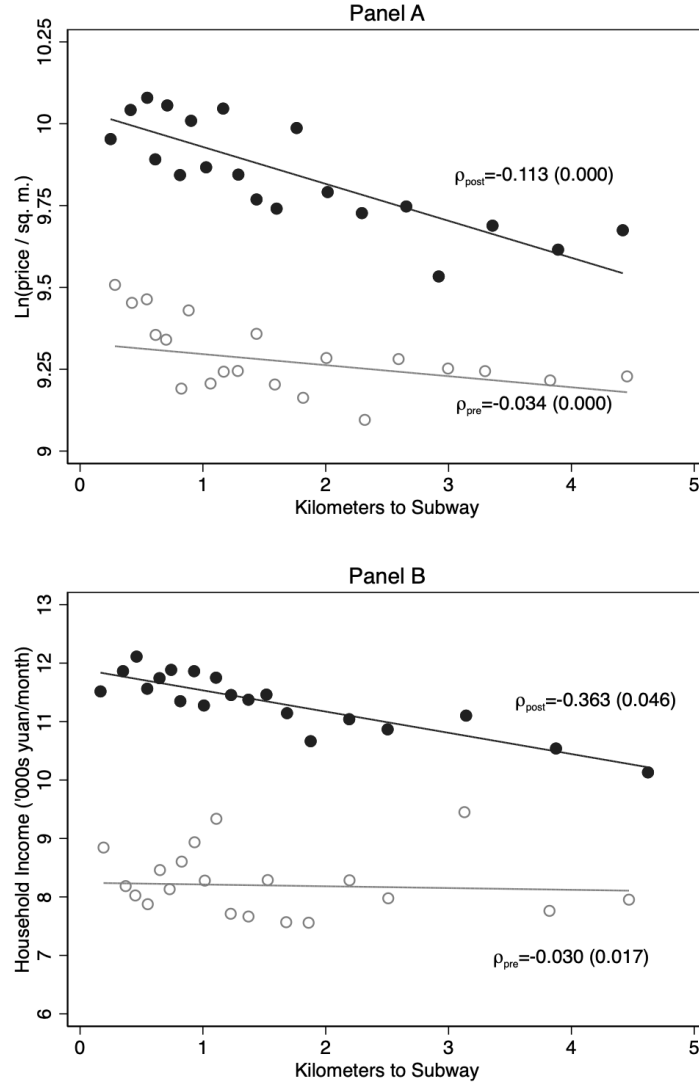
Note: Panel A shows the location of housing units purchased between 2005-2014 throughout Beijing. Source: Beijing Real estate data. Panel B shows a southwest section of central Beijing, between the second and third ring roads. Sources: Beijing Real estate data; mortgage application data.

Figure 3: Urban Land Use and Equilibrium Sorting with Income Heterogeneity



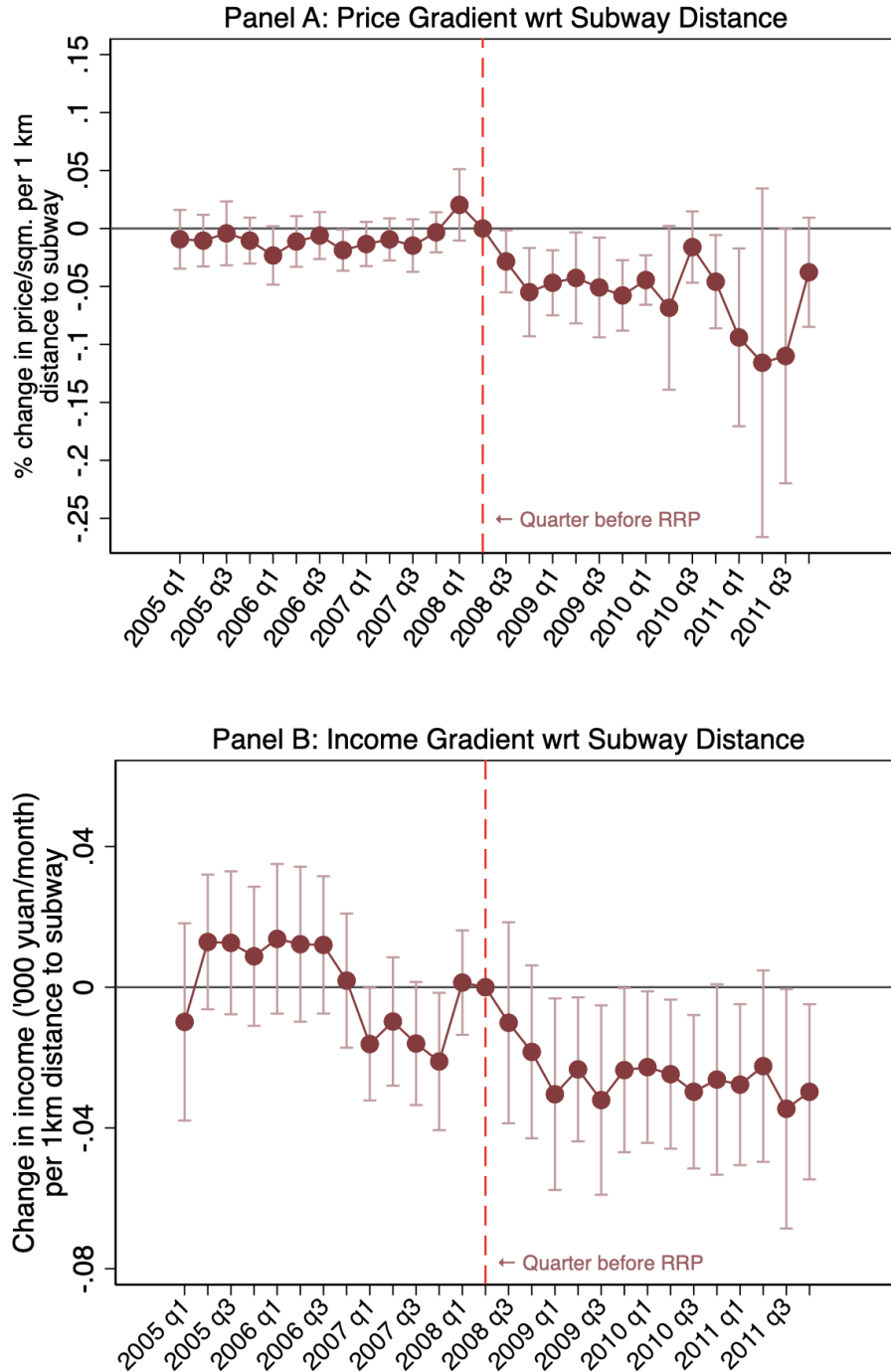
Note: y-axis is price per square meter and the x-axis is distance from the subway station. Each income group has a distinct bid-rent gradient. The poor have a steeper gradient than the rich, *ex ante*. The RRP increases the cost of commuting for the rich, thus they increase their demand for locations proximate to subway stations, depicted as a tilt from $Rich^0$ to $Rich^1$. The RRP causes the price per square meter to increase for all units from the intersection of *Poor* and $Rich^1$ to the intersection of *Poor* and $Rich^0$. This also causes the rich to outbid the poor for units along the x-axis within the horizontal dotted lines.

Figure 4: The Bid-Rent Gradient, Income Sorting, and Beijing's Road Rationing Policy



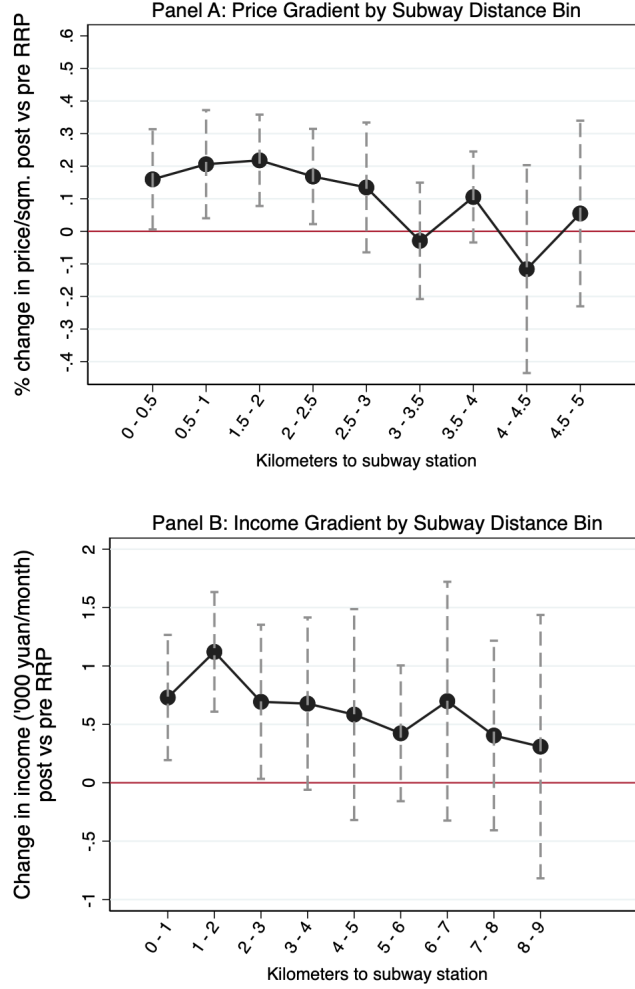
Note: **Panel A** plots the mean $\ln(\text{price}/\text{sqm}$ in ¥2007) for each of 20 distance bins. Each dot represents 4,927 and 12,664 observations per bin in pre- and post-RRP periods, respectively. Sourced from the real estate transaction data set. **Panel B** plots the mean monthly income ('000 ¥2007) for each of 20 distance bins. Each dot represents 328 and 2,715 observations per bin in pre- and post-RRP periods, respectively. Sourced from the Mortgage application data set. $\rho(\text{pre})$ and $\rho(\text{post})$ are regression coefficients. Figures exclude observations over 5km from a station for exposition purposes.

Figure 5: The Effect of Road Rationing on House Price Premiums and Income Sorting w.r.t Subway Distance



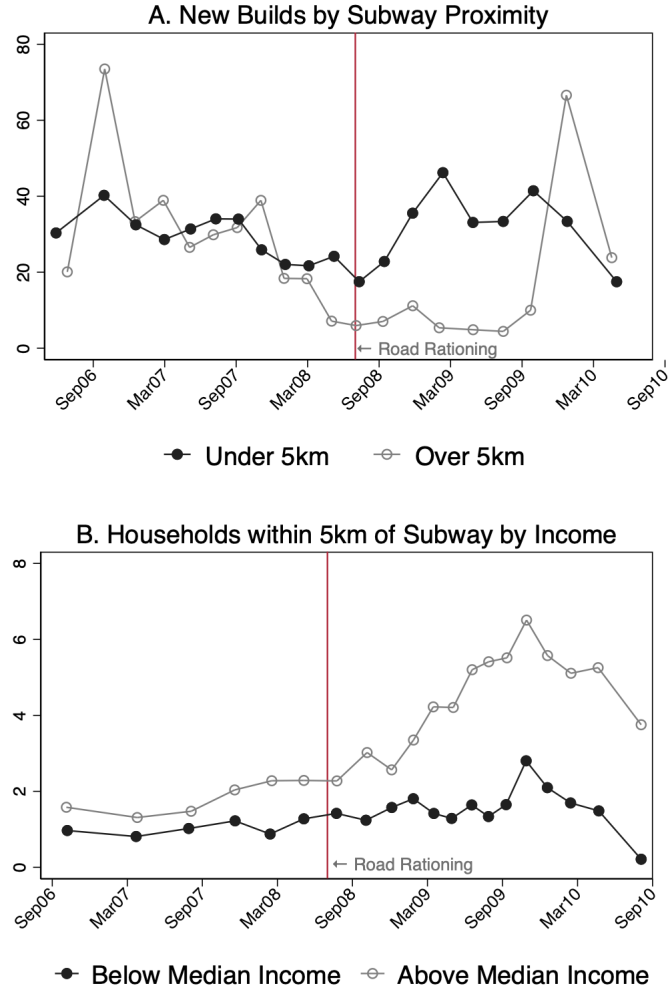
Note: **Panel A:** each dot shows the change in $\ln(\text{price/sqm in } \text{¥}2007)$ for a 1 km increase in distance to the closest subway station at each quarter between Jan 2005 and Dec 2011 relative to the omitted quarter, April-June 2008. Dashed lines show 95% confidence intervals. Controls include fixed effects for unit type (resale or newsale), jiedao, subway line of nearest station, and year-quarter of the transaction; as well as controls for age, age², size, floor-area ratio, green space, property management fee, parking fee, number of housing units and building units in complex, and unit size. Standard errors clustered at the jiedao level. **Panel B:** each dot shows the change in monthly household income (¥'000) for a 1km increase in distance to the closest subway station at each quarter between January 2005 and December 2011 relative to the omitted quarter, April-June 2008. Dashed lines show 95% confidence intervals. Controls include fixed effects for age, rank, employer type, education, and experience of buyer as well as year and month of the transaction, subway line of closest station, and zip code of housing unit. Standard errors clustered by zip code.

Figure 6: Road Rationing Effect by Subway Distance Bin



Note: Figures plot α_b , estimates of the average change in y_{ijt} following the RRP by distance bin to subway stations (B_b): $y_{ijt} = \sum_b \alpha_b(B_b \times RRP_t) + \sum_b \kappa_b(B_b) + \rho RRP_t + \mathbf{X}_{ijt}\theta + \gamma_j + \tau_t + \varepsilon_{ijt}$. Dashed lines represent 95% confidence intervals. Includes transactions from July 2005 through July 2011. In **Panel A**, $y = \ln(\text{price/sqm in } \text{¥}2007)$. The reference bin includes housing units over 5 kilometers from subway stations. Controls (\mathbf{X}) include fixed effects for unit type (resale or newsale), jiedao (γ_j), subway line, year, month of transaction (τ), total number of floors in building, decoration level, whether at top floor, and facing direction; as well as controls for age, age², size, floor-area ratio, green space, number of housing units and building units in complex, and unit size. Standard errors clustered at the jiedao level. In **Panel B**, y = household income (¥'000 /month in 2007 term). The reference bin includes housing units over 9 kilometers from subway stations. (Plot shows only estimates up through 8 km bins for exposition purposes). Controls (\mathbf{X}) include fixed effects for subway line, year, month of transaction (τ), and zip code (γ_j); as well as controls for age, rank, employer type, education, and experience of buyer. Standard errors clustered at zip code level.

Figure 7: Road Rationing & Supply of New Housing



Note: Panel (A) plots the mean new supply of housing by month for units under versus over 5km of subway stations. Sourced from the real estate transaction data set. Panel (B) plots mean number of households living within 5km of subway stations by income group. Median income is based on the pre-RRP income distribution and is calculated from all mortgage applications occurring from January 2005 through July 2008. Sourced from the mortgage application data set. Means in all panels residualized by neighborhood fixed effects.

Table 1: Estimation Sample Descriptive Statistics

	Real Estate Transactions		Mortgage Data	
	(1) Mean	(2) (S.D.)	(3) Mean	(4) (S.D.)
Total purchase price ('07 Yuan)	1,491,405.7	(1,090,428.9)	599,805.8	(303,918.9)
Price per sq.m	13,274.5	(4,763.8)	6,225.8	(2,805.9)
Unit size (sq.m.)	107.8	(48.7)	100.6	(33.3)
Km to subway	3.3	(2.7)	15.2	(14.7)
Km to nearest CBD	6.1	(3.9)	20.5	(14.6)
Km to City Center	9.2	(3.7)	26.9	(17.1)
Building Age	10.7	(7.1)	6.4	(7.4)
Building Floor-to-Area Ratio	2.9	(1.3)	2.2	(1.0)
Building Green space ratio	0.3	(0.1)	33.1	(7.4)
Household Monthly Income ('07 Yuan)			7,723	(4,502)
Age of household head			35.2	(6.6)
Years of work experience of household head			13.4	(7.8)
Education level of household head (mode)			Bachelor's	
No. Complexes	4,403		3,971	
No. Neighborhoods	188		183	
Total Observations	252,426		46,471	
Observations by year:				
2005	1,926		4,858	
2006	61,348		5,273	
2007	47,837		4,410	
2008	24,759		4,225	
2009	65,513		12,513	
2010	34,859		8,321	
2011	16,184		6,871	

Note: The unit of observation for the Real Estate Transaction data is a housing purchase transaction. The means calculated in column (1) are calculated using pre-RRP months July 2007 through July 2008. The unit of observation for the Mortgage Data is a mortgage loan application, or a household, equivalently. The means in column (3) are calculated using pre-RRP months July 2006 through July 2008.

Table 2: The Effect of Road Rationing on the House Price-Distance to Subway Gradient

	(1)	(2)	(3)	(4)	(5)
Km to Subway x RRP	0.055*** (0.018)	0.045** (0.018)	-0.061*** (0.010)	-0.075*** (0.009)	-0.063*** (0.012)
Km to Subway	-0.085*** (0.015)	-0.029** (0.014)	-0.005 (0.010)	0.010 (0.009)	0.016* (0.009)
Year and Month FE	Y	Y	Y	Y	Y
Jiedao FE		Y	Y	Y	
Subway Line FE		Y	Y	Y	
Controls			Y	Y	Y
DistrictxYear-Month Trend				Y	Y
Building Complex FE					Y
Observations	185138	185138	185138	185138	184787
Adjusted R^2	0.213	0.453	0.688	0.695	0.831
Avg Proximity Premium/Km	¥29941	¥24791	¥35007	¥43006	¥36306

Note: The dependent variable is $\ln(\text{price per square meter in 2007 real Yuan})$. Standard errors clustered at jiedao level. Sample spans July 20, 2006 - July 20, 2010. Average price premium per km is evaluated at a unit size of 115 sq.m., the size at the mean distance (between 2km and 4km) to the nearest subway station. Controls include fixed effects for unit type (newsale vs resale), top floor, floor level, facing direction, no. bedrooms, decoration level, ownership type, and total number of floors in building. Continuous controls include age, age², size, floor-area ratio, green space, property management fees, parking fees, and size, number of housing units and number of buildings of the complex. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

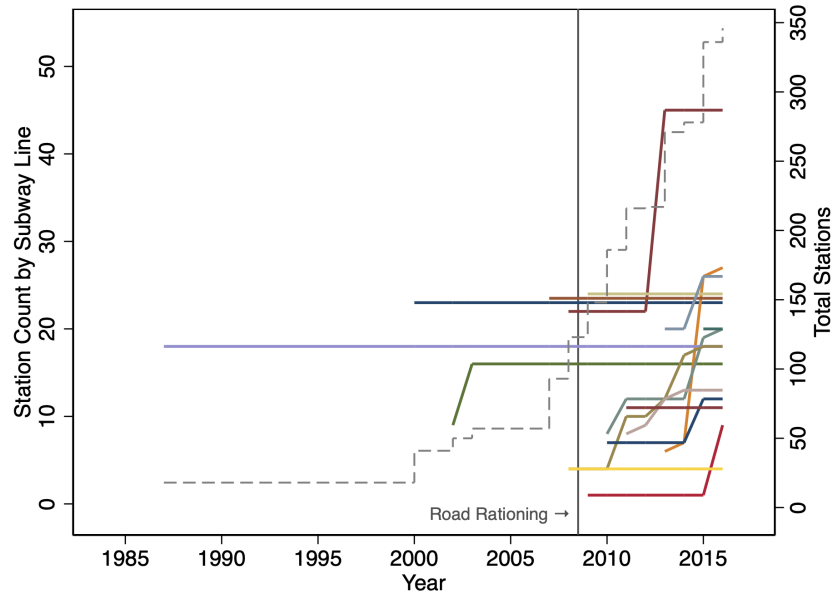
Table 3: The Effect of Road Rationing on the Income-Distance to Subway Gradient

	(1)	(2)	(3)	(4)	(5)
Km to Subway x RRP	-0.044*** (0.011)	-0.028*** (0.009)	-0.014** (0.007)	-0.014* (0.007)	-0.009* (0.005)
Km to Subway	-0.080*** (0.010)	-0.021 (0.017)	-0.001 (0.009)	0.005 (0.011)	0.065*** (0.012)
hline Year and Month FE	Y	Y	Y	Y	Y
Zip Code FE		Y	Y	Y	
Subway Line FE		Y	Y	Y	
Controls			Y	Y	Y
DistrictxYear-Month Trend				Y	Y
Building Complex FE					Y
Observations	18021	18021	18021	18021	17375
Adjusted R^2	0.156	0.258	0.464	0.466	0.494

Note: The dependent variable is monthly household income ('000 yuan). Standard errors clustered by zip code. Sample spans July 20, 2006-July 20, 2010. Controls include husband and wife age, employment rank, education, employer type, and tenure. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

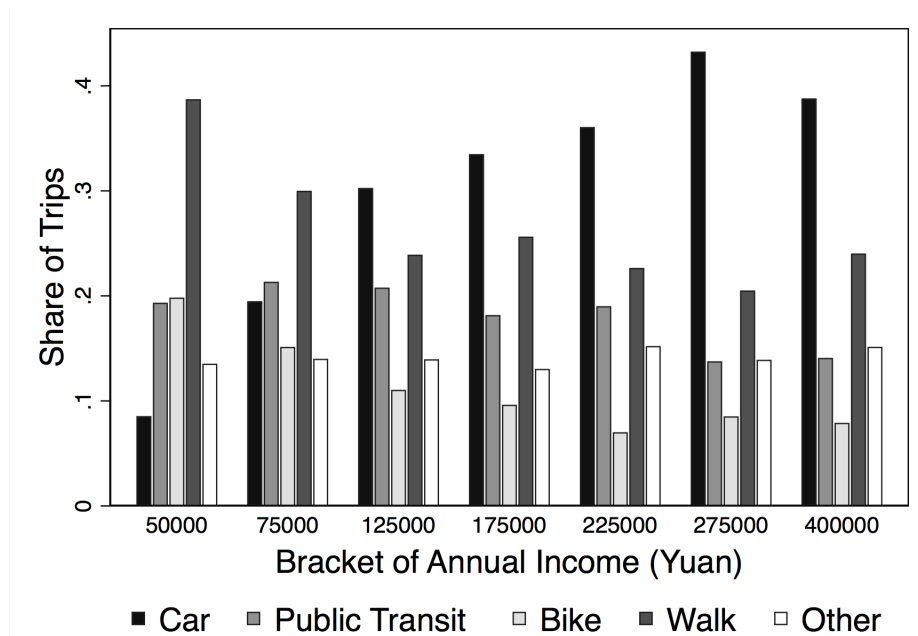
Appendix A Figures and Tables

Figure A1: Beijing Subway System Expansion



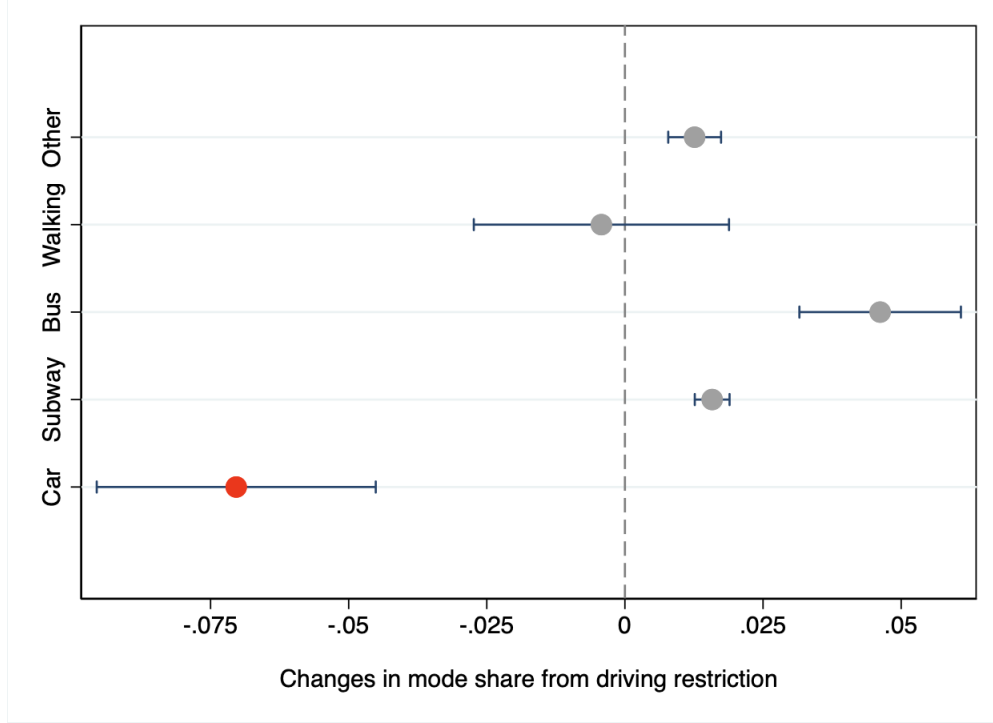
Note: Each color represents a subway line. Vertical shifts within a subway line show additions of stations connected to that subway line. The gray dashed line plots cumulative growth in stations over time.

Figure A2: Transit Mode Choice by Income



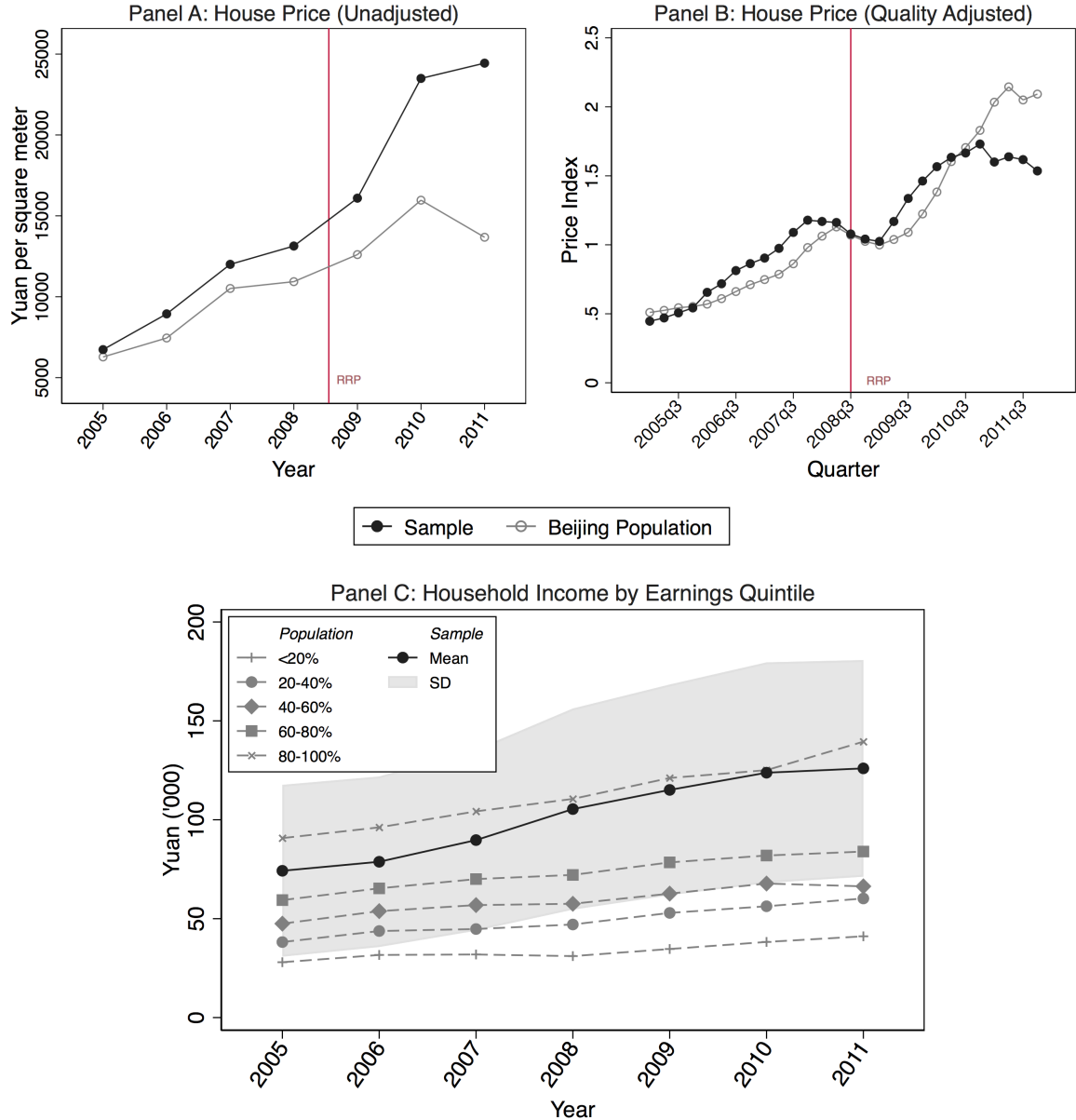
Note: Data sourced from 2010 Beijing Household Travel Survey ([Beijing Transport Institute 2010](#)). “Other” includes taxi, commercial vans and trucks, motorcycle, company shuttle, school bus, and mixed modes.

Figure A3: Effect of Driving Restriction on Mode Choice



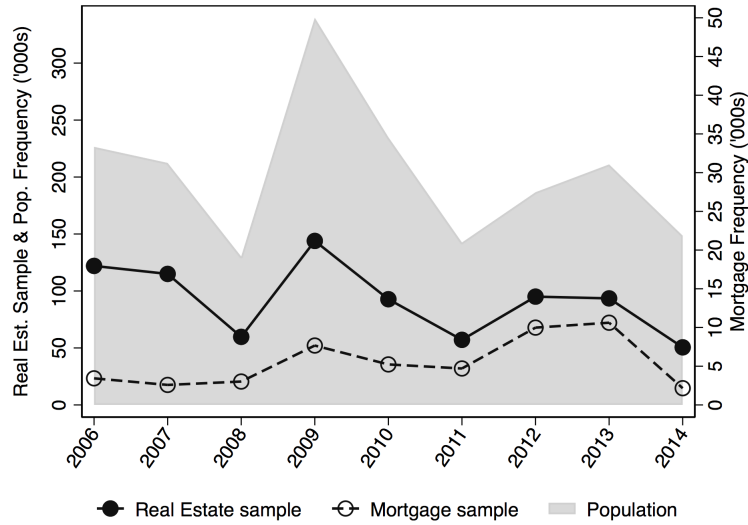
Note: The figure shows the partial effect of the driving restriction on travel mode choice and the 95% confidence bands from the multinomial logit regression. The utility from choosing mode j in trip i at time t is given by $u_{ijt} = \beta_j(\text{Restricted Hour}_t \times \text{Restricted Day}_{it}) + \alpha_j \text{Restricted Hour}_t + \gamma_j \text{Restricted Day}_{it} + \mu_{jt} + \delta_{jt} + \varepsilon_{ijt}$ where the choice set includes using a car, riding the subway or bus, walking, or using an “other” mode (e.g., biking, taxi, motorcycle, school bus, company van). “Restricted Hour” equals 1 for a trip i if the trip’s starting time or end time is within the window of 7am-8pm. “Restricted Day” equals 1 for a trip i if the traveler is from a household owning a vehicle that is restricted from driving on day t based on the last digit of the license plate number of the vehicle. A vehicle is restricted from driving during the restricted hour (7am-8pm) one day per week during workdays. The policy follows a preset rotation schedule in terms of which pair of numbers (one and six, two and seven, three and eight, four and nine, or five and zero) is restricted on a given day (Yang et al., 2020). Sample includes 86,989 trips from households owning a vehicle. μ_{jt} are month fixed effects and δ_{jt} are day-of-week fixed effects. Standard errors are clustered at the calendar day level. Data sourced from the 2010 Beijing Household Travel Survey.

Figure A4: Data Sample Relevance & Representativeness



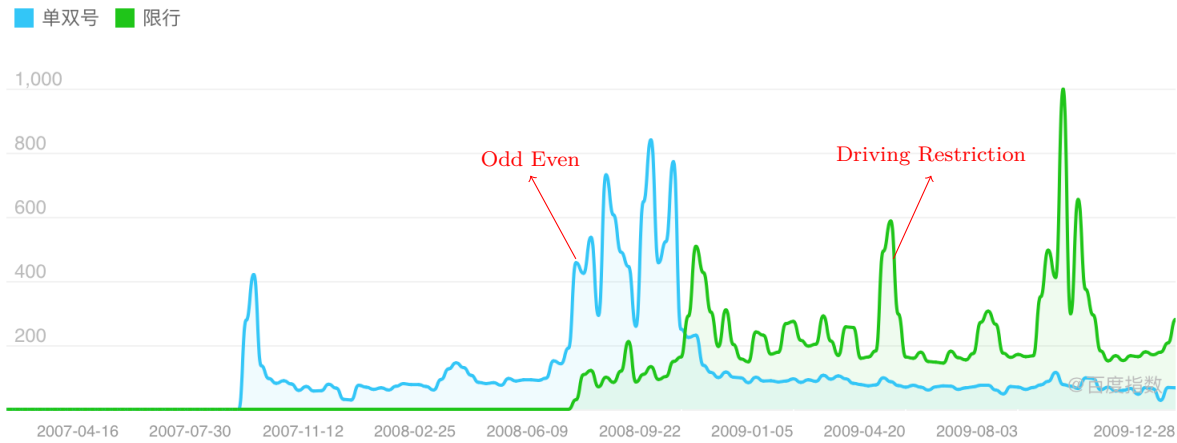
Note: Figure compares the sample data to Beijing population means of house prices and household income. Panel A plots the average (unadjusted) house price per square meter by year; Panel B plots the average quality-adjusted house price by quarter; and Panel C plots average household income by earnings quintile by year. In **Panel A**: source for the “Sample” data is the real estate transaction data set; source for the “Beijing population” data is the National Bureau of Statistics. In **Panel B**: source for the “Sample” house price index is the real estate transaction data set quality adjusted as follows. We first estimate the fixed effect coefficients μ_t from the regression $\ln(\text{unit sale price})_{ict} = \mu_t + \kappa_c + w'_{ict}\gamma + \varepsilon_{ict}$ where the dependent variable is the log transaction price for unit i in building complex c on date t . κ_c are building-complex fixed effects and μ_t are month \times year fixed effects. Property characteristics w_{ict} include floor fixed effects, unit size, unit size², unit type fixed effects (resale or new sale property), and number of bedroom fixed effects. μ_t captures the price difference among otherwise identical units sold across two months, assuming that within a building complex, differences in the units are fully described by our property characteristic controls. This approach mimics Fang et al. (2016)’s method for estimating quality adjusted house prices across Chinese cities. Second, we average monthly price indices (μ_t) to the quarter level. “Beijing Population” house price sample sourced from Fang et al. (2016). **Panel C**: source for the “Sample” average and standard deviation is the mortgage application data set. Source for annual means by earnings quintile is the National Bureau of Statistics. All numbers deflated to 2007 ¥.

Figure A5: Comparison of Sample vs Population of Housing Transaction Volume



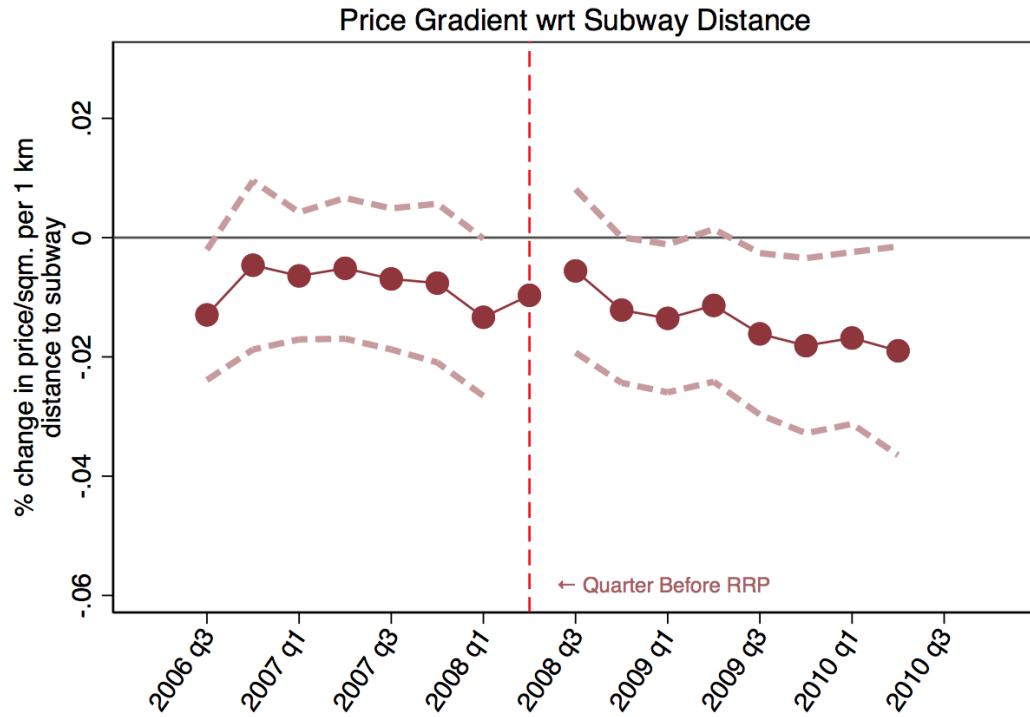
Note: Figure plots the population of housing transactions in Beijing from 2006 through 2014 in solid gray. Population transaction aggregates sourced from the Beijing Municipal Commission of Housing and Urban-Rural Development. Annual transaction volumes associated with the Real Estate data and Mortgage Application data shown in the solid and dashed black lines, respectively.

Figure A6: Baidu Search Trend for Driving Restriction Policies



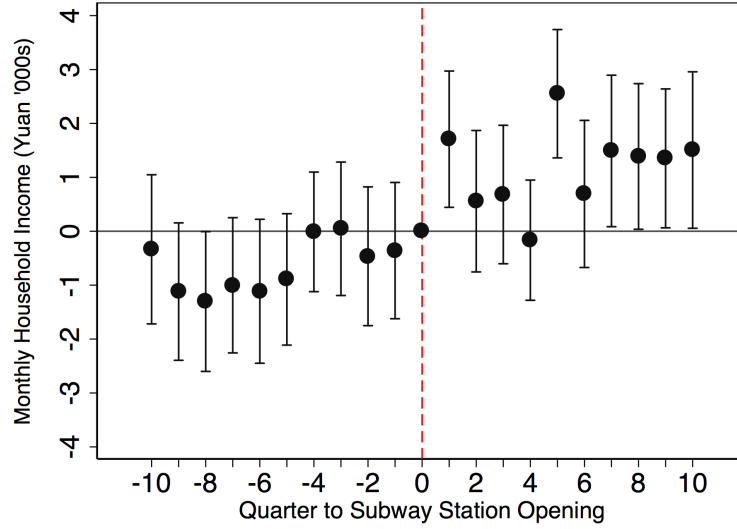
Note: Figure plots the search intensity from Jan. 2007 to Dec. 2009 for the terms “Odd Even” and “Driving Restriction” from personal computers (i.e., non-mobile devices) using Baidu, the dominant search engine in China. Baidu’s search index based on searches from mobile-devices began from 2011. Beijing had a test run of the odd-even policy during August 17-20, 2007 where about half of the vehicles were restricted from driving. The official policy started on July 20, 2008 before the 2008 Summer Olympics. Starting from October 11, 2008, the policy was relaxed and vehicles were restricted from driving one day per week based on the last digit of the plate number, which continues to today.

Figure A7: Hedonic Replication with Mortgage Loan Sample



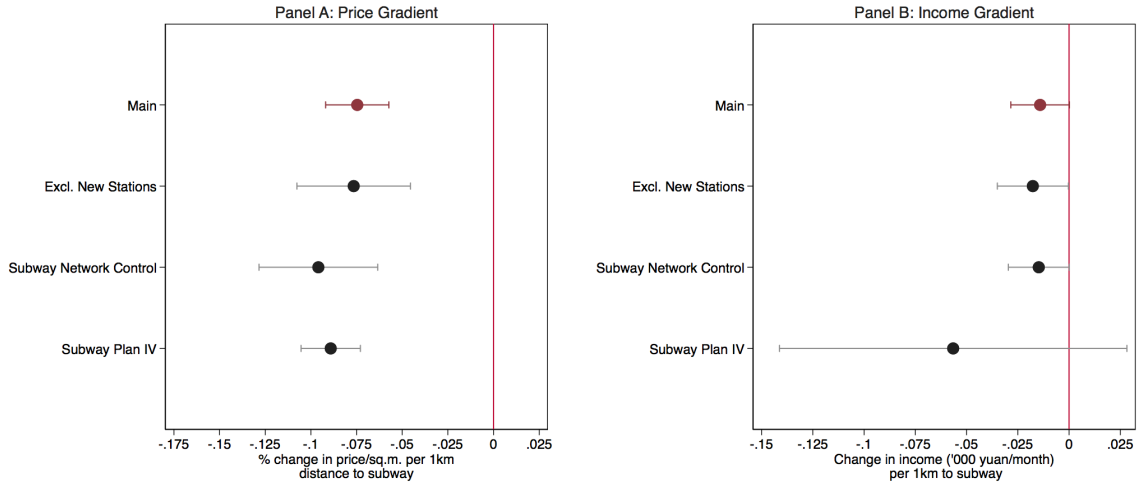
Note: Sample based on 9,640 observations from the mortgage loan data set. Each dot shows the change in $\ln(\text{price/sqm in } \text{¥}2007)$ for a 1 km increase in distance to the nearest subway station at each quarter between July 2006 and Sept 2010 relative to the omitted quarter, April-June 2008. Dashed lines show 95% confidence intervals. Controls include age, floor-to-area ratio, green space ratio, land area, and property management fee associated with the housing unit; number of units and number of buildings in building complex; zip code fixed effects, and year x month fixed effects. Standard errors clustered at zip code level.

Figure A8: Evolution of Neighborhood Income Before vs After New Subway Station Openings



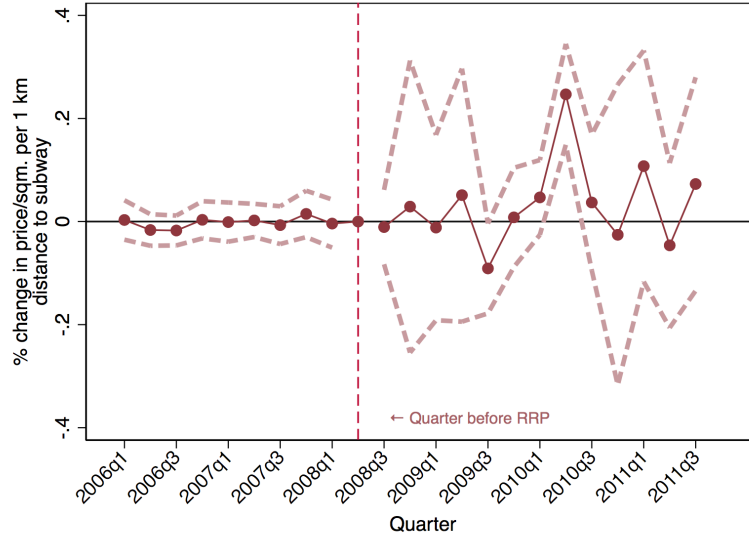
Note: Figure plots the difference in mean monthly household income in jiedao j in quarter q relative to quarter $q = 0$ when a subway station opens in jiedao j . Point estimates residualized by quarter-of-year fixed effects. Includes a balanced panel of 24 jiedao over 21 quarters. Sample spans 2005 through 2014. Bands show 95% confidence intervals. Source: Mortgage loan application data.

Figure A9: Sensitivity of RRP Effect Accounting for Subway System Growth & Network Effects



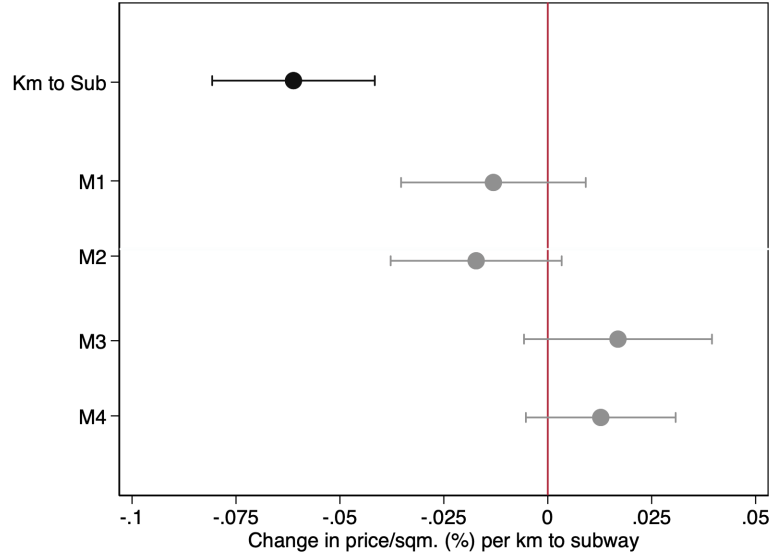
Note: Figure shows the effect of the RRP on the housing price gradient with respect to subway distance in **Panel A** as well as the RRP effect on the income gradient with respect to subway distance in **Panel B**. Each coefficient presents an alternative specification of Equation (1) in Panel A and Equation (2) in Panel B, respectively. Sample includes transactions spanning July 20, 2006 through July 20, 2010. **Panel A** models include fixed effects for unit type (resale or newsale), top floor, floor level, facing direction, no. bedrooms, decoration level, ownership type, total number of floors in the building, jiedao, month, year, subway line and district-specific linear time trends. Continuous controls include age, age², size, floor-area ratio, green space, property management fee, parking fees, and number of housing units and building units in complex. Standard errors clustered at the jiedao level. **Panel B** models include fixed effects for age, rank employer type, education, and experience of buyer as well as year and month of the transaction, subway line, zip code of housing unit, and district-specific linear time trends. “Main” is the RRP effect shown in column (5), Table 2. “Excl. New Stations” uses the sample of housing units in building complexes that do not change in their proximity to subway stations over the sample period. “Subway Network Control” includes a control of the subway network density and it’s interaction with distance to subway. Network density is the inverse distance-weighted sum of subway stations from each station location. “Subway Plan IV” instruments for distance to subway (and its interaction with RRP) using the locations of subway stations from Beijing’s 2003 plan following Li et al. (2019). See Section 5.3 for details. Bands show 95% confidence intervals.

Figure A10: Placebo Effect of Road Rationing on the House Price Gradient w.r.t. Subway Distance Among Housing over 3km from Station



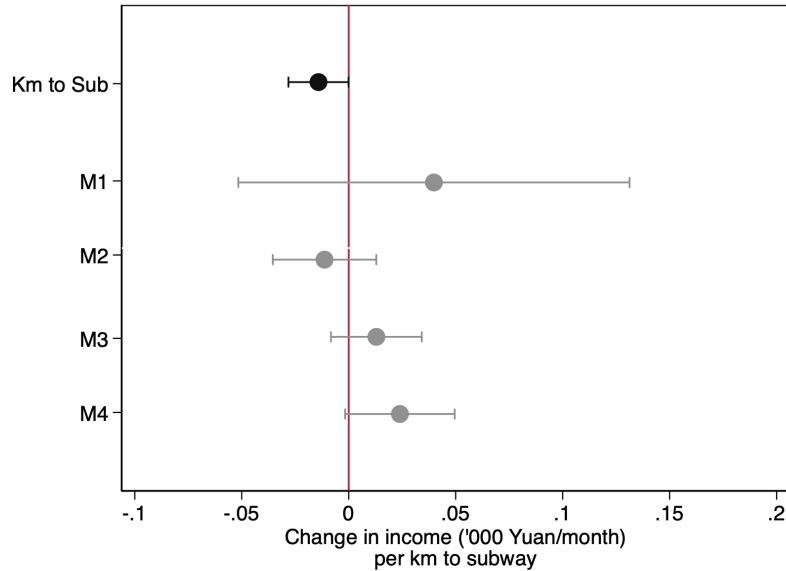
Note: Figure shows the partial effect of subway distance on housing price $\ln(\text{price/sqm in } \text{¥}2007)$ at each quarter between Jan 2006 and Dec 2011. The omitted quarter is April-June 2008. Dashed lines show 95% confidence intervals. Sample includes housing units located in building complexes that are over 3km from the nearest subway station through the event period; but are under 3km from a station after the event period ends (beginning in 2013). The sample includes 65,758 transactions. Controls include fixed effects for unit type (resale or newsale), jiedao, and year-quarter; as well as controls for age, age², size, floor-area ratio, green space, property management fee, parking fee, number of housing units and building units in complex, and unit size. Standard errors clustered at the jiedao level.

Figure A11: Placebo Effect of Road Rationing on the Price Gradient w.r.t Distance to Pollution Sites



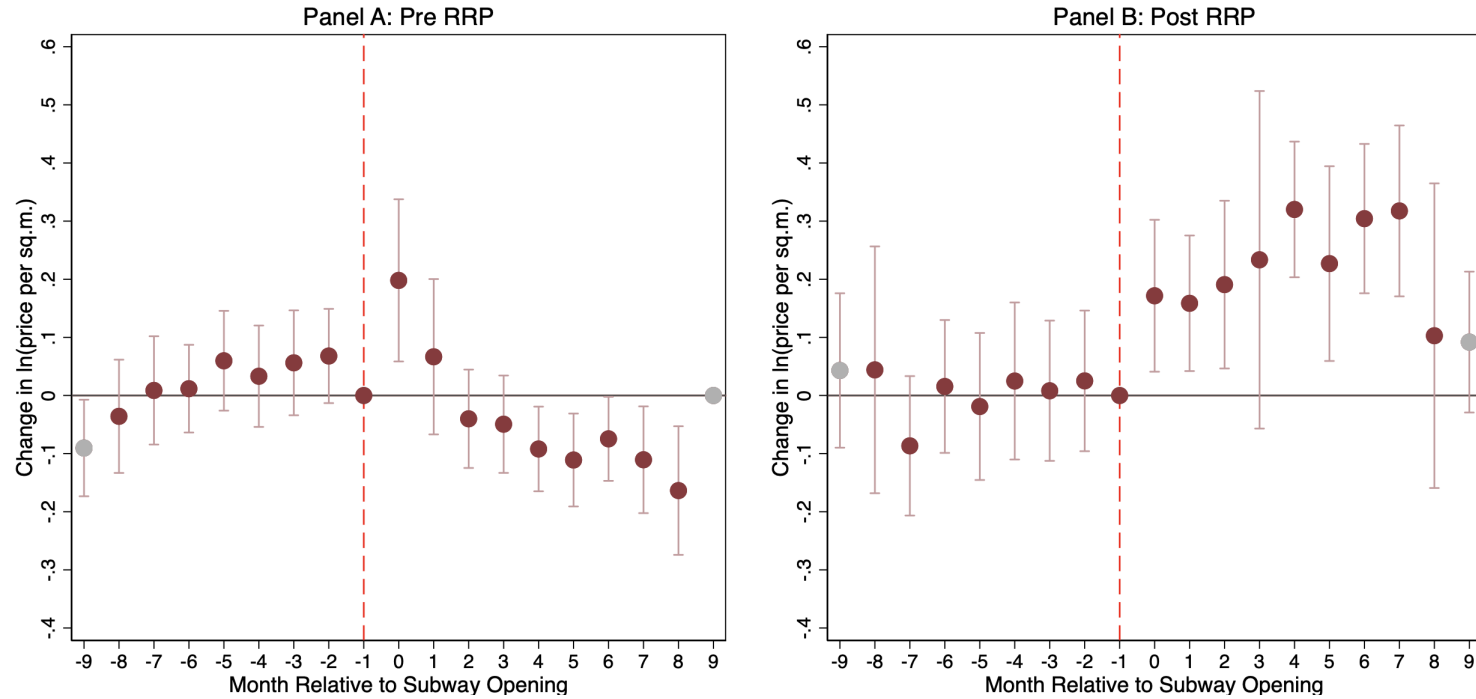
Note: The “Km to Sub” estimate in black shows the RRP effect on price per 1km change in distance to subway stations, and replicates column (3) in Table 2. The gray dots show estimates of the RRP effect on price per 1km change in distance to major pollution sites under various specifications. All specifications include year and month fixed effects. Bands show 95% confidence intervals. Sample includes 185,142 transactions, spanning July 20, 2006 through July 20, 2010. Model “M2” adds jiedao and subway line fixed effects and controls for: unit type (resale or newsale), age, age², size, floor-area ratio, green space, property management fee, parking fee, number of housing units and building units in complex, and unit size. “M3” adds year-by-month fixed effects. “M4” adds building complex fixed effects. Standard errors clustered at the jiedao level.

Figure A12: Placebo Effect of Road Rationing on Income Sorting w.r.t. Distance to Pollution Sites



Note: The “Km to Sub” estimate in black shows the RRP effect on household income per 1km change in distance to subway stations, and replicates column (3) in Table 3. The gray dots show estimates of the RRP effect on household income per 1km change in distance to major pollution sites under various specifications. All models include year and month fixed effects. Bands show 95% confidence intervals. Sample includes 18,021 transactions, spanning July 20, 2006 through July 20, 2010. “M2” adds neighborhood (zip code) fixed effects, subway line fixed effects, and controls for husband and wife age, employment rank, education, employer type, and tenure. “M3” adds year-by-month fixed effects. “M4” adds building complex fixed effects. Standard errors clustered by zip code.

Figure A13: The Effect of Station Openings on House Prices, Pre vs Post RRP



Note: Each dot shows the change in $\ln(\text{price/sqm in } \text{¥}2007)$ after a subway station opens within 1 km of a house at each month 9 months before and after the station opening, relative to the month prior to the opening. Controls include housing units that remain over 1km from a station as of December 2011. Dashed lines show 95% confidence intervals. Controls include fixed effects for the building complex, unit type (resale or newsale), year and month, and district-specific year-by-month trends; as well as controls for building age, age², size, floor-area ratio, green space, property management fee, parking fee, number of housing units and building units in complex, and unit size. Standard errors clustered at the jiedao level.

Table A1: Road Rationing Policy & Travel Mode Shifts

Panel A: Car share (%)			
	Restricted hour	Non-restricted hour	Difference
Restricted day	32.8	45.8	-13
Non-restricted day	42.0	47.7	-5.7
DID			-7.3

Panel B: Bus+subway share (%)			
	Restricted hour	Non-restricted hour	
Restricted day	22.9	12.7	10.2
Non-restricted day	15.9	12.3	3.6
DID			6.6

Note: This table shows the travel mode shares for trips during different hours. “Restricted Hour” applies if a trip’s starting time or end time is within the window of 7am-8pm. “Restricted Day” applies for a trip if the traveler is from a household owning a vehicle that is restricted from driving on the trip day. A vehicle is restricted from driving during restricted hours one day per week during workdays based on the last digit of the license plate number. The policy follows a preset rotation schedule in terms of which pair of numbers (one and six, two and seven, three and eight, four and nine, or five and zero) is restricted on a given workday (Yang et al., 2020). The shares are for 86,989 trips in vehicle-owning households from 2010 Beijing Household Travel Survey.

Table A2: Triple Difference Design: Effects of Subway Opening on Price, Pre vs Post RRP

	1km Threshold		2km Threshold	
	(1)	(2)	(3)	(4)
Treated Unit X Station Open x RRP	0.082*** (0.028)	0.107*** (0.034)	0.127* (0.070)	0.135** (0.066)
Treated Unit X Station Open	-0.023 (0.03)	-0.022 (0.029)	0.006 (0.044)	-0.006 (0.039)
Year and Month FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Building Complex FE	Y	Y	Y	Y
District x Year-Month Trend		Y		Y
Observations	66917	66917	43243	43243
Adjusted R^2	0.811	0.818	0.824	0.838

Note: The dependent variable is $\ln(\text{total price per square meter in 2007 real Yuan})$. “Treated Unit” is 1 for a housing unit which has ever been within 1km of a subway station during the sample period. “Station Open” is 1 if a station opens within the distance threshold of a housing unit. “Station Open” is 0 prior to a station opening and if a housing unit is always over the distance threshold from a station by the end of 2011. The distance “thresholds” are 1km for columns (1)-(2) and 2km for columns (3)-(4). Standard errors clustered at jiedao level. Controls include fixed effects for unit type (newsale vs resale), top floor, floor level, facing direction, no. bedrooms, decoration level, ownership type, and total number of floors in building; as well as building age, age², size, floor-area ratio, green space, property management fees, parking fees, and size, number of housing units and number of buildings of the complex. Sample includes transactions that occurred up to 9 months before and 9 months after station openings. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B Beijing’s Road Rationing Timeline

Beijing’s RRP went through several iterations between July of 2008 and January of 2011, generally leading to more lenient restrictions. There was some advance notice of this policy: the Beijing government enacted a 4-day trial period in August of 2007 and Beijing news media covered stories on the coming road rationing during early Spring of 2008. In June of 2008, government officials announced that road restriction would extend through the Olympic and Paralympic Games from July 20th to September 20th, 2008. This early iteration of the policy was the most restrictive: a driver could only use their car every other day based on whether the last number on their license plate was even or odd, and the restriction extended through the entire city of Beijing including the suburbs. The policy was enforced seven days a week nearly all day, except for three hours from midnight to 3am. At the end of this temporary restriction in September, the government decided to continue with the policy through April of 2009.

In its second phase from October 11, 2008 to April 20, 2009, officials relaxed the policy’s restriction on vehicle use to one day per week within (and including) the fifth ring road during week days from 6am until 9pm. A driver’s restricted day was based on the last digit of their vehicle’s license plate number. A driver’s relevant restricted day would rotate every four weeks. When this half-year trial ended, the government, again, announced an extension of the policy through April of 2020. This new policy restricted car use within, but excluding the fifth ring road, rotated restricted days every 13 weeks, and reduced the restricted hours from 7am to 8pm. This final iteration of the policy still stands today. Buses, taxis, and public-use vehicles for the police and military are not affected by the restriction ([Wang et al. 2014](#); [Viard and Fu 2015](#)).

Appendix C Comparison of the Road Rationing Price Effect with Prior Work

Our estimates of the price gradient with respect to distance to subway are larger than those of [Xu et al. \(2015\)](#), who find that the elasticity of price with respect to subway distance is -1.89%. We compare results directly in Appendix Table A3, where we employ the same 6-month time period, log-log specification, and start date for the RRP (October 11, 2008) as in [Xu et al. \(2015\)](#). We restrict our sample to only transactions executed by the same real estate firm used in their paper. Following [Xu et al. \(2015\)](#), we also exclude housing units located near newly-built subway stations, leaving 5,090 observations. We were not able to precisely match their sample of 5,990 observations, likely because our data originate from a different data provider. Columns (4) through (6) employ this paper’s RRP start date of July 20, 2008 for comparison purposes. We believe July 20th is the correct effective start date of the RRP because this was when the policy was implemented for the Olympic games. The policy restricted half of the vehicles from driving on a given work day based on the even or odd license numbers. The policy was modified and extended in October of that year.

In column (1), we attempt to replicate their main result by including all controls used in [Xu et al. \(2015\)](#), such as distance to the city center and dummies for whether the housing complex is located within a “key” school district. Specifically, we replicate column 8 of Table 2 in [Xu et al. \(2015\)](#). Our results are very close to their main estimate of -1.89%. The addition of jiedao fixed effects in column (2) and our detailed controls on the housing attributes in column (3) reduce the estimates slightly, but increase their precision.

After assigning July 20, 2008 as the start date of the RRP and employing our housing unit controls, the magnitude of the RRP effect is 30 to 50% larger than that of [Xu et al. \(2015\)](#) under this restricted sub-sample of 5090 observations. The reason we find a larger effect after using July 20 as the start date is likely due to the fact that the housing market responded to the RRP after it’s initial implementation between July and September, so estimates from [Xu et al. \(2015\)](#) may be biased toward a null effect. In further contrast, [Xu et al. \(2015\)](#) find that the post-RRP subway proximity premium was 50% larger than that of the pre-restriction premium. We interpret the pre-RRP premium coefficient with caution, as several confounding factors are likely correlated with subway proximity and housing desirability. However, for comparison purposes, our results suggest that the post-RRP premium is double the pre-RRP premium, substantially greater than [Xu et al. \(2015\)](#). This underscores that not only was the gradient shift larger than prior estimates suggest; but the magnitude of this gradient shift is of first order economic significance.

Table A3: Comparison of RRP Effects with Xu et al. (2015)

Outcome: ln(price/sq.m.)	RRP is October 11			RRP is July 20		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Km to Subway) x RRP	-0.017* (0.009)	-0.013* (0.007)	-0.012* (0.006)	-0.032*** (0.010)	-0.025*** (0.008)	-0.024*** (0.007)
Ln(Km to Subway)	-0.074*** (0.011)	-0.029*** (0.011)	-0.029*** (0.009)	-0.062*** (0.011)	-0.027** (0.011)	-0.023** (0.009)
Year and Month FE	Y	Y	Y	Y	Y	Y
Xu et al. Controls	Y			Y		
Jiedao FE		Y	Y		Y	Y
Full Controls			Y			Y
Observations	5096	5090	5090	3529	3524	3524
Adjusted R^2	0.301	0.503	0.601	0.287	0.471	0.568

Note: Dependent variable is ln(price per square meter in 2007 real Yuan). Standard errors clustered at building complex level. 'Xu et al. Controls' include ln(distance to city center), an indicator for locating within a key school district ln(size) age, age², indicators for decoration level, floor level, and top floor. 'Full Controls' include fixed effects for top floor, floor level, facing direction, no. bedrooms, decoration level, ownership type, and total number of floors in building; and continuous controls for ln(distance to nearest CBD), age, age², size, floor-area ratio, green space, property management fees, parking fees, and ln(size), ln(number of housing units) and ln(number of buildings of the complex). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$