Road Rationing Policies and Housing Markets

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June 11, 2020
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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 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 as well as central business districts. 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 and Beijing's central business districts shifts toward wealthier households, consistent with theoretical predictions of the monocentric city model with income-stratified transit modes. 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 markets, urban structure

JEL Classification Codes: R21, R41

1   Introduction

In an effort to combat congestion and air pollution, many governments of the world's largest cities have adopted road rationing policies that restrict drivers from entering urban centers.[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, Bogata, Colombia; Sao 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 central city driving restrictions.

Previous research has shown that urban driving bans can alleviate congestion and pollution (Viard and Fu 2015; Carrillo et al. 2016; Zhang et al. 2017; Barahona et al. 2018; Davis 2008). Yet, we know very little about the impacts 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. We show that urban road rationing policies can have potentially regressive consequences on access to economically important parts of cities through the housing market.

Our empirical approach relies on a monocentric model of urban land use that was first developed by Alonso (1964), Muth (1967), and Mills (1967) and later extended by LeRoy and Sonstelie (1983) (hereinafter, "AMM-LS"). The AMM-LS model remains the workhorse model in urban economics. A key prediction of the equilibrium outcome is that the rich will decentralize relative to the poor as long as faster modes of transit (e.g., a car) are more expensive than slower modes (e.g., public transit), a pattern which has been supported by several empirical studies of US cities (Margo 1992; Gin and Sonstelie 1992; Glaeser et al. 2008; Brueckner and Rosenthal 2009; Lee and Lin 2017). Surprisingly, however, this prediction has not been tested in a quasi-experimental setting. As Duranton and Puga (2015) state, whether the monocentric model can explain urban income sorting patterns is "still very much an open question."

To conduct a causal test of the AMM-LS model's predictions, two components are necessary: an

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[1]Over 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 air pollution in urban areas. For instance, they account for roughly 50% percent of particulate matter in Beijing (Viard and Fu 2015) and 20% in Delhi (Jain 2018).

exogenous shock to transportation costs and a shock that impacts one income group differently from others. Beijing's 2008 road rationing policy (hereinafter "RRP") uniquely affords both components. First, the policy generated an exogenous shock to transportation costs. People who previously drove to work were required to substitute to other modes of transit one day per week or else incur large fines. Second, transit mode choice is largely stratified by income group in Beijing. Personal automobiles are expensive relative to average income levels. Low income individuals generally rely on bus, subway, cycling or walking. These features make Beijing a particularly useful setting for testing predictions of the AMM-LS model because high income groups were more likely to be affected by the RRP than low income groups.

Our empirical analysis exploits a novel micro-level dataset that covers over 380,000 real estate transactions from 2005 through 2016, and 89,000 households from 2005-2014. We focus on two main predictions. First, an increase to the cost of commuting by car should increase demand for housing that is proximate to areas where people work and areas with easy substitution to alternative travel modes, namely subway stations. Second, as driving costs rise, the group formerly most likely to drive (in our case, the rich) should be more likely to outbid other groups for areas proximate to business districts and subway stations.

We proceed in two steps. Using a difference-in-differences research design, we first leverage the exogeneity of the RRP to measure how the premium for housing units proximate to business districts and subway stations changes as a consequence of the policy. Following Rosen (1974), we use the variation in house prices that is explained by variation in proximity as a measure for people's demand for proximity. Having examined the impact on the bid-rent gradient, we then estimate the causal effect of the RRP on the propensity of high income groups to live closer to business districts or subway stations than low income groups. Our unique data allow us to examine whether income-based sorting near business districts and subway stations is an important 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-distance gradients *within neighborhoods* of Beijing. These fine-scale fixed effects control for time-invariant differences across neighborhoods 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 observationally-similar properties that differ only with respect to their

distance to business districts or subway stations.

Despite the use of neighborhood 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. Beijing's subway network expanded considerably between 2000 and 2015.[2] Li et al. (2016) use variation in Beijing's subway expansion over this time to show that property values increased by almost 1% for every 1 kilometer reduction in distance to a station. In addition, Beijing's housing values experienced substantial appreciation during this time period. Prices grew an average of 13.6% per year, or roughly double that of the US housing market from 2000 to 2007 (U.S. Census 2019; Fang et al. 2016).[3] If Beijing's subway expansion and housing boom explain most of the change in housing prices 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 concerns via several robustness analyses, an out of sample validation test, and a placebo test. First, we estimate hedonic and sorting effects dynamically in order to test for the existence of pre-RRP trends. Our event studies show that the price premium (and sorting response) for proximity to subway stations and business districts changed only after the RRP, suggesting that general price appreciation in the housing market does not explain our results. Second, we limit our study sample to neighborhoods with pre-existing subway stations, 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 2.5 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 outside of our sample period. We find no differential trends in the bid-rent gradient directly following the RRP for houses in those neighborhoods. We interpret these findings as evidence that the post-RRP shift in demand for subway proximity observed in our main sample was unlikely to be driven by secular investment or gentrification trends in areas that received new subway stations. Finally, 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.

Our analysis provides two key findings. First, the RRP increased the premium for both proximity to subway stations and proximity to Beijing's central business districts. Our preferred estimates suggest that road rationing increased the price premium for a housing unit one kilometer closer to a

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[2]See Appendix Figure A1 for year-on-year expansions.

[3]Appendix Figure A2 plots the mean annual price per square meter for housing among transactions within our sample.

subway station by 3.7% (or \$3,032 for the average housing unit). Similarly, the price premium for a housing unit one kilometer closer to a central business district increased by 1.9% (or \$1,602 for the average housing unit) compared to before the policy. The baseline premium for proximity is stable in the periods prior to the RRP, but shifts significantly in the aftermath of the policy.

Second, we find modest but statistically significant evidence that the demographic composition of households near subway stations and business districts shifted toward high income households. This finding is consistent with our model's prediction and underscores income sorting as a mechanism that drives housing price growth in these areas following the RRP. Specifically, before the policy, a household earning 1,000 yuan more per month than another locates 7.7% closer (or about 1.4 kilometers) to the central business districts. After the policy, this disparity widens by 0.4% (or about one-tenth of a kilometer). These results imply that a household earning in the top 10th percentile of income would live approximately 1 kilometer closer to the central business district after the RRP relative to a median income household.

We find a similar magnitude for the sorting response near subway stations. At baseline, a 1,000 yuan difference in monthly household income is associated with a 9.7% difference in distance to subway stations. Post RRP, this imparity increased by 0.7% (or about one-tenth of a kilometer).[4] Data patterns suggest that these sorting results are driven by increased housing development and inflows of high income households 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.

This paper makes contributions to prior literature that are of theoretical and empirical consequence. A main prediction of the AMM-LS model is that there is a negative relationship between house prices per unit and distance from an urban core, yet empirical support of this pattern is mixed (Duranton and Puga 2015; McMillen 2006). A key contribution of our paper is that we utilize the RRP to estimate a *change* in the house price-distance gradient as opposed to a level estimate of this gradient. This quasi-experimental shock to transit costs combined with the fine spatial resolution of our data overcome limitations in prior literature by allowing us to compare outcomes within small, relatively homogeneous neighborhoods, thereby holding unobserved amenities fixed. In addition, we

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[4]The mean pre-RRP distance to a central business district (subway) in our sample is 18 km (13 km). A 0.4% (0.7%) effect size translates into one-tenth of a kilometer. The baseline difference in distance of 7.7% (9.7%) is derived from a regression of ln(Distance to CBD) (ln(Distance to Subway)) as a function of household income with year-by-month fixed effects.

provide the first causal evidence of the AMM-LS model's prediction that the distance-income gradient becomes steeper as travel costs increase. These results suggest that driving restriction policies, aside from impacting the housing market through the bid-rent gradient, can additionally impact the composition of income near transit accessible areas.

Our hedonic analysis of subway proximity fits into a large body of literature that demonstrates train and subway access capitalize positively into housing prices.[5,6] Redding and Turner (2015) provide a comprehensive review of this literature. Most of this literature relies on transit network *expansion* to infer valuation of access (Gibbons and Machin 2005; Billings 2011; Ahlfeldt and Wendland 2011; Li et al. 2016; Mayer and Trevien 2017; Tsivanidis 2019; Severen 2019). In contrast, our estimates can inform optimal provision of public transit when private vehicle use is *restricted*—a scenario increasingly prevalent among the world's largest cities.

The fact that our setting provides a mirror image shock to most prior work also has implications for understanding the relationship between inter-city location choices among the poor and public infrastructure access (Brueckner et al. 1999; Baum-Snow and Kahn 2000; Brueckner and Rosenthal 2009). While investment in transit infrastructure has been shown to attract the poor (Glaeser et al. 2008) to public transit and the city center, we demonstrate that increasing commute costs for the wealthy can actually disperse the poor as they are outbid by the rich in these areas. In general, our results underscore the importance of transportation infrastructure and transit regulation as determinants of urban spatial structure.

This paper also contributes to a nascent literature on China's subway expansion and infrastructure investment (Li et al. 2016; Chu et al. 2019). Xu et al. (2015) examines the impact of Beijing's RRP on demand for subway proximity. In addition to exploiting the most comprehensive dataset yet compiled on the housing market and home-owner demographics in Beijing, we provide new insight into this relationship by demonstrating that income sorting is an important mechanism explaining the increased premium for subway proximity.

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 develops our spatial equilibrium framework and explores patterns in the data that crudely support our model's predictions. We explain

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[5]In contrast, Manelici (2017) finds subway proximity capitalizes negatively into house prices following terrorist threats.

[6]More broadly, our paper contributes to a large literature on the role of public goods in determining house prices and residential location decisions. For example: Koster and Rouwendal (2017) shows that property values increase following public investment in historic amenities; Chay and Greenstone (2005) and Bayer et al. (2009) study the impacts of air quality improvements on housing values and gentrification; while Cellini et al. (2010) studies public school quality.

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 discuss data sources and sampling restrictions.

2.1 Road Rationing in Beijing

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). 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 as early as the Spring 2008 announcement or later in Fall 2008 once the policy was renewed following Olympic Games. In Section 5, we discuss robustness checks

that vary the start date to account for anticipation or delayed effects.

Viard and Fu (2015) found that Beijing's road rationing reduced particulate matter (PM_{10}) by 21%, with strong compliance and little evidence of inter-temporal substitution (i.e., driving more during non-restricted hours) or an increase in total number of vehicles in circulation, as in Davis (2008). In Beijing, it is difficult to evade detection because cameras throughout the city monitor the plates on vehicles. If an individual violates the restriction, they are fined 300 yuan (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. Precise statistics on the total number of violations that have occurred are difficult to find, however anecdotal evidence suggests that the probability of receiving a fine on a restricted day is between 20 and 40%. Beijing further restricts people's ability to purchase a second car, and purchase of a first car is regulated by a lottery system. The limited scope for noncompliance or substituting behavior were key to the success of Beijing's RRP in improving air quality.

Transit mode choice is stratified by income in Beijing. While we do not observe transit mode choice among the home buyers in our data, aggregated statistics on mode choice by income group from the 2010 Beijing Household Travel Survey (Beijing Transport Institute 2010) support our prediction that the RRP was binding mainly for the wealthy who can afford cars. Nearly 40% of earners in the top income bracket relied on cars to commute to work, while less than 20% of earners in the bottom income brackets relied on cars to commute. For public transit, just 14% of the top earners relied on public transit (subway or bus), whereas 20-21% of individuals in the bottom income bracket chose to commute by subway or bus. (Appendix Figure A3 plots the distribution of mode choice by income group as of 2010.) Although subway transit fares are heavily subsidized by the Beijing government at about $0.30 per trip,[7] walking and biking are the dominant modes of commute among most lower income groups (Beijing Transport Institute 2010).[8] Research by Gu et al. (2017) uses the 2010 travel survey and shows that wealthier commuters were twice as likely to switch from auto use to an alternative mode on restricted days compared to lower income groups who rely on public transit, biking or walking.

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[7]Fares became distance-based after 2014, however trips within the 4th Ring Road (under 22 kilometers) were still relatively cheap, averaging about 5 yuan per trip.

[8]Chu et al. (2019) use entry of bike sharing programs in 10 major Chinese cities as a natural experiment in a cross-city hedonic analysis. They find suggestive evidence that bikes serve as a complement to subway use by solving the "last mile problem."

2.2 Data Sources & Description

Our empirical analysis requires information on housing prices, residential locations, and household income. We assembled this information from two separate datasets. The first dataset consists of individual real estate transactions. Sourced from two major Beijing real estate firms, these data cover approximately 17% of all housing purchases within Beijing from 2006 through 2012. The second dataset consists of mortgage loan applications sourced from an anonymous government-backed mortgage provider.[9] Our sample of mortgage contracts cover the universe of all home purchases in Beijing that made use of a government-backed mortgage loan from 2006 to 2013. While this sample includes the majority of mortgage loans made in Beijing during the sample period, the precise market share is difficult to quantify because 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) are not represented in our data. Mortgage refinancing is uncommon in China, and there are no refinancing observations in the sample. As a result, each mortgage contract refers to a housing transaction.

Each housing transaction contains the sale or rental price as well as 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 mortgage loan data also provide information on housing unit price and—importantly—detailed information on the loan applicant's demographic characteristics, including their income, education levels, and place of employment.

Our sample of housing units are concentrated near the city center and near subway lines of Beijing (as shown in Appendix Figure A4). The city is divided into 18 districts indicated by the thick green lines, 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

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[9]The anonymous mortgage provider operates as a government-backed credit market to encourage home ownership. In the absence of a developed credit market, people can access loans through their employers (Tang and Coulson 2017). 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.

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 dataset. 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.

Figure 1 shows the location of Beijing's major employment centers. Like many major cities, Beijing does not have one singular central business district, but several subcenters.[10] The city's geographic center is a cultural and consumer-oriented district that, itself, is surrounded by several business district clusters. These clusters, like the "Financial District" (Xicheng) or the "Commercial District" (Wangfujing), contain most of the employment in Beijing. For this reason, we define the Beijing "central business district" (CBD) as a relative measure and assign the closest major employment center as a given housing unit's CBD. Our results are generally insensitive to alternative definitions of Beijing's CBD, as demonstrated in Section 5.

Our identification strategy exploits variation in the housing price-distance gradient before and after the RRP within a neighborhood of Beijing. 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 restrict our analysis to housing units within the Sixth Ring Road of Beijing which skews the sample of neighborhoods toward those with smaller square areas.[11] Our sample covers roughly two-thirds of the 300 jiedao within Beijing and over 90% of the 200 zip codes.

Table 1 provides summary statistics on a subset of the variables available within each of our datasets. The purchase price varies considerably more in the real estate data than in the mortgage dataset. The mortgage data housing units are also further from the city center and further from subway stations, on average. This is partially due to the selection of individuals represented by the mortgage data, which overrepresent the middle class demographic. Home buyers able to utilize the government-backed mortgage loan system are less likely to be entrepreneurs or wealthy households.

The Beijing government enacted several city-wide policies after 2011 that likely impacted the

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[10]While the economic literature on urban spatial structure often assumes cities are circular or radially symmetric, a growing literature has acknowledged the polycentric structure of cities (e.g., Anas et al. 1998; Lucas and Rossi-Hansberg 2002; Harari 2016). Lucas and Rossi-Hansberg (2002) shows that negative bid-rent gradients toward an urban core will theoretically arise even in a city with an initial uniform distribution of employment.

[11]Appendix Figure A5 shows the cumulative distribution of jiedao size within our sample compared to that of all Beijing.

housing market and demand for automobiles.[12] 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 A6. 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 it's 2009 level.

3 Theoretical Framework

We predict how a city-wide driving restriction policy will affect the sorting of low relative to high income individuals through the stylized AMM-LS monocentric city model and its extension by LeRoy and Sonstelie (1983). We then discuss application of our model to Beijing's RRP and explore relevant data patterns across house prices, proximity, and household income.

3.1 Residential Equilibrium & the Bid-Rent Function

Consider a monocentric city with a CBD where all residents supply work at wage w to earn y income. Residents live outside the CBD in the residential district of the city. Commute time increases monotonically with distance from the CBD. There are two possible modes of traveling to work: car or subway. Consumers decide where to live, choosing a pair of location parameters (x, δ) where x is the distance from an individual's residence to the CBD and δ is the distance from an individual's residence to the nearest subway station. Consumers choose the pair (x, δ) so as to maximize consumption of housing h with price p and the numeraire good z with price one. Individuals commute distance x if they commute via car and distance $(x + \delta)$ if they commute via subway. Transit time is denoted as $T(n, x, \delta)$ and is a function of distance to CBD x, distance to the nearest station δ, and the likelihood of commuting via subway n. Discussed in greater detail below, n can vary with commuter type (rich versus poor) and the existence of road rationing days. The monetary time-cost of commuting is $w \cdot T(n, x, \delta)$.[13]

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

[13] Realistically, commuting costs will include pecuniary costs like tickets for a subway or gas for a car in addition to time costs. For tractability, we model urban land use as dependent on time costs alone, however including these pecuniary costs in $T(n, x, \delta)$ does not alter the baseline model predictions as long as commuting costs are linear in distance (Duranton and Puga 2015).

Our empirical approach focuses on price and sorting effects over a relatively short time horizon of one to three years, consequently we assume that the city boundary is fixed, housing supply is fixed, and the city is closed, without in or out migration. Residents face the following problem where they maximize consumption of housing and the numeraire good subject to their budget constraint:

$$\max_{z,h} U(z,h) \text{ s.t.}$$

$$z + ph + w \cdot T(n,x,\delta) = y$$

The time-cost of commuting $w \cdot T(n,x,\delta)$ reduces income because residents forgo wage earnings by allocating part of their time each day commuting. Transit time $T(n,x,\delta)$ from any distance x to the CBD depends upon the transit mode and the speed of the transit mode. Driving transit time is x/ν, where ν is driving speed. Subway transit time includes two components. First, walking time to the subway station δ divided by walking speed ω; and second, time on the subway to the CBD, x/σ, where σ is subway speed. Together, total transit time via subway is $\frac{\delta}{\omega} + \frac{x}{\sigma}$. For any x, we assume driving is a faster mode of transit than taking the subway: $\frac{x}{\nu} < (\frac{\delta}{\omega} + \frac{x}{\sigma})$. We define transit time as follows:

$$T(n,x,\delta) = n\Big[\frac{\delta}{\omega} + \frac{x}{\sigma}\Big] + (1-n)\frac{x}{\nu} \tag{1}$$

In equilibrium, each resident has achieved their highest utility subject to their budget constraint; and all residents i must have equal utility levels: $\max_{z,h} u_i = \bar{u} \forall i$. Solving this system of equations, we have the condition that each resident chooses a housing location (x,δ) such that:

$$\frac{\partial p}{\partial x} = -\Big[\frac{n}{\sigma} + \frac{1-n}{\nu}\Big]\frac{w}{h} \qquad \text{and} \tag{2}$$

$$\frac{\partial p}{\partial \delta} = -\Big[\frac{n}{\omega}\Big]\frac{w}{h} \tag{3}$$

In Appendix B, we show the derivation of Equation 2 and 3 in greater detail. Equation 2 shows that the price of housing falls with distance enough to compensate individuals for their longer commutes. This is the spatial equilibrium condition that is the basis of the AMM-LS model. Ex ante, both choice of x and δ matter for individuals commuting via subway, but only the choice of x matters for individuals commuting via car. Equation 2 describes the bid-rent for distance from the CBD, holding δ constant, whereas Equation 3 describes the bid-rent for distance from a subway station holding x constant.

3.2 Heterogeneity by Mode Choice

Groups facing different preferences for housing consumption h and time costs $w \cdot T(n,x,\delta)$ will have distinct bid-rent gradients. LeRoy and Sonstelie (1983) develop the case where a high-income group, possessing greater opportunity cost of time, commute via car. Cars are more expensive but a faster

transit mode relative to public transit. The poor rely on public transit since it is not economical for them to commute via car. This setting is quite applicable to Beijing where wealthier people use cars and the lower income rely on public transit. Following LeRoy and Sonstelie (1983), assume there are two consumer types i: rich and poor. Each consumer type will differ in their wage, housing consumption, and likelihood of commuting via subway. We introduce an index i for each of these variables (w_i, h_i, n_i) where $i = p, r$. Subscript p denotes the housing consumption, wage, and transit mode of the poor and r the same for the rich. For the poor, n_p approximates 1 because the poor are very likely to rely on subway transit. The rich rely on cars, but the rich may use either mode depending on effective road rationing days, thus: $0 \le n_r < n_p \sim 1$.

Rearranging Equation 2, the poorer group will have a steeper bid-rent gradient than the rich and, therefore, live closer to the city center if and only if:

$$\frac{h_r}{h_p} > \frac{\partial T(n_r, x, \delta)/\partial x}{\partial T(n_p, x, \delta)/\partial x} \times \frac{w_r}{w_p} = \frac{n_r/\sigma + (1 - n_r)/\nu}{n_p/\sigma + (1 - n_p)/\nu} \times \frac{w_r}{w_p} \tag{4}$$
$$\approx \frac{n_r/\sigma + (1 - n_r)/\nu}{1/\sigma} \times \frac{w_r}{w_p}$$

Beijing's RRP, operationalized by an increase in n_r, serves to change the ratio of transit time costs for the rich car-users relative to the poorer subway-users. As road rationing pushes a rich resident to utilize the subway, the ratio $\frac{n_r/\sigma + (1-n_r)/\nu}{1/\sigma}$ moves closer to parity, and thus Equation 4 is *less* likely to hold following Beijing's RRP. This logic tells us that higher-income individuals will find housing close to the city center more attractive, and will consequently move closer to the city center following the road rationing.

Similarly, poor households will live closer to subway stations, conditional on x, if Equation 3 is steeper for the poor relative to that of the rich:

$$\frac{h_r}{h_p} > \frac{w_r}{w_p} \frac{n_r}{n_p} \tag{5}$$

As n_r increases, road rationing makes Equation 5 less likely to hold. Collectively, this logic tells us that higher-income individuals will move closer to the CBD and closer to subway stations, conditional on location x, following city-wide road rationing.

3.3 Application to Beijing's Road Rationing Policy

An advantage of our setting is that we isolate *shifts* to the slope, as opposed to the actual slope, of the bid-rent for distance to the CBD ($\frac{\partial p}{\partial x}$ in Equation 2) and distance to subway stations ($\frac{\partial p}{\partial \delta}$ in Equation 3). Beijing's RRP served to *shift* both gradients. Consequently, we can identify plausibly exogenous changes to the bid rent and changes in demographic composition through estimating the

second derivative of both equations with respect to n as follows:

$$\frac{\partial^2 p}{\partial x \partial n} = -\frac{w}{h}\Big[\frac{1}{\sigma} - \frac{1}{\nu}\Big] \tag{6}$$

and

$$\frac{\partial^2 p}{\partial \delta \partial n} = -\frac{w}{h}\frac{1}{\omega} \tag{7}$$

Equation 6 provides the rate of change in the price versus distance-to-city-center trade off, all else equal, following road rationing. Similarly, Equation 7 provides the rate of change in price versus distance-to-subway station trade off, all else equal, following road rationing. Our empirical approach will estimate changes in both bid-rent gradients, and then test predictions of the model on sorting of lower relative to higher income households near the CBD following Equation 4 and subway stations following Equation 5.

3.4 Preliminary Evidence

We show preliminary evidence that the bid-rent gradients have shifted over time based on our raw data. Figure 3 plots the bid-rent gradients before and after the RRP. The difference in slopes shown in these figures are, respectively, estimates of Equation 6 for the price-distance to CBD gradient and Equation 7 for the price-distance to subway gradient. The price premium per square meter for subway access clearly increases following the RRP in Panel B. The premium for access to the nearest CBD in Panel A is less striking, but still demonstrates a tilt from the pre-RRP period. These price dynamics are intuitive if both the rich and poor are competing for housing proximate to subway stations, but less so for proximity to the nearest CBD. The poor are not impacted by the road rationing, thus do not compete for housing close to the city center. Both rich and poor, however, prefer housing closer to subway stations, all else equal.

Figure 4 shows the relationship between income and distance to the nearest CBD in Panel A and the nearest subway station in Panel B, respectively. The decentralization of the lower-income relative to higher-income households is prevalent in the raw data. Both gradients have become steeper over time, particularly for subway proximity. In the post driving restriction period, Beijing appears similar to older US and European cities in its demographic spatial structure as average income declines with distance from the CBD (Brueckner et al. 1999). However, these patterns are a marked shift from the pre-driving restriction period, when the relationship between income and CBD proximity was weaker. Noticeably in Panel B, mean distance to a subway station fell over time for all income groups as the system expanded.

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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 CBDs; and second, the residential location choices of low- relative to high- income households. We use a difference-in-differences estimator to isolate the causal effects of the RRP. 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 during the 24-month window from July 2007 to July 2009. 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 2009. We show in Section 5 that results are robust to alternative window lengths.

4.1   Estimation Framework

We first explore how Beijing's transit policies impact demand for housing near the city's CBDs and subway stations. Let i index housing units, j index jiedao, t index the day of a transaction, and q index one of 28 quarters from 2005 to 2011. We estimate the effect of the RRP on the distance-to-subway (and the distance-to-CBD) price premium through the following event study specification:[14]

$$\ln(p_{ijt}) = \sum_{q=1(\ni 15)}^{28} \kappa_q (Km_{it} \times D_q) + \alpha Km_{it} + \mathbf{X_{ijt}}\theta + \gamma_j + \tau + \varepsilon_{ijt} \qquad (8)$$

where p is the price of housing per square meter, Km_{it} measures the distance to the nearest subway station or business district, and D_q is a quarter dummy. $\mathbf{X_{ijt}}$ 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. When Km denotes distance to the nearest subway station, we include a control for distance to the nearest CBD in $\mathbf{X_{ijt}}$, and vice versa for when Km denotes distance to the nearest CBD. The subway proximity regression further includes fixed effects for the subway line of the nearest station. These fixed effects remove variation in the connectivity of different subway lines. Specifically, we remove variation in demand across highly centralized lines like Line 10 (Daxing Line) relative to peripheral lines like the Airport Express. With these additional controls, we can interpret κ_q as the change in the bid-rent holding distance to other location amenities fixed, consistent with Equations 2 and 3. Lastly, γ_j and τ are jiedao and time fixed

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[14]Our results are robust to alternative specifications (log-log or linear-linear) and time windows (24 months or 6 months before and after the RRP). These results are available upon request.

effects, respectively. Jiedao fixed effects 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. τ includes year and month fixed effects in our main specification, but we apply finer-scale time fixed effects in various specifications.

The parameters κ_q measure the difference in the housing price-distance gradient in quarter q relative to the baseline, the third quarter of 2008. We cluster standard errors ε_{ijt} at the jiedao level. We estimate the coefficients of interest, κ_q using variation in CBD and subway proximity *within* a jiedao. The relevant variation that identifies κ_q differs slightly across the distance-to-CBD hedonic regressions versus the distance-to-subway hedonic regressions. Distance to the nearest CBD does not vary over time for a given housing unit. Consequently, for specifications where Km_{it} measures distance to the nearest CBD, κ_q is identified off of cross-sectional variation within a jiedao. In contrast, for specifications where Km_{it} measures the distance to the nearest subway station, parameters κ_q are also identified off of time-wise variation in subway proximity. This is because Beijing's subway system expanded during our time period of interest, thus some housing units became closer to the nearest subway station over time. As a robustness check in Section 5.3, we restrict our subway distance hedonic regressions to housing units that do not vary in their proximity to the nearest subway station. Our conclusions remain the same whether or not we identify the hedonic parameter off of housing units that become closer to stations over time.

Several factors can confound empirical estimates of price-distance and distance-income gradients, such as proximity to high quality schools or proximity to recreational amenities (i.e., parks or restaurants). Omission of such unobserved factors will bias the price-distance gradients, either downward in the case of positive correlation of proximity and desirable amenities; or upward in the case of a proximity and dis-amenities, such as the prevalence of noise and congestion near CBDs or subway stations.[15] The key to our empirical approach is that we estimate *shifts* in these gradients caused by the driving restriction rather than the gradients themselves: $\frac{\partial p}{\partial x}$ in Equation 2 or $\frac{\partial p}{\partial \delta}$ in Equation 3. Beijing's RRP served to shift these gradients. By exploiting this plausibly exogenous policy change, we can identify a causal impact of the driving restriction on proximity premiums Our underlying assumption to identify a causal effect in Equation 8 is that prices would have trended in parallel for observationally similar housing units within the same neighborhood in absence of the RRP.

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[15]Beijing's subway network is entirely underground, thus the infrastructure itself does not alter the visual appeal of a particular location. However, there may be some dis-amenity from living directly beside a subway station due to heavier foot traffic or noise.

We next explore how the RRP impacted location decisions of high income relative to low income households. In our model, house prices adjust as a result of higher income groups outbidding lower income groups for proximate locations. Inequalities in Equations 4 and 5 provide conditions for lower income groups to live closer to CBDs and subway stations, respectively. These conditions are *less* likely to hold following the RRP if the rich increase their use of the subway relative to the poor; that is if the ratio n_r/n_p increases. We predict n_r will increase relative to n_p following the RRP because wealthier income groups utilize private vehicles to commute more than lower income groups (as shown in Appendix Figure A3). The driving restriction will require that wealthier commuters adjust their commute mode choice away from driving toward public transit. In contrast, lower income households will be less directly affected by the policy since they are the incumbent public transit users. As the ratio n_r/n_p increases, our model predicts that the average income in areas proximate to subway stations and business districts will increase.

To test this prediction of our model, we employ data on household income, demographics, and location from the mortgage application data. We estimate the relationship between household income and proximity over 28 quarters spanning the RRP policy from 2005 through 2011 in a flexible event study specification:[16]

$$\ln(Km_{izt}) = \sum_{q=1(\ni 15)}^{28} \alpha_q(I_{izt} \times D_q) + \psi \ln I_{izt} + \mathbf{Z}_{izt}\theta + \zeta_z + \tau + \mu_{izt} \qquad (9)$$

where i indexes housing units (equivalently households), z indexes zip codes, t indexes the day of a mortgage application, and q indexes one of 28 quarters. Km_{izt} is the distance from housing unit i to the nearest subway station or business district, I_{izt} is the monthly household income for the primary and secondary earners purchasing housing unit i at time t, and D is, as before, a series of indicator variables equal to one for each quarter from 2005 through 2011. We use the same framework of controls in 9 as in 8 with a few exceptions. First, in place of housing unit characteristics, we include controls for demographics of the primary and secondary earners of the household in $\mathbf{Z}$, 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 in place of jiedao fixed effects. As discussed in Section 2, data limitations prevented us from creating a spatial correspondence between zip codes and

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[16]As with Equation 8, our results are robust to alternative time windows (24 months or 6 months before and after the RRP). The distance to CBD-income regression is robust to alternative log-log and linear-linear specifications, but the distance to subway - income regression becomes less precise with the log-log specification. This is because variation in distance to subway throughout Beijing is relatively limited, and the dispersion is further compressed by a log transformation. These results are available upon request.

jiedao, however, they both delineate small neighborhoods of Beijing. As before, we include controls for distance to the nearest subway in $\mathbf{Z}$ when Km denotes distance to the nearest CBD, and vice versa so as to isolate the partial effect of the RRP on sorting near CBDs or subway stations, respectively. Lastly, time fixed effects τ control for macro-economic trends effecting all housing units. In our main specification τ includes year and month fixed effects. The parameter α_q measures the change in distance to the nearest CBD or subway given a 1,000 yuan change in monthly income in quarter q relative to the third quarter of 2008, quarter 15 in our sample. We cluster standard errors μ_{izt} at the zip code level.

4.2 Identification Challenges

Our empirical approach is subject to two main threats to identification of κ_q in Equation 8 and α_q in Equation 9. The first one is unobservables that could have changed at the time of the policy and also affect the outcome variables (prices or location choices). That is, price changes could have occurred absent the RRP if areas near CBDs and subway stations were on a differential growth trajectory relative to areas far from CBDs and subway stations. For instance, if dining, shopping, and entertainment establishments grew over time in the same areas as subway stations and CBDs, then bid-rent gradients could have become steeper over time due to growth in these amenities, as opposed to the RRP. If these correlated time-varying trends capitalized positively into housing prices, an ordinary least squares (OLS) estimate of κ_q in Equation 8 would over-estimate the effect of the RRP. If, however, correlated time-varying trends are "bads", like noise and congestion, an OLS estimate of κ_q would under-estimate the effect of the RRP.

Similarly, OLS would not provide a consistent estimate for α_q in Equation 9 if mean proximity to CBDs or subway stations would have increased for higher income households absent the RRP. In other words, if amenities like high quality shopping and dining grew over time near subway stations or CBDs, and these types of amenities disproportionately attract higher income individuals, then the composition of income near these areas would have increased regardless of the road rationing policy.

The identifying assumption is that housing prices and location choices would have trended similarly with respect to their distances to subways and CBDs in absence of the RRP. We can test the plausibility of this assumption by examining trends in the price-distance or distance-income gradients in quarters leading up to the policy. If the difference in price or income between proximate and far housing units is stable in periods leading up to the RRP, it is likely that such differences would have remained stable in absence of the RRP. To test these assumptions, we present dynamic effects

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specifications that allow for the treatment effects of proximity on prices, and income on distance to flexibly vary over time. We test, for instance, whether news coverage of the driving restriction in early 2008 (Hooker 2008) generated a response in the housing market prior to the policy's start date. The dynamic effects specification also allows us to visually examine if amenities (or dis-amenities) have grown near CBD or subway stations over time, and if these are capitalized into housing prices prior to start of the RRP in July of 2008. We further address this identification threat by including controls for district–specific[17] linear time trends, as well as specifications with year-by-month fixed effects. Since some districts contain more stations and are closer to CBDs than others, these controls will remove unobserved growth (or decline) in proximity-correlated amenities that may impact prices or sorting.

The second main threat to identification is reverse causality. That is, proximity to CBDs or subways could have changed *as a consequence of* housing price or income growth over our time period. In this paper, we assign central business district centroids as static points that are unchanging over time by construction. Consequently, it is not possible in our setting for the location of CBDs to dynamically respond to gentrification patterns. Beijing's subway network, however, was growing over this time period, with additional lines and additional subway stops. 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. In general, we do not find evidence that new subway stations were more likely to open in gentrifying neighborhoods of Beijing.

5   Empirical Results

We now present evidence on the connection between housing demand and transit policies in Beijing. The RRP increased transit costs for drivers. Our model, following AMM-LS, predicts that changes in n_r - the likelihood of commuting via subway for the rich - should shift the price-distance gradient as well as the residential location choices of those formerly relying on personal car travel relative to those relying on public transit.

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[17]Beijing is divided into 18 districts, as shown in Appendix Figure A4.

5.1 Road Rationing and Property Values

In following with the AMM-LS model's original prediction, we begin by discussing results of how the RRP affected the distance-to-CDB premium. Throughout this paper, we display regression results in terms of the house price discount for distance. The absolute value of this discount can be equivalently interpreted as the house price premium for proximity. First, we test the assumption that housing prices for proximate relative to distant housing units would exhibit common trends absent the RRP. Figure 5 Panel A shows estimation results of Equation 8. Each dot shows the change in the distance-to-CBD housing price premium in each quarter relative to the RRP quarter of July-October 2008. Our event study uses all years in our sample up through 2012 to portray dynamic effects. We exclude years after 2012 because Beijing's a license plate lottery in 2012 would likely confound estimates of the RRP since both policies impacted commuting costs. Prior to the policy, the price discount for distance-from-the-CBD is not significantly different from the road rationing quarter. There is a reduction in price of 1 to 2% as distance increases by 1 kilometer following the RRP, which translates into a 1 to 2% increase in the premium for one kilometer of proximity. Because housing units do not have as much variation over time in their distance-to-nearest-CBD within a jiedao as they do distance-to-subway stations, each individual time-specific point estimate is often statistically insignificant. However, there is a visible trend break following the RRP.

Regression results in Table 2 show that CBD proximity commands between 1.3 and 1.9% (or between $1,079 and $1,602) higher price per kilometer after the road rationing. The use of jiedao fixed effects in column (2) significantly increases the magnitude of the policy effect, implying unobserved dis-amenities can attenuate estimates of the price-distance to CBD gradient. The jiedao fixed effects also serve to absorb cross-sectional variation between kilometers to CBD and price, which explains the insignificance of distance to CBD once jiedao fixed effects are included. The price premium remains similar in magnitude after adding controls in Column (3), although the standard error becomes smaller. Column (4) uses year-by-month fixed effects, such that variation is driven by transactions within the month of July 2008. In column (5), we apply controls for district-specific linear time trends to account for district-level growth that may be spatially correlated with subway proximity. The point estimate is stable across these various specifications.[18]

We next consider the effect of the RRP on demand for living close to a subway station. Panel

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[18]These results generally hold when we fix a CBD to be one of several business districts as shown in Appendix Table A1. Using our preferred specification with district-specific time trends (col (5) of Table 2), these alternative CBD estimates are smaller in magnitude as a result of measurement error in assigning the relevant CBD, but comparable to our main results.

B of Figure 5 shows estimates of κ_q from Equation 8 where Km is distance to the nearest subway station. Each dot shows the change in the distance-to-subway housing price premium in each quarter relative to the RRP quarter July-October 2008. Price per square meter across housing units that differ in their distance to subway stations did not vary significantly in the time periods leading up to the RRP. However, starting in the first quarter following the RRP, the discount for subway distance decreased by approximately 4% per kilometer. In other words, the premium for subway proximity increased by 4%. Around April of 2011, there is an additional decrease (uptick) on the price discount (premium) corresponding to tightened government restrictions on the circulation of license plates.

Our mean difference-in-differences estimates in Table 3 reaffirm that the price-distance to subway gradient became steeper following the RRP. Results are robust to alternative controls, and finer time-location fixed effects.[19] A one kilometer increase in distance (proximity) to a subway station renders a 1.8% to 4.8% price discount (premium) prior to the RRP policy. This level estimate may be biased if stations are sited endogenously, however we interpret the positive sign as consistent with subway distance (access) being undesirable (valuable) to city residents, all else equal. The interacted term shows that after the RRP, housing units one kilometer further from a subway station sell for 3.7% (or \$3,032) less than a comparable housing unit one kilometer closer to a subway station based on our preferred specification in column (5). These results suggest that demand for subway proximity increased by more than double that of pre-RRP levels.[20]

The RRP increased the premium for subway access only within walking distance of subway stations. Figure 6 displays the non-parametric relationship between subway proximity and price following the RRP by plotting the mean price change for each half-kilometer bin relative to housing units outside of five kilometers from subway stations. The premium falls to zero after approximately three kilometers, or about 1.8 miles. Intuitively, subway proximity has no impact on housing prices outside of a reasonable walking distance from the station. These results support our interpretation that demand for the subway network—as opposed to correlated, unobserved amenities—is the mechanism driving the price premium for proximity following the RRP.

We assess the magnitude of our results using a back–of–envelope value-of-time calculation. The

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[19]These results are similar in magnitude and significance after excluding subway line fixed effects. These results are available upon request.

[20]We replicate the main results of Xu et al. (2015) in Appendix C. Our estimates of the price-distance to subway gradient 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 attribute this difference to having a more representative dataset as well as using an RRP start date in July following the initial roll out of the policy.

magnitudes of our price premium results are between 22 and 68% of the hourly wage, based on the CBD proximity premium and the subway proximity premium, respectively.[21] These magnitudes seem plausible, considering prior literature finds a value-of-time to wage ratio of about 50% (Small 2012).

Our results show that Beijing's subway station access came to be in much higher demand relative to CBD access following the RRP. Expansion of the subway network made transit-oriented housing locations more desirable for all marginal subway riders. However, the policy made housing closer to the CBD more desirable only for marginal drivers with a high value of time. Those wealthy enough to drive their cars are a relatively smaller portion of Beijing's population compared to those reliant on public transit, thus it is not surprising that the subway price effect is much stronger than the CBD price effect. Both results, however, are consistent with the AMM-LS prediction that changes to transit costs will alter the price-distance gradient.

5.2   Road Rationing and Demographic Sorting

The prior section established that price-distance gradients for both business district and subway access became steeper following the RRP. We now explore whether gentrification is a potential mechanisms driving these price effects. This is an empirical question: lower income groups may continue to outbid the rich for locations proximate to CBDs and subway stations if developers supply smaller housing units with a higher price per square meter. On the other hand, if the RRP is more binding for the rich relative to the poor, Equation 6 and Equation 7 are less likely to hold and the average income of households proximate to either CBDs and subway stations will increase.

Figure 7 shows estimates of α_q from Equation 9. Each dot shows the relative change in the distance-income gradient relative to the RRP quarter. These event studies use all seven years from 2005 through 2011 to portray dynamic sorting effects. Each panel portrays sorting responses that are generally consistent with the model's prediction: wealthier (poorer) households move closer to (further from) both subway stations and the nearest CBD relative to lower (higher) income households after the RRP.

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[21]We calculate hours saved by moving one kilometer closer to either the CBD or a subway station as follows: walking speed of 15 min/km × 2 walks per day × 1 driving restricted day per week × 50 working weeks per year × 2 commuters per household × a 20-year housing tenure ÷ 60 min/hour = 1,000 total hours saved per household by moving 1km closer. Hedonic results in Tables 2 and 3 suggest the mean increase in purchase price for a housing unit 1 km closer to the CBD and subway was about $1,000 (6,800 yuan) and $3,000 (20,400 yuan) per kilometer, respectively. Dividing these premiums by the 1,000 hours saved gives an implied value of time of $3 (20.4 yuan) and $1 (6.8 yuan) per hour. As of 2007, the average resident in our sample living within 5 km of either the CBD or a subway station with non-zero income earned approximately 6,000 yuan per month, or about 30 yuan per hour (assuming they work 200 hours per month). Consequently, our hedonic CBD and subway estimates are about 22% and 68%, respectively, of the hourly wage in our sample.

In Panel A, the outcome variable of interest is distance to the nearest CBD. The difference in CBD proximity across the income distribution is generally stable, and not statistically different from the RRP quarter in periods prior to July of 2008. However, the two quarters prior to the RRP exhibit some pre-trends, suggesting that higher income individuals were more likely to move to locations closer to CBD relative to lower income groups before the policy went into effect. The Beijing government enacted a trial period in late 2007 and Beijing news media covered stories on the coming road rationing during the first quarter of 2008. Consequently, it is possible that these pre-trends reflect adjustment to new information on the RRP. Indeed, Google Search trends in Appendix Figure A7 suggest individuals may have anticipated the policy in late 2007 and early 2008. As a robustness check in Appendix Table A2, we excluded the first two quarters of 2008 from our sample and obtained estimates that are similar to each of our main results.

After the RRP, the composition of wealth increases (falls) in areas proximate to (far from) the nearest CBD. The estimated magnitudes imply that a household earning 5,000 yuan more per month than another household (approximately one standard deviation in our sample, or $8,500 per year) is likely to live 2.5 percentage points (or about half a kilometer) closer to the nearest central business district after the road rationing relative to the poorer household.[22]

Table 4 shows the mean change in the income-business district proximity gradient comparing 12 months before versus 12 months after the RRP. The level estimate of household income in column (1) is negative, suggesting that higher income households are more likely to live proximate to CBD (consistent with Panel A of Figure 4). The gradient becomes steeper after the policy, and is more than double the baseline gradient estimate in some specifications. Column (2) adds zip code fixed effects, column (3) adds controls for buyer demographics, while columns (4) and (5) add year-month fixed effects and district-specific time trends, respectively. The point estimate is stable across these specifications. Results in columns (2) to (5) show that a household that is 1,000 yuan wealthier (poorer) is likely to locate 0.4 to 0.5 percentage points closer to (further from) the nearest business district after the RRP compared to before. These results are similar after employing alternate definitions of the Beijing CBD, as shown in Appendix Table A3.

We show changes in the distance to subway-income gradient over time in Panel B of Figure

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[22]We calculate this magnitude as follows: one standard deviation of monthly income in our sample is 4,714 yuan. The average change in the distance-to-CBD - income gradient is 0.5% per thousand yuan (a point estimate of 0.005). Thus $4.714 \times 0.5\% = 2.4\%$. The average pre-CDR distance to CBD in our sample is 18 km, 2.4% of which is approximately half a kilometer.

7. There is an evident trend break. After the RRP, higher income households differentially sort near subway stations. The gradient shift estimate is small and statistically insignificant in the quarters immediately following the RRP, but becomes marginally significant and larger in magnitude after the second quarter of 2010. There is no significant evidence that higher income households differentially sort near subway stations in periods leading up to the road rationing policy.

Table 5 shows the average difference-in-difference estimate of the RRP effect on the subway distance-income gradient. As before, each column adds successive controls. Column (2) includes zip code and subway line fixed effects, column (3) includes demographic controls, and columns (4) and (5) include year-by-month and district-specific time trends, respectively. The results show that a household with 1,000 yuan more (less) income per month is likely to live 0.7 percentage points closer (further) from the nearest subway station after the RRP relative to before the policy. These estimates imply that a household earning 5,000 yuan more per month than another household (approximately one standard deviation, or $8,500 per year) will live approximately half a kilometer further from a subway station following the RRP.

While these results on demographic shifts appear small, they explain approximately 30% of the overall increase during our study period in the relationship between proximity and household income.[23] This suggests that city-wide policies aimed at reducing traffic and air pollution can be potentially regressive because they not only increase the premium for center-city locations, but they also increase competition for housing near public transit, the mode choice disproportionately utilized by lower income groups. In absence of Beijing's aggressive subway investments, the housing market and demographic sorting responses would likely be stronger.

The potential regressivity of a city-wide driving restriction will depend upon the level of enforcement and the potential for behavioral adjustments. For example, if the purchase of new cars were unregulated, wealthy car drivers could circumvent the license-plate based road rationing by purchasing a second car, as in the case of Mexico City (Davis 2008). Such offsetting behavior would likely mitigate a road rationing policy's effect on the housing market and demographic sorting. Beijing's rigorous enforcement meant that car owners could only adjust through a combination of using public transit and relocating within the city to reduce their total commute time.

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[23]The rate at which kilometers-to-CBD fell with income was -3.2% in 2005, and increased to 5.2% in 2011. The distance to CBD-income gradient estimate in Table 4 of -0.5% is approximately 25% of this difference. Similarly, the rate at which kilometers-to-subway fell with income was -3.5% in 2005 and -5.5% as of 2011. The distance to subway - income gradient estimate in Table 5 of -0.7% is approximately 35% of this difference. We estimate mean gradients using regressions of $\ln(Km)_{it} = \delta\text{Monthly Income}_{it} + \epsilon_{it}$ 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 housing 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.[24] 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).

We next 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. Relying purely on time-series variation, rather than spatial variation in subway expansion, results of the bid-rent gradient shift for subway proximity are consistent with our main results. The bid-rent gradient in Appendix Figure A9 demonstrates a substantial tilt following the RRP. Appendix Table A4 shows estimates of the price-distance to subway gradient for this restricted sample. The gradient change is attenuated but within a standard deviation of our main results in Table 3. For housing units with no change in their proximity to the nearest subway station, the RRP increased the premium for these units by an additional 2.7% per kilometer compared to 3.7% estimated from our full sample in Table 3 (based on our preferred specification in column (5) of both tables). We interpret these results as a lower bound on the increased demand for subway proximity. The subway stations near this housing subsample are some of the oldest lines and stations in the system network (Beijing's oldest lines were built in 1969 and 1971, without substantial additions until the mid-2000's) thus proximity to these areas may be

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[24]Appendix Figure A1 shows how the subway network has expanded substantially over the last two decades, particularly since 2010.

less desirable to the extent that these lines offer less network advantages and may run less efficiently compared to the newer lines.

Lastly, 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 outside of walking distance from a subway station up through 2013, but came to be within walking distance 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 walking 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 caused the price-distance to subway gradient shift, as opposed to increased demand for the subway itself. We define "walking distance" as three kilometers of a station, based on our non-parametric estimates in Figure 6. Appendix 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 upward trend, as in Figure 5.

Network Density—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. We construct a measure of a location-specific network density over time. We divide our study area within the 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. Appendix Table A5 shows that the additional subway network density control does not change our baseline results. Further, network density and its interaction with kilometers to subway appear to provide no additional information on the variation between distance and price. The coefficient estimates are near zero with small standard errors.

Placebo Analysis—We conduct a final 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 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 8 and 9. Some major pollution sources may also be employment locations. Unfortunately, we are unable to observe employment associated with the pollution sources. 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 3 kilometers away from a major CBD. This leaves a sample of 114 pollution sites throughout Beijing, out of a total of 449 sites. Appendix Figure A11 compares our main estimates on the price-distance gradient shifts for subway and CBD proximity shown in black to that of proximity to major pollution locations shown in gray. "M1" through "M4" denote various specifications of the hedonic regression. The placebo estimates are generally smaller in magnitude than our main hedonic estimates, and are statistically indistinguishable from zero. 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 the distance-income gradient for pollution sites are statistically indistinguishable from zero. These findings demonstrate that price-distance gradients and income sorting did not change in all parts of the city after the RRP. Rather, the RRP increased desirability of areas that are most accessible to major business districts of the city.

5.4   Sorting Mechanisms

While the prior analysis documented how the RRP increased the price and mean 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 subways and CBDs over time.

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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 and CBDs. We then take the mean of new builds across neighborhoods, and plot these values by month in Figure 8. Panel A shows that new development increased after the RRP in areas within 5 kilometers of subway stations more so than areas over 5 kilometers from subway stations. Panel B shows that new development exhibited a similar upward trend in areas both close and far from Beijing's CBDs. These figures suggest that housing supply adjusted to the increased demand for housing near city centers. This may explain why the price effects for proximity shown in Figure 5 reverted to pre-RRP levels in the three years following the RRP.

We repeat this non-parametric approach in Panels C and D of 8 with a focus on count of high relative to low income households. We first look only at households living within 5 kilometers of either subway stations or CBD. 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. Panels C and D show 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 these figures, it does not appear that lower income households were displaced from proximate locations. Instead, these patterns are suggestive that newly-built housing in desirable areas of Beijing are occupied by higher income households.

6   Conclusion

Road rationing policies are an increasingly common policy instrument used among major cities 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 the residential location decisions and urban spatial structure. Urban land use theory provides predictions on how such policies could impact the housing market and the sorting of demographic groups relative to one another. Although the monocentric city model remains a dominant tool to explain urban spatial structure, quasi-experimental tests of its predictions on bid-rent gradients are somewhat mixed. Quasi-experimental tests of its predictions of income sorting are fewer still.

This paper uses detailed, micro-level data on home purchases and buyer demographics in combination with a city-wide road rationing to empirically test these theories in the context of Beijing,

China. Our analysis relies on fine-scale spatial fixed effects to control for location-specific correlates of public transit. We find that Beijing's road rationing policy (RRP) required prospective home-buyers to pay an additional $3,000 on average for a one-kilometer reduction in distance to a subway station. The premium for central business district proximity also increased, but by a smaller magnitude of approximately $1,000 per kilometer. Considering that the average Beijing household earns approximately $12,000 (86,000 yuan) per year, these effect sizes are economically large. We additionally utilize novel micro data on household income and housing locations to explore how the RRP impacted the residential location choices of different income groups. Following the policy-induced shocks to housing prices, the composition of households living close to subway stations as well as close to Beijing's CBDs shifted toward wealthier households. We find that a household earning one standard deviation more in annual earnings was more likely to live half a kilometer closer to subway stations and business districts.

These results underscore the relevance of the transit-based explanation championed by LeRoy and Sonstelie (1983) to explain patterns of income sorting in cities. Further, by focusing on a scenario where urban commuting costs *increase*, our study provides rare insight into the universality of the monocentric city model. Where prior literature has shown that declining transportation costs have flattened bid-rent gradients over time (McMillen 1996; Ahlfeldt and Wendland 2011), our study shows the reverse holds and price-distance gradients become *steeper* when transportation costs increase.

From a policy perspective, our study provides evidence that city-wide road rationing policies can have the unintended consequence of limiting access to public transit for lower income individuals. Such effects are likely to be stronger in markets where car ownership is cost-prohibitive to the poor and when enforcement of road rationing is strict. Our findings expose 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 a welfare transfer process 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 welfare impacts and the distributional consequences of market-based versus command-and-control approaches warrants future research.

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Figure 1: Business Districts of Beijing
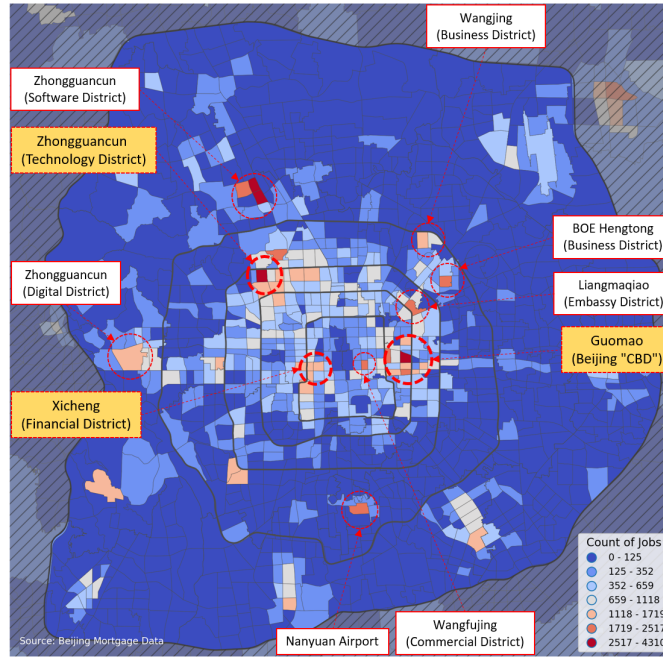
Note: Figure shows employment concentration by transportation analysis zone (TAZ), the sampling unit used by the Beijing Transportation Institute for their commuting surveys. "Count of Jobs" measures the frequency from 2005-2014 with which home buyers reported the location of their employer in that TAZ. Employer locations sourced from the mortgage application data. See Section 2 for details.

Figure 2: Neighborhood Variation
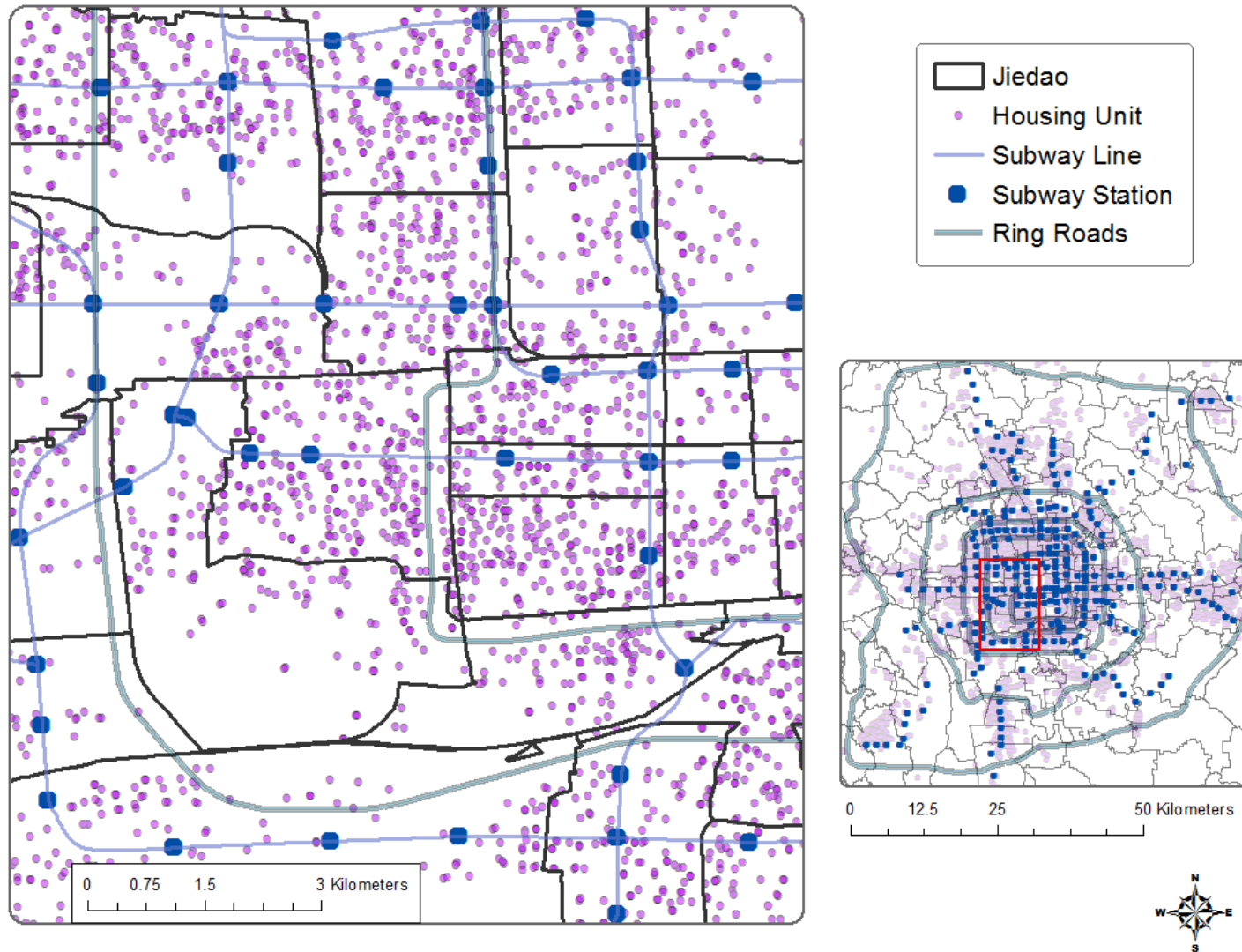
Sources: Beijing Real estate data; mortgage application data. Figure shows a southwest section of central Beijing, between the second and third ring roads. Roard Rationing Policy effects are identified based off of variation in distance to subway stations, or distance to the nearest CBD across housing units *within* a jiedao.

Figure 3: The Price-Distance Gradient and Beijing's Road Rationing Policy
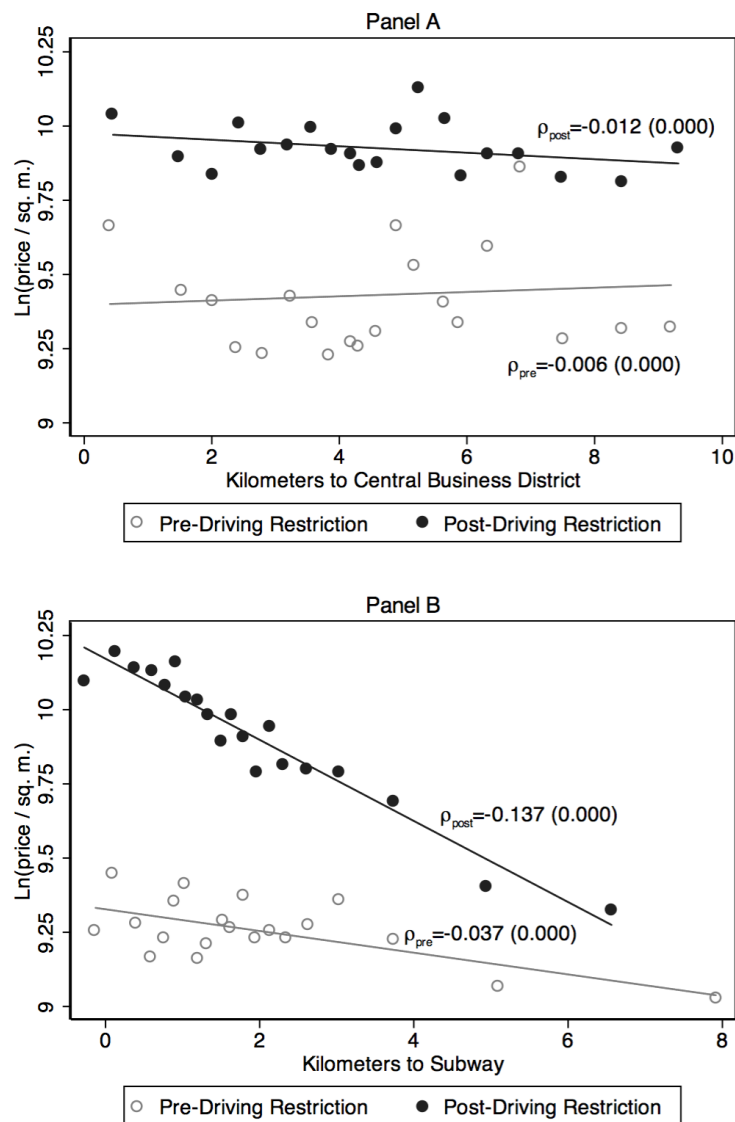
Note: Figures plot the mean ln(price/sqm in ¥2007) for each of 20 distance bins. Each dot represents 6,200 and 12,870 observations per bin in pre- and post-RRP periods, respectively. Panel A means are residualized by distance to the nearest subway. Panel B means are residualized by distance to the central business district. "Central Business District" defined as the closest of 7 main business districts. ρ(pre) and ρ(post) are regression coefficients. Includes years 2005-2014. Source: Real estate transaction dataset.

Figure 4: The Distance-Income Gradient and Beijing's Road Rationing Policy
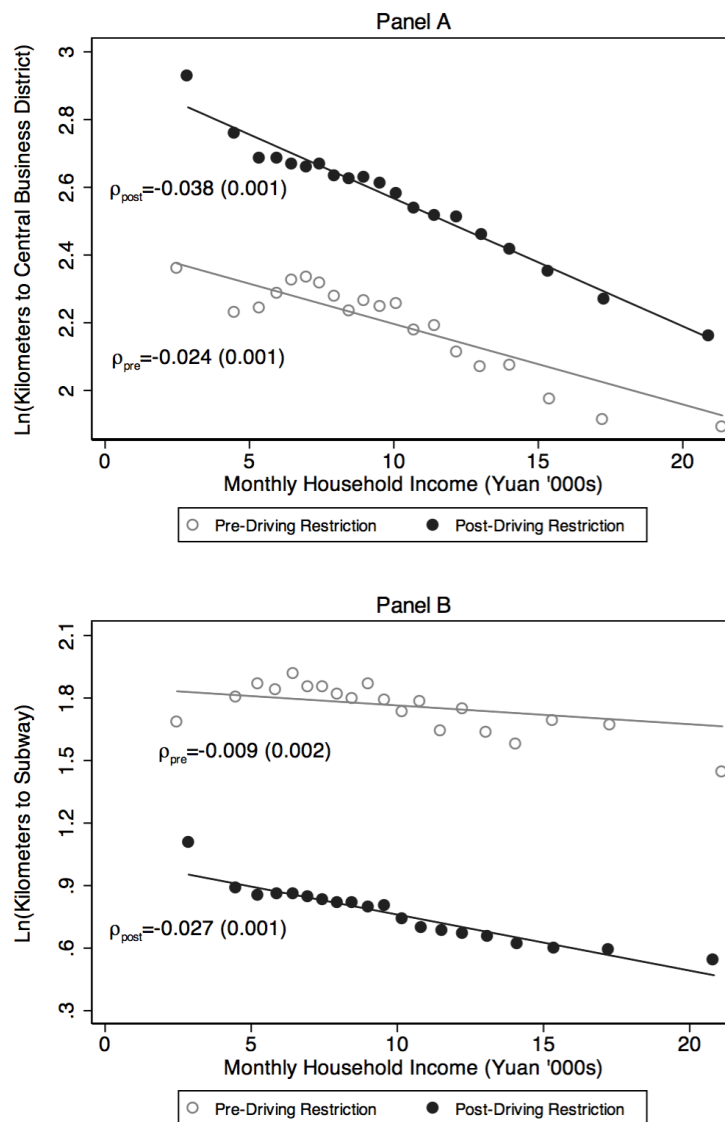
Note: Figure plots mean distance for each of 20 income bins. Each dot represents 840 and 3670 observations per bin in pre- and post-RRP periods, respectively. Panel A means are residualized by ln(distance to the nearest subway). Panel B means are residualized by ln(distance to central business district). "Central Business District" defined as the closest of 7 main business districts. ρ(pre) and ρ(post) are regression coefficients. Includes years 2005-2014. Source: Mortgage application dataset.

Figure 5: The Effect of the Road Rationing on the House Price Premium for Proximity
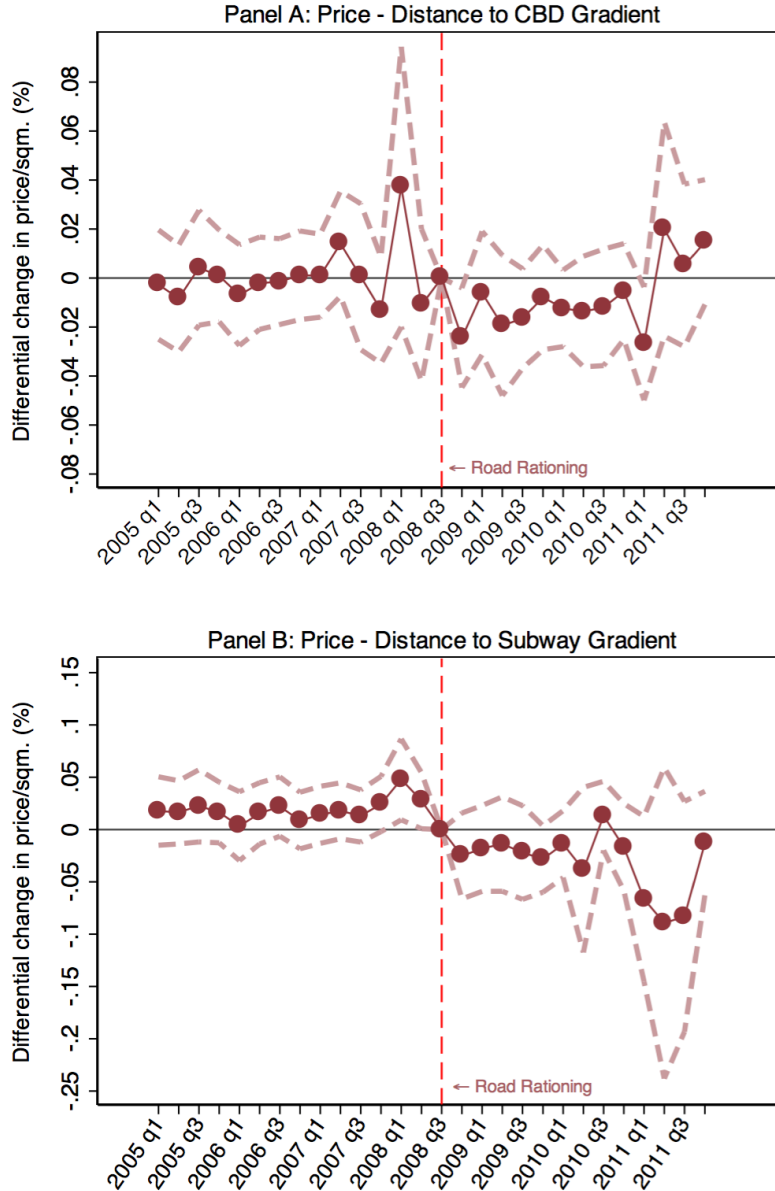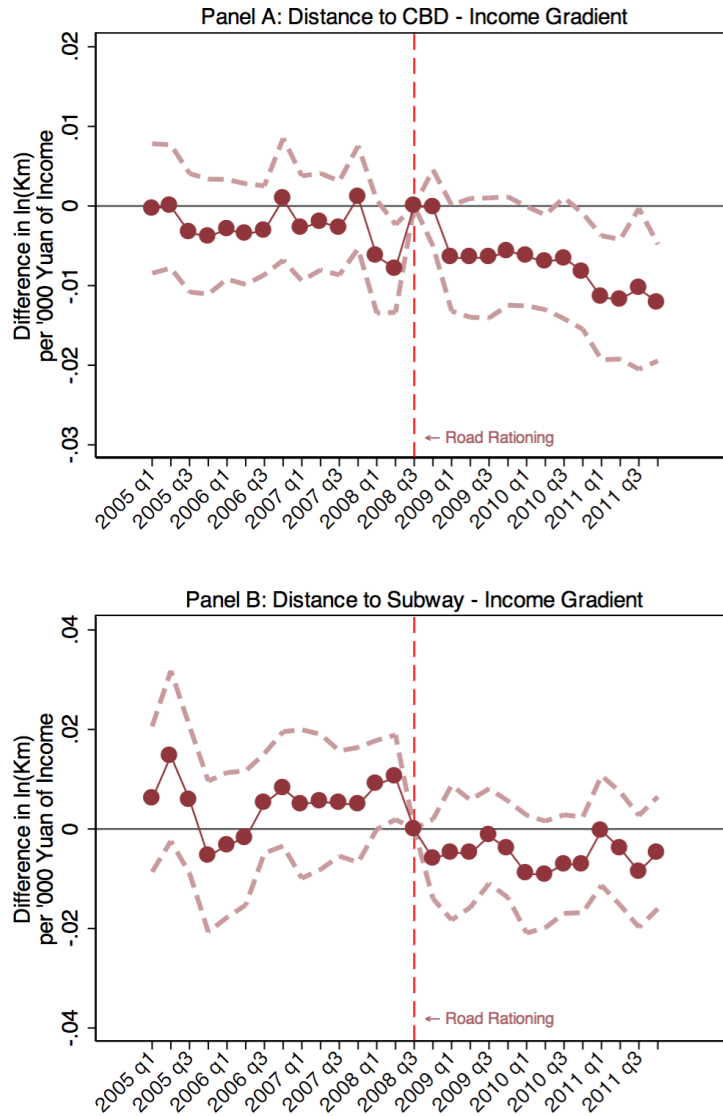

Note: For both figures: each dot shows the change in ln(price/sqm in ¥2007) at each quarter between Jan 2005 and Dec 2011 relative to the omitted quarter, July-October 2008. Dashed lines show 95% confidence intervals. Controls include fixed effects for unit type (resale or newsale), jiedao, and year-quarter; as well as controls for age, age^2, 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 jiedao level. Panel A controls for distance to subway. CBD defined as the closest of 7 main business districts. Sample restricted to units located within 10km of their closest CBD. Panel B controls for distance to CBD and subway line fixed effects to capture network heterogeneity across stations.

Figure 6: Average Effect of Road Rationing on Housing Prices by Subway Distance Bin
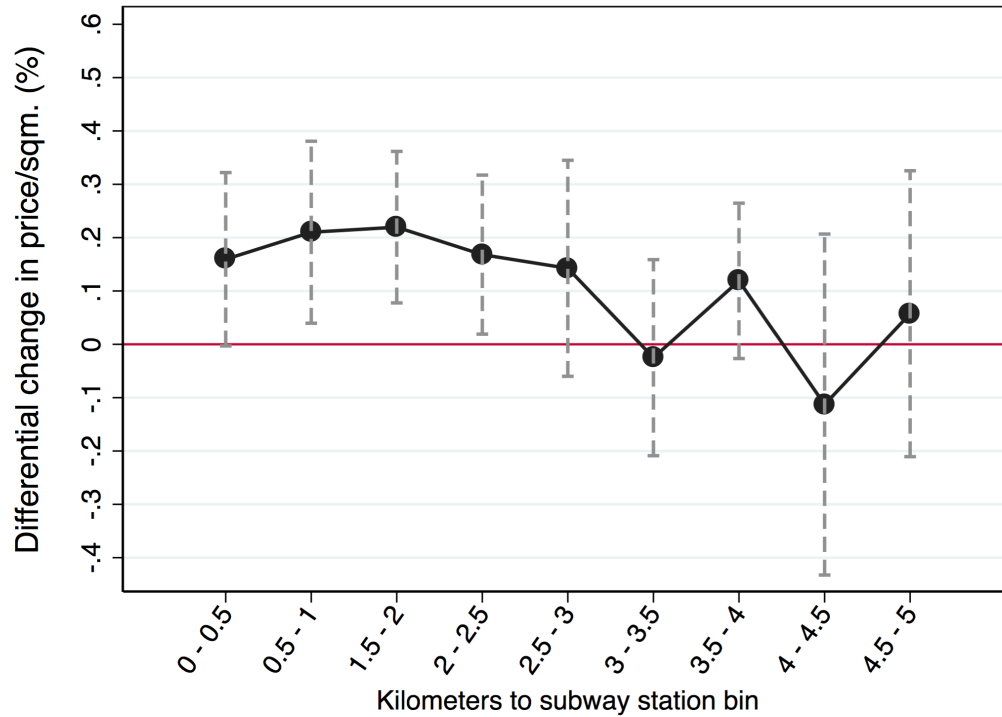

Note: Figure plots α_b, estimates of the average change in ln(price/sqm in ¥2007) following the RRP by half-mile distance bins to subway stations (B_b): $\ln(y_{ijt}) = \sum_b \alpha_b(B_b \times RRP_t) + \sum_b \kappa_b(B_b) + \rho RRP_t + \mathbf{X}_{ijt}\theta + \gamma_j + \tau_t + \varepsilon_{ijt}$. The reference bin includes housing units over 5 kilometers from subway stations. Dashed lines represent 95% confidence intervals. Includes transactions from July 2005 through July 2011. 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, age^2, size, floor-area ratio, green space, number of housing units and building units in complex, and unit size. Standard errors clustered at jiedao level.

Figure 7: The Effect of Road Rationing on the Distance - Income Gradients

Note: Panel A shows the partial effect of income on distance to the nearest CBD. Panel B shows the partial effect of income on distance to the nearest subway station. For both figures: each dot shows the change in ln(distance) per thousand yuan of monthly income at each quarter between January 2005 and December 2011 relative to the omitted quarter, July-October 2008. Dashed lines show 95% confidence intervals. We use similar controls as in Figure 5 but we replace housing unit controls with demographic controls, and we utilize zip code fixed effects. Demographic controls include fixed effects for year, month, neighborhood, age, rank, education, and experience of buyers. Standard errors clustered by zip code.

Figure 8: Road Rationing & Average Supply of New Housing by Neighborhood
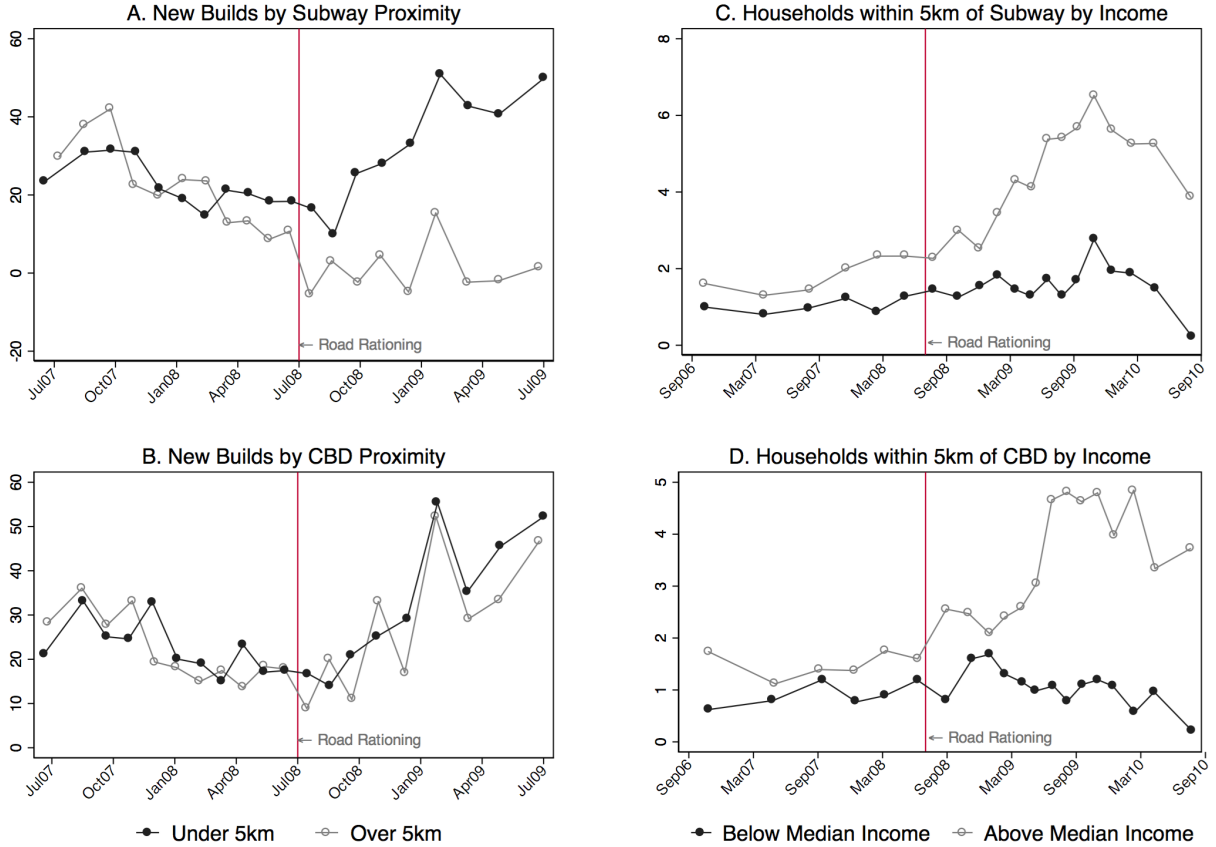
Note: Panels (A) and (B) plot the mean new supply of housing by month for units under versus over 5km of subway stations and CBDs, respectively. Sourced from the real estate transaction dataset. Panels (C) and (D) plot mean number of households living within 5km of both Subway stations and the nearest central business district by income group. Median income is based on the pre-RRP income distribution and is calculated from all mortgage applications occuring from January 2005 through July 2008. Sourced from the mortgage application dataset. Means in all panels residualized by neighborhood fixed effects.

Table 1: Estimation Sample Descriptive Statistics

	Real Estate Transactions		Mortgage Data	
	(1) Mean	(2) (St. Dev)	(3) Mean	(4) (St. Dev)
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)
Unitsize (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)			Bachlor'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 CBD Gradient: Mean Estimates

Outcome: ln(price/sq.m.)	(1)	(2)	(3)	(4)	(5)
Km to CBD x RRP	-0.005	-0.014**	-0.013***	-0.013***	-0.019***
	(0.010)	(0.007)	(0.005)	(0.005)	(0.007)
Km to CBD	-0.035***	-0.005	0.012	0.010	0.015
	(0.009)	(0.017)	(0.017)	(0.016)	(0.017)
Avg Proximity Premium / Km	$425.59	$1187.01	$1141.44	$1079.57	$1602.42
Jiedao FE		Y	Y	Y	Y
Controls			Y	Y	Y
Year-Month FE				Y	
DistrictxYear-Month Trend					Y
Observations	82002	82002	82002	82002	82002
Adjusted R^2	0.184	0.513	0.609	0.619	0.614

Note: Dependent variable is ln(total price per square meter in 2007 real Yuan). Standard errors clustered at jiedao level. Sample spans July 20, 2007 - July 20, 2009. All specifications include year and month fixed effects. Average price premium is evaluated at a unit size of 122 sq.m., the size at the mean distance to the nearest business district (5km and 7km), and at a conversion rate of 6.95 yuan per USD. 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 distance to nearest subway station, age, age^2, 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 House Price-Distance to Subway Gradient: Mean Estimates

Outcome: ln(price/sq.m)	(1)	(2)	(3)	(4)	(5)
Km to Subway x RRP	-0.046**	-0.041***	-0.038***	-0.033***	-0.037***
	(0.020)	(0.011)	(0.011)	(0.011)	(0.009)
Km to Subway	-0.048***	-0.004	-0.016	-0.011	-0.013
	(0.014)	(0.011)	(0.009)	(0.008)	(0.010)
Avg Proximity Premium / Km	$3772.96	$3371.82	$3111.84	$2683.51	$3032.76
Jiedao & Subway Line FE		Y	Y	Y	Y
Controls			Y	Y	Y
Year-Month FE				Y	
DistrictxYear-Month Trend					Y
Observations	82002	82002	82002	81995	82002
Adjusted R^2	0.161	0.524	0.614	0.619	0.617

Note: Dependent variable is ln(price per square meter in 2007 real Yuan). Standard errors clustered at jiedao level. Sample spans July 20, 2007 - July 20, 2009. All specifications include year and month fixed effects. Average price premium 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, and at a conversion rate of 6.95 yuan per USD. 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 distance to nearest CBD, age, age^2, 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 4: The Effect of Road Rationing on the Distance to CBD-Income Gradient: Mean Estimates

Outcome: ln(km to CBD)	(1)	(2)	(3)	(4)	(5)
Monthly Income× RRP	-0.001	-0.004*	-0.005**	-0.005**	-0.004*
	(0.012)	(0.002)	(0.002)	(0.002)	(0.002)
Monthly Income	-0.090***	-0.000	0.001	0.001	0.000
	(0.012)	(0.002)	(0.001)	(0.001)	(0.001)
Zip FE		Y	Y	Y	Y
Controls			Y	Y	Y
Year-Month FE				Y	
DistrictxYear-Month Trend					Y
Observations	8107	8107	8107	8107	8107
Adjusted R^2	0.196	0.942	0.944	0.944	0.946

Note: Dependent variable is ln(Km to CBD). Income is household monthly income ('000 yuan). Standard errors clustered by zip code. Sample spans July 20, 2007-July 20, 2009. All specifications include controls for year, month, and distance to subway. Controls include husband and wife age, employment rank, education, employer type, and tenure. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The Effect of Road Rationing on the Distance to Subway-Income Gradient: Mean Estimates

Outcome: ln(km to subway)	(1)	(2)	(3)	(4)	(5)
Monthly Income× RRP	0.004	-0.006	-0.007*	-0.007**	-0.007**
	(0.008)	(0.003)	(0.003)	(0.003)	(0.003)
Monthly Income	-0.018**	0.005*	0.006**	0.006**	0.006**
	(0.009)	(0.003)	(0.003)	(0.003)	(0.003)
Zip & Subway Line FE		Y	Y	Y	Y
Controls			Y	Y	Y
Year-Month FE				Y	
DistrictxYear-Month Trend					Y
Observations	8107	8107	8107	8107	8107
Adjusted R^2	0.649	0.927	0.928	0.928	0.930

Note: Dependent variable is ln(Km to Subway). Income is household monthly income ('000 yuan). Standard errors clustered by zip code. Sample spans July 20, 2007-July 20, 2009. All specifications include controls for year, month, and distance to nearest CBD. Controls include husband and wife age, employment rank, education, employer type, and tenure. Subway Line FE is a fixed effect for the subway line associated with the housing unit's closest subway station. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix A   Figures and Tables

Figure A1: Beijing Subway System Expansion
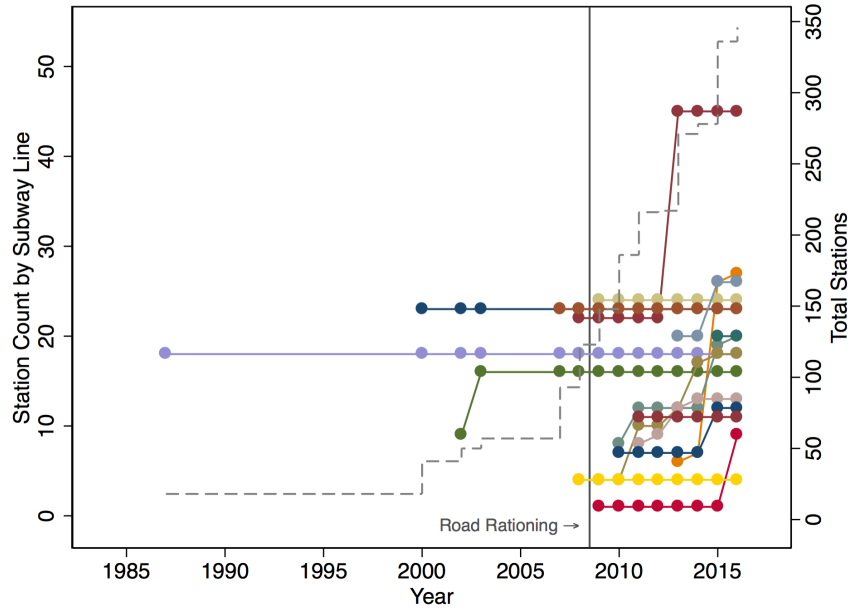
Note: Each color represents a subway line. Each dot denotes a date on which atleast one subway station was added across the Beijing system. Vertical shifts within a subway line show additions of stations connected to that subway line. The gray dashed line plots cumulative growth in stations over time.

Figure A2: Beijing Housing Price Appreciation, 2005-2016
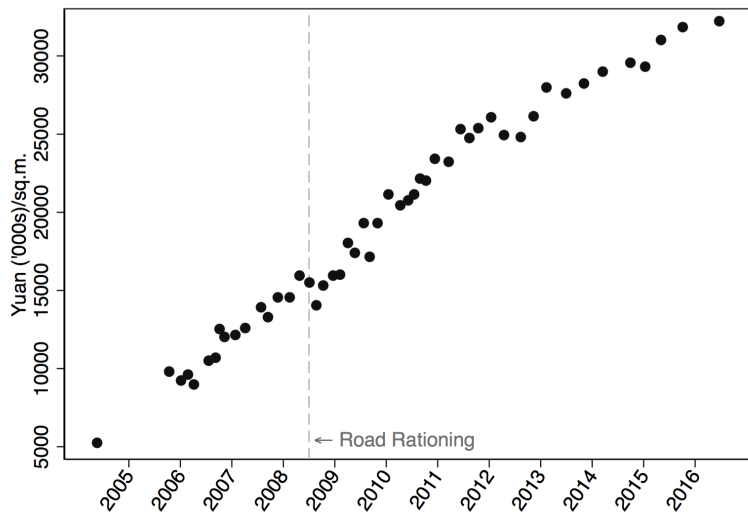
Note: Figure plots mean house price per square meter for 50 bins for transactions occuring from 2005 through 2016. Means residualized by jiedao fixed effects. Thus figure shows mean price appreciation within a jiedao over time. Source: Real estate transaction dataset.

Figure A3: Transit Mode Choice by Income
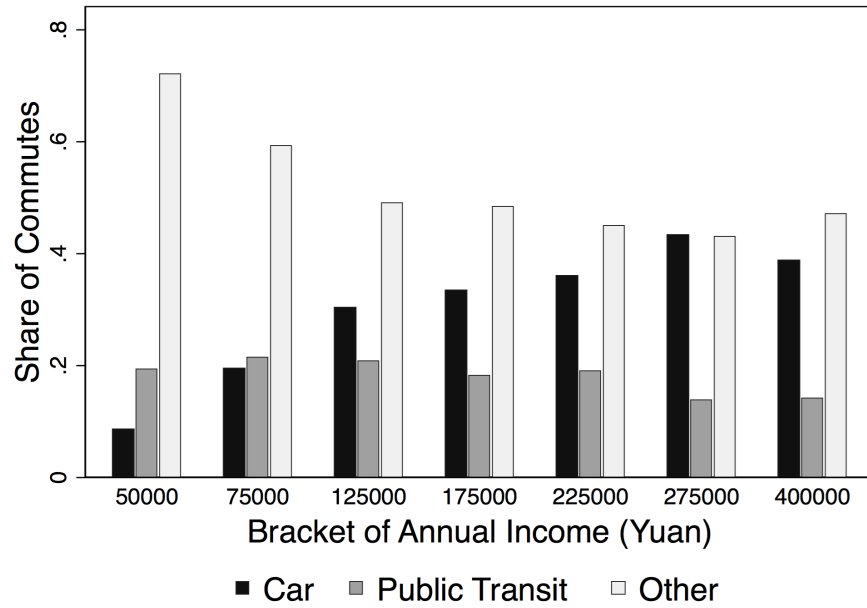
Note: Data sourced from 2010 Beijing Household Travel Survey (Beijing Transport Institute 2010). Based on 276,377 responses. "Other" includes walking, cycling, taxi, company shuttle, mixed modes.

Figure A4: Housing Units & Subway Stations in Beijing
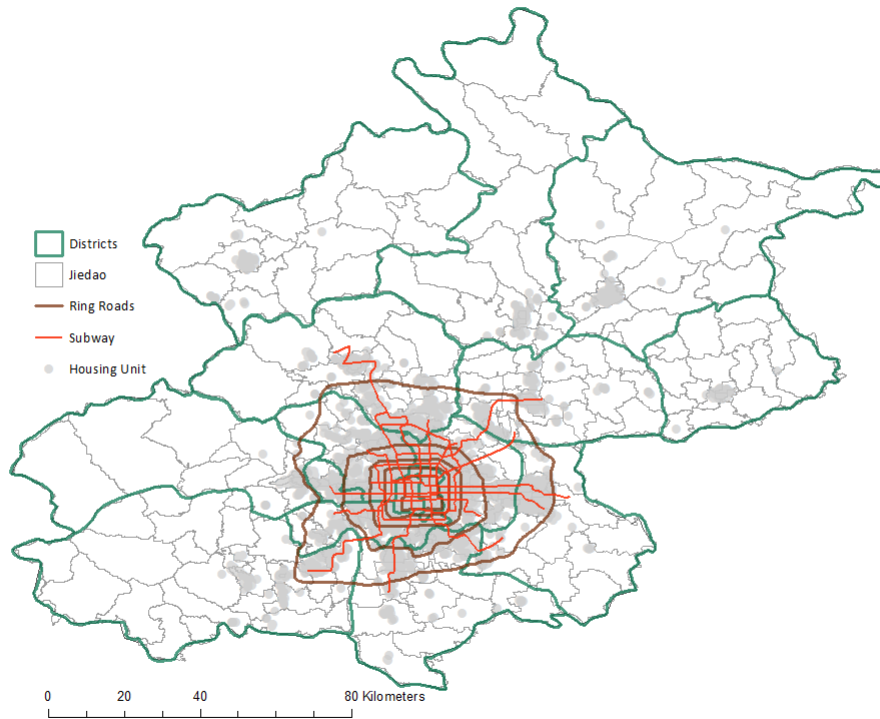
Note: Figure shows the location of housing units purchased between 2005-2014 throughout Beijing. Source: Beijing Real estate data.

Figure A5: Distribution of Jiedao Size
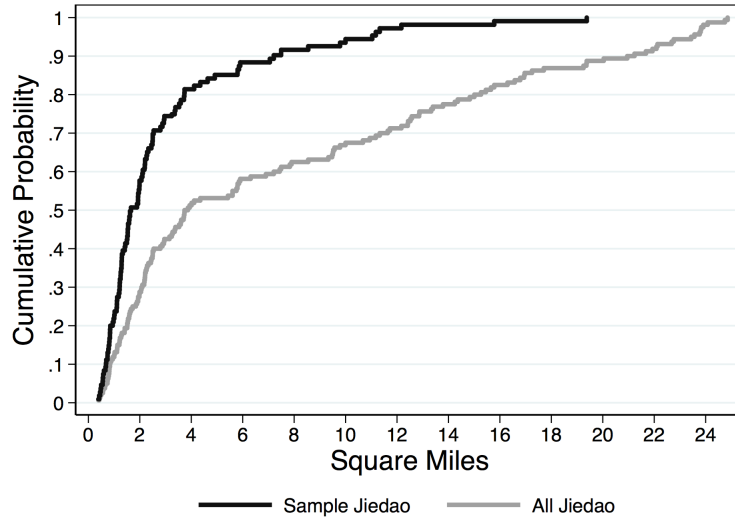
Note: Sample Jiedao include 188 neighborhoods. "All Jiedao" include population of neighborhoods within Beijing, or 307 jiedao.

Figure A6: Comparison of Sample vs Population of Housing Transaction Volume
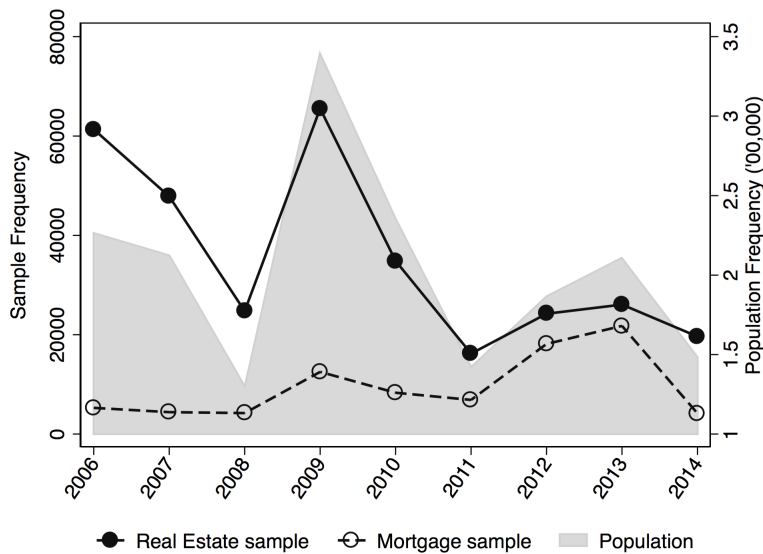
Note: Figure plots the population of housing transactions in Beijing from 2006 through 2014 in solid gray. Annual transaction volumes associated with the Real Estate data and Mortgage Application data shown in the solid and dashed black lines, respectively.

Figure A7: Google Search Trend Index for "Beijing Odd Even"
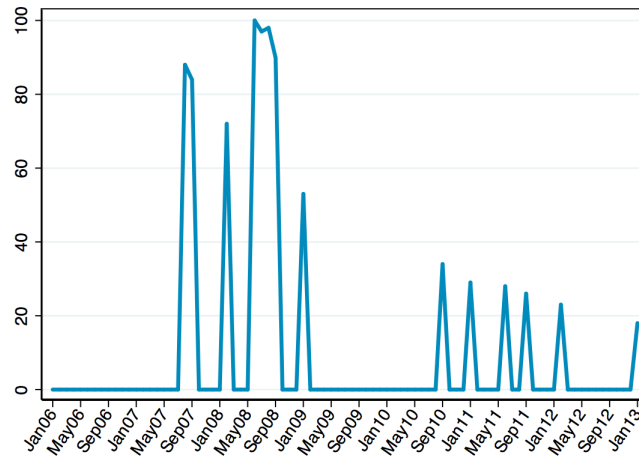
Note: Figure plots the relative popularity in Google searches worldwide for the term "Beijing Odd Even". Each data point is scaled on a range of 0 to 100 based on a topic's proprotion to all searches on all topics.

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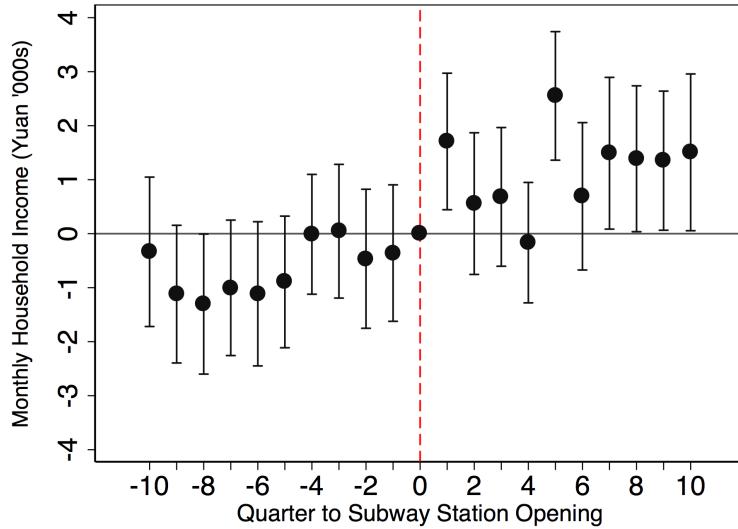

Note: Figure plots the difference in mean monthly household income in jiedao j in quarter q relative to quarter $q = 0$ when a subway station opens in jiedao j. Point estimates residualized by quarter-of-year fixed effects. Includes a balanced panel of 24 jiedao over 21 quarters. Sample spans 2005 through 2014 and includes 56 station openings. Bands show 95% confidence intervals. Source: Mortgage loan application data.

Figure A9: The Price-Distance Gradient and Beijing's Road Rationing Policy: Excluding Housing Near Newly-Built Stations
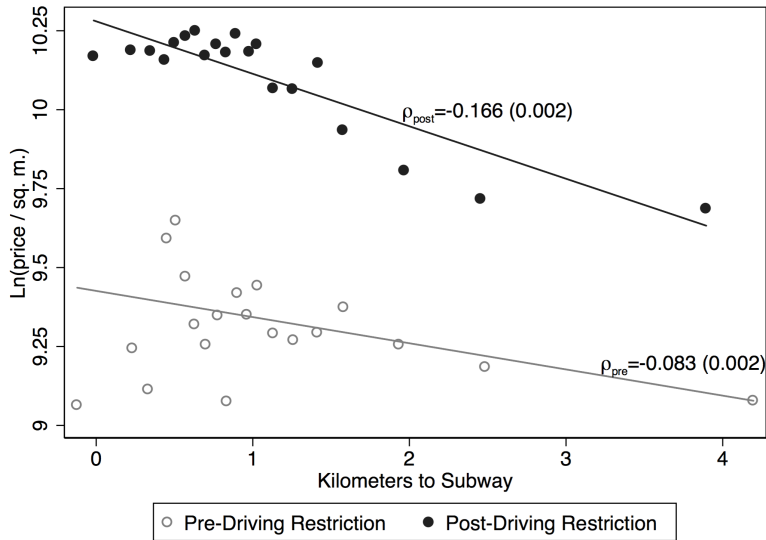

Note: Figure plots mean ln(price/sqm in ¥2007) for each of 20 distance bins. Each dot represents 600 and 3,830 observations per bin in pre- and post- road rationing periods, respectively. Means are residualized by distance to the central business district. ρ(pre) and ρ(post) are regression coefficients. Includes years 2005-2016. Source: Real estate transaction dataset.

Figure A10: Placebo Effect of Road Rationing on the House Price-Subway Distance Gradient Among Housing over 3km from Station
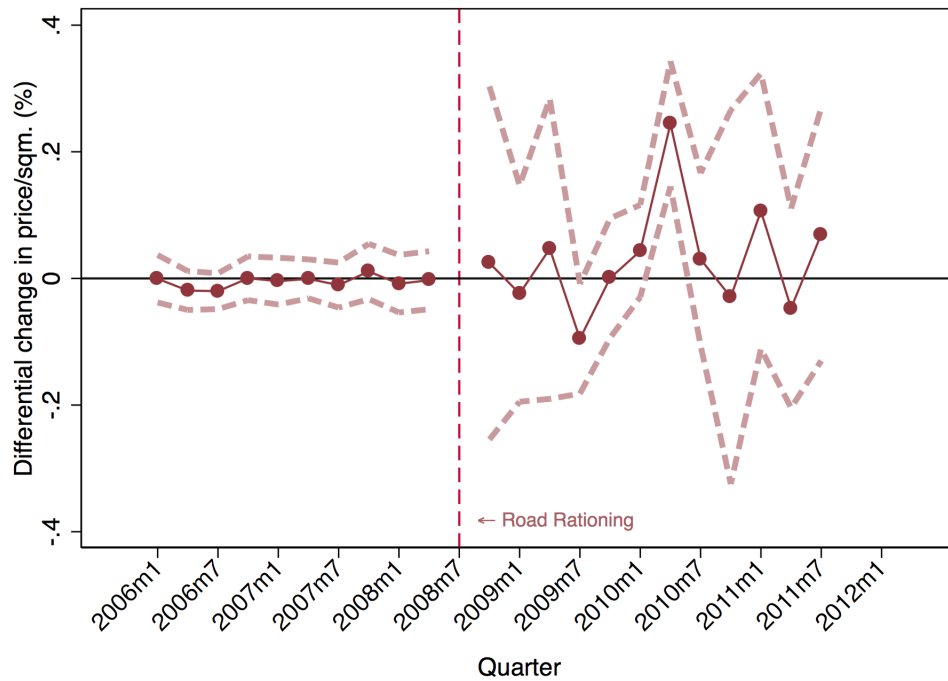
Note: Figure shows the partial effect of subway distance on housing price ln(price/sqm in ¥2007) at each quarter between Jan 2006 and Dec 2011. The omitted quarter is July-October 2008. Dashed lines show 95% confidence intervals. Sample includes housing units located in building complexes that are over 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 distance to nearest CBD, age, age^2, 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 jiedao level.

Figure A11: Placebo Effect of Road Rationing on House Price-Distance to Pollution Gradient
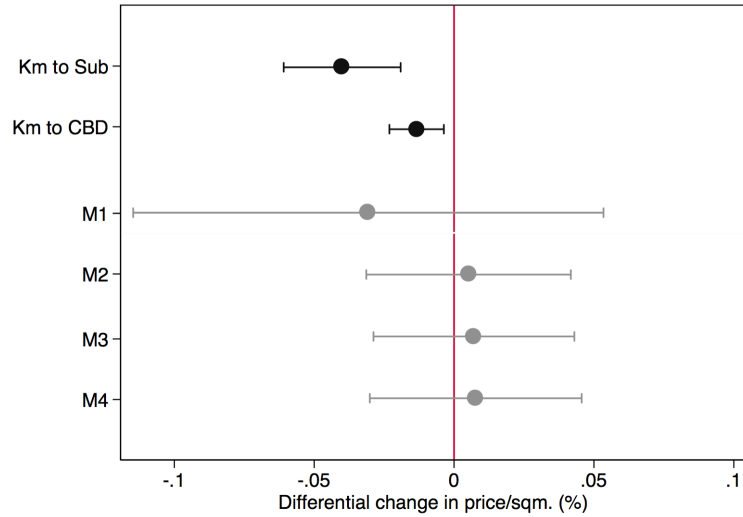
Note: Figure plots the effect of the RRP on the housing price premium for distance to one of three locations: the first and second estimates in black show the road rationing effect for distance to a subway station and distance to a central business district, respectively. These estimates replicate column (3) in Tables 3 and 2, respectively. The gray dots show estimates of the road rationing effect for distance to a major pollution site under various specifications. All models include year and month fixed effects. Bands show 95% confidence intervals. Sample includes 82,002 transactions, spanning July 20, 2007 through July 20, 2009. Model "M2" adds jiedao fixed effects and controls for: unit type (resale or newsale), distance to nearest CBD (or subway in the CBD regression), age, age^2, 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" includes all controls as well as district by year-month linear time trends. Standard errors clustered at jiedao level.

Figure A12: Placebo Effect of Road Rationing on Distance to Pollution-Income Gradient
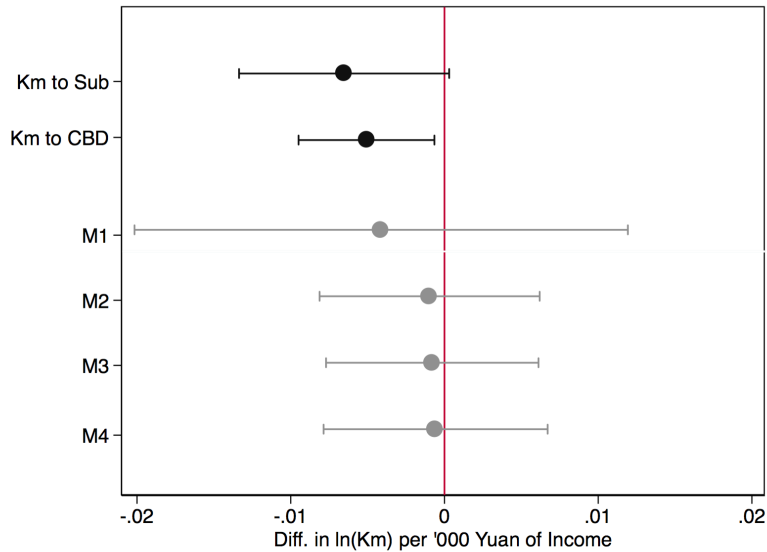
Note: Figure plots the effect of the RRP on the distance-income gradient. Distance is defined as kilometers to one of three locations: the estimates in black show the road rationing effect for distance to a subway station and distance to a central business district, respectively. These estimates replicate column (3) in Table 5 and 4, respectively. The gray dots show estimates of the RRP for distance to a major pollution site under various specifications. All models include year and month fixed effects. Bands show 95% confidence intervals. Sample includes 8,107 transactions, spanning July 20, 2007 through July 20, 2009. "M2" adds neighborhood (zip code) fixed effects, subway line fixed effects, and controls for husband and wife age, employment rank, education, employer type, tenure, and distance to nearest CBD (or subway in the second estimate.) "M3" adds year-by-month fixed effects. "M4" includes all controls as well as district by year-month linear time trends. Standard errors clustered by neighborhood.

Table A1: Effects of Road Rationing on Price-Distance to CBD Gradient with Alternative CBDs

Outcome: ln(price/sq.m.)	Technology District	Software District	Financial District	Beijing "CBD"	Business Park	Shopping District
Km to [CBD] x RRP	-0.006	-0.005	-0.016**	-0.010**	-0.011***	-0.018***
	(0.004)	(0.003)	(0.008)	(0.004)	(0.003)	(0.006)
Km to [CBD]	0.020	0.014	0.042**	0.008	0.010	0.027
	(0.013)	(0.013)	(0.018)	(0.014)	(0.013)	(0.017)
Year & Month FE	Y	Y	Y	Y	Y	Y
Jiedao FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
DistrictxYear-Month Trend	Y	Y	Y	Y	Y	Y
Observations	82002	82002	82002	82002	82002	82002
Adjusted R^2	0.613	0.612	0.616	0.613	0.614	0.615

Note: Dependent variable is ln(price per square meter in 2007 real Yuan). Standard errors clustered at jiedao level. Sample spans July 20, 2007 - July 20, 2009. Average price premium is evaluated at a unit size of 122 sq.m., the size at the mean distance to the nearest business district (5km and 7km), and at a conversion rate of 6.95 yuan per USD. 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 distance to nearest subway station, age, age^2, 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 A2: Hedonic and Sorting Results Excluding Anticipation Effects

	ln(price/sqm) (1)	ln(price/sqm) (2)	ln(km to CBD) (3)	ln(km to subway) (4)
Km to CBD × RRP	-0.012*			
	(0.007)			
Km to Subway × RRP		-0.034***		
		(0.010)		
Monthly Income× RRP			-0.006**	-0.008**
			(0.002)	(0.004)
Jiedao FE	Y	Y		
Zip Code FE			Y	Y
Subway Line FE		Y		Y
Housing Controls	Y	Y		
Demographic Controls			Y	Y
DistrictxYear-Month Trend	Y	Y	Y	Y
Observations	68658	68658	6506	6506
Adjusted R^2	0.668	0.672	0.946	0.927

Note: Dependent variable denoted in top row. Sample spans July 20, 2007 - July 20, 2009, excluding the first two quarters of 2008: January 1, 2008 through July 20, 2008. All specifications include year and month fixed effects. In columns (1) and (2), standard errors clustered at jiedao level. Housing 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; age, age^2, size, floor-area ratio, green space, property management fees, parking fees, and size, number of housing units and number of buildings of the complex. Demographic controls include husband and wife age, employment rank, education, employer type, and tenure. Columns (1) and (2) control for distance to the nearest subway. Columns (2) and (3) control for distance to the nearest CBD. * p<0.10, ** (p<0.05), *** (p<0.01).

Table A3: Effect of Road Rationing on Distance to CBD-Income Gradient-Varying Definition of CBD

Outcome: ln(km to [CBD])	Technology District	Software District	Financial District	Beijing "CBD"	Business Park	Shopping District
Monthly Income× RRP	-0.004**	-0.003***	-0.004*	-0.004***	-0.003*	-0.004**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Monthly Income	0.001	0.001	0.002	0.003**	0.001	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year & Month FE	Y	Y	Y	Y	Y	Y
Zip FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
DistrictxYear-Month Trend	Y	Y	Y	Y	Y	Y
Observations	18022	18022	18022	18022	18022	18022
Adjusted R^2	0.954	0.951	0.974	0.968	0.956	0.974

Note: Dependent variable is ln(Distance to CBD (km)). Income is household monthly income ('000 yuan). Standard errors clustered by zip code. Sample spans July 20, 2007-July 2009. Controls include distance to nearest subway, husband and wife age, employment rank, education, employer type, and tenure.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Effects of Road Rationing on House Price-Distance Gradient, Excluding Houses Near Newly-Built Stations

Outcome: ln(price/sq.m)	(1)	(2)	(3)	(4)	(5)
Km to Subway x RRP	-0.020	-0.022***	-0.025***	-0.012***	-0.028***
	(0.019)	(0.006)	(0.005)	(0.004)	(0.005)
Km to Subway	-0.109***	-0.053***	-0.043***	-0.050***	-0.040**
	(0.015)	(0.009)	(0.015)	(0.015)	(0.015)
Avg Price Premium / Km	$1694.84	$1902.35	$2105.61	$1053.99	$2418.66
Controls		Y	Y	Y	Y
Jiedao & Subway Line FE			Y	Y	Y
Year-Month FE				Y	
DistrictxYear-Month Trend					Y
Observations	12474	12474	12474	12474	12474
Adjusted R^2	0.191	0.635	0.733	0.742	0.734

Note: Dependent variable is ln(price per square meter in 2007 real Yuan). Sample includes housing units in building complexes that do not change in their proximity to subway stations from 2005 through 2016. Standard errors clustered at jiedao level. Sample spans July 20, 2007 - July 20, 2009. All specifications include year and month fixed effects. Average price premium 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, and at a conversion rate of 6.95 yuan per USD. 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 distance to nearest CBD, age, age^2, 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 A5: Effects of CDR policy on Subway-Price gradient - Controlling for Subway Network Density

Outcome: ln(price/sq.m.)	(1)	(2)	(3)	(4)	(5)	(6)
Km to Subway x RRP	-0.040*	-0.043***	-0.046***	-0.039***	-0.036***	-0.037***
	(0.020)	(0.013)	(0.013)	(0.012)	(0.012)	(0.011)
Km to Subway	-0.042***	0.002	-0.000	-0.012	-0.009	-0.010
	(0.013)	(0.013)	(0.011)	(0.009)	(0.008)	(0.009)
Km to Subway x Network Density	-0.001	0.001	0.001	0.000	0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Network Density	0.002	0.000	-0.001	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Avg Proximity Premium / Km	$3232.07	$3525.87	$3768.40	$3206.90	$2927.12	$3025.98
Jiedao FE		Y	Y	Y	Y	Y
Subway Line FE			Y	Y	Y	Y
Controls				Y	Y	Y
Year-Month FE					Y	
DistrictxYear-Month Trend						Y
Observations	70307	70307	70307	70307	70299	70307
Adjusted R^2	0.161	0.518	0.524	0.623	0.629	0.629

Note: Dependent variable is ln(total price per square meter in 2007 real Yuan). Standard errors clustered at jiedao level. Sample spans July 20, 2007 - July 20, 2009. Network Density is the inverse distance-weighted sum of subway stations from each station location. All specifications include year and month fixed effects. Average price premium 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, and at a conversion rate of 6.95 yuan per USD. 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 distance to nearest CBD, age, age^2, size, floor-area ratio, green space, property management fees, parking fees, and size, number of housing units and number of buildings of the complex.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B   Derivation of the Bid-Rent Gradient

Residents face the following maximization problem. As in Section 3.1, the resident maximizes consumption of a numeraire good z with price of \$1 and consumption of housing h with price of p subject to a budget constraint. They receive income y from working. With that income, they pay for the numeraire good, housing, and commuting. Commuting costs $(w \cdot T(n, x, \delta))$ are the opportunity cost of forgone earnings w. Time spent commuting is a function of distance from the CBD x, the likelihood of using the subway n, and walking distance to the subway station δ.

$$\max_{z,h} U(z,h) \text{ s.t.} \tag{10}$$

$$z + ph + w \cdot T(n, x, \delta) = y$$

The transit time $T(n, x, \delta)$ differs depending on how likely an individual is to utilize the subway versus driving a car. The variable n ranges between 0 and 1 and measures the likelihood of commuting via subway. For the rich, n approximates zero because they are more likely to rely on cars to commute, except on road rationing days. Thus, we can define transit time as in Equation 1 as follows:

$$T(n, x, \delta) = n\left[\frac{\delta}{\omega} + \frac{x}{\sigma}\right] + (1-n)\frac{x}{\nu} \tag{11}$$

The first term in brackets encompasses transit time for commuting via subway: walking time to the station, which depends on the distance of the walk δ divided by walking speed ω and distance traveled on the subway to the CBD x divided by subway speed σ. The second term encompasses transit time to the CBD via car where car speed is ν. We assume car speed is greater than subway speed, which is greater than walking speed ($\nu > \sigma > \omega$).

In equilibrium, each resident i has achieved their highest utility subject to their budget constraint, and all residents have equal utility levels, $\max_{z,h} u_i = \bar{u} \ \forall i$. This is achieved by each resident optimally allocating their income between housing and commutes where, at each location of the city, they make a tradeoff between higher per-unit housing with lower commuting costs. Substituting the first budget constraint into Equation 10, the equal utility condition is:

$$\max_{h} u_i(y - ph - T(n, x, \delta)w, h) = \bar{u} \forall i \tag{12}$$

Equations 10 and 12 are the two conditions required for equilibrium.

We derive the first order conditions of Equation 10 by setting up a Lagrangian equation:

$$\mathcal{L} = u(z, h) + \lambda[y - ph - T(n, x, \delta)w - z]$$

The first derivative of $\mathcal{L}$ with respect to choice variables h and z are as follows:

$$\frac{\partial \mathcal{L}}{\partial z} = \frac{\partial u}{\partial z} - \lambda = 0 \rightarrow \frac{\partial u}{\partial z} = \lambda$$

$$\frac{\partial \mathcal{L}}{\partial h} = \frac{\partial u}{\partial h} - \lambda p = 0 \rightarrow \frac{\partial u}{\partial h} = \lambda p$$

Dividing the first condition by the second:

$$\frac{\partial u / \partial h}{\partial u / \partial z} = p \text{ or, equivalently } \frac{v_2}{v_1} = p$$

where v_1 denotes the partial derivative of the first argument in Equation 10 and v_2 denotes that of the second argument.

Next, we totally differentiate Equation 12 with respect to x to solve the system of equilibrium conditions:

$$v_1 \frac{\partial y}{\partial x} - v_1 \Big[\frac{\partial p}{\partial x} h + \frac{\partial h}{\partial x} p \Big] - v_1 \Big[\frac{\partial T}{\partial x} w + \frac{\partial w}{\partial x} T \Big] + v_2 \frac{\partial h}{\partial x} = 0$$

Since y and w do not vary with x, we are left with:

$$v_1 \Big[\frac{\partial p}{\partial x} h + \frac{\partial h}{\partial x} p \Big] + v_1 \Big[\frac{\partial T}{\partial x} \Big] w = v_2 \frac{\partial h}{\partial x}$$

From the first order conditions, we can substitute $\frac{v_2}{v_1} = p$, or $v_2 = v_1 p$ into the above equation:

$$-\frac{\partial T}{\partial x} w = \frac{\partial p}{\partial x} h \tag{13}$$

From Equation 11, the left-hand side is equivalently:

$$-\Big[\frac{n}{\sigma} + \frac{1-n}{\nu} \Big] w = \frac{\partial p}{\partial x} h$$

Solving for the change in price with respect to distance from the CBD, we obtain the bid-rent function that demonstrates how housing prices fall with distance from the CBD at a rate proportional to the increased commuting costs required to reach the CBD from a distance x:

$$\frac{\partial p}{\partial x} = -\Big[\frac{n}{\sigma} + \frac{1-n}{\nu} \Big] \frac{w}{h} \tag{14}$$

which is Equation 2 in the main text.

We similarly derive the bid-rent gradient for subway station proximity by totally differentiating Equation 12 with respect to δ. Equation 13 is instead:

$$-\frac{\partial T}{\partial \delta} w = \frac{\partial p}{\partial \delta} h$$

From Equation 11, the left-hand side is equivalently:

$$-\frac{n}{\omega}w = \frac{\partial p}{\partial \delta}h \rightarrow \frac{\partial p}{\partial \delta} = -\frac{n}{\omega}\frac{w}{h}$$

which is Equation 3 in the main text. Appendix Figure A13 graphically depicts $\frac{\partial p}{\partial x}$ and $\frac{\partial p}{\partial \delta}$. The same framework is applicable whether the origin is the central business district with distance x or a subway station with distance δ. Rich and Poor having different slopes, dictated by their respective cost of commuting and time value, as discussed in the text. This paper's empirical approach estimates the magnitude of the change in slope following the road rationing policy. In the simplified case of the Figure, we empirically estimate the slope change from $Rich^0$ to $Rich^1$.

Figure A13: Urban Land Use and Equilibrium Sorting with Income Heterogeneity
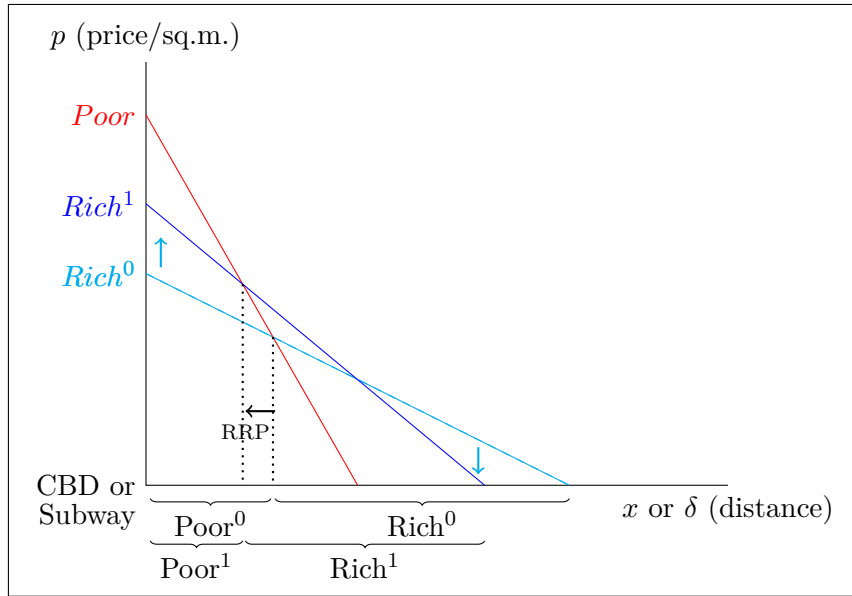
Note: y-axis is price per square meter and the x-axis is distance from the CBD or a subway station, respectively. Location in the city is defined by x conditional on δ and vice verca. Each income group has a distinct bid-rent gradient. Let $\epsilon_{h,y} > \epsilon_{t,y}$ (i.e., income elasticity of housing > income elasticity of time costs). Consequently, 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 both the CBD and subway stations, depicted as a tilt from $Rich^0$ to $Rich^1$. The RRP identifies $\frac{\partial p^2}{\partial x \partial (RRP)}$ and $\frac{\partial p^2}{\partial \delta \partial (RRP)}$. 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.

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

Our estimates of the price-distance to subway gradient 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 A6, 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 A6: 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*	-0.013*	-0.012*	-0.032***	-0.025***	-0.024***
	(0.009)	(0.007)	(0.006)	(0.010)	(0.008)	(0.007)
Ln(Km to Subway)	-0.074***	-0.029***	-0.029***	-0.062***	-0.027**	-0.023**
	(0.011)	(0.011)	(0.009)	(0.011)	(0.011)	(0.009)
Year x 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, age^2, 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, age^2, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$