

1 **Measuring policy leakage of Beijing’s car ownership restriction**

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1 **Measuring policy leakage of Beijing’s car ownership restriction**

2 **ABSTRACT**

3 In response to severe traffic congestion and air pollution, Beijing introduced a car ownership re-
4 striction policy to curb growth in the number of private cars in the city. However, Beijing residents
5 can still purchase and register their cars in neighboring cities and this “leakage” may substantially
6 reduce the policy’s effectiveness. Using city-level data collected from the CEIC China Premium
7 Database, we aim to quantify the spill-over effect: the impact of Beijing’s policy on the growth of
8 private car registrations in neighboring cities. We first deploy a synthetic control method to create
9 a weighted combination of non-treated cities for each treated city. We then employ a difference-in-
10 differences approach to estimate the policy leakage. Our models suggest that the policy resulted in
11 additional 443,000 cars sold in the neighboring cities (within 500 km of Beijing) from 2011-2013,
12 compared to if the policy had not been implemented. 35%-40% of the car growth reduction stipu-
13 lated by the policy simply spilled over to neighboring cities. The significance of the policy leakage
14 necessitates positioning Beijing’s urban transportation in a broader context and executing regional
15 collaboration.

16

17 *Keywords:* Beijing, car ownership, policy, policy leakage, license plate lottery, synthetic control,
18 difference-in-differences, treatment effects

1 1. INTRODUCTION

2 Beijing, China, has experienced rapid motorization over the past decades. Driven by urbanization
3 and extraordinary economic growth (with GRP growing at about 13% a year from 2003-2010), the
4 stock of Beijing's private cars increased from 929,000 in 2003 to 3,566,000 in 2010 (40). This
5 motorization has also given rise to problems such as air pollution and traffic congestion. With
6 an annual average PM10 concentration of 121 micrograms per cubic meter in 2010, Beijing often
7 ranked among the worst Chinese cities in terms of air quality, significantly exceeding World Health
8 Organization recommended limits (28). In 2010, Beijing was also ranked as one of the most con-
9 gested cities in the world (23).

10

11 In response to these issues, Beijing's city government developed a series of traffic demand man-
12 agement strategies. Early approaches included a parking restriction system in 1998, a vehicle
13 purchase tax in 2004, and a low-cost public transportation policy in 2007 (40). Realizing that these
14 approaches were not enough, Beijing used the 2008 Olympic Games as a unique opportunity to
15 implement the first city-wide private car use restriction in China. Due to the effectiveness of the
16 regulation during the Olympic period, the city then shifted to a long term one-day-per-week driv-
17 ing restriction policy (32). Despite temporary traffic relief in 2008 and 2009, continued private car
18 registrations in Beijing during 2009 and 2010 gradually brought Beijing's traffic congestion back
19 to the level observed prior to the implementation of the use restriction (40).

20

21 This led Beijing to adopt a complementary car ownership restrictions policy in 2011, which limits
22 the number of private car licenses that are allowed to be registered in the city through a lottery
23 allocation mechanism, effectively capping the number of new local car sales (39). According to
24 this policy, a license plate is required if a resident wants to buy a new car, buy a second-hand car,
25 accept a gifted car, or transfer a non-local car registration to Beijing. And there is a fixed quota of
26 car license plates that are allocated among residents monthly or bimonthly.

27 **Potential Effectiveness of Beijing's Car Ownership Restriction Policy**

28 From its implementation in January 2011, Beijing's car ownership lottery allocated about 240,000
29 licenses a year (20,000 a month) until 2013, after which the annual quota was reduced to 150,000.
30 Comparing this small number of private cars allocated to the high growth-rate of car numbers prior
31 to policy implementation (529,000 additional cars from 2008-2009, or an 23.11% annual growth
32 rate) as well as to the high number of entrants now waiting for a license in the lottery suggests
33 that the policy has been effective in curbing growth in new vehicle ownership (28). Informed
34 by studies of car ownership restriction auctions in Singapore and Shanghai (16, 24, 25, 38, 46),
35 researchers have attempted to estimate the impact of Beijing's car ownership restriction lottery
36 on the number of cars in the city. Yang et al. (2014) found that in the first few years after the
37 adoption of the policy, growth in the number of cars in Beijing has been sharply reduced (40).
38 Zhang (2014) estimated that the license plate lottery policy was responsible for a reduction of
39 approximately 1.053 million vehicles from 2011-2013 (42). However, these studies often fail to
40 account for potential noncompliant behavior that may reduce the effectiveness of the policy.

41 **Potential Ineffectiveness of Beijing's Car Ownership Restriction Policy**

42 Given the limited jurisdictional reach of Beijing's policy, there is potential for noncompliance by
43 residents. In Shanghai, survey studies investigating the issue of noncompliance at the individual-

1 level have shown that a significant proportion (28%) of drivers obtained their car licenses plates
2 from neighboring municipalities rather than participating in that city's auction policy (20). And
3 anecdotal evidence suggests similar behavior from residents of Beijing, who may purchase and
4 register cars in neighboring cities rather than participate in the lottery (31, 35). For instance, one
5 news article published in March 2011 reported that nearly 1,000 Beijing residents had applied for
6 new car license plates in the city of Langfang in Hebei Province specifically "to circumvent the
7 capital's car registration lottery" (19). In 2013, another article reported that drivers in Beijing
8 who spend an extra 1,500 yuan (\$250) can obtain a license plate from Zhuozhou in Baoding city,
9 Hebei—about an hour's drive from Beijing (26).

10

11 Procuring a residence permit may be one obstacle for Beijing residents to register their cars in
12 other cities to circumvent the ownership restriction. For example, to register a car license plate in
13 neighboring Hebei province, a residence permit in Hebei is required alongside with a car purchase
14 certificate and ID of the person who wants to obtain the license plate. In practice, however, these
15 residence permits are relatively easy to obtain for cities outside of Beijing. News articles suggest
16 that the property ownership or renter certificates needed were relatively easy to obtain in the early
17 years of the policy, with many individuals procuring them through friends or family in the area or
18 third-party agents who created a "black market" for these permits (7).

19

20 Therefore, there is significant anecdotal evidence that non-compliant behavior in response to Bei-
21 jing's car ownership restriction may have resulted in "policy leakage." However, to date no study
22 has attempted to test the significance or measure the magnitude of the policy leakage from Beijing's
23 car ownership restriction policy.

24 **Our Approach: Quantifying Policy Leakage**

25 In this study, we take an in-depth look at potential leakage around Beijing's car ownership restric-
26 tion policy to better understand the real effectiveness of this policy in reducing the total number
27 of new private cars in the city. Private cars are cars owned by individuals. In 2010, private cars
28 accounted for 91.2% of all the privately-owned vehicles (including coaches, motorcycles, and
29 three-wheelers) in Beijing according to the City Yearbook. Using difference-in-differences analy-
30 sis, this study isolates the causal effect of the implementation of Beijing's car ownership restriction
31 policy on the growth of private cars in neighboring cities. The underlying logic is that any statis-
32 tically anomalous growth of private cars in these neighboring cities can be attributed to Beijingers
33 obtaining non-local license plates from these cities to bypass the car ownership restriction in Bei-
34 jing. Acknowledging that other socio-economic factors may also contribute to the rise of private
35 car sales in neighboring cities, we use the synthetic control method to control for these confound-
36 ing variables. This approach allows us to assess the amount of policy leakage of Beijing's car
37 ownership restriction, and quantify the actual reduction of new private cars sold in and around
38 Beijing after the policy intervention. Then accounting for this policy leakage, we consider how ef-
39 fective Beijing's car ownership restriction actually was in reducing private car growth in the region.

40

41 The rest of the paper is organized as follows. Section 2 describes our data and econometric models.
42 Section 3 present our main results quantifying the policy leakage in neighboring cities. Section 4
43 then compares this policy leakage to the hypothetical number of private cars avoided by Beijing's
44 policy in the absence of any policy leakage (assuming no policy leakage). This gives us an esti-

1 mate of the actual effectiveness of Beijing’s policy, accounting for policy leakage. Section 5 then
 2 discusses potential implications of these findings for policymakers in Beijing and its surrounding
 3 cities. We conclude by summarizing our findings and noting areas for future research in Section 7.

4 **2. DATA AND ECONOMETRIC MODELS**

5 In this study, we use a difference-in-differences (DID) approach to estimate the impact of Beijing’s
 6 car ownership restriction policy on the number of private cars in Beijing’s neighboring cities. This
 7 method quantifies the differential effect of a “treatment” (the implementation of Beijing’s car own-
 8 ership restriction policy) on a “treatment group” (Beijing’s neighboring cities) versus a “control
 9 group” in a quasi-natural experiment. The validity of this approach rests on the assumption that
 10 the treatment and control groups would have followed the same trend in private car growth during
 11 the study period in the absence of the treatment. Therefore the choice of study period, treatment
 12 group, and control group are critical modeling decisions to ensure accuracy of the results.

13 **Data**

14 We take as our dependent variable the total number of cars in each city. The independent variables
 15 include:

- 16 1. Socioeconomic characteristics of the cities, including administrative area per capita,
 17 GDP per capita, total registered population, average wage per capita, and average gov-
 18 ernment expenditure per capita in yuan;
- 19 2. Urbanization rate;
- 20 3. Statistics on the coverage of the transportation network and availability of alternative
 21 modes, such as highway length (m) per 1,000 persons, road area (m^2) per capita, taxi
 22 number per 1,000 persons, and bus number per 1,000 persons.

23 We collect these data for 287 cities in China for the period 2006-2013 from CEIC’s China Pre-
 24 mium Database (15). We then manually verified and supplemented the data using information
 25 from China’s City Statistical Yearbook for each year provided by the National Bureau of Statistics
 26 and other provincial and municipal statistical yearbooks.

27
 28 Based on previous literature (27, 36, 41), we argue that this set of independent variables covers the
 29 main variables that might explain car ownership levels over time or across cities. By controlling for
 30 these variables, we can be confident that our measured difference in number of private cars in the
 31 pre- and post-treatment period is due to the implementation of the policy and not some time trends
 32 or city-to-city variations in socio-economic variables or urban and transport system characteristics.

33 **Study Period**

34 On December 13, 2010, Beijing announced an unofficial plan to cap private car registrations. Only
 35 11 days later (on December 24, 2010), Beijing froze all new car registrations until the license plate
 36 lottery policy took effect on January 1, 2011 (40). The time window between announcement and
 37 implementation of the policy was so short that anticipatory car purchasing at the end of 2010 was
 38 likely very limited. Therefore, we take the years up until 2010 as our pre-treatment period and
 39 2011 onward as our post-treatment period.

40

41 Specifically, we model private car ownership trajectories from 2006-2013 in each city, with 2006-
 42 2010 as the pre-treatment period and 2011-2013 as the post-treatment period. While data are

1 available for years after 2013, we choose this cutoff to avoid potential bias introduced by the
 2 adoption of similar car ownership restrictions in other Chinese cities starting in 2014: including
 3 Tianjin (December 15, 2013), Hangzhou (March 26, 2014), and Shenzhen (December 31, 2014)
 4 (34). To avoid sudden increase in car sales just before the implementation of these policies, each
 5 of these policies went into effect almost immediately after their announcements. The choice of
 6 treatment period also avoids any confounding effect from a change in Beijing’s related car use
 7 restriction policy in 2014, which reduced the valid period for non-local car driving permits from
 8 half a year to 7 days, making it more difficult for non-local cars to drive in Beijing.

9 **Treatment Group Selection**

10 *Using Geographic Distance to Define a Treatment Boundary*

11 We choose to use geographic distance to Beijing to define our treatment group. We assume that
 12 nearby cities will be more affected by Beijing’s car ownership restriction policy compared with
 13 other cities. The policy leakage is likely to be stronger in neighboring cities of Beijing than in
 14 other cities far from Beijing for two reasons. First, it is less cost and time consuming to travel
 15 to neighboring cities for car registration and annual inspection (30). Second, a great number of
 16 immigrants in Beijing originate from these neighboring cities. This might suggest that cities geo-
 17 graphically close to Beijing may also be close in terms of social distance; in other words, Beijing
 18 residents have stronger connections with nearby rather than far-away cities. For example, neigh-
 19 boring Hebei Province accounted for 21.3% of Beijing’s immigrant population in 2015, well ahead
 20 of second-place Henan which was the origin of 11% of Beijing’s immigrant population (1). Since
 21 local residential permits are needed for private car registration in Beijing, it is natural for these
 22 immigrants to get car licenses from their hometowns.

23

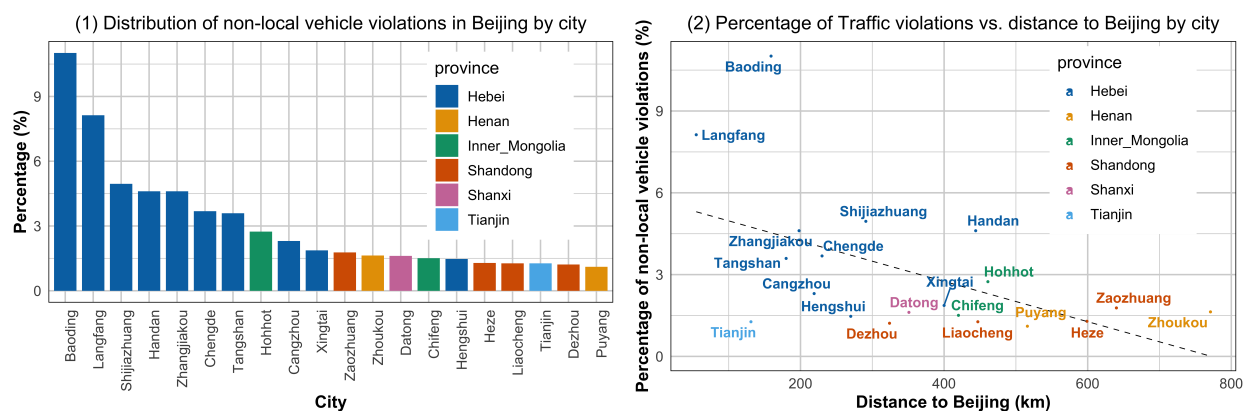
24 As a cross-validation, we check Beijing’s official records of cars that have committed traffic viola-
 25 tions and find that cars registered in the cities closest to Beijing in Hebei Province have the highest
 26 number of vehicle violations behind cars registered in Beijing. Furthermore, the percentage of car
 27 violation cases in Beijing drops significantly for non-local cars registered in city further away from
 28 Beijing—with the percentage of violations falling to below 2% for cities beyond 500 to 600km
 29 from Beijing (see Figure 1). Assuming that enforcement of traffic violations is independent of
 30 where the car is registered, we can infer that there is a significantly higher proportion of non-local
 31 cars driving on Beijing’s streets from nearby cities. This further substantiates the relationship be-
 32 tween geographical distances to Beijing and the magnitude of car inflows to Beijing and provides
 33 additional evidence for our choice of treatment boundary.

34 *Determining the Appropriate Boundary*

35 Next we consider the appropriate boundary for our treatment area. We begin by deriving a measure
 36 of the driving distance (D_i) between a Beijing and each city i . This driving distance is calculated
 37 using the Google Maps API, from which we extracted the shortest path driving distance using ex-
 38 isting road networks between each city’s geometric center (18).

39

40 Using this distance, we define our treatment group as those cities that lie within a 500 km driving
 41 distance of Beijing. This boundary of the treatment area makes sense for our application, since a
 42 500 km distance translates to roughly 5.5 hours of driving at an average speed of 91 km/hr (or 56
 43 mph); this means people can leave Beijing, complete their car registration procedures in a neigh-



Data: Beijing Public Security Traffic Administration website, <https://bj.122.gov.cn/views/viopub.html>

FIGURE 1: Percentage of traffic violations in Beijing by cars with non-local licenses from February 2016 to April 2019: top 20 cities

1 boring cities, and return home all in one day. With this 500 km boundary, our treatment group
 2 includes 31 cities located in 6 neighboring provinces: Hebei, Tianjin, Shandong, Shanxi, Inner
 3 Mongolia, and Liaoning (Figure 2). In 2013, the total population of the 31 cities was about 153
 4 million, which accounted for about 12% of China’s total population at that time.

5

6 Given that the choice of the boundary can be somewhat uncertain, we also test the sensitivity of
 7 our models to this choice of treatment boundary following an approach similar to that used by
 8 Zheng, et al. (2017) (44). In addition to the 500 km boundary, we also specify models with 400
 9 km and 600 km¹ boundaries. We also include cities’ driving distance to Beijing as a key variable
 10 to study the variation of the treatment effect over distance. As we show in Section 3, our model
 11 result suggests that 500 km is the appropriate boundary for treated city selection.

12 Control Group Synthesis

13 Given the heterogeneity of China’s cities, we use a synthetic control method to define our control
 14 group (5). By creating a weighted combination of non-treated cities for each treated city, this
 15 method creates a group of control cities that are approximately equivalent to the treatment cities in
 16 terms of the pre-intervention outcome (in this case, the number of private cars prior to the Beijing
 17 car ownership restriction) and the independent variables included in the model. This data-driven
 18 procedure reduces discretion in the choice of the control group (3) and also avoids problems arising
 19 in conventional DID analysis if the number of treated subjects is small relative to the number of
 20 control subjects (13).

21 The synthetic control method

We adopt the synthetic control based on recommendations in existing literature (4, 6). Our goal is to construct a single synthetic control city for each treated city such that the synthetic control city resembles the treated city for the pre-treatment period in all relevant predictors: administrative area per capita, GDP per capita, total registered population, annual wage per capita, urbanization rate,

¹This includes 5 more cities: Laiwu, Weifang, Panjin, Jining and Heze.

highway length per 1,000 people, road area per capita, taxi number per 1,000 people, bus number per 1,000 people, government expenditure per capita. Then, for each treated city, we solve for W^* that minimizes:

$$\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)} \quad (1)$$

where X_1 represents the vector of the pre-treatment predictors for the treated city, X_0 is a matrix with the same information for the donor cities. W is a weight vector indicating the weights of the donor cities in the synthetic city that sum to one. V is a diagonal matrix which specifies the relative importance of the predictors. The optimal V minimizes the mean square error of the outcome estimation. In other words, we choose V based on:

$$V^* = \arg \min_V (Z_1 - Z_0W^*(V))'(Z_1 - Z_0W^*(V)) \quad (2)$$

1 where Z_1 is the vector of the observed pre-intervention outcome for the treated city, and Z_0 is
 2 the matrix containing the same information for the donor cities. We use R package ‘synth’ (4) to
 3 generate the synthetic control cities from a pool of 188 donor cities discussed below.

4 *Donor pool construction*

5 To apply the synthetic control method discussed above, we must first construct a donor pool of non-
 6 treated cities from which to synthesize our control cities. We begin with the set of all non-treated
 7 cities, then exclude cities that may have been affected by the treatment (Beijing’s car ownership
 8 restriction policy) or that may have suffered other large, idiosyncratic shocks that could affect the
 9 outcome of interest (the total number of cars registered in the city).

10

11 First, we exclude cities that are within a 1,000 km driving distance of Beijing but outside our treat-
 12 ment buffer because these cities may be slightly affected by Beijing’s car ownership restriction
 13 policy due to their vicinity to the city (see Figure 2). Cities in this area are not considered “clean”
 14 enough to be included in the control group because they may experience some residual spillover
 15 effects from the treatment. Therefore, we follow the advice of Abadie (2019) and exclude cities in
 16 reasonably close geographical proximity (within 1,000 km driving distance) to Beijing which may
 17 provide a biased estimate of the counterfactual outcome without treatment (2).

18

19 Second, we exclude cities that are within a 500 km driving distance of Guangzhou, because these
 20 cities may be subject to the policy leakage of Guangzhou’s car ownership restriction policy that
 21 was adopted towards the end of our study period (in 2012). Note that Guiyang also implemented a
 22 license plate policy in 2011 during our study period, but this policy is fundamentally a car use re-
 23 striction rather than a car ownership restriction; therefore, Guiyang and its surrounding cities were
 24 not excluded from the donor pool of non-treated cities. Shanghai also adopted a car ownership
 25 restriction policy in 1994 and a non-local car use restriction policy in 2002. Both of these policies
 26 were adopted well before the start of our study period and therefore can be seen as “constant”
 27 throughout from 2006-2013. Therefore, we do not exclude Shanghai or its surrounding cities from
 28 our donor pool. We also include city fixed effects in all of our models to help control for residual
 29 shocks that could affect the outcome of interest (number of personal cars registered) in each city.

30

31 Even after excluding these cities, our donor pool of non-treated cities still adequately covers the

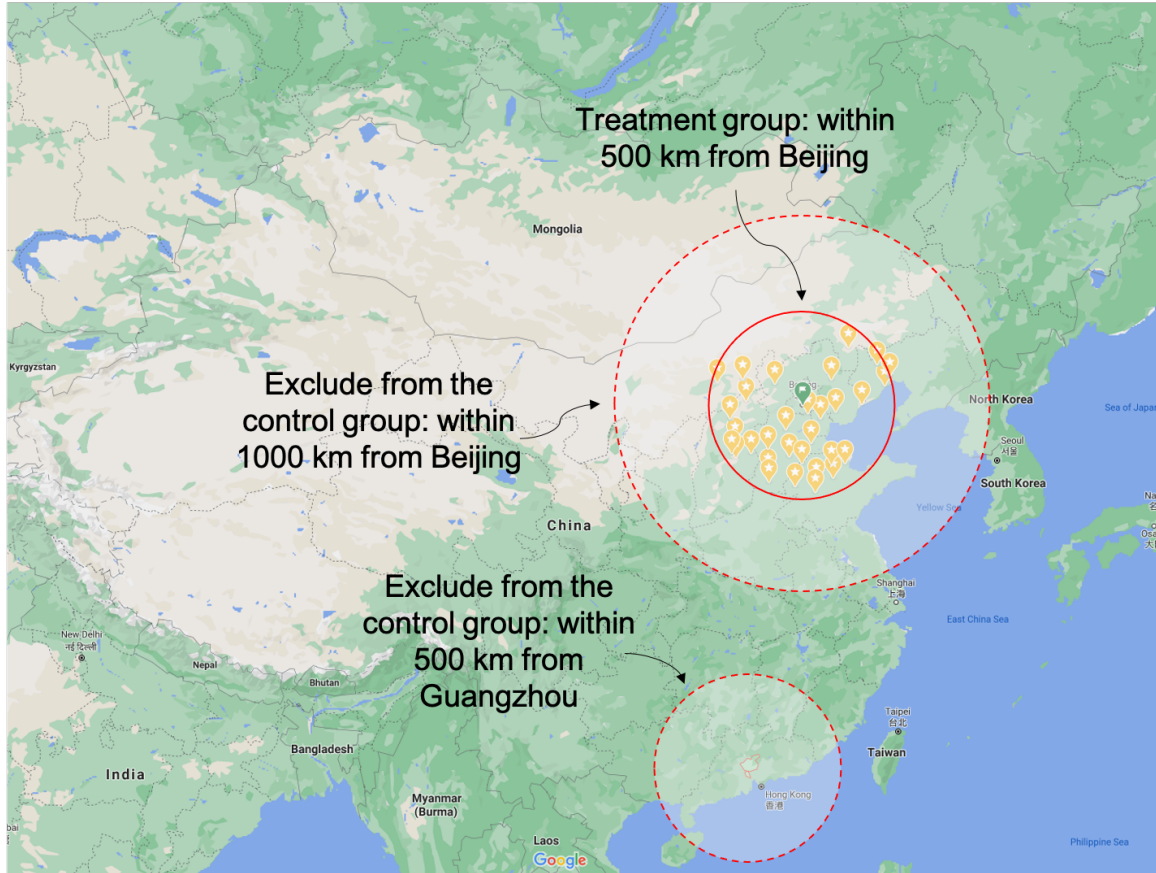


FIGURE 2: Geography of treated and non-treated cities (used for the synthetic control)

1 full distribution of pre-treatment characteristics observed in our treatment cities (see Figure 3).
 2 In fact, the plots show that the distribution of each characteristic in the donor group has a wider
 3 range than the distribution of that characteristic in the treated group. This means that there are no
 4 variable has an extreme values in the treatment group that could not be reflected in a synthetic city
 5 based on the donor group. We are thus confident that, using this donor pool, we can construct a
 6 synthetic control city that adequately matches each of our treated cities in terms of both dependent
 7 and independent variables within a 500 km driving distance of Beijing.

8 *Quality of the Synthetic Control Group compared to all Non-Treated Cities*

9 A good control group should exhibit similar characteristics to the treatment cities prior to the intro-
 10 duction of the car ownership restriction in 2011. Therefore, to check the quality of our controls, we
 11 can begin by comparing the pre-treatment characteristics of our treated cities with the non-treated
 12 cities in our donor pool and our synthetic control cities (see Table 1 and Figure 3). The summary
 13 statistics in Table 1 suggest that, prior to the implementation of Beijing's car ownership restric-
 14 tion, the synthetic control cities generally resemble their treated counterparts on all dependent and
 15 independent variables. In addition, the similarity of the variable distributions between the treated
 16 and synthetic cities in Figure 3 confirms that the synthetic control cities closely approximate the
 17 values of all variables for the treated cities during the pre-treatment period. This exploration of
 18 pre-treatment characteristics clearly show that the synthetic control approach produces a better

- 1 comparison group for the treated cities than the naïve control group of all non-treated cities in the
- 2 donor pool.

TABLE 1: Pre-treatment characteristics averaged over cities and years (2006-2010)

	Treated	Synthetic	Donor (non-treated)
Number of private cars (millions)	243.15 (173.71)	246.54 (171.82)	121.5 (149.4)
Administrative area (m^2) per capita	4541.8 (4714.95)	4912.61 (5232.66)	6771.79 (17461.68)
GDP (K RMB) per capita	29.13 (20.29)	24.21 (12.44)	22.8 (18.47)
Total registered population	4.8 (2.59)	5.02 (2.55)	4.18 (3.17)
Annual total wage (K RMB)/total population	2.64 (1.88)	2.27 (1.15)	2.21 (1.94)
Urbanization rate	0.31 (0.21)	0.3 (0.13)	0.32 (0.21)
Highway length (m) per 1,000 people	2994.32 (1265.4)	2890.95 (956.43)	2950.93 (1512.09)
Road area (m^2) per capita	3.31 (2.52)	2.79 (1.8)	2.85 (2.76)
Taxi number per 1,000 people	1 (0.71)	0.73 (0.44)	0.63 (0.66)
Bus number per 1,000 people	0.24 (0.21)	0.25 (0.17)	0.22 (0.24)
Government expenditure (RMB) per capita	3114.1 (1533.85)	2903.7 (932.26)	2957.13 (1748.01)
Total number of cities	31	31	188

Note: Table values represent means with standard deviations in parentheses.

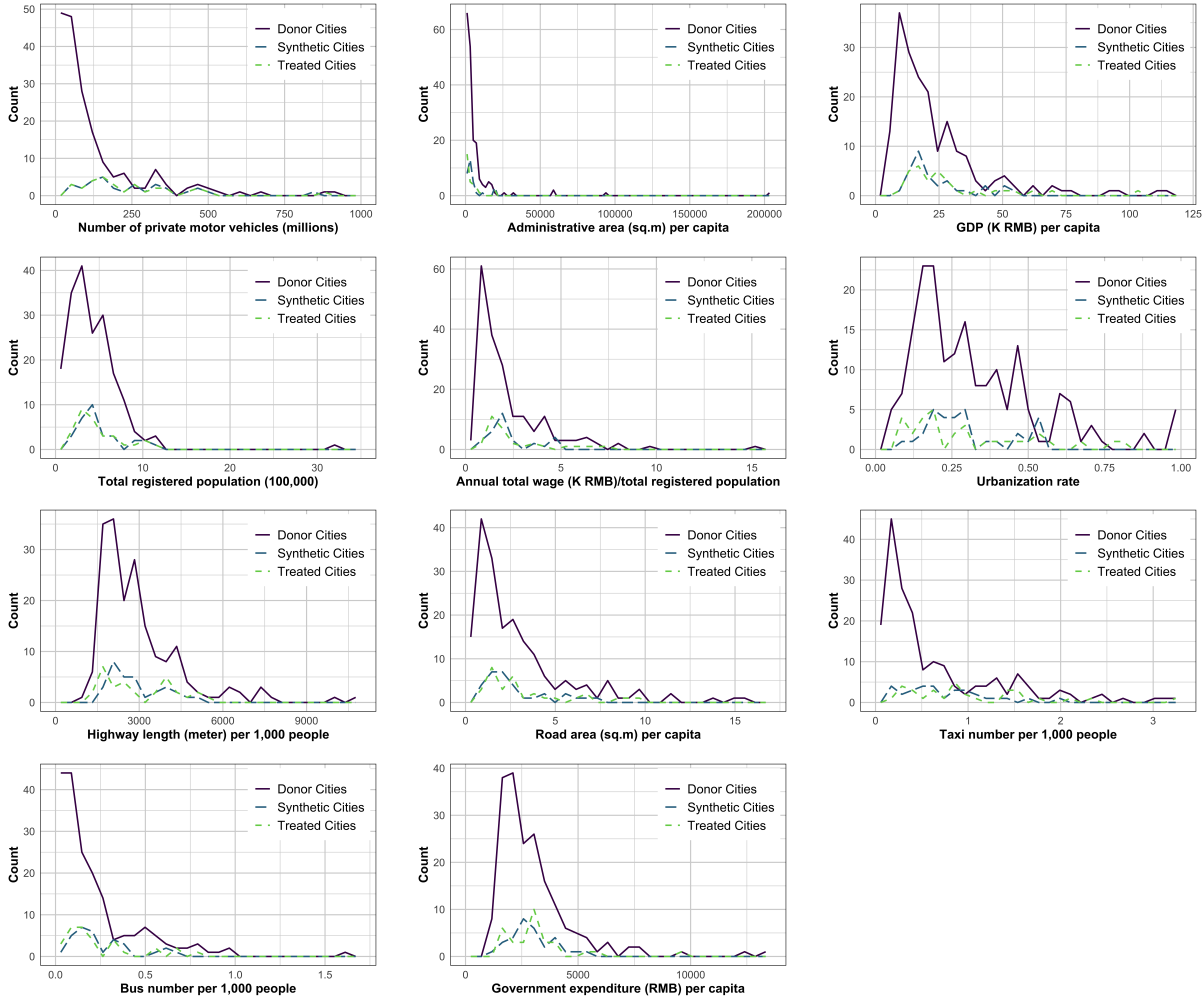


FIGURE 3: Distributions of pre-treatment characteristics averaged over cities and years (2006-2010): donor vs. synthetic vs. treated cities

1 As further evidence, we compare the time trends of the average number of private cars between the
 2 treated cities and our two different control groups (all non-treated cities and the synthetic cities) in
 3 Figure 4. The first panel in Figure 4, shows that using all non-treated cities as the control could
 4 be problematic, since their private car ownership trends overtime are not parallel, even before the
 5 implementation of Beijing’s policy in 2011 (the treatment). The second panel in Figure 4 shows
 6 that when using the synthetic cities as the control, the time trends between the treated cities and
 7 synthetic groups fit well before the policy implementation in 2011. After 2011, the treated cities
 8 appear to show an increase in private car population compared to the synthetic controls, which
 9 suggests the presence of some policy leakage (which we will measure using the DID models intro-
 10 duced in the next section). Overall, these visualizations suggest that the synthetic cities provide a
 11 much better control than the group of all non-treated cities in the donor pool.

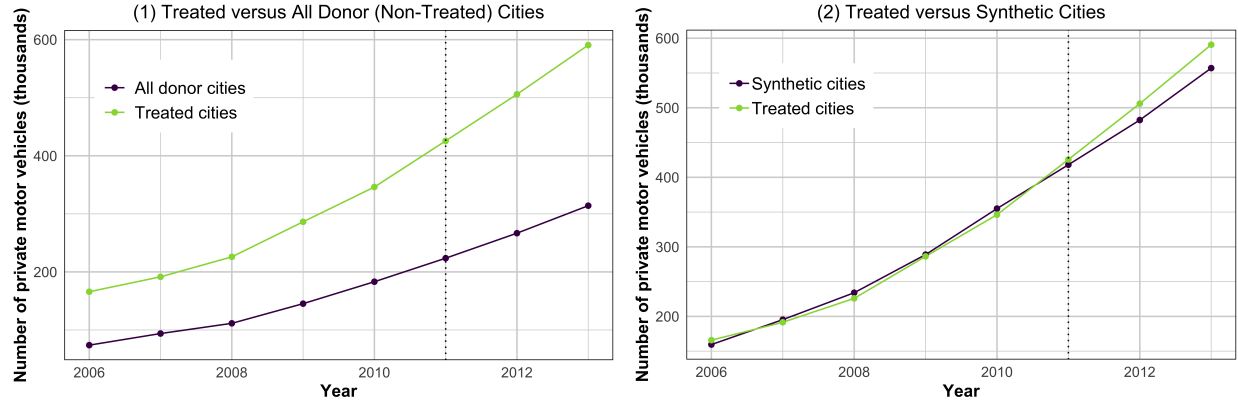


FIGURE 4: Trend in average number of private cars for (1) treated vs. donor cities and (2) treated vs. synthetic cities

1 Model Specifications

2 Having determined the appropriate time period, treatment group, and control group, we can now
3 estimate our DID models. The base model is expressed mathematically as in Equation (3):

$$Y_{it} = \rho_1[After2010_t * T_i] + \rho_2[Year2012_t * T_i] + \rho_3[Year2013_t * T_i] + \rho_4(t - 2010) + \rho_5[(t - 2010) * T_i] + \alpha_1 \mathbf{X}_{it} + \mathbf{c}_i + \mu_t + \varepsilon_{it} \quad (3)$$

4 where the subscripts i and t refer to city and year, respectively; T_i is a dummy variable that is
5 1 for the treated cities and 0 otherwise; $After2010_t$ is a dummy variable index for years after
6 2010; $Year2012_t$ and $Year2013_t$ takes a value of 1 for those years and 0 otherwise. To remove
7 the underlying time trend, we include $(t - 2010)$, which takes a value of zero in 2010.² We also
8 control for the heterogeneity in time trend across the treatment and control groups by incorporating
9 $[(t - 2010) * T_i]$. X_{it} denotes the control variables; \mathbf{c}_i is the city fixed effect; μ_t denotes the time
10 fixed effect which takes the value 1 after year 2010 and 0 otherwise; and ε_{it} is a random error term.
11 Of particular interest is the estimated value of ρ_1 , which represents the average treatment effect of
12 the policy intervention through 2011 to 2013.

13

The impact of Beijing's car ownership restriction policy for a treated city could depend on its distance to Beijing. It is reasonable to assume that the cities closer to Beijing are more likely to be affected by Beijing's car ownership restriction policy than those further away. To capture the distance variation, we further interact the distance and squared distance of the city with the DID term. This distance-decayed treatment model can be specified as in Equation (4), where D_i represents the driving distance from Beijing to city i derived from the Google Maps API:

$$Y_{it} = \rho_1[After2010_t * T_i] + \rho_2[After2010_t * T_i * D_i] + \rho_3[After2010_t * T_i * D_i^2] + \rho_4(t - 2010) + \rho_5[(t - 2010) * T_i] + \alpha_1 \mathbf{X}_{it} + \mathbf{c}_i + \mu_t + \varepsilon_{it} \quad (4)$$

²While it is conventional practice to include fixed effects for each individual year, literature has suggested that including a time trend and an indicator of the post-intervention period (17) is often sufficient for capturing the time effect. Recognizing that we have a somewhat limited sample size (with only 31 treated cities and 31 synthetic controls for each year), we value the model parsimony of including a time trend term rather than individual year dummies. Figure 4 also empirically suggests that the actual time trend can be well approximated with a linear term.

1 3. TOTAL POLICY LEAKAGE IN NEIGHBORING CITIES

2 In this section we present the results of our difference-in-differences models evaluating the growth
3 in private cars in Beijing's neighboring cities as a result of Beijing's car ownership restriction. The
4 choice of boundary of the treatment area can have an impact on our treatment city selections and
5 model estimation results. Therefore, we first compare results for the average treatment effect using
6 400 km, 500 km, and 600 km treatment boundaries and Equation (3). We then model the spatial
7 decay of the treatment effect by including an interaction term with distance using Equation (4).
8 Having determined the most appropriate treatment boundary, we translate our estimated average
9 treatment effect into a total number of private cars registered in neighboring cities as a result of
10 Beijing's car ownership restriction policy.

11 Treatment Effects with Different Boundaries

12 We present results estimated from Equation (3) in Table 2. We test the sensitivity of these results to
13 the boundary distance used to define the treated cities. Columns (1), (3) and (5) test for the yearly
14 average treatment effect of Beijing's car ownership restriction policy on neighboring cities across
15 2011-2013, whereas Columns (2), (4) and (6) take into account the heterogeneity of the three years
16 in the post-treatment period.

17
18 The result shows that if we only include cities within 400 km driving distance from Beijing as
19 the treated cities as in Column (1), the impact of Beijing's car ownership restriction policy on car
20 numbers in the treated cities is statistically insignificant. This insignificant effect may be partially
21 attributed to the limited number of cities within 400 km, which reduces the model sample size and
22 statistical power. When we change the treatment boundary from 400 km to 500 km, the treatment
23 effect becomes significant. Column (3) shows that, on average, the number of private cars in treated
24 cities within 500 km driving distance from Beijing grew by about 5% more than in their control city
25 counterparts after Beijing's policy was implemented. The magnitude and significance of the result
26 are consistent even if we take the yearly heterogeneity into account as in Column (4). However, the
27 coefficients for $Year2012_t * T_i$ and $Year2013_t * T_i$ are negative but not statistically significant, which
28 suggests that the leakage effects in 2012 and 2013 do not differ significantly from the treatment
29 effect in 2011. Further expanding the treatment boundary to 600 km as in Columns (5) and (6),
30 we see that the estimated treatment effect is slightly lower, but still significant. The decrease
31 of treatment effect with the expansion of the treatment boundary makes intuitive sense because
32 Beijing residents may not want to take the trouble to register their cars in cities that are further
33 away. The insignificant coefficient for $(t - 2010) * T_i$ (ρ_5) shows that the heterogeneity in time
34 trend across the treatment and control groups is insignificant.

TABLE 2: Treatment effect of Beijing's car ownership restriction policy on number of private cars in neighboring cities estimated using Equation (3)

	The maximum distance to Beijing for the treatment cities:					
	400 km		500 km		600 km	
	(1)	(2)	(3)	(4)	(5)	(6)
$After2010_t * T_i (\rho_1)$	0.104 (0.064)	0.110 (0.080)	0.050** (0.021)	0.056*** (0.018)	0.048* (0.025)	0.054** (0.026)
$Year2012_t * T_i (\rho_2)$		-0.012 (0.024)		-0.020 (0.021)		-0.020* (0.012)
$Year2013_t * T_i (\rho_3)$		-0.036 (0.044)		-0.003 (0.028)		-0.005 (0.016)
$t - 2010 (\rho_4)$	0.232*** (0.082)	0.243*** (0.092)	0.176*** (0.026)	0.174*** (0.024)	0.173*** (0.029)	0.172*** (0.029)
$(t - 2010) * T_i (\rho_5)$	-0.011 (0.023)	-0.009 (0.027)	-0.004 (0.010)	-0.004 (0.012)	-0.007 (0.010)	-0.006 (0.012)
Administrative area	1.106 (0.735)	1.089 (0.745)	0.720*** (0.213)	0.715*** (0.215)	0.438 (0.384)	0.438 (0.386)
GDP	0.207 (0.126)	0.178 (0.131)	0.520*** (0.111)	0.516*** (0.117)	0.372*** (0.125)	0.367*** (0.126)
Population	1.385*** (0.489)	1.348*** (0.499)	1.054 (0.885)	1.049 (0.870)	0.680 (0.473)	0.675 (0.465)
Wage	-0.176 (0.110)	-0.175 (0.113)	-0.314*** (0.107)	-0.311*** (0.113)	-0.282*** (0.079)	-0.279*** (0.081)
Urbanization rate	0.178 (0.177)	0.165 (0.185)	0.111 (0.167)	0.113 (0.166)	0.090 (0.111)	0.090 (0.110)
Length of highway	0.072 (0.109)	0.095 (0.120)	0.069 (0.066)	0.068 (0.066)	0.044 (0.064)	0.044 (0.064)
Road area	-0.073 (0.050)	-0.073 (0.054)	-0.148*** (0.051)	-0.147*** (0.052)	0.019 (0.085)	0.020 (0.086)
Taxi number	-0.036 (0.084)	-0.039 (0.093)	-0.068 (0.069)	-0.073 (0.078)	-0.033 (0.067)	-0.037 (0.073)
Bus number	0.017 (0.049)	0.022 (0.047)	0.067** (0.027)	0.067** (0.028)	0.004 (0.036)	0.005 (0.036)
Government expenditure	-0.187 (0.179)	-0.222 (0.208)	0.018 (0.109)	0.024 (0.106)	0.069 (0.095)	0.072 (0.090)
Observations	240	240	496	496	576	576
Adjusted R^2	0.991	0.991	0.983	0.983	0.983	0.983

Note: Standard errors in parentheses are clustered on cities. The dependent variable and the socio-economic variables are log transformed. Administrative area, GDP, wage, road area, and government expenditure are all expressed per capita; length of highway, taxi number and bus number are expressed per 1,000 people. All the models take into account city fixed effect and time fixed effect (which assigns 0 to pre-treatment time and 1 to post-treatment time).

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

TABLE 3: Variation of the treatment effect in terms of cities' distance to Beijing estimated using Equation (4) with a 600 km treatment boundary

	<i>Interaction term between distance and treatment:</i>		
	None	Linear	Quadratic
	(1)	(2)	(3)
$After2010_t * T_i (\rho_1)$	0.048* (0.025)	0.218*** (0.051)	0.118 (0.097)
$After2010_t * T_i * D_i (\rho_2)$		-0.00044** (0.00019)	0.00025 (0.00072)
$After2010_t * T_i * D_i^2 (\rho_3)$			-0.0000 (0.0000)
$t - 2010 (\rho_4)$	0.173*** (0.029)	0.179*** (0.027)	0.175*** (0.026)
$(t - 2010) * T_i (\rho_5)$	-0.007 (0.010)	-0.008 (0.010)	-0.007 (0.009)
Administrative area	0.438 (0.384)	0.457 (0.362)	0.452 (0.357)
GDP	0.372*** (0.125)	0.414*** (0.133)	0.421*** (0.130)
Population	0.680 (0.473)	0.620 (0.466)	0.639 (0.465)
Wage	-0.282*** (0.079)	-0.302*** (0.068)	-0.289*** (0.065)
Urbanization rate	0.090 (0.111)	0.115 (0.120)	0.118 (0.121)
Length of highway	0.044 (0.064)	0.045 (0.063)	0.049 (0.064)
Road area	0.019 (0.085)	0.008 (0.070)	-0.007 (0.063)
Taxi number	-0.033 (0.067)	-0.065 (0.082)	-0.056 (0.080)
Bus number	0.004 (0.036)	0.024 (0.035)	0.025 (0.036)
Government expenditure	0.069 (0.095)	0.027 (0.118)	0.032 (0.118)
Observations	576	576	576
Adjusted R ²	0.983	0.984	0.984

Note: D_i denotes the distance of city i to Beijing (in km). The dependent variable (total number of private cars) and the socio-economic variables are all log transformed. Administrative area, GDP, wage, road area, and government expenditure are all expressed per capita; length of highway, taxi number and bus number are expressed per 1,000 people. All the models take into account city fixed effect and time fixed effect.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

1 **Spatial Decay of Treatment Effect**

2 We also test for the spatial decay of the treatment effect by including the interaction term between
 3 the distance to Beijing and the DID estimator using the 600km treatment boundary and Equation
 4 (4). Columns (2) and (3) in Table 3 employ linear and quadratic relationships between the distance
 5 to Beijing and the policy effect, respectively. With the linear relationship, the coefficients of the
 6 DID term and the interaction term are both significant as shown in Column (2), while in the model
 7 with the quadratic relationship, Column (3), the DID term and the interaction terms are both in-
 8 significant. Therefore, we focus on the spatial decay of the treatment effect using the linear model.
 9 The negative coefficient of the interaction term reflects that the effect declines by 0.04% every 1
 10 km of driving distance away from Beijing within the 600 km boundary.

11

12 Using the intercept and slope coefficients in Column (2) of Table 3, we can estimate a linear
 13 relationship between the treatment effect and distance from Beijing, D_i (from 0 km to 600 km).
 14 Our model suggests that the impact of Beijing's car ownership restriction policy on the private
 15 car population in neighboring cities declines with increasing distance to Beijing. We find that the
 16 treatment effect decays to zero around 500 km driving distance from Beijing, providing further
 17 evidence that it is reasonable to choose 500 km as the boundary for selecting the treated cities
 18 since the treatment effect vanishes beyond this boundary.

19 **Estimated Treatment Effect in Terms of Total Number of Private Cars**

20 Based on the above analysis, we revisit model (3) in Table 2 and use it to estimate the increase in
 21 total number of private cars in Beijing's neighboring cities resulting from Beijing's car ownership
 22 restriction policy. We find that the 5.0% increase in private cars translates to approximately 443,000
 23 additional cars sold in the 31 cities within a 500 km driving distance of Beijing in the three-year
 24 period from 2011 to 2013. This number is calculated by taking 300,000 (the average number of
 25 private cars in treated cities in the pre-treatment period before 2011 and in synthetic cities during
 26 2006-2013), multiplying it by 31 (the number of treated cities), and accounting for the average
 27 treatment effect from 2011 to 2013 which is 0.050: $300,000 * 31 * (1 - 1/(1 + 0.05)) = 442,857$
 28 cars. Note that the variation of the effect across 2011-2013 is insignificant as evidenced by Column
 29 (4) in Table 2, thus the 443,000 cars is the increase in the number of cars in treated cities due to
 30 the cumulative treatment effect of Beijing's policy from 2011 up until 2013.

31 **4. EFFECT OF BEIJING'S CAR OWNERSHIP RESTRICTION ON PRIVATE CAR GROWTH**

32 Given this significant policy leakage in Beijing's neighboring cities, a natural follow-up question
 33 is: how effective is Beijing's car ownership restriction in reducing private car growth discounting
 34 this leakage?

35

36 To answer this question, we must first estimate a no-policy counterfactual—a projection of the
 37 growth in private car registrations that would have occurred from 2011-2013 had no policy been
 38 implemented. We follow the approach developed by Yang et al. (2014) that calculates the coun-
 39 terfactual sale of private cars in Beijing under the no-policy scenario relying on the elasticity of
 40 demand for cars with respect to Beijing's Gross Regional Product (GRP) (40).³ With this method,
 41 we first record Beijing's GRP from 2003 to 2013. Next, we calculate the historical elasticity of

³Many previous studies have shown that GDP/GRP is a key variable used to model and predict car ownership growth at the city and national level (21, 37, 40).

1 growth in the number of private cars with respect to GRP growth in our pre-treatment period 2003-
 2 2009, which gives us an average elasticity of 1.37 during these 7 years. We remove 2010 from
 3 our calculation in case there was a surge in demand at the end of the year when news of the car
 4 ownership restriction policy surfaced, but it was not yet in effect (although as noted previously the
 5 period for this anticipatory response was very short). Instead, we assume that the elasticity is 1.37
 6 in 2010. Due to uncertainty in these projections, three scenarios are tested for sensitivity analysis
 7 based on Yang et al. (2014): (1) the elasticity remains constant from 2010 to 2013 (the high-growth
 8 scenario); (2) the elasticity falls linearly to 0.8 in 2020 (the medium-growth scenario), and (3) the
 9 elasticity falls linearly to 0.4 in 2020 (the low-growth scenario). The projected private car numbers
 10 in 2011-2013 for these three scenarios are shown in Figure 5. The medium-growth scenario is
 11 treated as the counterfactual for the following discussion. Under the medium-growth scenario, we
 12 project that private car registrations would have reached 5,647,000 in 2013 in the absence of any
 13 car ownership restriction policy.
 14

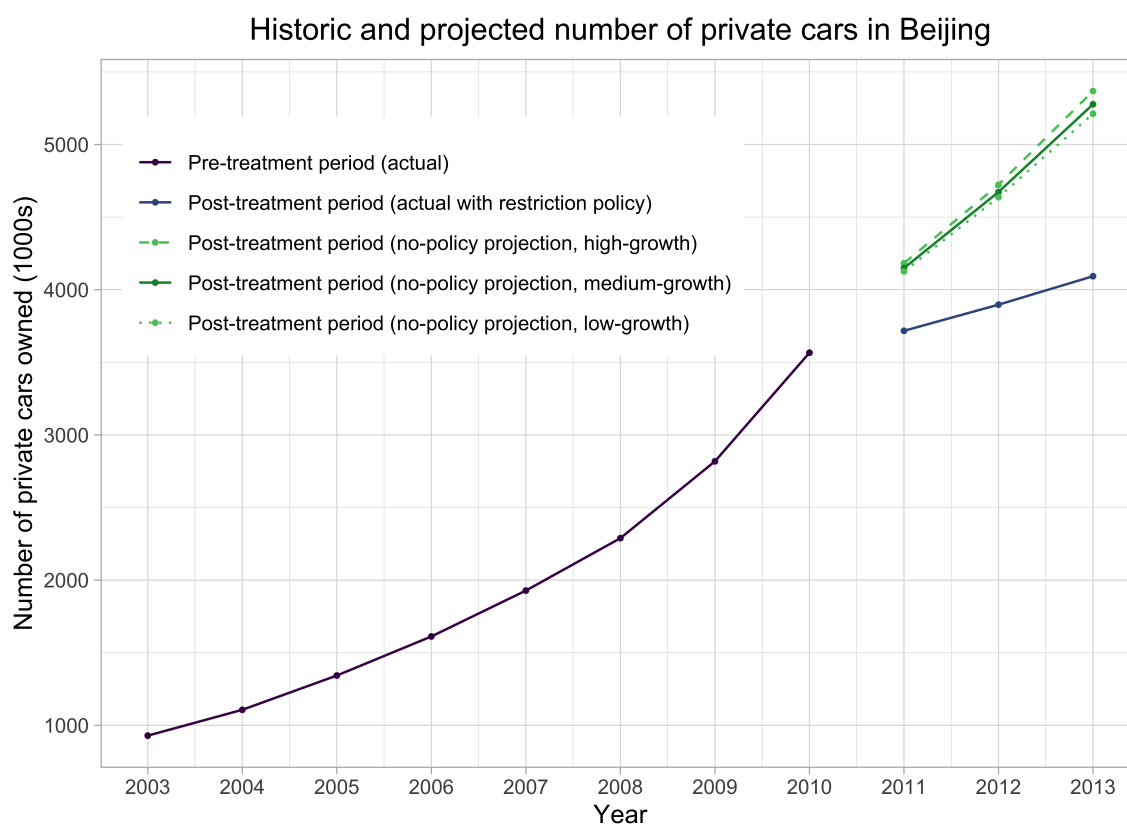


FIGURE 5: Projection of private car population in Beijing under multiple no-policy scenarios and under the policy assuming no leakage

15 We then compare these no-policy projections of the number of car registrations to actual car regis-
 16 trations in Beijing under the policy as diagrammed in Figure 5. Assuming no policy leakage, only
 17 as many new cars as there are license plates allocated by quota would be sold: an increase of only
 18 527,000 cars from 3,566,000 in 2010 to 4,093,000 in 2013. Comparing this to our no-policy pro-
 19 jections, we calculate the impact of Beijing's car ownership restriction policy in reducing growth

1 of private car sales assuming no policy leakage (i.e., the policy is 100% effective). We estimate
 2 that, ignoring leakage, Beijing’s policy blocked the registration of 1,119,000-1,275,000 new pri-
 3 vate cars (see Table 4.)

4
 5 However, our DID models suggest that an additional 443,000 private cars were sold in neighboring
 6 cities in response to Beijing’s car ownership restriction policy. Comparing this estimated magni-
 7 tude of policy leakage to the calculated car reduction assuming no policy leakage suggests that
 8 as much as 35%-40% of the growth in private car ownership that could have been reduced by the
 9 policy simply spilled over to neighboring cities (Table 4). In particular, taking the medium-growth
 10 scenario as an example, our study suggests that the actual reduction of private car registrations
 11 from 2011 to 2013 (accounting for policy leakage to neighboring cities) was 741,000 cars—only
 12 62.6% of the 1,184,000 predicted assuming complete policy effectiveness.

TABLE 4: Estimated reduction in private car numbers in Beijing with and without accounting for policy leakage

No-policy scenario (counterfactual projections)	Reduction of Beijing cars assuming no leakage	Policy leakage as % of reduction of Beijing cars assuming no leakage
High-growth—elasticity remains constant at 1.37 from 2010-2013	1,275,000	34.7%
Medium-growth—elasticity decreases linearly from 1.37 in 2010 to 0.8 in 2020	1,184,000	37.4%
Low-growth—elasticity decreases linearly from 1.37 in 2010 to 0.4 in 2020	1,119,000	39.6%

Note: Elasticity refers to the percentage change in the number of private cars with a 1% change in GRP.

13 5. DISCUSSION

14 Although the existence of policy leakage around car ownership restrictions have been covered by
 15 some existing literature (16, 20, 43), this research is the first to quantify this effect for Beijing’s car
 16 ownership restriction policy. We find that policy leakage undermines the effectiveness of Beijing’s
 17 policy by as much as 35%-40%. This presents a challenge for policymakers trying to curb growth
 18 in car ownership (and, indirectly, use) in the capital. This policy leakage fundamentally exists
 19 because the jurisdictional boundary of the policy (Beijing city) does not match the metropolitan
 20 scale of personal car ownership and use. In other words, there is an inherent tension between the
 21 local rationing of car ownership and the mobility of vehicles and people. Accordingly, we discuss
 22 two potential ways to address this policy leakage: engaging in collaborative policymaking at the
 23 regional scale or enacting additional local policies that make it more difficult for those registering
 24 cars in neighboring cities to bring their cars back into Beijing.

25 Regional Cooperation

26 One way to cope with the policy leakage issue would be to implement bureaucratic regimes that
 27 foster regional cooperation surrounding car ownership restrictions (43). This would require cities

1 neighboring Beijing to either join in implementing or at least enforcing Beijing's car ownership
2 restriction policy. However, there are three reasons why enforcing Beijing's policy in neighboring
3 cities may be difficult. First, the policy leakage actually generates positive fiscal gains for Beijing's
4 neighboring jurisdictions, which have the incentive to collect fees from cars that do not travel on
5 their roads (33). Second, the channel of non-local licenses registration stimulated speculative
6 activities, which included many car dealers and traders providing agent services. These activities
7 not only generated a black market, but also removed barriers for the non-local license registrations
8 (45). Finally, the ineffectiveness of the car ownership restriction policy may lead to loss of public
9 faith in the ability of local government to enforce this type of regulation (20), potentially de-
10 legitimizing further regulations.

11 **Local Actions to Address Noncompliance**

12 The alternative to collective, regional enforcement is for policymakers in Beijing to enact addi-
13 tional policies within their limited jurisdiction that make non-local licenses less attractive. We
14 already see evidence of this trend. During the first 3 years of policy implementation, the penalties
15 for non-compliance were not very severe (35). Before 2014, permits allowing non-local cars to
16 drive within Beijing's 5th ring road only needed to be renewed once every 6 months (11). This
17 made registering a new car in a neighboring city and then driving in Beijing on that non-local rel-
18 atively simple and convenient. However, recently the government has taken measures to curb the
19 inflow of non-local cars using Beijing's streets. For example, in November 2013, the penalty for
20 driving non-local cars in central areas during peak hours has been intensified, now amounting to 3
21 points deduction in addition to a fine of 100 yuan (10). At the beginning of 2014 the validity period
22 of the permits for non-local cars to drive in Beijing was reduced from 6 months to 7 days (although
23 the permits can be renewed without restrictions). Starting in November 2019, the authority has
24 announced that cars not licensed by Beijing will only be allowed to renew their permits to enter the
25 city 12 times a year, with each permit remaining effective for 7 days (9), and the restriction area
26 for non-local cars was enlarged from the 5th ring road to the 6th ring road (8). This means that
27 cars with non-local licenses will only be able to drive a maximum of 84 days of the year within
28 Beijing's 6 ring area.

29

30 This entire suite of "closure" policies discourages non-local cars from driving in Beijing. While
31 making noncompliance with the car ownership restriction policy less attractive, these additional
32 policies come with a cost. In addition to additional administrative overhead, these use restric-
33 tions may curtail the free flow of labor and materials between Beijing and its neighboring cities.
34 Though originally designed for non-compliant Beijingers, these policies may unintentionally pre-
35 vent people who occasionally ran errands in Beijing from entering the city, which raised new equity
36 concerns (45). In addition, it raised debate about regional discrimination, since Beijing cars can
37 drive in other cities without restrictions but not vice versa.

38 **Other Potential Complementary Policies**

39 The loopholes of the current policy design necessitate future policy adjustment. In the near-term,
40 non-local car policies could be better nuanced. The evolution of Beijing's non-local car related
41 policies show that policymakers are aware of potential policy leakage and trying to address it.
42 However, these additional policies come at a cost that may not be fully weighted against the ben-
43 efits of improving the efficiency of the car ownership restriction. In order to avoid negative con-

1 sequences of these stricter non-local car policies, Beijing officials may want to consider careful
2 differentiation of "localized" non-local cars, which belong to Beijingers and stay in the city, and
3 normal non-local cars which occasionally enter Beijing for purposes such as tourism, business, and
4 freight transport (45).

5

6 In the longer-term, dependence on cars for travel needs will need to be addressed. The popularity
7 of private cars in Beijing is often attributed to the imbalance of job-housing locations, since a great
8 number of population live in the periphery area and work in central Beijing (12). Policymakers
9 should work on the co-location of jobs and housing through urban planning as a way to reduce
10 commuting. And complementary improvements to the public transit system could help meet res-
11 idents' travel needs more efficiently than private cars. In essence, the policy leakage dilemma
12 reflects the unbalanced development between Beijing and its neighboring cities. As such, more
13 coherent transportation policies should be adopted as part of the regional integration process in the
14 future.

15 **6. LIMITATIONS AND FUTURE WORK**

16 While this study included the careful determination of the most appropriate study period and choice
17 of treatment and (synthetic) control cities, there are still a few limitations to this work that warrant
18 further research. A key identification challenge is that the treated cities are not randomly located
19 in space, but are rather within a small distance from Beijing. Therefore, some unobserved spatially
20 correlated characteristics may have a similar impact on the car populations if they also took effect
21 in 2011. Though according to our knowledge, no policy shocks that could have affected car popu-
22 lations in Beijing's neighbor cities except Beijing's car ownership restriction policy took effect in
23 2011, we recognize that this could be a limitation of our study.

24

25 Second, we recognize that reliance on geographical proximity to determine treatment and control
26 cities may be imperfect. There are ways in which Beijingers can obtain non-local license plates
27 from anywhere (not just neighboring cities), particularly through 4S shops and car dealerships.
28 While anecdotal evidence suggests that the majority of non-local car plates obtained through 4S
29 shops in Beijing still come from neighboring provinces such as Hebei (22), these other avenues for
30 policy leakage could present a challenge to our modeling approach.

31

32 Finally, there are other channels through which Beijing residents may attempted to circumvent the
33 car ownership restriction policy. Rather than registering cars in neighboring cities, anecdotal evi-
34 dence suggests that some Beijing residents turned to buying vans, motorcycles, or other vehicles
35 not restricted by the license plate quotas (14, 29). While private cars account for a majority of
36 the vehicles on Beijing's roads, future research could measure the magnitude of policy leakage
37 through the purchase of other vehicle types. In addition, while our study focuses on the mar-
38 ket for individually-owned cars, Beijing also has a separate license plate policy for official cars,
39 with a much smaller monthly quota (around 3,000-4,000 official cars compared to 17,500-20,250
40 individually-owned cars). If appropriate data sources can be identified, an interesting extension of
41 our work could be an investigation of policy leakage around the official car quota and potential
42 interplay between individually-owned and official cars.

43

1 **7. CONCLUSION**

2 Beijing's car ownership restriction was enacted to curb rapid motorization in the city. Setting strict
3 quotas on the number of new private cars that could be purchased and registered within the city,
4 Beijing ostensibly appeared to reduce the number of new cars sold by 1,119,000-1,275,000 vehi-
5 cles under the first three years of the policy. However, these estimates of the efficacy of the policy
6 do not account for rampant non-compliant behavior, with significant anecdotal evidence pointing
7 to the fact that Beijing residents circumvented the policy by purchasing cars in neighboring cities
8 and then driving them back to Beijing. This research is the first to empirically test for the existence
9 of this policy leakage, to measure its magnitude, and to explore how much it undermined the pol-
10 icy's effectiveness.

11

12 To investigate the causal effect of Beijing's car ownership restriction policy on the growth of pri-
13 vate cars in neighboring cities, we conduct a difference-in-differences analysis of car registrations
14 in neighboring cities using a distance-based treatment. To mitigate the selection bias of the con-
15 trol group, we use a synthetic control method to create a weighted combination of non-treated
16 cities for each treated city based on the pre-intervention outcome and other predictors of the post-
17 intervention outcome. Our results provide strong empirical evidence that car registrations increased
18 in neighboring cities after the implementation of Beijing's policy, with an average causal effect of
19 about 5%. Factoring in variation by distance, we find that the magnitude of the policy leakage
20 declines by 4% every 100 km of driving distance from Beijing. From our models, we estimate
21 that approximately 443,000 additional private cars were sold in Beijing's neighboring cities from
22 2011 to 2013 compared to what we would expect had Beijing not implemented the car ownership
23 restriction.

24

25 In fact, we find that this policy leakage likely undermined the effectiveness of Beijing's policy by
26 as much as 35%-40% from 2011-2013. The evidence of policy leakage raises concerns regarding
27 consequences of policy non-compliance, including dilution of the ability to control the number of
28 private cars both in Beijing and in its neighboring cities, failure to alleviate traffic congestion, and
29 loss of public faith in the government's ability to enforce regulations. These issues necessitate pol-
30 icymakers and researchers to put Beijing's congestion issues and car ownership and use policies in
31 a regional context, confronting potential noncompliance at their source. That being said, additional
32 research is needed to explore the impacts of the policy leakage measured here (in terms of new car
33 sales) on related issues such as air pollution and traffic congestion. To explore these impacts, fur-
34 ther information would be needed on how non-locally licensed cars are driven in Beijing and its
35 neighboring cities. If the cars purchased in neighboring cities are driven less and during non-rush
36 hour times compared with locally-licensed cars, then primary policy goals of congestion and air
37 pollution mitigation might still be accomplished.

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