The Effects of Car Driving and Purchasing Restrictions on Air Quality and the Use of Public Transportation in Beijing, China

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Abstract

During the Olympic Games in 2008, a driving restriction based on vehicle license plate numbers was implemented in Beijing to mitigate air pollution and traffic congestion. Following the Games, the restriction was modified several times. This paper investigates the effects of two policy changes: a weakening policy change due to a shorter restricted time period, and a strengthening policy change due to a higher penalty for violators and the complementary car purchasing restriction. By employing a regression discontinuity design, I find that the weakening policy change led to more pollution in restricted areas only, while the strengthening policy change improved air quality in both restricted and non-restricted areas. One possible explanation for the second result is that driving in restricted areas and non-restricted areas are complements. Several robustness checks also confirm the results. I also provide suggestive evidence that driving restrictions increased the use of public transportation and alleviated traffic congestion.

Contents

1 Introduction

As the capital city of one of the oldest countries around the world, Beijing attracts the eyes of the world for its long history and splendid culture, its centuries-old parks and palaces, and its rapid economic development in recent decades. However, its severe air pollution also draws a lot of attention. Beijing is jokingly called "Greyjing" for its grey sky caused by air pollution. According to the Economist Intelligence Unit (EIU)'s global liveability survey in 2012, Beijing was ranked 32 among the best cities to live in. However, its score for pollution was the worst among the cities ranked in the top 50, which was 4.5 out of 5 (1=best, 5=worst). Air pollution has long been considered an important issue around the world, since it affects human health and even leads to deaths. Epidemiological studies have shown that air pollution could cause respiratory infections, heart diseases, and lung cancer, etc.[1] According to the World Health Organization (WHO), urban outdoor air pollution is estimated to cause 1.3 million deaths worldwide per year, and those living in middle-income countries disproportionately experience this burden.[2] Some recent studies also show that air pollution results in an increase in the mortality rate, especially in developing countries (e.g. Greenstone and Hanna, 2011; Tanaka, 2010).

The main sources of air pollution come from industrial plants, power plants, vehicles, and natural processes such as wildfires and volcanic eruptions. Since most of the factories have been moved out of Beijing, and wildfires and volcanic eruptions do not happen frequently in Beijing, most of Beijing's air pollution could be attributed to vehicle emissions. Chai Fahe, deputy head of the Chinese Research Academy of Environmental Sciences, said that more than 5 million vehicles are currently registered in Beijing, and the number keeps climbing.[3] In the meantime, traffic congestion itself is also a severe problem in Beijing. As an easy-to-implement regulation, driving restrictions based on license plate numbers have been used in many cities to alleviate air pollution and traffic congestion, such as Mexico City, Sao Paulo and Bogota. To reduce air pollution and traffic congestion in advance of the Olympic Games in 2008, a series of driving restrictions was implemented in Beijing to mitigate air pollution and traffic congestion. During the Olympic and Paralympic Games, cars with license plate numbers ending with odd and even digits could not drive on the road on alternate days. Following the Games, the policy was modified several times, including a window with no restrictions, change in restriction areas and time periods, change in penalty, and the implementation of a

[1]For a review of epidemiological studies on the respiratory effects of air pollution, please refer to Lebowitz (1996). For a review of epidemiological studies on the relationship between particulate matter and heart disease, please refer to Annette Peters (2005). For a review of epidemiological studies on the relationship between air pollution and lung cancer, please refer to Cohen and Pope (1997).

[2]From WHO website: http://www.who.int/mediacentre/factsheets/fs313/en/

[3]From China Daily, Jun.11, 2012. http://www.chinadaily.com.cn/regional/2012-06/11/content_16133037.htm.

complementary policy to restrict vehicle purchase.

Previous studies of a similar policy in Mexico City do not show a significant effect of driving restriction. Gasoline demand in Mexico City increased because of the policy, since people tended to respond by purchasing another car, using older cars, and increasing their driving on weekends and nonpeak weekdays (Eskeland and Feyzioglu, 1997). Households adjusted their stock of vehicles rapidly, within a year (Gallego et al., 2012). The restriction has engendered a relative increase in air pollution during weekends and nonpeak weekdays, but there is no evidence of an absolute improvement in air quality during peak weekdays (Davis, 2008). Studies on a series of other cities like Sao Paulo and Bogota also show mild or trivial alleviation of air quality (Lin et al., 2011).

Some studies have also focused on the restrictions in Beijing. Chen et al. (2011) find a significant but temporary reduction in air pollution that was associated with the restriction during the Olympic Games. Their analysis on satellite based AOD data also confirms that the air quality improvement in Beijing existed but was temporary. But Chen et al. (2011) only focus on the restriction during the Olympic Games, when there were many confounding factors, such as blockage of the roads, limitation of traveling, etc. They do not take all of these factors into account, which could severely bias their results. Viard and Fu (2011) also find a significant pollution reduction that was coincided with Beijing's driving restrictions. The effects were also related to the distance between the air quality monitoring stations and the main roads. They also consider the effect of the driving restrictions on labor supply by measuring substitution to TV viewership and find that workers with discretion over their work time increased their viewership during restricted hours. Similar to Chen et al. (2011), Viard and Fu (2011) do not consider the confounding factors during the Olympic Games. Moreover, their study on policies in other periods is also problematic. They compare all the policies with baseline periods without restrictions. Many things changed during the time periods that they study on, but they do not control for such factors as population, oil prices, etc, all of which could have impacts on air quality. Therefore, previous studies using OLS without controlling for all of these factors should be taken with caution.

Distinguishing from previous studies on driving restrictions in Beijing, this paper investigates the effects of two policy changes: a weakening policy change due to a shorter restricted time period, and a strengthening policy change due to a higher penalty for violators and the complementary restriction on purchasing a second car. By employing a regression discontinuity design, the confounding factors mentioned above have been taken into account and the results would no long be biased. The primary air quality data used in this paper is the daily station-level API panel data from the Beijing Municipal Environmental Protection Bureau

(BMEPB) for Jan.1, 2008-Oct.10, 2012. Among the 27 monitoring stations, eight of them are located within the 5th ring (restricted areas) and nineteen lie outside the 5th ring (non-restricted areas).[4] To evaluate possible spillover effects, I compare the results from restricted areas and those from non-restricted areas. The results show that the weakening policy change led to more pollution in only restricted areas, while the strengthening policy change improved air quality in both restricted and non-restricted areas. It seems to be surprising that both restricted and non-restricted areas were influenced by a strengthening policy change. One possible explanation is that driving in restricted areas and non-restricted areas are complements. Robustness checks including weather covariates, month dummies, station fixed effects, and employing different time windows confirm the main results. To investigate into the mechanisms underlying the effects, I also try to analyze the possible substitution effect between private car and public transportation caused by the driving restrictions. The results show some evidence that the restrictions were coincided with an increase in the use of public transportation and a reduction in traffic congestion, but the results should be taken with caution because of data limitations and weak identification problem under OLS regression.

This paper contributes to the existing literature in three ways. First, by employing a RD design, I fix the potential problems existing in previous Beijing studies as mentioned above, so that I could get more reliable results. Second, I investigate the effects of two different policy changes, which gives some implications on how changes in policy making could affect air pollution. This provides a useful reference for policy makers, as no one has ever studied these changes before. Finally, no previous research has studied the effects of driving restriction on transportation and congestion in Beijing. Although this paper could not investigate this question deeply due to data restrictions and weak identification problem, it fills in the gap in the literature.

The rest of the paper is organized as follows. Section 2 reviews previous literature. Section 3 introduces more detailed background on the policies and describes the data. Section 4 investigates the empirical strategy used in this paper. Section 5 and 6 report main results and robustness checks, respectively. Section 7 concludes.

2 Literature Review

Driving restrictions have been used in many cities around the world to alleviate air pollution and traffic congestion, such as Mexico City, Sao Paulo, Bogota, and some cities in China including the capital Beijing. In November 1989, a policy restricting drivers from using their vehicles one weekday per week was imposed in

[4]Ring roads in Beijing are the main roads that surround the center of the city. The fifth ring is regarded as the threshold between the main city and suburbs, within which is the main city, while outside are suburbs.

Mexico City. In the winter 1995, a driving restriction which restricted the use of 20% of the car fleet between 7:00 a.m. and 8:00 p.m. on each weekday was introduced in Sao Paulo and lasted for three years. In 1997, a new driving restriction in a 152 km^2 area in Sao Paulo was adopted, which limited the circulation of 20% of the vehicles in peak hours, between 7:00–10:00 a.m. and 5:00–8:00 p.m. on weekdays. Vehicular restrictions were first implemented in Bogota in August 1998. Each vehicle was restricted from circulation during peak hours on 2 days per week between 7:00 and 9:00 a.m. and between 5:30 and 7:30 pm, i.e. 40% of private vehicles were restricted from operating in the city during weekdays. During the Olympic and Paralympic Games, driving restriction was implemented in Beijing and its neighbor Tianjin, which restricted half of the car use. After the Games, a modified restriction similar to the one in Mexico city was expanded in some other cities in China.

Some studies have focused on the effectiveness of the driving restriction in Mexico City. Eskeland and Feyzioglu (1997) are the first to study the effect of the driving restriction based on car plate numbers. They estimate a gasoline demand function based on aggregate time-series data during 1983-1992 to analyze the effect of the driving ban in Mexico City. They find that demand for gasoline was larger because of the driving restriction compared to the simulation case without driving restrictions. They also establish a vehicle ownership model to investigate how the driving restriction could increase gasoline consumption. They find that households increased their vehicle ownership, especially through purchasing old cars, and increased their driving due to more available vehicles. Therefore, the total car use in Mexico increased rather than decrease because of the regulation. They conclude that the driving restriction in Mexico City imposed higher compliance costs than those of alternative market-based policies such as gasoline taxes. As the pioneers, Eskeland and Feyzioglu (1997) lay a solid foundation for subsequent studies on driving restrictions in Mexico City and other cities.

Unlike Eskeland and Feyzioglu (1997), Davis (2008) examines whether the driving restriction in Mexico City improved air quality. He uses hourly air pollution data during 1986-1993, an 8-year symmetric window around the implementation of the driving restriction. He takes advantage of a regression discontinuity (RD) design to address possible confounding factors, by adding a highly flexible polynomial time trend (seventh, eighth, ninth-order polynomial time trends). Davis (2008) also compares different subsamples by time of the day and day of the week, since the driving restriction was only in place on weekdays between 5am and 10pm. The results indicate that the policy has engendered a relative increase in air pollution during weekends and nonpeak weekdays, and there is no evidence of an absolute improvement in air quality in peak weekdays.[5]

[5] Peak weekdays were defined as 5am-10pm in weekdays, and non-peak weekdays were defined as 10pm-5am in weekdays.

Other specifications, such as the effects of the driving restriction on maximum pollution levels (maximum daily air pollution, and days when pollution levels exceed WHO standards), are consistent with the basic specification, indicating no improvement of air quality. He also finds more gasoline consumption, more car registrations, more new car sales, and less public transportation ridership during the driving restriction, which suggests that people tended to buy more cars and turn to used high-emissions vehicles instead of substituting to low-emissions public transportation. The social costs of the driving restriction are large, likely in excess of $300 million annually. The results coincide with Eskeland and Feyzioglu (1997), which suggests that the driving restriction policy is socially costly in Mexico City.

Gallego et al. (2012) complements the literature by introducing an adaptation model to investigate how households reacted to the driving restriction in Mexico City. They contribute both theoretically and empirically. They use hourly measures of CO as a proxy for vehicle use. The data set they use is from 15 of the network stations in Mexico City, which were operating during the entire period of their analysis which is a four-year window symmetrically spaced around the time of policy implementation (Nov.1987-Nov.1991 for the driving restriction). They find that the driving restriction decreased CO concentration at peak hours by about 7 percent within the first month of implementation, but in long run the driving restriction has increased CO by about 13 percent. The adaption period was about 12.5 months. They find slightly smaller effects for off-peak hours and Sundays. In all, after a period of adaption between 8 to 12 months, the driving restriction had long-lasting positive impacts on CO, and hence on car use. They also find that the driving restriction had its largest impact in middle-income neighborhoods where households were more likely to buy a second car to bypass the driving restriction. The theoretical model of car ownership they establish to explain the mechanisms behind the adaption are highly consistent with the empirical results. Results from analyzing gasoline sales, number of registered cars, sales of new cars, trade of used cars, traffic flows and taxi medallions are also consistent with the basic results on CO. Again, Gallego et al. (2012) confirms that the driving restriction in Mexico City is ineffective.

Lin et al. (2011) investigate the effectiveness of the driving restrictions in Sao Paulo, Bogota, Beijing and Tianjin respectively. For the driving restriction in Sao Paulo, they use annual averages and maxima of the air pollution pollutants during 1990-1997, and find that carbon monoxide and PM10 reduced at different levels in most of the specifications. For the following restriction in a 152 km^2 area in Sao Paulo, they use hourly air pollution records on the period of 1998-2008 for 15 stations and find no significant evidence of an overall improvement in air quality. For Bogota, they use hourly level pollution records during the period 1997-2009 for 14 stations, using a RD design. They find that even though the driving restriction was not effective in improving the overall air quality, it could be associated with some mild reductions in air pollution levels during week nights and weekends. The restriction in Bogota also significantly reduced the daily maximum of several air pollutants. For Beijing, they use the API data for time period Jul.20, 2007-Oct.31, 2009 and focus their analysis on the concentrations of PM10 derived from API, using both OLS and RD design. They set two indicator variables for the period of Olympics and after Oct.11 respectively, and they find 38% reduction in PM10 concentrations during Olympics but no evidence afterwards. For Tianjin, they use the same method to construct the PM10 sample of Tianjin for the time period of Aug.6, 2007-Oct.31, 2009, and find that the restriction could only be associated with mild but not significant reductions in PM10. Lin et al. (2011) study comprehensively across different cities for similar driving restrictions, and provide a comparison for the effects of driving restrictions in different places.

A few studies have also focused on the effect of driving restrictions in Beijing. Wang, W. et al. (2009) and Wang, X. et al. (2009) both use data collected themselves. Wang, W. et al. (2009) use the PM data sampled on the roof of the 7-story Geology Building on the Peking University campus during Jul.28, 2008-Sep.3, 2008 and Sep.13, 2008-Oct.7, 2008. They compare PKU data and API data, Olympic and Non-Olympic periods, and source control and non-source control group periods. They find that the source control efforts, which include the traffic restriction and some other controls on factories and construction activities, have resulted in lower PM10 concentrations. Wang, X. et al. (2009) use the black carbon (BC) data sampled on the PKU Health Science Center campus. Two aethalometers were installed at two different elevations. One was 6m above the ground and recorded data from Jul.25 to Oct.2. The other was 20m above the ground and recorded data from Jul.26 to Sep.5. They find that there is a consistent decrease in BC concentrations as the height increases from the ground level. Besides, they find significant increase of both BC median and maximum concentrations in non-traffic-control (NTC) days, which indicates the positive impact of traffic control regulations. They also find that diesel trucks are a major contributor to the summertime BC levels by observing a sharp rise of BC after midnight in NTC days when non-local trucks were banned in TC days. Both Wang, W. et al. (2009) and Wang, X. et al. (2009) find significant effects of driving restriction in Beijing.

Unlike Wang, W. et al. (2009) and Wang, X. et al. (2009), Chen et al. (2011) use the API data during Jun.5, 2000-Oct.31, 2009, and the aerosol optical depth (AOD) data during Feb.26, 2000-Dec.31, 2009. They set up four time windows: the benchmark period (Jun.5, 2000-Dec.12, 2001), seven-year preparation period (Dec.13, 2001-Aug.7, 2008), one month during Olympic and Paralympic Games (Aug.8, 2008-Sep.17, 2008), 13 months after the Games (Sep.18, 2000-Oct.31, 2009). They also compare API in Beijing with 36 other cities by adding city fixed effects. They find that the environmental actions, especially plant closure and traffic control, effectively reduced the API by 29.65 percent, but 60 percent of the improvement dissipated one year after the Games. Their analysis on satellite based AOD data also confirms that air quality improvement in Beijing existed but was temporary. But their study only focuses on the restriction and control during the Olympic Games when there were many confounding factors, such as blockage of the roads, limitation of traveling, etc. They do not take all of these factors into account, which could severely bias their results.

Viard and Fu (2012) also use the API data for time period Jan.1, 2007-Dec.31, 2009 and focus on the concentrations of PM10 derived from API. To study the effects of driving restrictions on aggregate pollution levels, they set three indicator variables for the period of odd-even restriction, one-day restriction from 6am-9pm, and one-day restriction from 7am-8pm respectively. They also add indicators for weekends, holidays and month-fixed effects, and find that compared with non-restricted periods, the pollution levels were 19.3 percent lower during the odd-even restriction period and 7.9 percent lower during the one-day restriction period with a 9.7 percent increase in weekends (i.e. there seems to be a substitution between weekdays and weekends). To study the effect on station-level pollution, they add station-level fixed effects and a polynomial function of distance between each station and the nearest major road interacted with the policy variables, and find that during the odd-even (one-day) restriction policy pollution dropped by 20.6% (8.8%) at the ring roads but the effects dissipated by 9.1% (5.8%) with each kilometer from the roads. They also study the effects of driving restrictions on TV viewership, and find that workers with discretionary work time increased their TV viewership during the restricted hours, while workers with fixed work time did not change much. Viard and Fu (2011) complement Chen et al. (2011) by considering a wider range of restrictions, geographic effects and the effects on labor market. It is so far the most comprehensive study on driving restrictions in Beijing. Similar to Chen et al. (2011), Viard and Fu (2011) do not consider the confounding factors during Olympic Games. Moreover, their study on policies in other periods are also problematic. They compare all the policies with baseline periods without restrictions. Many things changed during the time periods that they study on, but they do not control for such factors as population, oil prices, etc, all of which could have impacts on air quality. Above all, previous studies using OLS without controlling for all of these factors should be taken with caution.

3 Beijing's Air Pollution Policies

3.1 Introduction to Traffic Policies

China had long been infamous for its poor air quality before the Olympic Games. In order to establish a positive image in front of the world during the Olympic Games in 2008, a series of traffic policies including driving restrictions was implemented in Beijing to mitigate air pollution and traffic congestion. Figure 1 shows a timeline of traffic policies in Beijing since 2007.

Bus and subway fares were reduced by introducing bus and subway passes in January and October 2007 respectively. Bus fare was reduced from 1 RMB per trip to 0.4 RMB for regular bus pass holders and 0.2 RMB for student pass holders. Subway fares was reduced from 2 RMB per transfer to 2 RMB per trip regardless of number of transfers. Besides, several new subway lines were open and put into use as shown in Figure 1. These changes reduced the cost of taking public transportation and encouraged citizens to use public transportation instead of private cars.

To further alleviate air pollution and traffic congestion, the Beijing Municipal Government (BMG) announced a document on Jun.19, 2008 through its official website about a temporary traffic policy during Jul, 1 to Sep.20, 2008 (TP0 and TP1 in Figure 1). Specifically, cars with license plate numbers ending with odd and even digits could not be driven on alternate days during Jul.20 to Sep.20, 2008. During Jul.20 to Aug.27, the restriction was effective in the whole administrative area of Beijing; During Aug.28 to Sep.20, it was only effective within (and including) the 5th ring road. People could learn the details about the policy through BMG's website and TV news.

After a short break, another document was announced on Sep.28, 2008 by BMG on a modified version of the restriction during Oct.1, 2008 to Apr.10, 2009 (TP2 in Figure 1). Specifically, cars should be taken off the road one day per week according to the last number of their license plate between 6am and 9pm within (and including) the 5th ring road of Beijing during Oct.11, 2008 to Apr.10, 2009, without restrictions on weekends and holidays. The sequence of numbers to be restricted changed every month. For example, if this month cars with license plate numbers ending with digits 1 or 6 cannot drive on Mondays, 2 or 7 on Tuesdays, 3 or 8 on Wednesdays, and so on, then next month, 2 or 7 are restricted on Mondays, 3 or 8 on Tuesdays, and so on.

Another modification of the restriction was announced on Apr.3, 2009 (TP3 in Figure 1). The restriction time was narrowed to 7am-8pm, and the area was also narrowed to within (but excluding) the 5th ring road during Apr.11, 2009 to Apr.10, 2010. The sequence of numbers to be restricted changed every 13 weeks instead of every month. Since then, the restriction remains similar. Citizens can check online which numbers are restricted.

To ensure the implementation of the restrictions, people who were caught violating the restrictions had to pay 100RMB as a punishment, which is a relatively a small amount.[6] Before Jan.4, 2011, violators were only required to pay once in a day regardless of the times and length of the violation in the day, since the traffic cameras could only keep one record of a certain car in a day. Since Jan.4, 2011, the traffic camera system has been improved so that violators could be charged whenever he/she violates the restrictions, which means the implementation becomes stricter. In the following analysis, I split TP3 into two parts, and define TP3-1 as the less strictly implemented policy and TP3-2 as the stricter one.

Another complementary policy was passed on Dec.23, 2010. Citizens who intend to purchase cars in Beijing have to participate in a lottery to win the permits beginning in January 2011. This policy is aimed at reducing current car ownership, thus relieving air pollution and traffic jams. The lottery takes place every month to render permits to individuals and institutions that intend to purchase cars. Permits cannot be transferred and would expire in 6 months. Expired permits will be put back to the pool again and increase the number of winners in the month when the old permits expire.

In this paper, I focus on two of these policy changes: the one between TP2 and TP3, and the one between TP3-1 and TP3-2. The former was a weakening policy change, while the later was a strengthening policy change. I discuss in Section 4 why I do not focus on other policies or policy changes and how I analyze the two policy changes in detail.

3.2 Data

3.2.1 Air pollution data

The primary air quality data set used in this paper is the daily station-level API panel data from the Beijing Municipal Environmental Protection Bureau (BMEPB) during Jan.1, 2008 to Oct.10, 2012. There are 27 monitoring stations (Figure 2 shows the distribution of the stations), of which 8 stations lie within 5th ring areas and 19 stations are outside 5th ring areas. A total of 47,090 observations are included in the data set.

The ideal data set to study this problem is daily or hourly concentration data of every relevant pollutant (CO, SO_2, NO_x, PM2.5, PM10, etc). But historical daily (or hourly) concentration data for individual

[6] The 2011 monthly average income in Beijing was RMB 4,672, and those who have cars would have much higher incomes. The average cost to maintain a car is RMB 20-25 thousand per year in Beijing, and the cost varies a lot by different types of cars.

pollutants is not publicly available in China. The daily air pollution index (API), an index ranging from 0 to 500, is the best substitute publicly available in China. It is calculated from concentrations of SO_2, NO_2 and PM10. Specifically, concentrations of SO_2, NO_2 and PM10 are translated into pollutant-specific APIs according to Table 1 and API only reports the highest pollutant-specific API of the three. If the maximum pollutant-specific API exceeds 500, it is capped at 500. In most days, the maximum pollutant-specific API is the one for PM10, because particulate matter is the most severe pollutant in Beijing (3722 out of 3967days for Jun.5, 2000-Aug.14, 2012 when the maximum pollutant was available).[7] A day with API below or equal to 100 is defined as a "blue sky" day. More specifically, air quality is divided into five levels according to the API: 0-50 is "excellent", 50-100 is "good", 100-200 is "slightly polluted", 200-300 is "moderately polluted", and 300-500 is "heavily polluted".

The daily aggregate API data is available from the Ministry of Environmental Protection of the People's Republic of China (MEP) since Jun.5, 2000, and the daily station-level API data is available from the Beijing Municipal Environmental Protection Bureau (BMEPB) since Jan.1, 2008. The data set used in this paper is the daily station-level API panel data from BMEPB for Jan.1, 2008-Oct.10, 2012. I do not use the aggregate API data from MEP for two reasons. First, most of the polices I study on are only effective within the 5th ring areas, so using the aggregate data covering both stations within and outside the 5th ring areas may misstate the effects of the policies. Second, the station-level API data is less possibly being manipulated compared to the aggregate one, since no one cares about the "blue sky days" in a particular station, so it is not necessary to manipulate it. In addition, there are so many stations, so it is more difficult to manipulate it. When I aggregate the station-level API, it is not exactly the same as the reported aggregate API.[8] Figure 3 shows the density distribution of the aggregate API data from MEP directly (Panel A) and the aggregate API data calculated from station-level API from BMEPB (Panel B). We can see that the density just below 100 is higher in Panel A while the density just above 100 is lower in Panel A, which suggests that there are some manipulations from just above 100 to just below 100 (100 is the cutoff for the blue sky days as discussed above) in the aggregate API data from MEP directly. So I use the aggregate API data calculated using the stations within the 5th ring areas only to study the overall effects of the policies. In this paper, the main analysis is based on the station-level data, but it is also useful to consider the aggregate analysis

[7] The maximum pollutant is not reported when API is below 50, i.e. when the day is "excellent".

[8] For the data used in the following analysis, the aggregate API data are calculated from station-level API data by the formula: $API_t = \frac{1}{8}\sum_{s=1}^{8} API_{st}$, where s=1-8 if station s is within the 5th ring areas, s=9-27 if station s is outside the 5th ring areas. For the data used to compare with the aggregate API data directly from MEP in this section, the aggregate API data are calculated from station-level API data by the formula: $API_t = \frac{1}{27}\sum_{s=1}^{27} API_{st}$, since the aggregate API data directly from MEP includes data from all stations.

as a reference and comparison.

Before proceeding, I first discuss the quality of the API data I use in this paper. Andrew (2008) finds inconsistency between "blue sky days" and the API data in Beijing and that there exists some manipulations in the aggregate API data from MEP in the following three channels: First, the government lowered the air quality standard since 2000. Second, the government changed stations from dirty places to clean places in 2006. Finally, the government manipulated the data near the threshold of 100 in order to report more blue sky days.[9] One reason I do not use the aggregate API data as mentioned above is that the aggregate API data is more possibly being manipulated compared to the station-level data. Even though less likely, station-level API data still could be manipulated by the channels mentioned above. From my perspective, the standard change does not affect my study at all since all data sets I use are after 2000, so the standard change makes no difference. The station change from dirty places to clean places does have some influence since the station distribution may result in bias if the stations are not selected randomly. But for comparison purposes, if I use the data after 2008 and focus on the stations that are being used during the whole study period, it does not make much difference, especially after adding station fixed effects. Even if there are manipulations as shown by Andrew (2008), it seems that the government could always manipulate data, so it is independent of the policy implementation. It might underestimate the effects of the policy, but it is reasonable for us to get a conservative estimate. Above all, the station-level API data is less possibly being manipulated and even if being manipulated, it does not have much effect and is still good enough to study on the effects of the policies.

3.2.2 Ridership and congestion data

The public transportation data set used is the monthly subway ridership, bus ridership and congestion index data during Jan 2007 to Jun 2011 from the Beijing Transportation Research Center (BTRC). Ideally, daily or even hourly data of subway and bus ridership for every transportation line in Beijing would be better to study this problem. But it is not available. The data set used in this paper is the monthly subway ridership, bus ridership and congestion index data during Jan 2007 to Jun 2011 from the Beijing Transportation Research Center (BTRC). The monthly subway ridership and bus ridership data are aggregated from all the subway and bus lines in Beijing respectively. They are calculated as the average ridership per day during a month for all roads in Beijing, and the unit of measurement is ten thousand passengers per day. The congestion index is a 0-10 scale index indicating the level of congestion in Beijing. Congestion is divided into five levels:

[9] For more details, please refer to Andrew (2008).

0-2 is "very smooth", 2-4 is "smooth", 4-6 is "slightly congested", 6-8 is "moderately congested", and 8-10 is "severely congested". The monthly congestion index is also the average index during a month.

3.2.3 Weather data

The weather data used in this paper is the daily weather data from the China Meteorological Data Sharing Service System (CMDSSS) during Jan.1, 2008 to Oct.10, 2012, including windspeed, wind direction, dry-bulb temperature (DBT), dew-point temperature (DPT), precipitation, hours of sunshine, atmospheric pressure, etc. There are data from two stations, one is in Haidian District, and the other is in Miyun District. Higher wind speeds can remove particulates but also import them from neighboring areas. Beijing's air quality is also greatly affected by wind direction. Temperature has an indeterminate effect on particulate matter depending on whether a temperature inversion is created. Humidity (dew-point temperature is a measure of humidity) can interact with pollutants to create secondary ones. Precipitation has opposing effects. Rain can interact with existing pollutants to create secondary ones, but can also wash particles from the air and minimize their formation. I also include the daily hours of sunshine to control for the amount of atmospheric solar radiation, which creates ozone and more particulate matter. One limitation of the weather data is that it is the average data across different stations, so when merged with station-level API data, there is one weather data for 27 stations, which ignores the effects of different weather conditions across stations. But Beijing is not large, the weather conditions should be similar across the whole municipal areas, so it does not have much effect. Another concern is that weather could also be influenced by pollution level (e.g. more particulate matters in the air could result in fog and haze), so the weather data are only used to check robustness and serve as additional reference for the main results.

4 Empirical Strategy

Ideally, if there were no other confounding factors during the Olympic Games such as blockage of the roads, limitation of traveling, and more visitors from all over the world, I would study the effects of each restriction compared with non-restriction periods by the following model.

$$log(API_{st}) = \alpha_0 + \sum_{i=1}^{3} \alpha_i TP_{it} + \theta' X_{st} + \varepsilon_{st}$$

where $log(API_{st})$ is the natural logarithm of API at station s on day t, TP_{it} are the indicators for traffic policy i (as shown in Figure 1) on day t, X_t is a vector of covariates, ε_t is the error term. α_i's are the coefficients of interest, which can be interpreted as the approximate percent change of API due to policy i. Viard and Fu (2011) have done a similar analysis, except that they also include the lag term of the dependent variable. I explain below why I do not include the lag term.

However, there were many confounding factors during the Olympic Games as mentioned above. Without taking these factors into account, the results would be severely biased. Not considering other traffic policies, the results could be overestimated since other policies could also help alleviate air pollution and congestion; Not considering the increasing visitors during the Olympic Games could underestimate the results, since there would be more pollution if restrictions were not implemented. For the driving restrictions after the Olympic Games, there were still many confounding factors that changed with time., such as people's thoughts about driving, population growth, household income, oil prices, etc. Comparing different policies with baseline periods without restrictions is not appropriate without controlling for all of these factors. However, it is impossible to control for all confounding factors, so previous OLS estimates may result in omitted variable bias and should be taken with caution. RD design could overcome this problem by comparing air quality just before the policy change (i.e. discontinuity) and just after the policy change. In this way, the change of confounding factors could be ignored since the time was so close that all things that were changing continuously could be seen as unchanged. Therefore, the policy changes were the only changes that happened so that the changes of air quality could be attributed to the policy changes.

In this paper, I focused my study on two of the changes in the driving restrictions in Beijing: the one between TP2 and TP3, and the one between TP3-1 and TP3-2. The former was a weakening policy change, while the later was a strengthening policy change. I do not focus on the policies during Olympic Games, because some other policy changes happened during the same time period that were not changing continuously either. So It was difficult to distinguish between the effects of the driving restriction and those of other policies even when applying a RD design.

Classically, by narrowing the window around the threshold (the critical value where the discontinuity happens), RD design rules out the effects of confounding factors. As modern econometrics develops, it has been shown that by adding a high order polynomial or local polynomial smoothing of the assignment variable, we can also control for the confounding factors.[10] In the Mexico City case, Davis (2008) took advantage of RD design studying on a four-year symmetric time window by adding seventh, eighth, and ninth order polynomial time trends. I do not use high order polynomials because there is not a standard method to choose the order of polynomials that has been proven to be most appropriate. The results might be vulnerable to

[10]For more details, please refer to Lee and Lemieux, 2010.

different orders of polynomials. Moreover, when the time window is short, high order polynomials tend to overfit the trend. In this paper, I employed a sharp RD design by adding local polynomial plots based on triangle kernel which has been proved to be an appropriate kernel for RD context.[11]

Before implementing a RD design, one should check the validity of other relevant variables, i.e. to make sure that there is no difference in other relevant variables before and after the policy change. In this case, I have weather variables that are related to air pollution level. Table 2 reports the comparison of weather variables one year before and after the two policy changes, which indicates no significant difference before and after the policy changes. So it is appropriate to apply RD design in this scenario.

The RD model I applied for both station-level data and aggregate data in this paper can be expressed as follows.

$$log(API_t) = \alpha_0 + \alpha_i TP_{it} + k(D_t) + \theta' X_t + \varepsilon_t$$

where $log(API_t)$ is the natural logarithm of API on day t, TP_{it} is the indicator variable of policy i on day t, $k(D_t)$ is the local polynomial functions of D_t, D_t is normalized time where D_t=0 at the cutoff value of policy changes, and X_t is a vector of covariates, ε_t is the error term. α_i is the coefficient of interest, which can be interpreted as the percent change in API due to the policy change. I use the log form of API as dependent variable so that α_i can be interpreted as the approximate percent change in API due to the policy change.[12] Moreover, the distribution of the log form of the dependent variable is close to a normal distribution, so that the inference statistics including p-values are valid. However, direct transformation to log form results in 143 missing values for the original zero values in API. To include the missing values, I replace all zero values in API by 1 and then transform them to the log form. By doing this, all observations would be included and the results would not be affected as the difference between 0 and 1 in API is tiny especially after transforming to the log form. Empirically, the results are exactly the same regardless of including or not including the missing values. Lag terms of the dependent variable do not appear in this equation because PM10 usually stays in the air no longer than a day (As mentioned above, PM10 is the main determinant

[11] The sharp RD design is a concept relative to a fuzzy RD design, which requires that the identification of causal effects hinges on the crucial assumption that there is indeed a sharp cut-off, around which there is a discontinuity in the probability of assignment from 0 to 1. In contrast to the sharp RD design, a fuzzy RD does not require a sharp discontinuity in the probability of assignment but is applicable as long as the probability of assignment is different. See Cheng, Jianqing, and Marron (1997) for more detials.

[12] The accurate percent change of API change because of policy change i is given by the formula: $100(e^{\alpha_i} - 1)\%$. Since $\alpha_i = log(API_{TP_i=1}) - log(API_{TP_i=0}) = log(\frac{API_{TP_i=1}}{API_{TP_i=0}})$, we can get $\frac{API_{TP_i=1}}{API_{TP_i=0}} = e^{\alpha_i}$ by taking exponential on each side. By further transformation, $\frac{API_{TP_i=1} - API_{TP_i=0}}{API_{TP_i=0}} = e^{\alpha_i} - 1$. The left side is the exact percent change of API due to policy change i.

factor of API).[13] Besides, the restrictions were continuous in most of the case, so the auto correlation of log (API) might be also due to the effectiveness of the restrictions. It is difficult to tell how much is due to the pollutants persistency. Davis (2008) also showed that the pollutants were not persistent in Mexico City. To allow correlations of error terms within a station, standard errors are clustered by stations. It is reasonable because there could be some common unobservable variables for individual stations, such as measurement errors due to monitoring machines in the stations. To estimate the coefficients α_i, we can either use a wide or narrow time window. The main results shown in next section report windows of 2 months, 4months, and 6 months on each side of the first policy change, and 2 months, 11 months, and 20 months on each side of the second policy change.[14] The time window shown in the main results reflect results for a reasonably small window, the largest available symmetric window and a median window. In section 6, robust checks including adding month dummies, weather covariates, fixed effects, and using different time windows are also reported.

5 Main Results

As described in Section 4, I employ a sharp RD design to estimate the effects of the two policy changes (one is a weakening change, and the other is a strengthening change). In this section, the main results are reported. The time window shown in the main results reflect results for a reasonably small window, the largest available symmetric window and a median window. Figure 4 and Figure 5 show the local polynomial plots of the weakening policy change for the three chosen windows (2 months, 4months, and 6 months on each side).[15] Figure 4 shows the graph for data within the fifth ring. We can see that a sudden increase of API happens at the point of the first policy change for all three windows, which indicates that the weakening policy change increases air pollution even after partialling out a continuous time trend for restricted areas. Figure 5 shows the graph for data outside fifth ring. Contrary to the graph for data within fifth ring, there is no such a sharp discontinuity for all three windows, which suggests that for non-restricted areas, the weakening policy change does not have significant effects on air pollution level.

Figure 6 and Figure 7 show the local polynomial plots of the strengthening policy change for the three

[13] For particulate matters, the smaller the particle, the longer it can remain suspended in the air before settling. PM2.5 can stay in the air from hours to weeks and travel very long distances because it is smaller and lighter. PM10 can stay in the air for minutes to hours and can travel shorter distances from hundreds of yards to many mile because it is larger and heavier.

[14] 6 months is the largest window available for the first policy change, while 20 months is the largest symmetric window available for the second policy change.

[15] Figures in this section use station-level API data, as there are more observations. But aggregate API data gives similar shapes.

chosen time windows (2 months, 11 months, and 20 months). Figure 6 is shows the graph for data within the fifth ring, while Figure 7 shows the graph for data outside the fifth ring. Unlike the weakening policy change, the figures indicate a sharp discontinuity (decrease) of API at the point of the strengthening policy change for all three windows both within the fifth ring and outside the fifth ring, which suggests that both restricted areas and non-restricted areas have air quality improvement because of the strengthening policy change.

Table 3 shows the effects of the two policy changes for station-level API data. The benchmark results include no covariates and use a triangle kernel as mentioned and explained in Section 4. The bandwidth is chosen to minimize MSE (mean squared error). Imbens and Kalyanaraman (2009) show that bandwidth chosen to minimize MSE is the most accurate choice for a sharp RD context. The bandwidth chosen in this analysis is about 8 days, which may change up and down a little bit according to different specifications. Panel A shows the results for the weakening policy change, while Panel B shows the results for the strengthening policy change. For each policy change, I show 3 windows (from a narrow to a wide one) for both within fifth ring areas and outside fifth ring areas. Robust standard errors which are clustered by monitoring stations are reported in parentheses. The results coincide with the figures above. There was a 17.7%-18.7% increase in API due to a weakening policy change (shorter restricted time period) in restricted areas. The effects in non-restricted areas are about 4 percent, which is statistically insignificant and small. There was a 31.6%-34.7% decrease in API due to a strengthening policy change (more penalty to violators and restriction on purchasing a second car) in both restricted areas. [16]

It seems to be surprising that both restricted areas and non-restricted areas have an air pollution level decrease because of the strengthening policy change. There are four possible reasons to explain why both restricted areas and non-restricted areas show a decrease in API. First, the average API is higher in restricted areas than non-restricted areas, so a higher percent change does not necessarily mean a higher change in absolute value. Second, the restriction on purchasing a second car is in effect for both areas, so maybe it is an indicator that the restriction on purchasing a second car plays an important role. But as I discuss in the last section, even if the restriction on purchasing a second car plays an important role, it is not possible to be completely captured by the RD analysis. So the results mainly reflect the effects of a greater penalty. Third, driving within fifth ring areas and outside fifth ring areas are complements. For example, people who want to drive across the two areas would not drive even on the non-restricted areas if his/her car is restricted. But further study is needed to determine which factor is most important and plays a dominant role. Finally, as

[16] The percent changes above and below are calculated from the formula in footnote 12.

shown in the next section, weather variables have some influence on air pollution levels, which offsets some of the policy change effects, especially in non-restricted areas. But the results controlling for weather should be also taken with caution, with details shown in Section 6.

I do not add station fixed effects for the station-level estimates, because it is also not necessary to add fixed effects in this scenario.[17] Theoretically, in order for pooled panel data to produce consistent estimates, the unobserved effect (fixed effect) should be uncorrelated with the explanatory variables (the policy changes). The policy changes in this analysis are all exogenously determined by the government, which are at least uncorrelated with any station factor. So the pooled data could provide consistent estimates even without considering station fixed effects. However, there is no harm to check it with the data. As robustness checks, the results of manually adding fixed effects are shown in next section, which is also consistent with this argument.

Table 4 presents the effects of policy changes for the aggregate API data. Note that the data are averaged only across stations within the fifth ring. There was an about 10 percent increase in API due to the weakening policy change, while there was a 8.4%-17.5% decrease in API due to the strengthening policy change. The signs are all as expected, but the effects are smaller than those using station-level data. It makes sense because the aggregate data tends to average everything and gets rid of some important variations. The results are all not significant, because the standard errors tend to be larger due to fewer observations. But it still confirms that there are positive effects on air quality under a stronger restriction and negative effects under a weaker restriction.

In addition to the analysis above, it is necessary to check whether these results are robust to all reasonable empirical specifications, which are shown in the next section.

6 Robustness Checks and Extensions

In this section, I first show some robustness analysis to check the stability of estimates to all reasonable specifications. The local linear plots used in the main analysis can catch the time trend and some fluctuations in API, but it is possible that there are still some certain modes associated with seasons.[18] So it is reasonable

[17] Another reason that I do not add fixed effects is that the "rd" command used in this paper does not provide such an option, and manually adding fixed effects would make the standard errors incorrect. To manually add fixed effects, I use two methods. The first is to regress the dependent variable on station dummies and save the residuals, and then regress the residuals using the "rd" command. The second is to demean the dependent variable by stations, and regress the demeaned dependent variable using the "rd" command. Both methods give the same results.

[18] I use month dummies instead of season dummies because they could better describe the data. There are enough observations for the station-level data, so adding a few more independent variables would not lose much degree of freedom.

to check whether adding month dummies would significantly affect the results. Weather covariates, as described in Section 3, could influence air pollution levels in multiple ways, so it is also necessary to see whether adding these variables would change the results or not. Although I explain that adding fixed effects are not necessary theoretically in Section 5, there is no harm to check directly from the data that there would be no difference. So the first subsection shows robustness checks including adding month dummies, weather covariates, fixed effects (by demeaning within stations and adding station dummies).

Public transportation are substitutes to private vehicles in theory, so it is possible that the driving restriction could increase public transportation use, especially in the case that Beijing's public transportation is really cheap as shown in Section 3. In addition, another important goal of the driving restriction is to mitigate congestion. So the second subsection analyzes the effects of the driving restrictions on public transportation use and congestion.

6.1 Robustness Checks

In this subsection, robustness checks including adding month dummies, weather covariates, station fixed effects (by demeaning within stations and adding station dummies) are reported. To partial out the influence of month dummies, weather covariates, and station fixed effects, I first regress API on those variables for the whole data set, and get the residuals, which is the API after controlling for those variables. Then I use the "rd" command to analyze the effects of the policy changes on the chosen time windows.

Table 5 shows the results. Panel A shows the results for the weakening policy change, while Panel B shows the results for the strengthening policy change. For each policy change, I show 3 windows (same windows as Section 5) for both within fifth ring areas and outside fifth ring areas. Column (1)-(3) include data from stations within the fifth ring, while column (4)-(6) include data from stations outside the fifth ring. The first row includes month dummies, the second row includes weather data, the third row reports estimates based on demeaned log(API) by stations, the fourth row includes station dummies. The last two rows actually consider the same thing, i.e. the station fixed effects. Robust standard errors which are clustered by monitoring stations are reported in parentheses. The results suggest that there was a 11%-18% increase in API due to the weakening policy change in restricted areas for all specifications, which are consistent with the main results in Section 5. The effects in non-restricted areas fluctuate a little bit for different specifications, but all the results are statistically insignificant and small. So it indicates there is no effect in non-restricted areas because of the weakening policy change, which is also consistent with the main results. For the strengthening policy change, there is an about 30 percent decrease in API in both restricted and non-restricted areas for all specifications except the one controlling for weather covariates. By controlling weather covariates, the effects in restricted areas become smaller (about 27 percent decrease in API) but still statistically significant; While the effects in non-restricted areas become small (about 7 percent decrease in API) and statistically insignificant. The results suggest that weather covariates do influence air pollution level to some extent (especially in non-restricted areas). But weather could also be influenced by pollution levels (e.g. more particulate matters in the air could result in fog and haze), so the results may overestimate the effects of weather variables. The real effects of the policy change might lie between the main results and the results after controlling for weather, but it is hard to say without sufficient evidence.

Table 6 shows results from different time windows (from one months on each side of the policy changes to the largest available time windows). Panel A shows the results for the weakening policy change, while Panel B shows the results for the strengthening policy change. The results are also highly consistent with the main results (11%-18% increase in API for the weakening policy change in restricted areas, and 30 percent decrease in API for the strengthening policy change in both restricted and non-restricted areas), except the smallest time window (one month on each side) for the weakening policy change. It may be because when the time window is really narrow, there are not enough observations to capture the trend accurately.

6.2 Public Transportation Ridership and Congestion

The ridership and congestion data is not frequent enough to implement a RD design, so in this part I use OLS by adding policy dummies. Similar to the air pollution analysis, there are problems using simple OLS, so the results in this part should be taken with caution. But it is still useful to have some implications about the effects of driving restrictions on public transportation and congestion.

To study the effects of driving restrictions on the use of public transportation (subway and bus) and congestion, I employ the following model.

$$log(Y_t) = \alpha_0 + \sum \alpha_i TP_{it} + \theta' X_t + \varepsilon_t$$

where $log(Y_t)$ is the natural logarithm of the dependent variables which could be bus ridership, subway ridership, total public transportation ridership, and congestion index. TP_{it} are policy indicators defined as the fraction of the days that the policy was in effect, since the data are monthly but almost none of the policies started on the first day of the month. For example, TP1 started on Jul.20, 2008, the indicator TP1 in Jul.2008 is defined as (31-19)/31, since there are 31 days in July and the policy was not in effect for the first 19 days (i.e. the policy was in effect in the following 12 days). For Jun.2008, TP1 is just 0 since it was not effective in the whole June, and for Aug.2008, TP2 is just 1 since it was effective in the whole August. X_t is a vector of covariates, including seasonal dummies, for which I define Mar.-May as spring, Jun.-Aug. as summer, Sep.-Nov. as fall, and Dec.-Feb. as winter. ε_t is the error term. α_i's are the coefficients of interest, which can be interpreted as the approximate percent change in the dependent variables (bus ridership, subway ridership, total public transportation ridership, and congestion index) due to policy i.

Table 7 shows the effects of the policy changes on public transportation (including bus and subway) and congestion. Column (1)-(2) report the results for bus ridership, column (3)-(4) report the results for subway ridership, column (5)-(6) report results for total public transportation ridership, i.e. the sum of bus and subway ridership, and column (7)-(8) report results for congestion index. Column (1)(3) (5)(7) consider TP3 as whole, while column (2)(4)(6)(8) split TP3 into two parts, i.e. less penalty and more penalty policies. Standard errors are shown in parentheses. After controlling for seasonal effects, the restrictions were associated with an increase in public transportation use and a decrease in congestion. The policy with less penalty was correlated with an increase in total public transportation use by 34 percent and a decrease in congestion by 25 percent compared with non-restriction periods, while the policy with more penalty was correlated with an increase in total public transportation use by 38 percent and a decrease in congestion by 63 percent. It indicates that more penalty could possibly improve the effects of the policy, i.e. to increase public transportation use and reduce congestion. Note that there may exist substitution between bus and subway, so the results for the two should be considered together. One concern is that the increase in the use of public transportation may have resulted from other factors, such as more population, more subway lines in use, etc. Future work is needed to ensure causality.

7 Discussion and Conclusion

This paper investigates the effects of two policy changes: a weakening policy change due to shorter restricted time period, and a strengthening policy change due to more penalty to violators and the complementary restriction on purchasing a second car. By employing a regression discontinuity design, I find that the weakening policy change led to more pollution in only restricted areas, while the strengthening policy change improved air quality in both restricted and non-restricted areas. Several robustness checks confirm the results. I also find that driving restrictions increased the use of public transportation and alleviated traffic congestion.

Theoretically, the effects of the strengthening policy change include two parts: one is the stricter penalty on the violators, the other is the complementary policy to restrict purchasing a second car. Both of the two changes happened in Jan, 2011, so it is difficult to distinguish the effects from the two changes. But as far as I am concerned, the effects from the RD analysis are mostly the effects of the stricter penalty. RD design is not very appropriate to evaluate the effects of the car purchasing restriction, as the car purchasing restriction has long run effects, while the RD analysis mainly captures the immediate effects. By the local linear plots, the RD design has ruled out most of the long run effects. The trends of the numbers of applicants and winners along with the rates of winners since January, 2011 also support the argument, which are shown in Figure 6. According to Figure 6, the number of applicants increased while the number of winners did not. So the rate of decreased with time, which indicates that the effects would be larger in long run than short run.[19] However, if regarding the results in Table 3 Panel B as the effects of stricter penalty, it would still be overestimated. Further study is needed to distinguish the effects of stricter enforcement and car purchase lottery.

The comparison between the stations within the fifth ring areas and those outside the fifth ring areas shows that both the restricted areas and non-restricted areas have air quality improvement because of the stricter penalty and the complementary policy on car purchasing. One possible explanation to the results is that driving within 5th ring areas and outside 5th ring areas are complements. The complements property brings some positive externality to non-restricted areas. People tend to substitute some trips to weekends as shown in Mexico City (Davis, 2008). There might also be some substitute between restricted weekdays and non-restricted weekdays for a particular car. But since the restricted day of one car is the non-restricted day for another car, data is not available to check such substitutes. Above all, driving in restricted areas and non-restricted areas are complements, while driving in restricted days and non-restricted days are substitutes. The policies are effective when the substitute effects is not so much, because people just canceled some of the less important trips instead of moving it to another day, and some other trips are done exactly the restricted day by public transportation (since some trips cannot be moved, such as important conference). Another useful and policy relevant implication is that the government should put more efforts into monitoring and punishing violators properly as well as making complementary policy to ensure the effectiveness of the policy.

Although the evidence in this paper cannot prove directly that driving restrictions are effective in Beijing, policy changes could significantly affect air quality, which indicates restrictions especially restrictions with strict penalty and proper complementary policy could improve air quality. Then a question naturally arises

[19]The long run effects may also be overestimated since many people who participated the lottery would not buy a car urgently if there was not the lottery. Since the winner rate is quite low, they just want to have an option whether or not to buy a car.

as to why driving restrictions are effective in Beijing but not in Mexico City. There are three possible reasons. First, the car purchase lottery in Beijing reduces the possibility for people to buy a second car to some extent, which has an important policy implication that policies that enforce each other can have larger effects than a simple policy. Second, many households in Beijing are struggling to buy houses and cannot afford to buy a second car just to substitute for the previous car on the restricted days. Third, Beijing has very cheap public transportation, so there would be a large number of people who would turn to low emission public transportation instead of driving. Finally, Davis (2008) points out that in Mexico City, public transportation and private cars are kind of complements, because many subway and bus stations are remote and people should drive private cars to get there. But in Beijing, it is not the case. Public transportation system is dense and convenient, so it should be substitute of private cars instead of complement as in Mexico City. However, Beijing's public transportation system also needs further construction, as many of the buses and subway lines are rather crowded, especially during peak hours.

Driving restrictions seem to be effective, but we cannot say it is a good policy. The cost of implementing the restrictions and the loss of utility by reducing driving should be also considered when measuring a policy is whether effective or not. The cost of implementing the restriction is not high, since could observe the violators through traffic cameras, which should be used no matter there is a restriction or not. But as Davis (2008) indicates, driving restrictions impose high social costs as they prevent people from using a preferred way of traveling. The utility loss should be taken into account when policy makers evaluate a policy. Davis (2008) uses total increased vehicle expenditures associated with the driving restriction as a proxy for social costs. However, social costs are difficult to evaluate in Beijing as people did not increase car expenditures because of the restriction. Moreover, the proxy is also inaccurate. It could be overstated and understated as explained by Davis (2008). So the benefits from better air quality resulted from the driving restriction would be offset at least partially by the social costs.

To conclude, there are still several caveats of my analysis in this paper and need some future work. First, the effects of the strengthening policy change include two parts: one is the stricter penalty on the violators, the other is the complementary policy to restrict purchasing a second car. Although I have argued above that the effects from the RD analysis are mostly the effects of the stricter penalty, further study is still needed to distinguish precisely the effects of stricter enforcement and car purchase lottery. One way to do so is to assess the effects of the car purchase lottery on people's behaviors (including car purchase, car use, and how many times they participate the lottery, etc) using household level data. However, the data is not currently available. Second, the ridership analysis is less reliable due to data limitation. Besides, subway open-ups have not been considered, which could also affect transportation ridership in both short run and long run (not just subway, since bus and subway are substitutes and also complements in some sense). Finally, more work is needed to study on people's behaviors to confirm the underlying mechanisms, such as whether there are substitutes or complements between driving in different areas and at different time periods.

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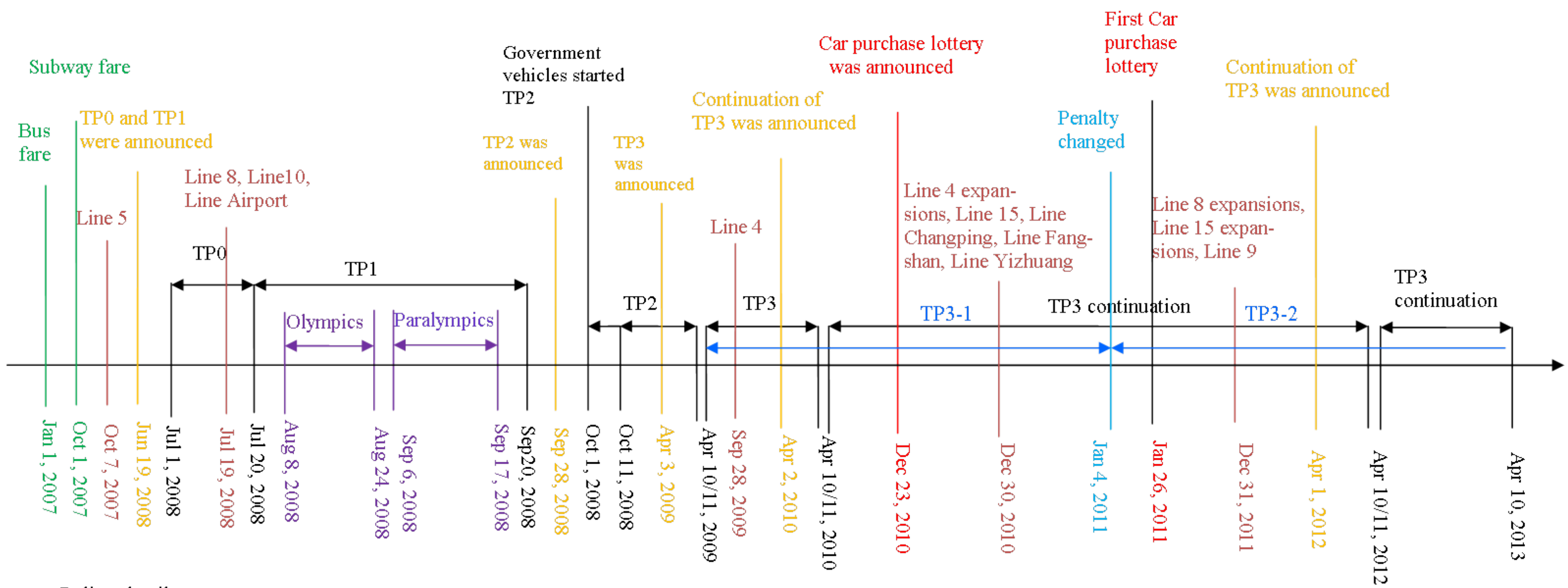


Policy details:

Bus fare	Bus fares reduced from 1 RMB per trip to 0.4 RMB for regular bus pass holders and 0.2 RMB for student pass holders.	
Subway fare	Subway fares reduced from 2 RMB per transfer to 2 RMB per trip regardless of number of transfers.	
TP0	Local vehicles	Yellow environmental protection label vehicles (YLV) are banned. (YLVs are vehicles with very high emissions)
	Non-local vehicles	Dangerous vehicles (such as trucks, tractors and those with dangerous chemicals) are banned; Other vehicles must satisfy certain emission standard.
TP1	TP0 and odd-even restriction within Beijing administrative area during Jul 20-Aug 27, and within fifth ring during Aug 28-Sep 20.	
TP2	One-day restriction during 6am-9pm within fifth ring, including the fifth ring. (Government vehicles started the restriction on Oct 1 for all day within Beijing administrative area, and 30% of them are stopped being used since Oct 1 to present)	
TP3	One-day restriction during 7am-8pm within fifth ring, without the fifth ring. The plate numbers being restricted each day in a week are changed every the 13th week.	
Car purchase lottery	Local residents who have driving license and do not have a local car can participate in the lottery to get a permit to buy a car. The permit will expire in 6 months, and cannot be transferred. Before the expiration date, residents cannot participate in another lottery.	
Penalty	Penalty for violating the restrictions changed from 100 RMB per day to 100 RMB per time in three hours.	

Note: TP1 means traffic policy 1, and Line 5 means Line 5 opened, and so on. Vehicles cooperating with these policies will receive some tax reductions for reward.

Figure 1: Policy Timeline

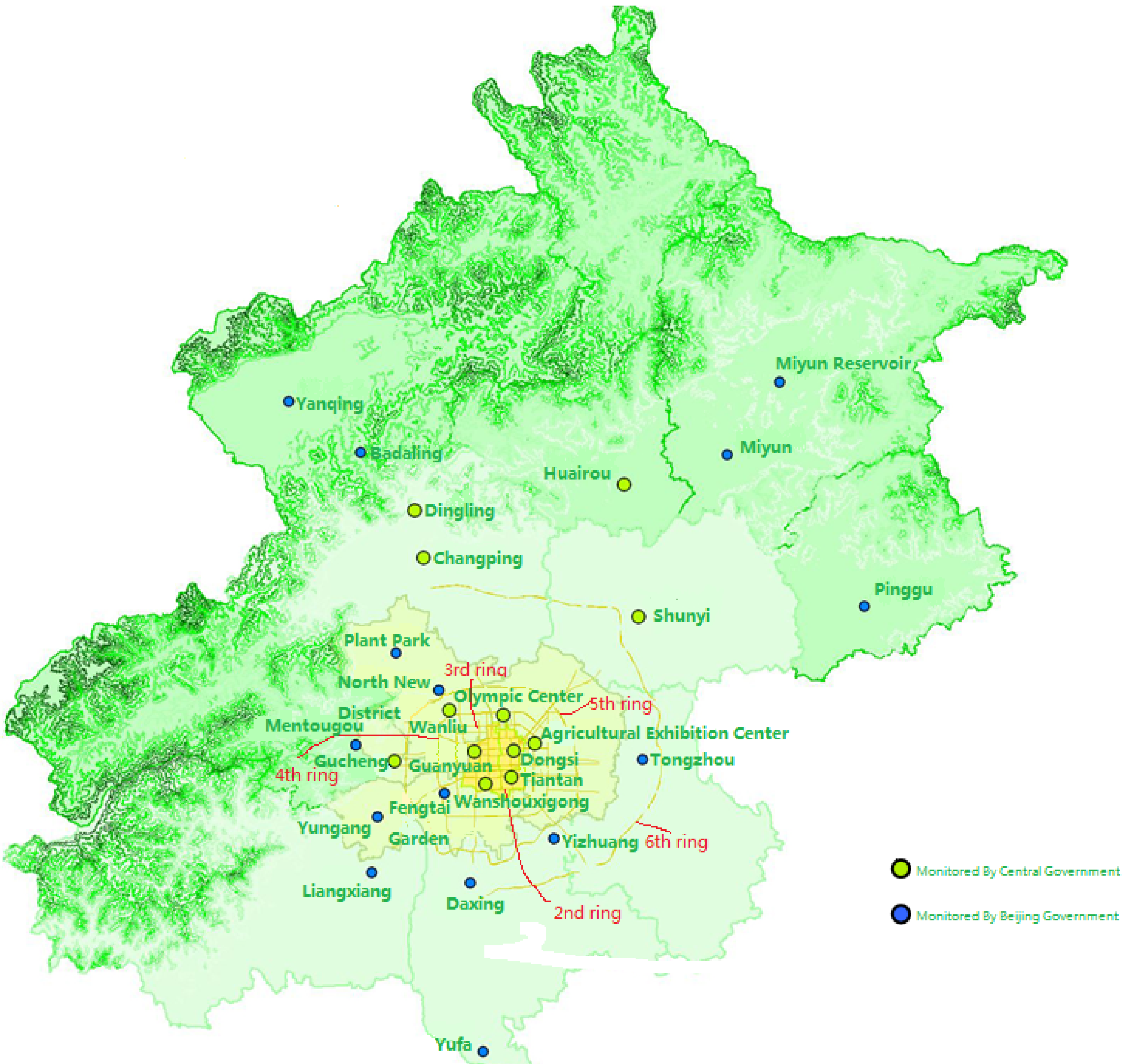
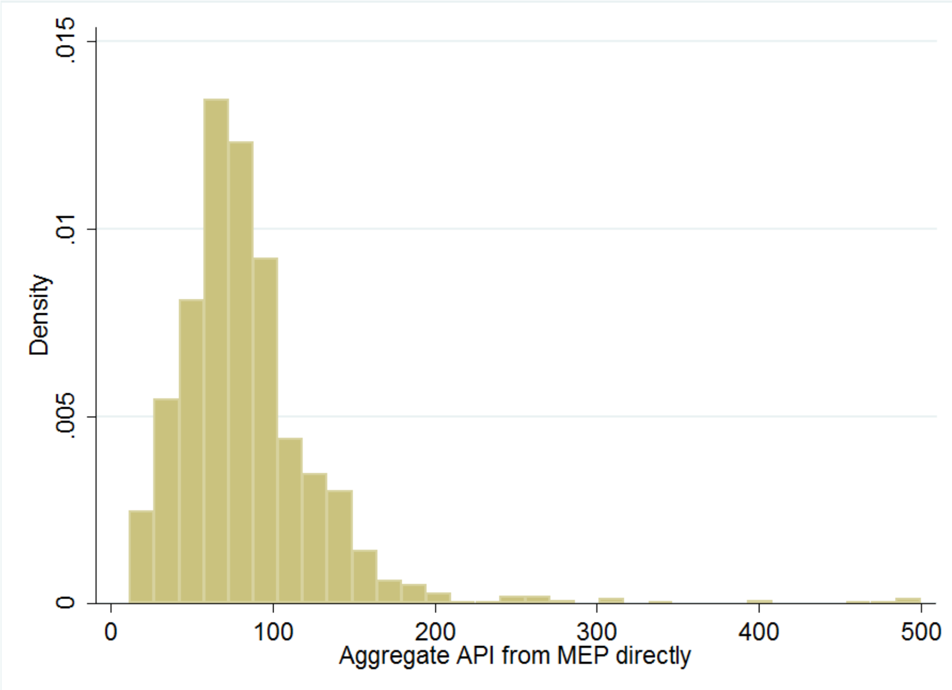
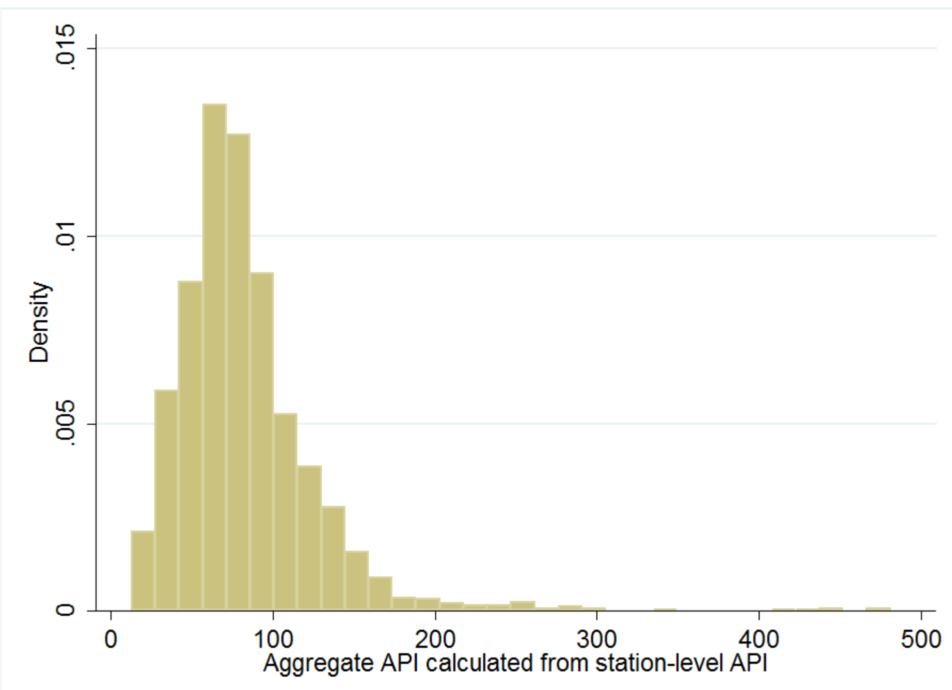


Figure 2: Distribution of Beijing Air Quality Auto Monitoring Stations

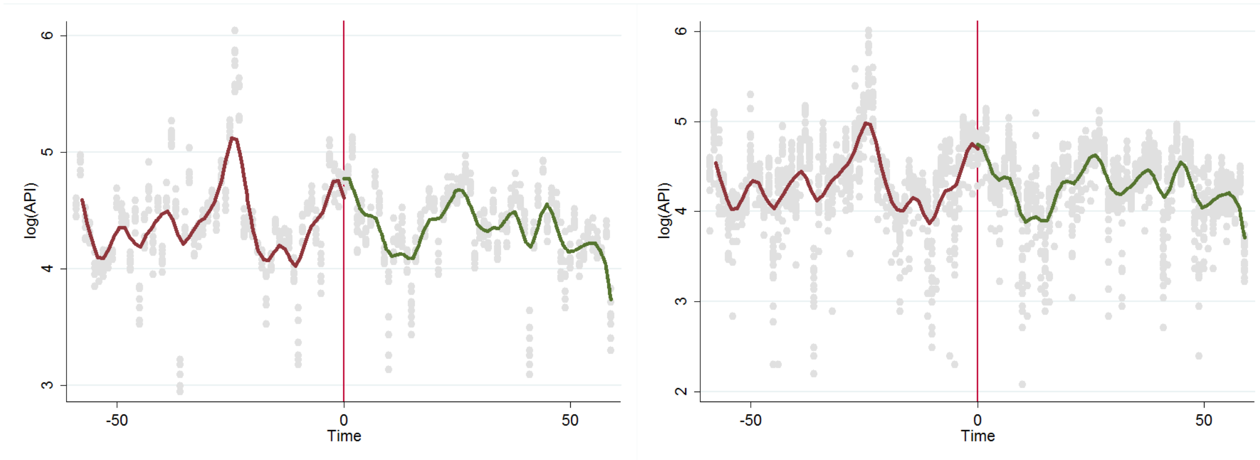


(a) Density of the aggregate API data from MEP directly

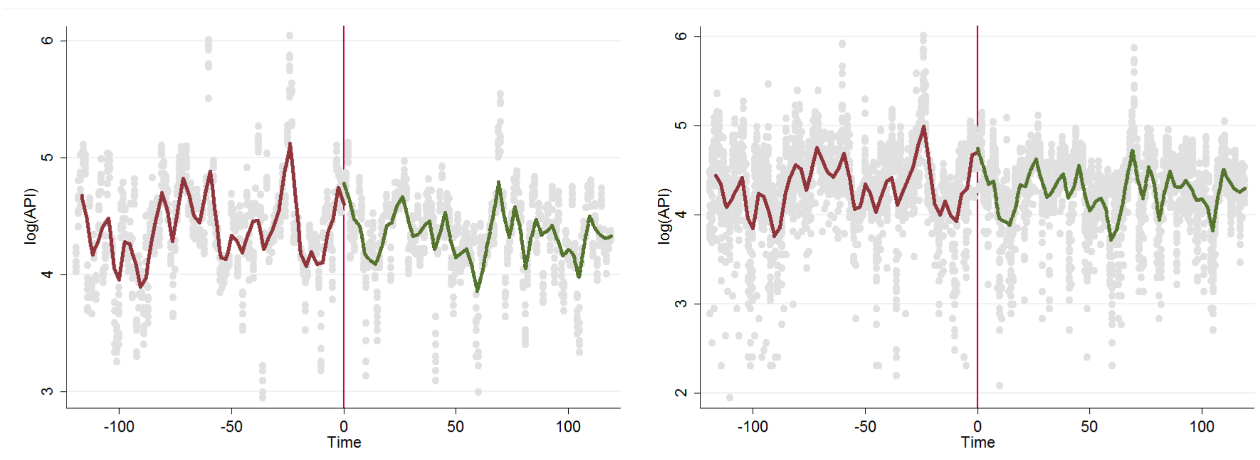


(b) Density of the API data calculated from station-level API from BMEPB

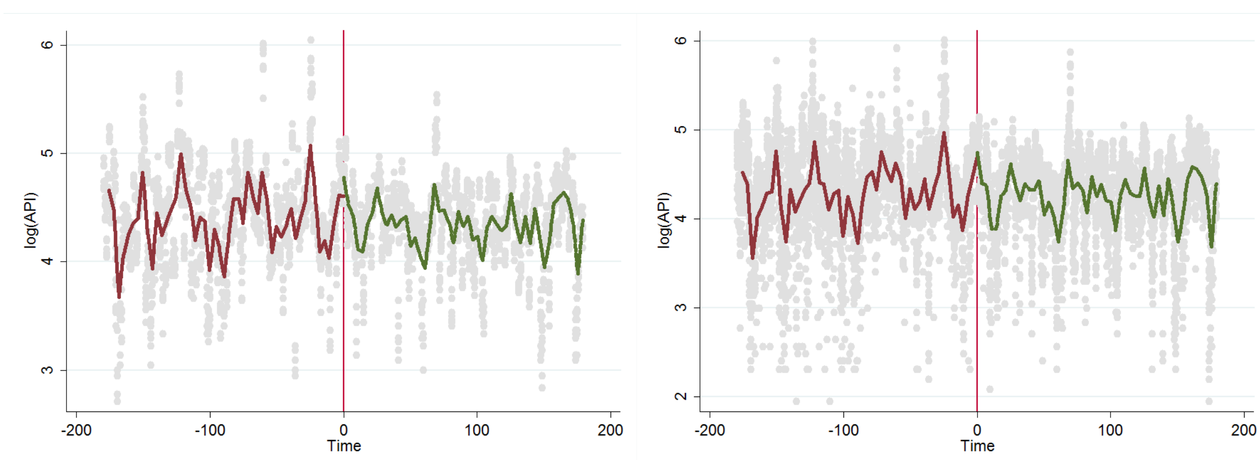
Figure 3: Comparison of Density Distributions of Two Sources of Aggregate API Data



(a) Two months on each side (Left for within fifth ring areas, right for outside fifth ring areas)

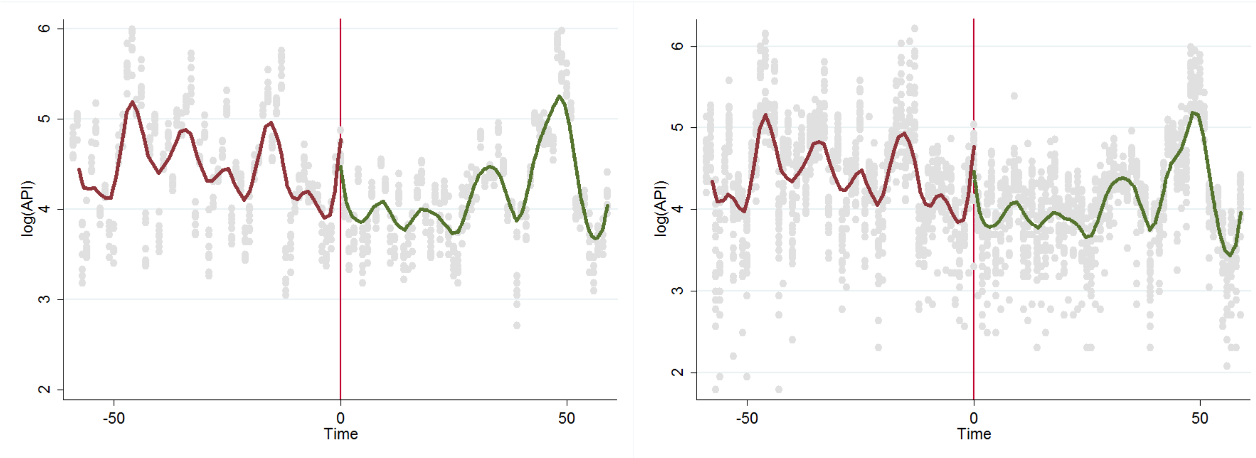


(b) Four months on each side (Left for within fifth ring areas, right for outside fifth ring areas)

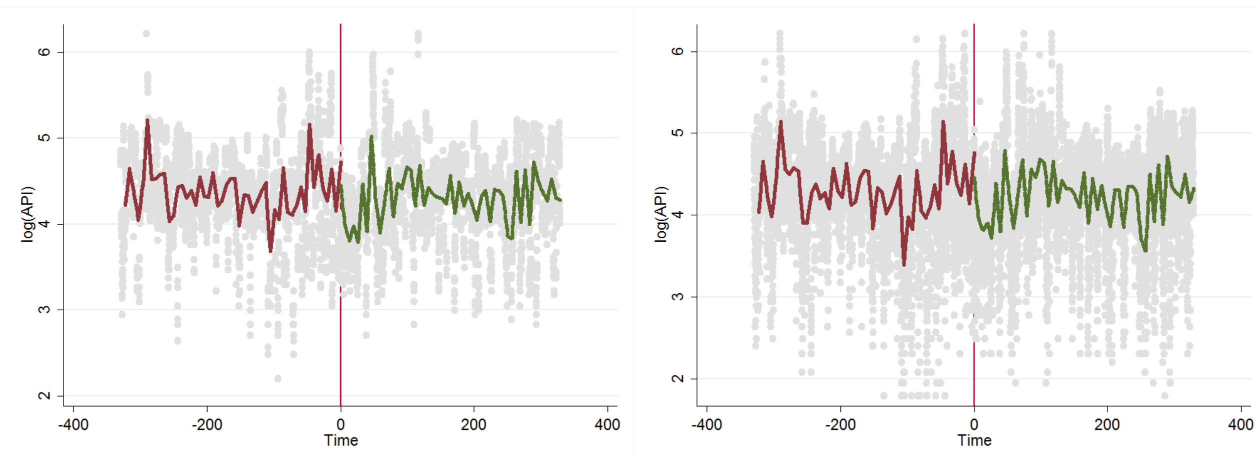


(c) Six months on each side (Left for within fifth ring areas, right for outside fifth ring areas)

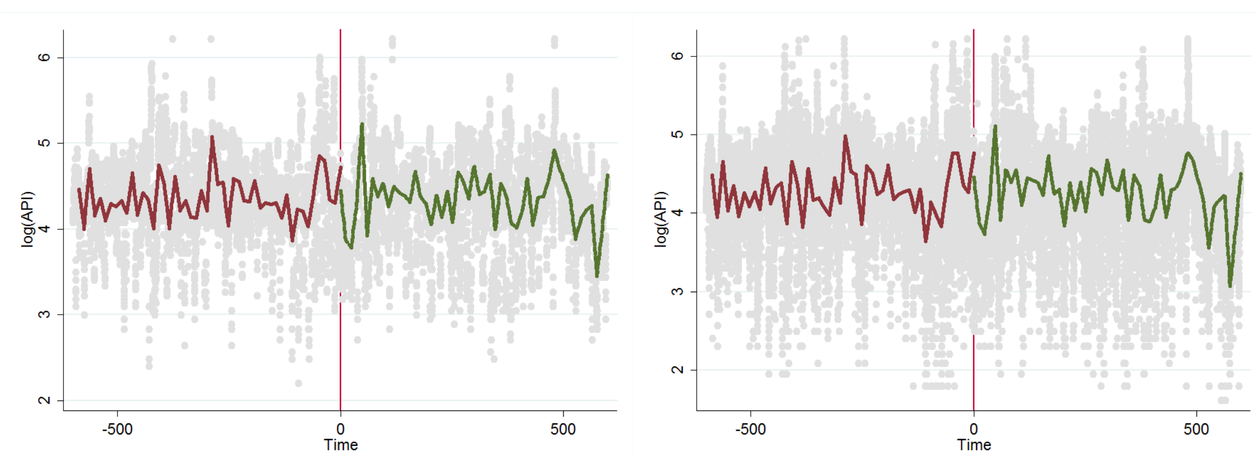
Figure 4: First Discontinuity (Weakening Policy)



(a) Two months on each side (Left for within fifth ring areas, right for outside fifth ring areas)



(b) Eleven months on each side (Left for within fifth ring areas, right for outside fifth ring areas)



(c) Twenty months on each side (Left for within fifth ring areas, right for outside fifth ring areas)

Figure 5: Second Discontinuity (Strengthening Policy)

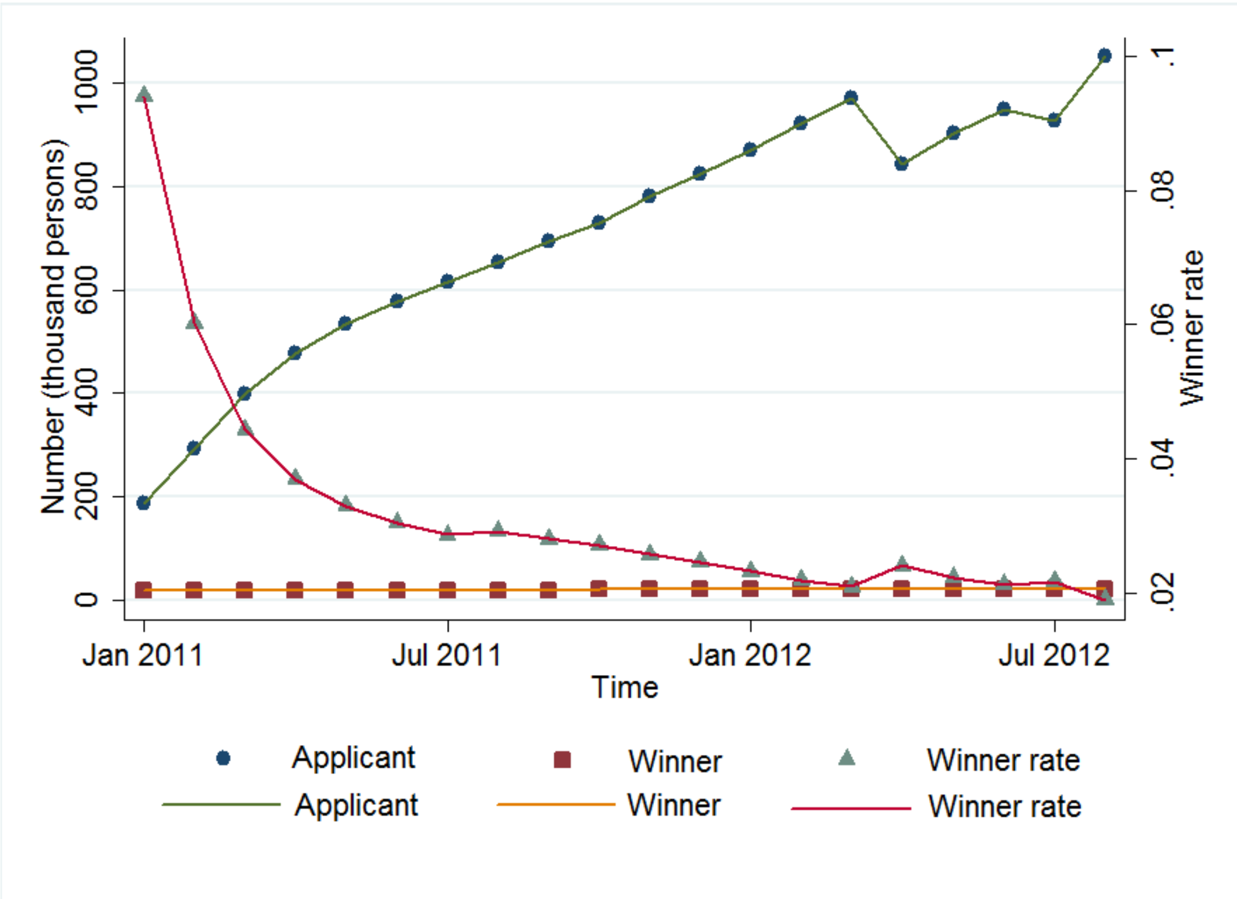


Figure 6: Numbers of Applicants and Winners of Car Purchasing Lottery

Table 1: Transformation from Pollutant Concentration to API[1]

API	PM10 (μg/m3)	NO2 (μg/m3)	SO2 (μg/m3)
0-50	0-50	0-80	0-50
50-100	50-150	80-120	50-150
100-200	150-350	120-280	150-800
200-300	350-420	280-565	800-1600
300-400	420-500	565-750	1600-2100
400-500	500-600	750-940	2100-2620

[1] Table 1 is from Andrew (2008). Andrew pointed out that there was a standard change since Jun. 2000. This is the one after Jun. 2000.

Table 2: Validity Check of Weather Variables

	Weakening Policy Change			Strengthening Policy Change		
Dependent Variable	Before (1)	After (2)	Difference (3)	Before (4)	After (5)	Difference (6)
Wind speed	2.051***	2.087***	0.0346	2.114***	2.040***	-0.0736
	(0.0365)	(0.0376)	(0.0515)	(0.0392)	(0.0363)	(0.0553)
Wind direction	147.7***	148.0***	0.201	158.5***	160.2***	1.782
	(2.567)	(2.531)	(3.628)	(2.629)	(2.473)	(3.713)
Dry-bulb temperature	124.6***	116.7***	-7.812	117.5***	125.6***	7.598
	(6.170)	(6.491)	(8.719)	(6.216)	(6.016)	(8.779)
Dew-point temperature	26.40***	15.79**	-10.34	16.71**	16.60**	-0.637
	(7.350)	(7.321)	(10.39)	(7.572)	(7.487)	(10.69)
Precipitation	4.446***	3.654***	-0.798	3.630***	4.485***	0.843
	(0.729)	(0.698)	(1.030)	(0.968)	(1.165)	(1.367)
Hours of sunshine	49.62***	51.31***	1.789	53.47***	51.88***	-1.621
	(1.706)	(1.733)	(2.410)	(1.735)	(1.749)	(2.450)
Atmospheric pressure	10,164***	10,167***	3.232	10,168***	10,177***	8.894
	(5.369)	(5.543)	(7.588)	(5.520)	(5.882)	(7.795)
Observations	365	364	729	364	364	728

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows validity checks of weather variables. Column (1)-(3) report the comparison before and after the weakening policy change, while column (4)-(6) report the comparison before and after the strengthening policy change. The time window used is one year on each side of the policy changes. Standard errors are reported in parentheses.

Table 3: Effects of Policy Changes (Station-level API)

(a) Effects of The Weakening Policy Change

	Dependent Variable: log(API)					
	Within 5th ring areas			Outside 5th ring areas		
Time window	4 months (1)	8 months (2)	12 months (3)	4 months (4)	8 months (5)	12 months (6)
Weakening policy change	0.163***	0.171***	0.172***	0.0447	0.0392	0.0403
	(0.0337)	(0.0340)	(0.0340)	(0.0350)	(0.0353)	(0.0352)
Observations	952	1,912	2,872	2,261	4,534	6,808

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from six separate regressions. Column (1)-(3) include data from stations within fifth ring, while column (4)-(6) include data from stations outside fifth ring. Time windows for column (1)&(4), (2)&(5), (3)&(6) are 2 months, 4 months, and 6 months on each side of the policy change respectively. 6 months on each side is the largest window available for the first policy change. Robust standard errors are reported in parentheses, which are clustered by stations.

(b) Effects of The Strengthening Policy Change

	Dependent Variable: log(API)					
	Within 5th ring areas			Outside 5th ring areas		
Time window	4 months (1)	22 months (2)	40 months (3)	4 months (4)	22 months (5)	40 months (6)
Strengthening policy change	-0.298***	-0.275***	-0.275***	-0.303***	-0.306***	-0.306***
	(0.0403)	(0.0417)	(0.0417)	(0.0535)	(0.0542)	(0.0543)
Observations	951	5,265	9,577	2,243	12,444	22,662

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from six separate regressions. Column (1)-(3) include data from stations within fifth ring, while column (4)-(6) include data from stations outside fifth ring. Time windows for column (1)&(4), (2)&(5), (3)&(6) are 2 months, 11 months, and 20 months on each side of the policy change respectively. 20 months on each side is the largest window available for the second policy change. Robust standard errors are reported in parentheses, which are clustered by stations.

Table 4: Effects of Policy Changes (Aggregate API)

	Dependent Variable: log(API)					
	Weakening change			Strengthening change		
Time window	4 months (1)	8 months (2)	12 months (3)	4 months (4)	22 months (5)	40 months (6)
Effects of policy changes	0.0997	0.100	0.104	-0.162	-0.0813	-0.106
	(0.150)	(0.150)	(0.148)	(0.366)	(0.404)	(0.392)
Observations	119	239	359	119	659	1,199

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from six separate regressions. Column (1)-(3) include aggregate API data around the first policy change, while column (4)-(6) include aggregate API data around the second policy change. Time windows for column (1)-(3) are 2 months, 4 months, and 6 months on each side of the first policy change respectively. 6 months on each side is the largest window available for the first policy change. Time windows for column (4)-(6) are 2 months, 11 months, and 20 months on each side of the second policy change respectively. 20 months on each side is the largest window available for the second policy change. Standard errors are reported in parentheses.

Table 5: Effects of Policy Changes (Robustness Checks)

(a) Effects of The Weakening Policy Change

	Dependent Variable: log(API)					
	Within 5th ring areas			Outside 5th ring areas		
Time window	4 months (1)	8 months (2)	12 months (3)	4 months (4)	8 months (5)	12 months (6)
Include month dummies:						
Weakening policy change	0.163***	0.171***	0.172***	0.0445	0.0390	0.0403
	(0.0337)	(0.0340)	(0.0340)	(0.0351)	(0.0353)	(0.0352)
Observations	952	1,912	2,872	2,261	4,534	6,808
Include weather covariates:						
Weakening policy change	0.0153	0.105***	0.104***	---	-0.0744	-0.00995
	(0.0238)	(0.0326)	(0.0326)	---	(0.0461)	(0.0222)
Observations	936	1,816	2,680	2,223	4,308	6,354
Demean log(API) by stations:						
Weakening policy change	0.163***	0.171***	0.172***	0.0396	-0.0717	-0.0721
	(0.0337)	(0.0340)	(0.0340)	(0.0353)	(0.0459)	(0.0459)
Observations	952	1,912	2,872	2,261	4,534	6,808
Include station dummies:						
Weakening policy change	0.163***	0.171***	0.172***	0.0396	-0.0717	-0.0721
	(0.0337)	(0.0340)	(0.0340)	(0.0353)	(0.0459)	(0.0459)
Observations	952	1,912	2,872	2,261	4,534	6,808

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: The table shows estimates from 24 separate regressions. Column (1)-(3) include data from stations within fifth ring, while column (4)-(6) include data from stations outside fifth ring. Time windows for column (1)&(4), (2)&(5), (3)&(6) are 2 months, 4 months, and 6 months on each side of the policy change respectively. 6 months on each side is the largest window available for the first policy change. The first row includes month dummies, the second row includes weather data, the third row reports estimates based on demeaned log(API) by stations, the fourth row includes station dummies. The last two rows actually consider the same thing, i.e. the station fixed effects. Robust standard errors are reported in parentheses, which are clustered by stations.

(b) Effects of The Strengthening Policy Change

	Dependent Variable: log(API)					
	Within 5th ring areas			Outside 5th ring areas		
	4 months	22 months	40 months	4 months	22 months	40 months
	(1)	(2)	(3)	(4)	(5)	(6)
Include month dummies:						
Strengthening policy change	-0.343***	-0.312***	-0.314***	-0.297***	-0.300***	-0.301***
	(0.0395)	(0.0406)	(0.0405)	(0.0523)	(0.0527)	(0.0528)
Observations	951	5,265	9,577	2,243	12,444	22,662
Include weather covariates:						
Strengthening policy change	-0.237**	-0.237**	-0.237**	-0.0709	-0.0709	-0.0709
	(0.0933)	(0.0933)	(0.0933)	(0.0684)	(0.0684)	(0.0684)
Observations	831	5,031	9,024	1,959	11,894	21,359
Demean log(API) by stations:						
Strengthening policy change	-0.298***	-0.275***	-0.275***	-0.301***	-0.305***	-0.305***
	(0.0403)	(0.0416)	(0.0417)	(0.0530)	(0.0539)	(0.0541)
Observations	951	5,265	9,577	2,243	12,444	22,662
Include station dummies:						
Strengthening policy change	-0.298***	-0.275***	-0.275***	-0.301***	-0.305***	-0.305***
	(0.0403)	(0.0416)	(0.0417)	(0.0530)	(0.0539)	(0.0541)
Observations	951	5,265	9,577	2,243	12,444	22,662

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from 24 separate regressions. Column (1)-(3) include data from stations within fifth ring, while column (4)-(6) include data from stations outside fifth ring. Time windows for column (1)&(4), (2)&(5), (3)&(6) are 2 months, 11 months, and 20 months on each side of the policy change respectively. 20 months on each side is the largest window available for the second policy change. The first row includes month dummies, the second row includes weather data, the third row reports estimates based on demeaned log(API) by stations, the fourth row includes station dummies. The last two rows actually consider the same thing, i.e. the station fixed effects. Robust standard errors are reported in parentheses, which are clustered by stations.

Table 6: Effects of Policy Changes (Different time windows)

(a) Effects of The Weakening Policy Change

Dependent Variable: log(API)				
Time windows	Within 5th ring areas	N	Outside 5th ring areas	N
1 month on each side	-0.00858		-0.0685	
	(0.0238)	472	(0.0462)	1,121
2 months on each side	0.163***		0.0447	
	(0.0337)	952	(0.0350)	2,261
3 months on each side	0.158***		0.0426	
	(0.0335)	1,432	(0.0351)	3,395
4 months on each side	0.171***		0.0392	
	(0.0340)	1,912	(0.0353)	4,534
5 months on each side	0.168***		0.0417	
	(0.0339)	2,392	(0.0352)	5,671
6 months on each side	0.172***		0.0403	
	(0.0340)	2,872	(0.0352)	6,808

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from 12 separate regressions using different time windows for within fifth ring areas and outside fifth ring areas respectively. Robust standard errors are reported in parentheses, which are clustered by stations.

(b) Effects of The Strengthening Policy Change

	Dependent Variable: log(API)			
Time windows	Within 5th ring areas	N	Outside 5th ring areas	N
1 month on each side	-0.375***		-0.302***	
	(0.0402)	472	(0.0532)	1,113
2 months on each side	-0.298***		-0.303***	
	(0.0403)	951	(0.0535)	2,243
3 months on each side	-0.304***		-0.305***	
	(0.0401)	1,431	(0.0539)	3,383
4 months on each side	-0.313***		-0.304***	
	(0.0398)	1,911	(0.0536)	4,519
5 months on each side	-0.298***		-0.303***	
	(0.0403)	2,391	(0.0535)	5,658
6 months on each side	-0.296***		-0.303***	
	(0.0404)	2,871	(0.0535)	6,789
7 months on each side	-0.291***		-0.304***	
	(0.0406)	3,351	(0.0537)	7,916
8 months on each side	-0.275***		-0.303***	
	(0.0417)	3,831	(0.0535)	9,054
9 months on each side	-0.271***		-0.304***	
	(0.0419)	4,311	(0.0537)	10,192
10 months on each side	-0.272***		-0.305***	
	(0.0419)	4,791	(0.0540)	11,325

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: The table shows estimates from 40 separate regressions using different time windows for within fifth ring areas and outside fifth ring areas respectively. Robust standard errors are reported in parentheses, which are clustered by stations.

(b) Effects of The Strengthening Policy Change (continued)

Dependent Variable: log(API)				
Time windows	Within 5th ring areas	N	Outside 5th ring areas	N
11 month on each side	-0.275***		-0.306***	
	(0.0417)	5,265	(0.0542)	12,444
12 month on each side	-0.275***		-0.306***	
	(0.0416)	5,745	(0.0543)	13,584
13 month on each side	-0.273***		-0.306***	
	(0.0418)	6,225	(0.0543)	14,715
14 month on each side	-0.272***		-0.306***	
	(0.0419)	6,705	(0.0542)	15,840
15 month on each side	-0.274***		-0.306***	
	(0.0417)	7,185	(0.0542)	16,970
16 month on each side	-0.276***		-0.307***	
	(0.0416)	7,665	(0.0544)	18,105
17 month on each side	-0.274***		-0.307***	
	(0.0417)	8,137	(0.0544)	19,245
18 month on each side	-0.273***		-0.307***	
	(0.0418)	8,617	(0.0544)	20,383
19 month on each side	-0.274***		-0.306***	
	(0.0417)	9,097	(0.0543)	21,522
20 month on each side	-0.275***		-0.306***	
	(0.0417)	9,577	(0.0543)	22,662

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: The table shows estimates from 40 separate regressions using different time windows for within fifth ring areas and outside fifth ring areas respectively. Robust standard errors are reported in parentheses, which are clustered by stations.

Table 7: Effects of Policy Changes on Public Transportation and Congestion

	Dependent Variable							
	log(bus)		log(subway)		log(transport)		log(congestion)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TP1	0.150**	0.155**	0.745***	0.722***	0.254***	0.251***	-1.070***	-1.043***
	(0.0632)	(0.0630)	(0.205)	(0.199)	(0.0724)	(0.0730)	(0.120)	(0.0993)
TP2	0.185***	0.185***	0.526***	0.528***	0.240***	0.240***	-0.409***	-0.412***
	(0.0360)	(0.0359)	(0.117)	(0.113)	(0.0413)	(0.0415)	(0.0681)	(0.0565)
TP3	0.166***		0.828***		0.297***		-0.288***	
	(0.0219)		(0.0711)		(0.0251)		(0.0414)	
TP3-1		0.176***		0.775***		0.291***		-0.226***
		(0.0233)		(0.0736)		(0.0270)		(0.0367)
TP3-2		0.133***		0.999***		0.319***		-0.487***
		(0.0346)		(0.109)		(0.0400)		(0.0545)
N	54	54	54	54	54	54	54	54
R-squared	0.691	0.701	0.747	0.767	0.785	0.788	0.724	0.814

*** p<0.01, ** p<0.05, * p<0.1

Note: The table shows estimates from 8 separate regressions. Column (1)-(2) report the results for bus ridership, column (3)-(4) report the results for subway ridership, column (5)-(6) report results for total public transportation ridership, i.e. the sum of bus and subway ridership, and column (7)-(8) report results for congestion index. TP1 is referred as the odd-even restriction during Olympic Games. TP2 is referred as the one-day restriction between 6am to 9pm. TP3 is referred as the one-day restriction between 7am to 8pm. Column (1)(3)(5)(7) consider TP3 as whole, while column (2)(4)(6)(8) split TP3 into two parts, i.e. restriction with less penalty (TP3-1) and restriction with more penalty (TP3-2). All regressions include seasonal dummies. Standard errors are shown in parentheses.