The Effect of Beijing's Driving Restrictions on Pollution and Economic Activity*

Abstract

We evaluate the pollution and labor supply reductions from Beijing's driving restrictions. Causal effects are identified from both time-series and spatial variation in air quality and intra-day variation in television viewership. Based on daily data from multiple monitoring stations, air pollution falls 21% during one-day-per-week restrictions. Based on hourly television viewership data, viewership during the restrictions increases by 9 to 17% for workers with discretionary work time but is unaffected for workers without, consistent with the restrictions' higher per-day commute costs reducing daily labor supply. We provide possible reasons for the policy's success, including evidence of high compliance based on parking garage entrance records.

Keywords: Driving restrictions; externalities; environmental economics; air pollution; commute costs
JEL Classification: Q52, H23, L51, J22, R41.

V. Brian Viard
Cheung Kong Graduate School of Business
Beijing 100738 China
brianviard@ckgsb.edu.cn
Tel: 86-10-8518-8858

Shihe Fu
Wang Yanan Institute for Studies in Economics (WISE)
Xiamen University
Xiamen 361005 China
fushihe@xmu.edu.cn
Tel: 86-0592-2188255

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1. Introduction

Driving restrictions are used in numerous cities around the world to reduce pollution and congestion.[1] Such restrictions may be ineffective either due to non-compliance or compensating responses such as inter-temporal substitution of driving or adding second vehicles. If effective, they may lower economic activity by increasing commute costs and reducing workers' willingness to supply labor. There is little empirical evidence of driving restrictions' effect on pollution and none about their effect on economic activity. We examine both under driving restrictions imposed by the Beijing government since July 20, 2008. The restrictions, based on license plate numbers, initially prevented driving every other day and later one day per week.

On the benefits side, the restrictions significantly reduce particulate matter, a pollutant estimated to claim 6.4 million life-years annually worldwide (Cohen, *et al.* 2005) and a severe air pollutant in Beijing and many other cities worldwide. Using daily data and a regression discontinuity design (RD), our point estimates indicate that the every-other-day restrictions reduced particulate matter by 18% and one-day-a-week restrictions by 21%. Given that motor vehicles create roughly 50% of particulate matter in Beijing this is consistent with strong compliance. We find little evidence of inter-temporal substitution of driving.

Particulate matter's ambient properties dictate that it is deposited within a few kilometers of its release. We exploit this to develop a differences-in-differences (DD) approach that combines time-series variation with spatial variation in monitoring stations' locations and eliminates other explanations for the pollution reduction. Pollution drops more at stations closer to a major road.[2] This means potential confounding factors are related to proximity to a major road and therefore traffic flow. We consider, and rule out, changes in gasoline prices, parking fees, number of taxis, emissions standards, and government-imposed working hours. Papers that use variation in distance from pollution sources for DD identification include Currie and Walker (2011) (response to toll traffic changes based on distance from toll plazas); Schlenker and Walker (2012) (response to airport congestion changes in areas downwind and upwind of airports); and Hanna and Oliva (2011) (response to a factory closure based on distance to the erstwhile factory).

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[1] These include Santiago, Mexico City, São Paulo, Bogotá, San Jose, La Paz, Athens, Barcelona, Amsterdam, Tokyo, all of Honduras, and several Italian cities. See Mahendra (2008), Wolff and Perry (2010), and "With Mixed Results, Cities Battle Traffic and Pollution," *Spiegel Online*, April 4, 2005.

[2] As we explain later, we define a major road as a Ring Road.

On the cost side, we investigate how the driving restrictions' higher commute costs affect economic activity. Lacking direct measures of work time or traffic flows, we rely on consumption of a major substitute – leisure time watching television (TV). Viewership as a proxy biases against finding an effect because the restrictions reduce auto congestion and pollution making outdoor activities more attractive relative to indoor TV viewership.[3] To rule out confounding factors that affect viewership, we compare responses of workers with discretionary work time (self-employed) to those whose days worked and daily hours are fixed in the short run (hourly employees). Since the one-day-a-week driving restrictions apply (initially from 6:00 a.m. to 9:00 p.m. and later 7:00 a.m. to 8:00 p.m.) during most workers' regular working hours, we examine viewership during the restricted hours to measure the effect on days worked but also examine viewership outside the restricted hours to determine if changes in work day length more than compensate.

Using an RD design, viewership by self-employed workers increases by 8.9 to 16.9% during the restricted hours of the one-day-a-week policy, consistent with substitution from days worked to leisure in response to higher commute costs. Viewership changes little outside the restricted hours ruling out the possibility that longer daily work hours offset the fewer work days. Output is reduced unless efficiency increases during the fewer remaining work hours. Hourly employee viewership decreases during restricted hours consistent with their having no choice over days worked but experiencing fewer at-home sick days due to reduced pollution. Although daily work hours for these workers should remain unchanged, their leisure time could change depending on changes in commute modes and congestion. We find minor adjustments in viewership outside the restricted hours.

Using back-of-the-envelope calculations, we estimate the annual benefits from reduced morbidity and fewer reduced activity days due to the one-day-a-week restrictions to be RMB 2.56 to 3.47 billion while the cost of reduced output is RMB 0.52 to 0.94 billion. The remainder of the paper is organized as follows. Section 2 reviews Beijing's driving restrictions policies and related work. Section 3 develops a simple model of driving restrictions' effects on pollution and labor supply. Section 4 describes the data. Section 5 contains the pollution results and Section 6 the

[3] TV viewing on mobile devices is extremely limited during our sample period and not included in our viewership measure.

viewership results. Section 7 provides some cost-benefit calculation while Section 8 provides reasons for the policy's effectiveness. Section 9 concludes.

2. Background

Air pollution and its health consequences are a major concern in Beijing, which was ranked thirteenth "most polluted city" in the world in 2004 for suspended particulates.[4] The economic cost of suspended particulates to China is estimated at 22.4 billion (1997 USD) in 2005 (Matus, *et al.*, 2012). Although a particularly acute problem in developing economies (Greenstone and Hanna, 2014), particulate matter is a major concern worldwide (Watkiss, Pye, and Holland (2005) provide European Union evidence). Particulate matter is linked to cardiopulmonary diseases, respiratory infections, and lung cancer (EPA, 2004), and increases infant mortality (Chay and Greenstone, 2003). Other air pollutants also have negative health effects linked to infant mortality (Currie and Neidell, 2005) and childhood asthma (Neidell, 2004).

We focus on PM_{10} which is the ambient concentration (in μg/m^3) of particulates smaller than 10μm. Various sources create PM_{10}, but autos are the major contributor in most urban areas. Autos create PM_{10} through emissions and by creating road dust.[5] Jiang (2006) reports that approximately 53% of Beijing's PM_{10} is attributable to motor vehicles – 23% due to emissions and 30% road dust.[6] Therefore, autos create roughly half of the air pollution we examine. As this is fairly consistent across countries, reducing auto pollution is important more generally.[7]

Beijing's driving restrictions began on July 20, 2008 with an odd-even ("OddEven") policy restricting cars to drive only every-other-day. This policy applied seven days a week and to all hours except midnight to 3:00 a.m. These restrictions ended on September 20, 2008. On October 11, 2008 the government re-instated driving restrictions, preventing cars from driving one-day-per-week ("OneDay"). This policy applied on weekdays and initially between 6:00 a.m. and 9:00 p.m. We call this period "OneDay69." On April 11, 2009 the daily restriction period narrowed to 7:00 a.m. through 8:00 p.m. and remained unchanged beyond our sample period. We call this

[4] "Beijing Pollution: Facts and Figures," *BBC News*, August 11, 2008 based on 2004 World Bank data.
[5] Some cities measure $PM_{2.5}$, which includes particulates below 2.5 μm and does not capture road dust.
[6] Citing "Beijing's Strategy to Control Air Pollution" by the Beijing Environmental Protection Bureau. Cui, *et al.*, (2009) estimate that autos create 62% of all air pollutants, including PM_{10}.
[7] In the U.S., the EPA's 2005 National Emissions Inventory Data attributes 10.7 (53.5%) of the 20.0 million tons of PM_{10} particulate matter nationwide to "Road Dust" and "On Road Vehicles."

period "OneDay78" and use "OneDay" to apply to the combined OneDay69 and OneDay78 periods.

The policies restricted vehicles based on the last digit of their license plate numbers. During the OddEven policy, odd-numbered license plates could drive only on odd-numbered dates and even-numbered only on even-numbered. The OneDay policy restricted two out of the ten plate numbers each weekday so that the restrictions followed a weekly cycle. The pairing of digits remained the same week-to-week ((0, 5), (1, 6), (2, 7), (3, 8), (4, 9)) but the assignment of these pairs to weekdays were initially rotated each month and, beginning April 11, 2009, every thirteen weeks.

The OddEven and OneDay69 policies applied to all roads (regardless of size) within and including the 5th Ring Road while the OneDay78 policy applied to all roads within but not including the 5th Ring Road (Figure 1 shows these areas). Police cars, taxis, ambulances, postal vehicles, and embassy cars were exempt although these are few in number.[8]

As Figure 2 shows, other pollution-relevant policies occurred around the time of the driving restrictions. These included bus fare reductions and subway line openings. In addition, during the Olympic Games many non-essential businesses and factories were closed; and migrant workers (those without Beijing *hukous*) were sent home. These all may affect air pollution.[9] Factory closures and migrant worker relocation coincide with the Olympic Games and we include a dummy variable and separate time trend in our estimates to capture that period. We address the other policies – bus and subway fare reductions and subway openings – in a variety of ways. In our RD estimates, we include flexible time trends to control for these policies and perform robustness checks to test their flexibility. We also estimate the driving restrictions' effects using small windows around their beginning (bus and subway fare reductions and Subway Line 4 and 5 openings are not within these windows). Finally, for policies beginning near the time of the driving restrictions (Subway Line 8, 10, and Airport openings), our DD results showing viewership increasing for workers with

[8] Two-wheel, combustion-engine vehicles such as mopeds and motorcycles were banned from Beijing's 2nd, 3rd, 4th, and 5th Ring Roads beginning December 8, 2000.
[9] Air travel also likely changed during this period but particulate matter is less than 1% of aircraft engine emissions ("Aviation & Emissions: A Primer," Federal Aviation Administration Office of Environment and Energy, January 2005, page 1). Also, since particulate matter dissipates within a few kilometers, the small amount of PM_{10} measurable by ground sensors would be produced during takeoff and landing near the Beijing airport which is 10.5 kilometers from the nearest station in our sample.

discretionary work time but not for those without are inconsistent with public transit expansion.

The only other detailed economic analyses of driving restrictions are Davis (2008), Carillo, Malik, and Yoo (2013) and Wolff (2014).[10] Davis (2008) finds no discernible effect on several pollutants (not including particulate matter) from a similar policy in Mexico City.[11] Our work differs in three key respects. First, we use geographic in addition to time-series variation to identify the effects. Second, we examine the impact on work time. Third, while Davis (2008) only describes the penalties and detection methods used in Mexico City, we provide direct compliance evidence. Lacking publicly-available violations data, we gathered data from a centrally-located Beijing parking garage not responsible for enforcing the restrictions. Using minute-by-minute data on license plates entering the garage, we find high compliance. Carillo, Malik, and Yoo (2013) exploit both temporal and geographic variation and find a significant reduction of carbon monoxide due to driving restrictions in Ecuador. Unlike our approach, the authors use monitoring stations outside the restricted area as a control group which may overstate the effects if traffic shifts to these areas as a result of the restrictions. Wolff (2014) finds pollution reductions in Germany's low-emission zones but uses control cities for identification.

Chen, *et al.* (2013) examine which, if any, of the policies implemented during and shortly after the Olympics affected Beijing pollution. This paper complements ours in that it concludes that the driving restrictions were one of two effective policies. They use two different DD approaches to show this. One uses only Beijing data and finds that aerosol optimal depth (AOD) – a satellite measure of atmospheric particulates – drops more in areas with higher road densities during the OddEven policy.[12] The other DD approach uses nearby cities as a control group and finds that pollution drops 17% during the OddEven policy – similar to our estimates.[13]

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[10] Policy papers examining driving restrictions include Osakwe (2010); Cropper, *et al.* (2014); and Cambridge Systematics, Inc. (2007). Chen and Whalley (2012) employ an RD approach to evaluate the effect of a Taiwan subway opening on auto emissions.

[11] Salas (2010) finds that the Davis (2008) results are sensitive to assumptions about time window and time trends. Eskeland and Feyzioglu (1997) use data on gasoline consumption to conclude that the Mexico City restrictions increased driving but they do not control for any pre-existing time trend.

[12] The paper does not explicitly test for the effects of the OneDay policy. However, it concludes that it was ineffective based on a regression that tests whether pollution remains lower in the months after the Olympics (a time period which includes the OneDay policy). Contrary to the conclusion in the paper the results of this test (the paper's Table 12) show pollution levels 14% lower even at the end of the sample period – similar to the magnitude of our estimates.

[13] This is from the specification closest to ours (Column 4 of Table 11).

Our paper differs in two ways. First, our approach more conclusively identifies driving restrictions as the cause of the pollution decline. Comparing areas with different road densities cannot rule out confounding factors that lower both auto and public transit congestion. Our TV viewership results fulfill this role. Unlike our station-level data which can detect sub-kilometer changes, satellite data is not precise enough (roughly accurate up to 10 kilometers) to evaluate within-city policies affecting pollution sources in close proximity. Comparing Beijing to control cities cannot rule out other coincident policy effects within Beijing. Second, the paper does not consider labor supply effects. Lin, Zhang, and Umanskaya (2011) examine driving restrictions in three cities including Beijing. They find a 20 to 29% pollution reduction during the OddEven restrictions and no change during the OneDay.[14] Their results may differ because they use a single time trend throughout the sample period rather than allowing for asymmetric time trends before, during, and after policies.

Our study adds to the very small empirical literature relating commute costs to labor supply. This is important for evaluating how transport changes affect worker productivity. That driving restrictions reduce work time implies that shifting to a commuting-related tax will not necessarily reduce the work-time distortion from an income tax. We know of one study that relates commute cost changes to work time changes while properly controlling for endogeneity. Gutiérrez-i-Puigarnau and van Ommeren (2010) find a very small elasticity of labor supply with respect to commute distance. In contrast, we distinguish workers with and without discretion over work time, allowing us to compare control and treatment groups as well as separately identify the effect on those with discretion.

3. Theoretical Model

Appendix A contains a model that predicts the short-run effects of Beijing's driving restrictions on pollution and economic activity. We outline the model and discuss its main results here but refer to the appendix for details. It incorporates the choice of commute mode in a labor supply model. There are two groups of workers:[15] those with discretionary work time and those with fixed. Since most Beijing workers with

[14] This is from the specification closest to ours (Columns 2 to 4 of Table 14b).
[15] The restrictions apply to non-commuters but they likely have greater flexibility for inter-temporal substitution. Including non-commuters, as our pollution data does, will bias us toward finding no effect. Since our viewership data is comprised only of workers the model applies directly to it. According to the 3rd Beijing Transportation Comprehensive Survey (Beijing Transportation Research Center, 2006), 48% of daily Beijing travelers across all modes are commuters.

fixed work times must arrive at work by 8:30 a.m. and stay until 5:30 p.m.,[16] we assume a fixed daily schedule for them. Each group includes a distribution of workers with heterogeneous commute properties, wages, and non-wage income. Each worker chooses an optimal commute mode (auto, public transit, or not working if they have discretion) considering its effect on their labor-leisure choice. Commute properties are defined by the monetary cost, time, and non-monetary disutility for each mode. Non-monetary disutility allows for the fact that some workers prefer a commute mode even though it requires more time and greater monetary cost. Examples are expending effort to walk, bearing the burden of a crowded subway, or inhaling exhaust fumes.

The model considers workers' total utility over restricted and non-restricted days. Absent the policy the two are identical. With the policy, workers suffer a penalty for driving on restricted days. We assume perfect compliance and that workers do not purchase a second car to comply; presence of these in the empirical data biases against finding an effect.[17] The model considers only short-run effects and therefore ignores changes in workforce participation,[18] transitioning between discretionary and fixed work-time jobs, changes in housing prices and wages, and changes of residential or work locations. The appendix considers only first-order effects but we comment below on second-order effects due to congestion changes. Driving restrictions affect work time on both an extensive (days worked) and an intensive margin (daily work hours conditional on working that day). Extensive margin changes affect pollution because they change the number of auto trips. Leisure is affected on both margins.

Extensive Margin: For those with fixed work times, the restrictions have no impact on the extensive margin since they must work. They will use public transit when restricted regardless of their preferred mode when unconstrained. Therefore,

Implication 1: Across all workers with fixed work times, days worked and therefore days spent entirely on leisure are unchanged due to the policy.

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[16] After our sample period (beginning April 12, 2010) official working hours became 9 a.m. to 6 p.m.
[17] Eskeland and Feyzioglu (1997) model the latter. Due to the integer nature of car purchases, some households are on the margin between zero and one car while others between one and two. Driving restrictions reduce the service flow from owning a single vehicle and can lead the former to sell their vehicle but the latter to buy another. Gallego, Montero, and Salas (2011) find that middle-income households in Mexico City and Santiago respond more in the long run to driving restrictions than low- or high-income consistent with middle-income households being on the margin between one and two vehicles absent the restrictions but low- and high-income being infra-marginal.
[18] Gibbons and Machin (2006) discuss the theoretical effect of increased commute costs on labor participation. Black, Kolesnikova, and Taylor (2014) find that female labor force participation rates are lower in cities with longer commute times consistent with women as the primary margin of labor supply adjustments.

The extensive margin effect for workers with discretionary work time depends on their preferred commute mode absent the restrictions. Those who prefer public transit are unaffected and will continue to work "full time" and take public transit on both restricted and non-restricted days. Workers who prefer to drive can either take public transit or not work on their restricted day ("reduced time"). Those with high public transit commute costs (in terms of time, money, or discomfort) will choose the latter and substitute to leisure activities. Therefore,

Implication 2: Across all workers with discretionary work time, days worked decrease and days spent entirely on leisure increase due to the policy.

Second-order effects may attenuate this. Auto congestion will decline and public transit congestion will increase. This will induce some people to drive who otherwise would take public transit on their non-restricted day. Given Implications 1 and 2, the pollution effects are straightforward:

Implication 3: Total auto commutes and pollution decrease due to the policy.

Because the model does not consider non-work driving and assumes all days are work days, inter-temporal substitution is not possible. In a more general model, workers may drive more on the non-restricted day because they cannot on the restricted.[19] This will attenuate the pollution reduction and lower empirical estimates.

Intensive Margin: Workers with fixed work times who take public transit absent the restrictions will also do so when restricted and their daily leisure time is unaffected. For those who drive absent the restrictions, they must take public transit on restricted days. Their leisure time increases if public transit commuting is faster than auto and decreases if not. Since our data includes all workers with fixed work times,[20]

Implication 4: Daily leisure time across all workers with fixed work times could either increase or decrease due to the policy.

Workers with discretionary work time who take public transit absent the restrictions will still do so when restricted and their daily leisure time is unchanged. Those who prefer driving and choose to work "full time" must commute by public transit on restricted days. Their daily leisure time changes depending on how public transit commute times and costs compare to those by car. Those who prefer driving and

[19] The OneDay policy restrictions also do not apply on weekends allowing for more inter-temporal substitution. We allow for this in our empirical tests.

[20] The second-order effects (increased public transit ridership and decreased auto commute times) of the restrictions also impact Implications 4 and 5 but do not change their ambiguity.

choose to work "reduced time" decrease their leisure time on non-restricted days to compensate for working fewer days unless their non-wage income is high.[21] Since our data includes all workers with discretionary time,

Implication 5: Daily leisure time across all workers with discretionary work time could either increase or decrease due to the policy.

4. Data

We use two primary data sets. The first is a daily measure of Beijing air pollution at both aggregate and monitoring-station levels. The second is an hourly measure of TV viewership by different categories of Beijing workers. We supplement these with control variables thought to affect air pollution and viewership. Our sample is from January 1, 2007 to December 31, 2009. This provides us with 1,096 total days of which 566 days occur before OddEven, 63 during OddEven, 20 between OddEven and OneDay, 182 during OneDay69, and 265 during OneDay78. This provides roughly 1.5 years both before and during the policy regimes. Appendix E provides descriptions and Table 1 summary statistics for the main variables.

Pollution Data: We use the daily Beijing Air Pollution Index (API) published by the State Environmental Protection Agency and Beijing Environmental Protection Bureau.[22] The API ranges from 0 to 500 with higher values indicating higher pollution concentrations and more harmful effects (EPA, 2009). Its value depends on three different pollutants which affect breathing: particulate matter (PM_{10}), nitrogen dioxide (NO_2), and sulfur dioxide (SO_2). An API is calculated for each of the three pollutants but only the maximum is reported. The concentration of each is rescaled for comparability before the maximum is chosen. In our sample, the API ranges from 12 to 500 and averages 91. The maximal pollutant is identified if the API exceeds 50. PM_{10} is the maximal pollutant on 917 of the 953 days with an API above 50.

The aggregate API is based on readings at multiple monitoring stations around Beijing whose composition varied slightly over time. In 2007 it is based on 28

[21] This is consistent with Gutiérrez-i-Puigarnau and van Ommeren (2009), who consider a general, concave wage function. Commute costs are fixed per daily trip so workers reduce the number of trips and spread these costs over longer daily hours. Allowing for a concave rather than linear wage function in our model leads to a smaller share of workers working "reduced time" and a smaller increase in daily work hours because declining marginal productivity leads to lower wages with longer daily work hours.
[22] Our description of the pollution data is based on Andrews (2008). Chen, *et al.* (2013) provide evidence on the accuracy of the aggregate Beijing API using independent satellite data.

stations. Five stations are dropped and four added for a net total of 27 stations in 2008 and 2009. Figure 1 shows the 2008 and 2009 locations. The station-level API ranges from 6 to 500 and averages 90. As with the aggregate, the station-level API is based on the maximal pollutant which is identified if the API is above 50. This prevents us from constructing an uninterrupted daily, station-level measure of PM_{10} although it is the "major pollutant" on 68% to 91% of days depending on the station. The API's construction prevents us from fully verifying the relationship between the aggregate and station-level APIs or creating an alternative aggregate index (see Appendix F).

TV Viewership Data: We use viewership to measure how driving restrictions affect economic activity. In the absence of data on work and total leisure time, viewership is a good proxy – it is a large fraction of leisure and a big substitute for work time.[23] Our viewership measure is CSM Media Research's "Television Audience Measurement" (TAM) database, China's most comprehensive TV ratings data. TAM measures the number of people watching each TV program and commercial. We aggregate to the hourly level across all channels. TAM's ratings are based on a household panel although data is collected for each household member. A "PeopleMeter," an electronic device installed inside the TV, detects when it is on and, if so, the channel displayed. Panelists use a remote-control device to enter which members are watching, displayed on the screen for confirmation. CSM's Beijing data covers an area very similar to that subject to the driving restrictions – all areas inside the 5th Ring Road and only a small part outside.

TAM provides viewership data by employment categories. We use two categories for which we know the degree to which its members control their work time. Those in the "self-employed" category have great discretion, while those in the "hourly workers" category have fixed work times. "Hourly workers" could increase work time but only at their employer's discretion and with overtime pay.[24] Based on a survey, CSM estimates the number of individuals in each category with TV access so that viewership rates can be translated into number of people watching TV. Table 1 provides summary statistics for each category. Across all hours an average of 91 thousand "self-employed" and 149 thousand "hourly workers" watch TV.

[23] A 2008 survey conducted by the Beijing Statistics Bureau (2009) estimates that the average Beijing resident spends 7.6 hours working, 1.4 hours commuting, 1.8 hours on household chores, and 3.5 hours on leisure activities during a work day. TV watching comprises 1.9 hours or 54% of total leisure time.

[24] For brevity we call TAM's "proprietor/private" category as "self-employed." We choose two categories to limit the cost of data. We prioritized the other categories ("unemployed," "cadres/ managers," "junior civil servants/office clerks," "students," "other") lower either because we do not have specific predictions for them or we are less certain whether they control their work time.

Control Variables: Our pollution regressions include daily weather variables known to affect particulate matter (EPA, 2010) all taken from China Meteorological Data Sharing Service System. We include dummies for the four quartiles of the daily maximum wind speed.[25] Higher wind speeds can remove particulates but also import them from neighboring areas. Beijing air quality is greatly affected by wind direction. Northerly winds carry local pollutants while Easterly and Southeasterly bring pollutants from the Eastern coastal and mid-China cities (Wiedensohler, *et al.*, 2007). To control for this flexibly, we use dummies for the four directional quadrants and interact them with the four wind quartiles. We include daily hours of sunshine to control for atmospheric solar radiation, which creates ozone and particulate matter. Humidity can interact with pollutants to create secondary ones. Rain can interact with existing pollutants to create secondary ones but can also wash particles from the air and minimize their formation. Daily maximum surface temperature has an indeterminate effect on particulate matter depending on whether a temperature inversion is created.

For the viewership regressions, we include daily weather measures that might affect the desire to remain indoors watching TV. These include total rainfall, average wind speed, total sunshine hours, and average surface temperature. We use daily measures even though our regressions are at the hourly level because we assume households decide whether to travel to work based on daily weather.

5. Effect of Driving Restrictions on Pollution

Implication 3 predicts that traffic density and therefore pollution declines during the policy periods. To test this we employ an RD method using aggregate API. Intuitively, our test determines if any pre-existing time trend is altered at the onset of policy periods conditional on the control variables. Since coincident factors may confound these results, we provide additional evidence based on DD estimates using station-level API data.

Effect on Aggregate Pollution: Our RD method tests for a potential discontinuity in the aggregate API due to the driving restrictions:

[25] Maximum is across averages during all ten-minute periods of the day. We experimented with using average daily speed, wind gusts (maximum speed during any three-second period), and maximum level directly. Quartiles of maximum daily speed provided the best fit.

(1) $$\log\left(API_t^A\right) = \alpha + \sum_{i=1}^{51} \beta_{1i} w_{t\in i} + \beta_2 WE_t + \beta_3 HO_t + \beta_4 BR_t + \sum_{k=1}^{K} \beta_{5k} Z_{tk} + Policy_t + f(t) + \varepsilon_t^A.$$

API_t^A is the aggregate API on day t, w_t are week-of-year dummies to capture seasonality not captured by the weather controls, WE_t is a weekend dummy, HO_t is a holiday dummy, and BR_t is a dummy for the break period between the OddEven and OneDay policies. Besides weather controls, Z_t includes a dummy for the Olympic Games period.

The policy effects are captured by:

(2) $$Policy_t = \rho_1 OE_t + \rho_2 OD_t + \rho_3 OD_t * WE_t,$$

where OE denotes the OddEven and OD the OneDay policy. ρ_1 and ρ_2 are of primary interest. ρ_3 allows for inter-temporal substitution to weekends within the OneDay policy period so we expect it to be non-negative. The identifying assumption is that, conditional on the covariates, unobserved factors affecting the API are uncorrelated with time. If they are, it may induce a correlation between ε_t^A and time and thus with $Policy_t$ biasing our estimates. This could occur due to changes in economic activity or other policies that alter API's long-term trend. To control for this we include an L th-order time trend within each of the regimes: "Before OddEven," "During OddEven/Before Olympics," "During Olympics," "During OddEven/After Olympics," the "Break" between the OddEven and OneDay policies, and "During OneDay:"

(3) $$f(t) = \sum_{l=1}^{L}\Big[\gamma_{1l}\mathrm{I}_{t<\bar{t}_{OE}}\left(t-\bar{t}_{OE}\right)^l + \gamma_{2l}\mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OLB}}\left(t-\bar{t}_{OE}\right)^l + \gamma_{3l}\mathrm{I}_{\bar{t}_{OLB}<t<\bar{t}_{OLE}}\left(t-\bar{t}_{OLB}\right)^l + \gamma_{4l}\mathrm{I}_{\bar{t}_{OLE}<t<\bar{t}_{BR}}\left(t-\bar{t}_{OLE}\right)^l + \gamma_{5l}\mathrm{I}_{\bar{t}_{BR}<t<\bar{t}_{OD}}\left(t-\bar{t}_{BR}\right)^l + \gamma_{6l}\mathrm{I}_{t>\bar{t}_{OD}}\left(t-\bar{t}_{OD}\right)^l\Big],$$

where I is an indicator variable for the statement being true, $\bar{t}_{OE}$ is the first day of the OddEven policy, $\bar{t}_{OLB}$ the first day of the Olympics period, $\bar{t}_{OLE}$ the day after the last day of the Olympics period, $\bar{t}_{BR}$ the first day of the break period, and $\bar{t}_{OD}$ the first day of the OneDay policy. This allows for different time trends during the pre-treatment and policy regimes as suggested in Angrist and Pischke (2009) and allows for interruption of the OddEven time trend by the Olympics. Identification requires:

(4) $$E\left[Policy_t \bullet \varepsilon_t^A \middle| w_t, WE_t, HO_t, BR_t, Z_t, f(t)\right] = 0.$$

This requires correctly specifying $f(t)$. While our specification is fairly flexible, this could be violated if unobserved factors affect the API in a nonlinear way over time that is not captured by the time trend. Therefore, we provide robustness checks below.

Before estimating the policy effects using the full sample we inspect them visually in small windows around the policy initiation. Figure 3 plots the residuals from regressing log API on the weather control variables and weekend and holiday dummies in a thirty-day window around the start of the OddEven policy along with fitted values from a local linear regression. The fitted trend line shows a slight upward trend just prior to the policy and a discontinuous drop of 31% on the first day of the policy period. The upward trend continues for a few days after the policy begins before leveling off. Figure 4 shows a similar plot using a twenty-day window around the beginning of the OneDay policy. The fitted trend line displays an upward trend in the week before the policy begins and a discontinuous drop of 42% at the policy threshold with the trend leveling off after the policy.

Columns 1 and 2 of Table 2 show Equation (1) estimated with linear $(L=1)$ and quadratic $(L=2)$ time trends for each of the six regimes. A regression of residuals on lagged residuals revealed that they exhibited order-one autocorrelation so we use Newey-West standard errors with a one-day lag in all full-sample aggregate API regressions.[26] Consistent with the gradual trends in Figures 3 and 4 the policy effects are highly significant using linear time trends. With quadratic time trends the OddEven effect is very significant and somewhat greater than with linear time trends. The OneDay effect remains negative but it is no longer significant. Since the API displays only gradual trends close to the OneDay policy (Figure 4) the quadratic trend may over-fit the data. Regardless, this highlights the importance of our station-level and viewership evidence which do not rely exclusively on time-series variation.

We focus on the linear time trend results since Figures 3 and 4 display only gradual trends and these estimates are close to our later DD results.[27] Both policy variables are

[26] A Durbin-Watson test did not reveal any significant serial autocorrelation in the residuals nor did a test of partial autocorrelation of the residuals using a Portmanteau (Q) test for white noise with different numbers of lags. An OLS baseline regression produced very similar standard errors. A Tobit regression constraining the API to a maximum of 500 produced almost identical results. We do not use this as the primary specification because it does not control for autocorrelation. There was also a four-day period (August 17 to 20, 2007) including a weekend when odd-even restrictions were partially tested. Setting the OddEven variable to one for these days yields similar results.

[27] We also re-estimated the regression in Column 1 distinguishing the OneDay69 and OneDay78 policies. All policy variables were highly significant. The OddEven coefficient was similar and the two OneDay coefficients were both statistically indistinguishable from a single coefficient.

highly statistically significant and show a decrease in pollution during the restricted periods. The aggregate API was 18% lower during the OddEven restrictions. With perfect compliance, no substitution to non-restricted hours, and a linear relationship between the number of cars and pollution, we would expect about a 25% decrease during the OddEven period (traffic reduced by 50% and 50% of PM_{10} produced by motor vehicles).[28] The aggregate API was 21% lower during the OneDay policy. We would expect about a 10% decline (traffic reduced by 20% and 50% of PM_{10} created by motor vehicles) which is close to the lower end of the 95% confidence interval. These estimates are consistent with high compliance.

The API is significantly lower on weekends but there is no significant substitution to weekends during OneDay relative to weekends absent the policy.[29] A one-degree temperature increase is associated with a 5.3% increase in the API – consistent with greater ozone and secondary pollution creation. A one-percent increase in humidity increases the API by 0.5%, consistent with humidity creating secondary particulates. Rainfall has no significant effect, but each additional hour of sunshine decreases the API by 3.2%.

Column 3 estimates using logarithm of PM_{10} as the dependent variable based on the transformation in Appendix F to convert from the API and controls for time-varying unobservables using a linear trend. The number of observations falls to 917 because there are 143 days when the API is below 50 and the maximal pollutant unknown, 29 days when the worst pollutant is other than PM_{10}, and 7 days when the API is above 50 but the pollutant identity is missing. Since the data is not contiguous, we cluster standard errors in rolling two-day blocks. The policy effects are larger but not statistically different than the linear time-trend results using the API.

We perform two tests of whether the time trends are sufficiently flexible to ensure the unobserved factors are uncorrelated with time. First, we test for discontinuities at the

[28] Substitution effects are likely small since the restrictions applied except from midnight to 3:00 a.m. Pollution rises convexly with congestion because cars spend more time idling and a longer time traveling the same distance (see Arnott and Kraus, 2003; Small and Verhoef, 2007). During the OneDay policy, a larger adjustment for inter-temporal substitution is possible because the OneDay restrictions do not apply in the late evening and early morning hours.

[29] Drivers may also inter-temporally substitute to non-restricted weekdays. RD estimates control for part although not all of this substitution. Drivers restricted on the first day of the OneDay policy (which was a Friday) will begin driving more on the Thursday before the policy starts and the estimates will capture this. On the other hand, no drivers are restricted on the Thursday before the policy begins so these drivers will not need to drive more on the following day even though such inter-temporal substitution will occur in future weeks of the policy. Therefore, RD estimates may overestimate the policy effects relative to what will occur on an ongoing basis.

median of the two subsamples on either side of the policy as recommended by Imbens and Lemieux (2008). The closest analogy in our setting is the midpoint of the sample prior to the OddEven and after the OneDay policy. We supplement this by testing for a discontinuity at the ¾ point before the OddEven policy and the ¼ point after the OneDay. These placebo test results are shown in Online Appendix G: "Pre-OddEven" in the top and "Post-OneDay" in the bottom panel. They suggest that including at least a linear trend is sufficiently flexible to control for time-varying unobservables.

Second, we check our results using "discontinuity samples" (Angrist and Lavy, 1999) and employ a two-step procedure.[30] We first regress log API on the control variables (except time trend) in a thirty-day window around the OddEven policy start and then in a second step use a local linear regression to estimate the policy effect. In a sufficiently small window identification requires only that no confounding factors change discontinuously at the cutoff. The top panel of Table 3 shows results using a rectangular kernel with varying bandwidths. Above a bandwidth of three, the predicted policy effect is significant and the magnitude is fairly robust.[31] Given the standard errors, the range of estimates (23 to 31%) overlaps significantly with our full-sample RD estimate (18%).

The bottom panel shows analogous estimates in a twenty-day window around the OneDay policy start (the longest possible to avoid overlapping with the OddEven policy). The predicted policy effects are significant above a bandwidth of three. The point estimates (26 to 42%) are larger than our full-sample RD estimate of 21%. However, the standard errors are large enough that all of the point estimates are within 1.8 standard deviations of the midpoint of the full-sample estimates.

Effect on Station-Level Pollution: The RD results depend only on time-series variation and could be confounded by contemporaneous factors. To address this, we use geographic variation in monitoring stations' locations[32] and test whether pollution

[30] We use a two-step procedure with a local linear regression because in a one-step procedure the policy effects were sensitive to the time trend order raising concerns about the proper functional form for the control variables (Lee and Lemieux, 2010, p. 333).
[31] Although the discontinuities are not significant at the optimal bandwidth (Imbens and Kalyanaraman, 2012) for either the OddEven or OneDay policies these are identified from sample sizes of six.
[32] Another DD approach would use any non-uniformity in the plate number distribution and allow for differential effects in which plate numbers were restricted on a given day. However, plate numbers were assigned randomly by the Beijing Traffic Management Bureau for a uniform fee through March 9, 2009. Only after that could a plate number be selected from a set of available numbers for a fee. Since April 10, 2009 plates can be exchanged at no cost but only from a list of ten numbers.

decreased more for monitoring stations located closer to major roads than for those further away in response to the policies:

$$\log\left(API_{st}^{S}\right)=\alpha_s+\sum_{i=1}^{51}\beta_{1i}w_{t\in i}+\beta_2 WE_t+\beta_3 HO_t+\beta_4 BR_t+\beta_5 OE_t+\beta_6 OD_t$$
$$+\beta_7 OD_t*WE_t+\sum_{k=1}^{K}\beta_{8k}Z_{stk}+DD_{st}+f_s\left(t\right)+\varepsilon_{st}^{S}, \tag{5}$$

where API_{st}^{S} is the daily API at station s on day t. As before, we include week-of-year dummies to capture seasonality along with weekend, holiday, and break dummies. The control variables $\left(Z_{st}\right)$ include the same daily weather controls and Olympic dummy as the RD estimates. Of primary interest is the treatment effect $\left(DD_{st}\right)$ which captures the driving restrictions' effect as a function of distance $\left(Dist_s\right)$ between each station and the nearest major road.

We include station-level fixed effects $\left(\alpha_s\right)$ to capture time-constant unobserved factors that cause some stations to have higher pollution levels. These include nearby stationary pollution sources as well as the baseline effect of distance to a major road. These fixed effects prevent the treatment effect from being biased upward by the fact that stations closer to a major road have higher pollution levels, both before and after the treatment, than do further stations. The BR, OE, OD, and $OD*WE$ terms prevent the treatment effect from being biased by the fact that all monitoring stations, regardless of distance from a major road, may be affected by the driving restrictions. We use the wild bootstrap of Cameron, Gelbach, and Miller (2008) to produce 95% confidence intervals allowing for group clustering of standard errors by station.[33] We also report significance levels using percentile p-values.

The identifying assumption is that, conditional on the covariates, station-specific unobserved factors affecting the API are uncorrelated with the treatment. That is, unobserved factors do not vary systematically with distance from a major road during the policy periods relative to before. This assumption may not hold if stations closer to a major road have different long-term pollution trends than those further away. For example, if traffic patterns changed differently over time on major relative to smaller roads. To control for factors that affect closer and further stations differentially over time we include separate, station-specific time trends for the regimes: "Before OddEven," "During OddEven/Before Olympics," "During Olympics," "During OddEven/After Olympics," "Break," and "During OneDay:"

[33] This was implemented using Stata code by Malde (2012).

(6) $$f_s(t)=\sum_{s=1}^{S}\sum_{l=1}^{L}\Big[\gamma_{1sl}\mathrm{I}_{t<\bar{t}_{OE}}\left(t-\bar{t}_{OE}\right)^l+\gamma_{2sl}\mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OLB}}\left(t-\bar{t}_{OE}\right)^l+\gamma_{3sl}\mathrm{I}_{\bar{t}_{OLB}<t<\bar{t}_{OLE}}\left(t-\bar{t}_{OLB}\right)^l+$$
$$\gamma_{4sl}\mathrm{I}_{\bar{t}_{OLE}<t<\bar{t}_{BR}}\left(t-\bar{t}_{OLE}\right)^l+\gamma_{5sl}\mathrm{I}_{\bar{t}_{BR}<t<\bar{t}_{OD}}\left(t-\bar{t}_{BR}\right)^l+\gamma_{6sl}\mathrm{I}_{t>\bar{t}_{OD}}\left(t-\bar{t}_{OD}\right)^l\Big].$$

Identification of the treatment effect requires:

(7) $$E\left[DD_{st}\bullet\varepsilon_{st}^{S}\,\middle|\,\alpha_s,w_t,WE_t,HO_t,BR_t,OE_t,OD_t,Z_{st},f_s(t)\right]=0.$$

While $f_s(t)$ is fairly flexible, this condition could be violated if unobserved factors affecting station-level pollution change in a way over time that is not captured by the time trend and is correlated with the timing of the driving restrictions and distance from a major road. We provide some evidence against this possibility below.

Before presenting our DD results we perform RD estimates $(DD_{st}=0)$ using the station-level data and linear time trends $(L=1)$. Column 1 of Table 4 uses a panel of 24 stations, 22 of which operated the entire time and two of which operated from 2008 to 2009. We include the additional two stations because they are present during most of our sample period and are located within the restricted area which adds identifying variation to our DD estimates. The OddEven policy reduces the API by 17% and the OneDay policy by 20% both of which are close to our estimates using the aggregate API. Unlike the aggregate API estimates these results show substitution to weekend driving during the OneDay policy. Andrews (2008) argues that compositional changes in monitoring stations over time may reflect systematic government decisions to close stations in highly polluted places. To ensure that this does not introduce compositional bias in the unbalanced panel we compare to balanced panel results in Column 2. The results are similar.

Columns 3 and 4 provide separate RD estimates for stations inside and outside the restricted area. The restricted area is inside the 5th Ring Road and during the OddEven and OneDay69 policies the 5th Ring Road itself. Outside the restricted area, pollution could either increase or decrease depending on whether these roads are substitutes or complements to those inside. The restrictions decrease traffic outside the restricted area if, absent the restrictions, traffic primarily feeds into the restricted area. Traffic increases if drivers use roads outside the restricted area more intensively to travel from one side of the city to the other while complying with the restrictions.[34] Since no stations are located on the 5th Ring itself, Column 3 includes the eight stations inside

[34] This ambiguity also rules out using monitoring stations outside the restricted area as a control group for those inside in a DD specification.

the 5th Ring Road and Column 4 the 16 stations outside. The policies significantly decrease pollution in both areas suggesting roads outside are complements.

For our DD estimates, we use the minimum distance “as the crow flies” between a monitoring station and the nearest major road (Ring or Class I Road).[35] We use only the eight monitoring stations within the restricted area for two reasons. First, there is no ambiguity about how traffic is affected within the restricted area if the driving restrictions have an impact. Second, including stations too far from major roads will bias against finding an effect because they will be outside PM_{10}’s dispersion radius (most PM_{10} is deposited within a few kilometers of its release according to EPA (2000), p. 2-3). All of the stations within the restricted area are not only inside the 5th Ring Road but also within the 4th Ring Road (see Figure 1). This is where Beijing’s road network is densest and ensures that monitoring stations are sufficiently close to a major road to identify an effect if it exists. Table 1 confirms that stations within the restricted area are much closer to the nearest Ring or Class I Road than those outside.

Our first DD estimator uses linear $(L=1)$, station-specific time trends separate for each of the six regimes and allows for differential effects of the policies on “near” and “far” stations (defined by median distance) inside the restricted area:

$$(8) \qquad DD_{st} = \left(\lambda_1 OE_t + \lambda_2 OD_t\right) \mathrm{I}_{Dist_s < \overline{Dist}},$$

where $\overline{Dist}$ is the median distance to the nearest Ring Road across all eight stations.

Figure 5 plots the residuals from regressing station-level log API on the weather control variables and weekend and holiday dummies in a 45-day window around the start of the OddEven policy averaged on each day for “near” and “far” stations along with their grand means before (normalized to zero) and after the policy. Prior to the policy the residual averages for “near” and “far” track each other closely (correlation of 0.948 with a significance level below 0.01%) consistent with the unobservables trending similarly. The residual averages also track each other closely after the policy (correlation of 0.815 with a significance level below 0.01%). However, the grand means for both groups of stations are lower after the policy and the grand mean for

[35] The Ring Roads are large, multi-lane highways that loop around Beijing. The segments (East, West, North, or South) of four of the Ring Roads (2nd, 3rd, 4th, or 5th) are the busiest roads in Beijing according to 2006 data from the Beijing Transportation Research Institute. A Class I Road is a multi-lane highway in each direction with controllable entries and exits and a divider in the median. We use the Geographic Information System (GIS) software’s ARCINFO command “Near” to compute the distance between the monitoring station and the nearest point on the road.

the “near” stations lies below that for the “far.” The residual averages are similar the three days before the policy. Both drop on the first day of the policy but the “near” more than the “far” (16.4% versus 11.9%) and with the exception of a few outliers the “near” residuals lie below the “far” after the policy whereas before the policy they are close together.[36]

Column 1 of Table 5 shows full-sample regression results. The OddEven policy reduces pollution by 18.8% at “far” stations but 23.4% at “near” stations. For the OneDay policy, pollution drops 18.2% at the “far” stations and 21.2% at the “near” stations. In contrast to the RD estimates, these estimates display a 6.3% increase in weekend pollution during OneDay consistent with inter-temporal substitution. Although statistically significant, the magnitude is small.[37] The estimates for OddEven and OneDay in Column 3 of Table 4 lie between the “near” and “far” effects as expected. Column 1 of Table 5 allows for a separate time trend at each station. Column 2 allows for separate time trends for “near” and “far” stations within each of the six regimes to provide suggestive evidence as to whether they differ.[38] The policy effects are close to those in Column 1 and the “far” time trends are not statistically different from the “near” in any regime (see Appendix H for the full time trend results).

Column 3 of Table 5 uses the same specification as Column 1 but substitutes a quadratic distance function for the near/far distinction:

(9) $$DD_{st} = \sum_{j=1}^{2}\left(\lambda_{1j}OE_t + \lambda_{2j}OD_t\right)*\left(Dist_s\right)^j.$$

Pollution drops by 34.5% at the Ring Roads during OddEven. Although not individually significant, the distance terms are jointly significant at the 1.9% level and the effect diminishes with distance to a 15.3% drop at 1.3 kilometers (stations inside the restricted area are all within 1.3 kilometers of a Ring Road). Pollution drops by 27.5% at the Ring Roads during OneDay. The linear distance term is individually significant and the distance terms are jointly significant at the 1.0% level. The effect diminishes to a minimum drop of 18.7% at 1.0 kilometers. Although the quadratic

[36] Using a linear time trend in this discontinuity sample yields similar results although greater in magnitude – a drop of 16.7% at “far” stations and 20.7% at “near.”

[37] Using the results inside the restricted area (Column 3 of Table 4), on a weekly basis the decrease during weekdays equals 90.8 points (a drop of 19.6% over five weekdays at an average weekday API of 92.8) while the increase on weekends is only 10.8 (an increase of 6.3% over two days at an average weekend API of 85.6).

[38] As additional supporting evidence the correlation between the average log API at “near” stations and “far” stations prior to the OddEven policy is 0.988 with a significance level below 0.01%.

functional form implies that pollution increases beyond this point this is near the maximum distance of 1.3 kilometers for stations inside the restricted area.

In theory, our DD test could be applied using any size road. In practice, there is a tradeoff. Using smaller roads shrinks the average distance to the nearest road improving identification. However, smaller roads have less traffic and generate less pollution. This and the fact that neighboring large-road pollution may overwhelm that from small roads may make identification harder. Column 4 uses distance to the nearest Class I Road. These are smaller and less trafficked than Ring Roads. Using the same specification as Column 2, the OddEven policy reduces pollution by 20.5% at "far" stations. "Near" stations show an additional drop of 3.0% but it is not significant. For the OD policy, pollution drops 21.3% at the "far" stations and significantly more (25.4%) at the "near" stations. These results are weaker than those for the Ring Roads consistent with the traffic volume on these roads being less sufficient to identify the distance effects. The time trend coefficients are not significantly different for "far" and "near" stations except during the break.[39]

Appendix I shows the effect of the OddEven policy on stations inside the restricted area and the differential effect on "near" stations in discontinuity samples. Identification is less demanding in a small window – it requires only that no confounding factors change discontinuously at the cutoff differently for the "near" and "far" stations. The overall effects are highly significant with 45- and 60-day windows showing declines of 11.9% and 19.0% for "far" stations. The decline at "near" stations is even greater but is significant only at the 10.8% level in the 60-day window.

Policy Comparisons: "Back-of-the-envelope" calculations can be used to approximate the increase in gasoline prices or auto registration fees necessary to achieve the same pollution reduction as our estimate for the OneDay policy (21%). Cheung and Thomson (2004) estimate a long-run gasoline price elasticity of -0.56 in China using data from 1980 to 1999. The gas price at our sample midpoint is about RMB 6 per gallon, implying that a long-run price increase of RMB 2.25 per gallon

[39] We tried the same regression using distance to the nearest Class II Road – the next largest class of roads. These allow speeds between 60 and 80 kilometers per hour and have at least two lanes in each direction but, unlike Class I Roads, have no barrier in the median. Although we still found significant drops in the API due to the OddEven and OneDay policies (21.4% and 19.9%) we did not find significant differential effects for "near" and "far" stations. This is possibly because pollution from nearby larger roads overwhelms the pollution from these roads.

(37.5%) would achieve the same pollution reduction if pollution falls linearly with gas usage.[40]

An alternative is to increase registration fees to reduce the stock of cars. If registration is one-time and transferrable across owners, a fee increase is equivalent to a vehicle price increase. Deng and Ma (2010) estimate an own-price elasticity of -9.2 for autos in China using annual data from 1995 to 2001. This is about three times greater than estimates for the U.S., possibly due to less elastic demand at higher incomes. Given income increases in China since 2001 it is useful to consider elasticities ranging from -3.0 to -9.2. If pollution falls linearly with car ownership and assuming an average car price of USD 15 thousand,[41] a license fee increase of USD 342 to 1,050 (RMB 2,253 to 6,908) would lead to a 21% pollution reduction. This compares to the current RMB 500 (USD 76) registration price in Beijing.[42]

Robustness and Alternative Explanations: Our DD approach provides a convenient procedure to confirm that the policy effects are associated with the driving restrictions rather than proximate policy changes. We estimate a fixed-effects regression using the station-level data and interact the fixed effects with the policy variable. The coefficients on the interaction terms provide the station-specific policy changes. These station-specific changes should be correlated with the distance to pollution sources affected by the policy and uncorrelated with those not. In our case, the correlation between the station-specific OddEven effects and distance to the nearest Ring Road is 0.750 significant at the 3.2% level; while the correlations with distance to the airport and nearest subway line are insignificant at the 90.7% and 42.9% levels.

This implies that any confounding factors are related to proximity to major roads and therefore traffic flow. These could include gasoline prices, parking rates, vehicle emission standards, and subway capacity changes. The National Development and Reform Commission (NRDC) regulates retail gasoline prices and changed them somewhat during our sample period. Prior to December 19, 2008, the NRDC set a baseline price and allowed firms to charge a retail price within 8% of it. After this, NRDC imposed a retail price ceiling. The timing of price changes is generally

[40] Auffhammer and Kellogg (2011) find that precisely-targeted, inflexible regulation of gasoline elements most prone to form ozone is effective in reducing ozone pollution.
[41] Unless otherwise noted, all exchange rate conversions performed at January 2011 rates (1 RMB = 0.152 USD). Most 2009 car purchasers were in the range of RMB 50 to 150 thousand according to "Annual Report of China Car Industry 2009 – 2010," An, *et al.*, (2010). The midpoint of this range is USD 15.2 thousand.
[42] See "Beijing's Plan to Steer Clear of Traffic Jams," *China Daily*, December 14, 2010.

different than the driving restriction policy changes, although there was a significant price drop around the start of the OneDay policy which would bias against our findings. Adding the logarithm of retail gas price to the aggregate API regression with linear time trends produces very similar results.[43]

Regulated parking rates at public garages did not change during our sample period.[44] Private garages are allowed to charge market rates but this would bias against a reduction in driving under the restrictions. The number of official taxis in Beijing has remained constant at 66,646 since 2006.[45] Taxi cab emissions have declined over time through replacement of older taxis and upgrading of existing ones but this has occurred gradually. Staggered working hours were implemented in Beijing for those employed by social organizations, non-profit institutions, state-owned enterprises, and urban collective-owned enterprises but this did not take effect until April 12, 2010, after our sample period.

China's auto emissions regulations are similar to European Standards I to V. From the beginning of our sample through February 28, 2008 autos registered in Beijing had to conform to the Level III standard. After this and through the end of our sample, new vehicles had to meet the stricter Level IV standard. This timing differs from those of the driving restrictions and since the change applied only to new vehicles any effects occurred gradually.

Beijing added subway capacity during our sample period (see Figure 2). The timings did not coincide with the driving restriction policies and we showed earlier no significant correlation between station-specific effects and distance to subway; however some of our estimated policy effect could result from substitution from auto to public transit commuting. As a partial test of whether these policies confound our estimates we examine the opening of subway Line 5 and reduction of subway fares on October 7, 2007 and the reduction of suburban bus fares on January 15, 2008.

[43] The OddEven effect is -17.8% and the OneDay is -21.2% with both significant at better than the 3.6% level. The price coefficient is insignificant. Price data taken from NDRC documents at the Beijing Development and Reform Council website (http://www.bjpc.gov.cn).
[44] According to parking regulations in, "Notice of Adjusting the Rates for Non-Residential Parking Lots in Beijing," Beijing Municipal Commission of Development and Reform (2010), File No. 144 (in Chinese) and "Notice of Adjusting the Rates of Motor Vehicle Parking Lots in Beijing," Beijing Bureau of Commodity Prices (2002), File No. 194 (in Chinese).
[45] Under a decision by the Beijing Council of Transportation as part of the "Tenth Five-Year Plan" (according to *Beijing Statistic Yearbook* (2007, 2008, 2009), China Statistics Press).

The top panel of Online Appendix J, Columns 1 to 3 adds policy dummies for these to the specification in Column 1 of Table 2. The OddEven and OneDay effects remain similar and the subway and bus policy effects are close to zero and very insignificant. This suggests that the RD estimates do not confound the subway and bus policies with the driving restrictions. Columns 1 to 3 of the bottom panel perform RD estimates on the subway and bus policies without controlling for the driving restrictions. The policy effects are insignificant consistent with them occurring gradually enough that our time trends capture them. Columns 4 and 5 of the top panel implement a DD specification for the subway and bus policies while controlling for the driving restriction policies. We find no significant evidence of monitoring stations closer to Ring Roads being differentially affected during these policies. Columns 4 and 5 of the bottom panel implement a DD estimator not controlling for the driving restrictions. The estimates reveal a significant increase in pollution with the suburban bus fare decrease but the distance effects are insignificant for both policies. The baseline increase could result from these large buses running more frequently as ridership increased with the drop in fares. The following viewership results also eliminate the possibility of substitution to public transit in explaining our results.

6. Effect of Driving Restrictions on TV Viewership

We examine viewership for two reasons. First, it provides evidence on how the restrictions affect economic activity. Implications 1 and 2 predict that the restrictions should have different extensive margin effects for those with and without labor supply discretion. We test this using viewership for two different employment categories: "self-employed" and "hourly workers." Second, it provides a means to rule out confounding factors that might explain the pollution reductions. Factors that reduce both auto and public transit congestion, such as greater subway capacity, should decrease viewership on the extensive margin for those with discretionary work time.

Our comparison embeds RD estimation within a DD design. We estimate the policy's effect on each worker category using an RD. This estimates whether there is a discontinuity in viewership with the policy onset relative to any pre-existing trend conditional on control variables. We then use a DD design to see if the policy change affects the two groups differently.

Since regular work hours generally occur during the restricted hours, we measure extensive margin effects by changes in aggregate viewing during restricted hours.

Although extensive margin changes may extend outside the restricted hours if work day length exceeds them, they will certainly affect viewership inside them. The model in Section 3 predicts that during the policy period TV viewership across all workers with fixed work times is unchanged during restricted hours (Implication 1) while it increases across all workers with discretionary work time (Implication 2).

Since the intensive margin will adjust primarily outside restricted hours, we measure intensive margin effects by changes in aggregate viewership outside them. Given the less-than-perfect correspondence between regular work and restricted hours and since theory is ambiguous about the intensive margin effects (Implications 4 and 5), our primary goal in estimating the intensive margin effects is to see if they overwhelm those on the extensive margin.

Our RD design allows for a potential discontinuity for each of the three policies (OddEven, OneDay69, and OneDay78). For the OneDay69 and OneDay78 policies we allow for intra-day discontinuities to estimate the effect on the extensive and intensive margins. We allow for only a daily discontinuity for the OddEven policy because the Olympic Games greatly disrupted intra-day work patterns. For the same reason, we focus on the OneDay results. We estimate

$$\begin{aligned}\log\left(View_{th}^{c}\right) &= \sum_{i=1}^{24}\beta_{1i}\alpha_{i=h} + \sum_{i=1}^{23}\beta_{2i}BR_t\alpha_{i=h} + \sum_{j=1}^{11}\beta_{3j}m_{t\in j} + \beta_4 WE_t + \beta_5 HO_t + \\ &\beta_6 OE_t + \left(\beta_7 OE_t + \beta_8 OD69_t + \beta_9 OD78_t\right)*WE_t + \left(\beta_{10}OE_t + \beta_{11}OD69_t + \right. \\ &\left.\beta_{12}OD78_t\right)*HO_t + Policy_{th} + \sum_{k=1}^{K}\beta_{13k}Z_{tk} + g\left(t\right) + \varepsilon_{th}^{c}.\end{aligned} \tag{10}$$

$View_{th}^{c}$ is thousands of people watching TV on day t during hour h for worker category c (“self-employed” and “hourly workers”). The hourly dummies (α) capture intra-day variation in the appeal of other leisure activities (including sleep) and TV program quality. We allow these hourly effects to differ before the driving restrictions begin (β_1) and during the break period (β_2). We include month-of-year dummies to capture seasonality in outdoor activity. β_4 and β_5 capture differences in weekend and holiday viewership (due to programming differences or differential appeal of outdoor options) before the policy and β_6 captures change in viewership during the OddEven policy. β_{7-9} capture difference in viewership on weekends during the different policy regimes while β_{10-12} do the same for holidays. Besides weather controls, Z_t includes a dummy for the Olympic Games when programming differed greatly. We cluster standard errors by day to capture intra-day correlation among the hourly unobservables.[46]

The policy effect contains the primary coefficients of interest. These capture intra-day viewership differences during the OneDay periods relative to the pre-existing trend:

$$(11)\qquad \begin{aligned} Policy_{th} = &\left(\theta_1 OD69_t + \theta_2 OD78_t\right) * RH_{th} + \left(\theta_3 OD69_t + \theta_4 OD78_t\right) * NMH_{th} + \\ &\left(\theta_5 OD69_t + \theta_6 OD78_t\right) * NEH_{th}. \end{aligned}$$

We divide the day into three time segments to separately estimate the effects on the extensive and intensive margins. RH_{th} equals one during restricted hours and zero otherwise. For non-restricted hours, NMH_{th} equals one during morning hours (midnight to 6:00 a.m. during OneDay69 and midnight to 7:00 a.m. during OneDay78) and NEH_{th} equals one during evening hours (9:00 p.m. to midnight during OneDay69 and 8:00 p.m. to midnight during OneDay78) and zero otherwise. Morning and evening segments are a parsimonious way to distinguish non-restricted periods with very different viewing patterns. We expect the extensive margin effects to be positive for "self-employed" $\left(\theta_{1-2} > 0\right)$ and zero for "hourly workers" $\left(\theta_{1-2} = 0\right)$. θ_{3-6} capture intensive margin effects and theory is ambiguous about these.

Identification again requires controlling for unobservables affecting viewership to ensure they are uncorrelated with the error. To do so we include separate daily time trends for the regimes "Before OddEven," "During OddEven/Before Olympics," "During Olympics," "During OddEven/After Olympics," "Break," and "During OneDay:"

$$(12)\qquad \begin{aligned} g(t) = \sum_{l=1}^{L} \Big[&\gamma_{1l} \mathrm{I}_{t<\bar{t}_{OE}} \left(t - \bar{t}_{OE}\right)^l + \gamma_{2l} \mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OLB}} \left(t - \bar{t}_{OE}\right)^l + \gamma_{3l} \mathrm{I}_{\bar{t}_{OLB}<t<\bar{t}_{OLE}} \left(t - \bar{t}_{OLB}\right)^l + \\ &\gamma_{4l} \mathrm{I}_{\bar{t}_{OLE}<t<\bar{t}_{BR}} \left(t - \bar{t}_{OLE}\right)^l + \gamma_{5l} \mathrm{I}_{\bar{t}_{BR}<t<\bar{t}_{OD69}} \left(t - \bar{t}_{BR}\right)^l + \gamma_{6l} \mathrm{I}_{t>\bar{t}_{OD69}} \left(t - \bar{t}_{OD69}\right)^l \Big]. \end{aligned}$$

Appendix K shows the impact of the time trend order on estimates of the policy effect during restricted hours.[47] The top panel shows that for "self-employed," the coefficients on the OneDay69 interactions are positive and highly significant but decline at higher-order polynomials. The OneDay78 interactions are positive, highly

[46] The residuals exhibit autocorrelation with a maximum lag of four hours, so we also estimated using Newey-West standard errors with a four-hour lag. The standard errors of the significant viewership effects in Table 6 are all smaller and none of the insignificant viewership effects become significant.
[47] The appendix only includes time trends up to the fourth order because above fifth-order time trends create collinearities and require omitting trends for some regimes.

statistically significant, and fairly stable across time trend orders.[48] In the bottom panel, the "hourly worker"-OneDay69 interaction coefficient is small, variable, and insignificant beginning with second-order trends. The OneDay78 interaction is significantly positive but small with linear trends and significantly negative and very stable for second- through fourth-order trends.[49]

We focus on the fourth-order time trend results for three reasons. First, as shown in Appendix K, values of Akaike's information criterion model selection test drop dramatically in moving from first- to second-order trends and continue to decline monotonically through fourth-order trends for both self-employed and hourly workers.[50] Second, we show below that results from discontinuity samples are more consistent with fourth- than first-order trend results. Third, the fourth-order results are more conservative in estimating the viewership effects for the self-employed.

Viewership by Workers with Discretionary Work Time: Columns 1 and 2 of Table 6 display the "self-employed" results using a fourth-order time trend $(L = 4)$. Relative to weekdays before the restrictions, viewership during the OneDay69 and OneDay78 restricted hours is 8.9% and 16.9% higher. On the extensive margin, workers with discretionary labor supply work less and enjoy more leisure during the restricted hours. This is consistent with marginal workers who normally drive finding it too costly to do so when restricted. "Self-employed" viewers average 102.1 thousand during the OneDay69 restricted hours, implying an increase of 9.1 thousand viewers per hour. Assuming that preferences for viewing and commute cost sensitivity are uncorrelated, this extrapolates to 1.4% of the 656 thousand self-employed people and 0.10% of the 9.2 million employed people in Beijing.[51] Viewers average 98.1 thousand during the OneDay78 restricted hours so our estimates imply an increase of 16.5 thousand additional "self-employed" viewers or 2.5% of all self-employed.

Viewership outside the restricted hours (the intensive margin) can either increase or decrease. Those who do not work on their restricted day may compensate by working

[48] During OneDay69 restricted hours viewership increases by 21.1% using a first-order time trend and the effect declines to 8.9% using fourth-order trends. Viewership during OneDay78 restricted hours increases by between 16.9 and 20.4% using first- through fourth-order time trends.
[49] During OneDay78 restricted hours viewership increases by 5.2% using first-order trends and decreases by between 8.3 and 9.2% using second- through fourth-order trends.
[50] Lee and Lemieux (2010, page 326) recommend using AIC to select the optimal order of polynomial terms.
[51] Population data according to *The China Urban Statistic Yearbook 2009*, China Statistics Press. These calculations assume all Beijing residents have access to a TV. There were 134 color TVs per 100 households in Beijing in 2008 according to *Beijing Statistics Yearbook 2009*, China Statistics Press.

longer hours on non-restricted days; therefore, it is important to check whether intensive margin changes offset some or all of the extensive margin effects. During OneDay69, viewership decreases 24.5% during the morning hours. Although large in percentage terms this represents only 5.7 thousand additional viewers. This could reflect a shift to an earlier commute to comply with the restrictions and increased work time. During OneDay78, viewership remains unchanged in both the morning and evening hours. The intensive margin effects do not offset those on the extensive margin and the increased commute costs under the driving restrictions raise total viewership each restricted day by 102.7 thousand person-hours during OneDay69 and by 215.0 during OneDay78.[52] Work time would decrease less than this if TV viewing became more attractive relative to other leisure during the policy periods. The opposite is more likely because lower traffic congestion increases the appeal of leisure activities other than TV watching. Overall, output fell unless productivity increased during the fewer hours not spent watching TV.

Viewership by Workers with Fixed Work Times: Columns 3 and 4 of Table 6 display the results for "hourly workers" using a fourth-order time trend. Consistent with predictions for the extensive margin (Implication 1), viewership is unaffected during the OneDay69 restricted hours relative to weekdays before the restrictions. This is a fairly "tight zero" – it is not due to lack of variation. These workers must commute to work despite the restrictions and their leisure during required working times is unaffected. There is a decrease (8.3% or 13.7 thousand-viewers) during the OneDay78 restricted hours. This is consistent with these workers or their children experiencing fewer sick days from pollution. Hanna and Oliva (2011) find such evidence from pollution reduction following a Mexico City factory closure.

Theory is ambiguous about intensive margin changes. Work day length will not be affected given fixed work times, but leisure time may decrease or increase depending on whether public transit takes more or less time than car commuting. Viewership is unaffected during OneDay78 non-restricted hours. For OneDay69, viewership decreases 11.8% in the morning. Although large in percentage terms, because of the small viewership in the morning hours this represents only 2.8 thousand fewer viewers per hour. This could reflect workers leaving earlier to arrive at work by car before the restrictions begin. Overall viewership for "hourly workers" decreases by 35.9 thousand person-hours during OneDay 69 and 178.3 thousand during OneDay78.

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[52] For OneDay69 this equals 9.1 thousand additional viewers for 15 restricted hours less 5.7 thousand viewers for 6 morning hours and for OneDay78 16.5 thousand viewers for 13 restricted hours.

The significant control variables are generally of the expected sign. Fewer daylight hours and windier days are associated with more viewership by the "self-employed." For "hourly workers," viewership is lower on warmer days and those with more sunshine hours. Viewership is higher on weekends and holidays relative to weekdays before the restrictions and these effects are greater for "hourly workers" consistent with their having less discretion over when they work. Viewership is higher during the Olympics. Although we do not have specific predictions because work-leisure patterns were greatly altered around the time of the Olympics, viewership increases during the OddEven policy. The one unexpected result is that rainfall decreases viewership but the effects are very small.

Robustness and Alternative Explanations: Appendix L shows the effect of the OneDay policy on viewership in discontinuity samples using a two-step procedure. We first regress hourly log viewership for all hours on the control variables except time trend in a twenty-day window around the OneDay policy start. In the second step we use local linear regressions to estimate the policy effect separately for the restricted, morning non-restricted, and evening non-restricted hours. To allow for correlation among hours within a day we cluster standard errors by day. The results largely match those in Table 6 for the full sample using fourth-order time trends. During restricted hours, self-employed viewership increases 18.5% significant at the 8.5% level while for hourly workers it drops 8.7% but insignificantly (23.1% level). During evening hours, viewership displays statistically insignificant drops for both self-employed and hourly workers. The results differ from the full-sample results for the morning hours – there is an insignificant effect. The residuals are quite volatile during the morning hours suggesting caution in the conclusion that viewership declines during this time.

To ensure robustness to the grouping of hours into three segments, we re-estimate Equation (10) but interact the OneDay69 and OneDay78 policy variables each separately with hourly dummies. The results confirm our main estimates. Panel A of Figure 6 plots the coefficients on the interaction terms between OneDay69 and the hourly dummies for the "self-employed" category. The magnitudes of the coefficients are plotted on the y-axis only if significant at the 5% level or better. Relative to weekdays before the restrictions, viewership is higher during fourteen of the fifteen restricted hours and all are significant at better than the 1% level.[53] The decrease in the first restricted hour (6:00 – 7:00 a.m.) is consistent with workers who otherwise would have driven during this hour shifting their commute earlier to comply with the restrictions.

Panel B of Figure 6 provides the same graph for the "hourly workers." The results again confirm our main estimates although these results are fairly noisy. Viewership is largely unaffected during the restricted period with only six of the fifteen hours showing an increase. Although not displayed, the results for the OneDay78 policy are qualitatively similar but stronger. For "self-employed," viewership is significantly higher during all thirteen restricted hours with significance at the 1% level or better. For "hourly workers" viewership is significantly higher in only three of the thirteen restricted hours of which two are only marginally significant.

The viewership results further corroborate our pollution results. They preclude confounding factors inconsistent with the differing policy effects for those with and without discretionary work time. This includes increased subway capacity which would directly decrease public transit and indirectly decrease auto commute times as commuters substitute from buses, taxis, or private cars to subways. While this could partially explain the pollution results if its timing overlapped with the driving restrictions, it cannot explain the intra-day viewership results.[54] It contradicts the "self-employed" increasing their viewership during restricted hours. Also, shorter commute times should increase leisure time in non-restricted hours for both groups of workers (Appendix D shows this formally). We find no evidence of this in Table 6.

7. Cost-Benefit Quantification

We combine our pollution and TV estimates to quantify some of the driving restrictions' short-run costs and benefits. While we cannot perform a full welfare analysis, our results could serve as inputs to one. We focus on the OneDay policy since we are unable to estimate viewership effects for the OddEven policy and use the midpoint of our range of estimates.

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[53] The three significant effects in the morning hours are large in percentage but small in absolute terms. The average decrease from 1:00 to 4:00 a.m. is 7.6 thousand viewers per hour. The effect on absolute viewership is much greater during the restricted hours – 32.2 thousand viewers per hour.

[54] The Subway Line 4 opening during the OneDay period provides an opportunity to test whether subways had a differential effect on pollution. We added a policy dummy equal to one after the opening of Line 4 to the regressions in Columns 1 and 2 of Table 2. The coefficient was negative but insignificant (at the 21.4% and 51.3% levels). This is a low-powered test as Line 4 has low-volume.

The primary benefits of the driving restrictions are reduced morbidity and mortality from lowering PM_{10} by 30.8 µg/m^3 (21% drop in an average level of 147 µg/m^3). To estimate these we rely on Matus, *et al.* (2012). In their model, the welfare costs of pollution exposure include reduced activity days, acute mortality, and chronic mortality.[55] In the short run, chronic mortality effects are negligible as they depend on lifetime PM_{10} exposure[56] so we ignore these benefits.

Restricted activity days occur when pollution exposure confines adults to bed because of shortness of breath from even slight exertion. Epidemiological data relating pollution exposure to health outcomes implies 15.3 (13.5, 17.2)[57] million fewer restricted activity days annually due to the driving restrictions (Appendix M has detailed calculations). Restricted activity days involve a loss of both work and leisure time. We follow Matus, *et al.* (2012) and assume that leisure is valued at the wage rate. Beijing's average daily wage in 2007 was RMB 189,[58] implying a mean annual gain of RMB 2.89 (2.54, 3.25) billion.

Acute mortality is death resulting from current-year pollution exposure. Applying epidemiological data implies 1,114 (743, 1,485) fewer deaths annually due to the driving restrictions. Valuing the hastened mortality is controversial so we use both a lower-bound, human-capital and an upper-bound, value-of-statistical-life (VOSL) approach. Death from acute exposure normally only occurs to those that are close to death from other causes and hastens death by around one-half year (Matus, *et al.*, 2008). As a lower bound, we value each acute mortality case at one-half year of foregone wages. This yields a gain from the driving restrictions of RMB 26 (18, 35) million. For an upper bound, we value a life using VOSL estimates from Hammitt and Zhou (2006) and assume it is independent of life expectancy. This yields a gain from the driving restrictions of RMB 164 (109, 219) million. Reduced mortality benefits are small compared to those from fewer restricted activity days – at most 8.6% – so our conclusion is not very sensitive to value-of-life assumptions.

The primary cost of the driving restrictions is the lost output from reduced work time by the "self-employed" less the value of their leisure time. Daily output per Beijing

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[55] Medical costs are also estimated but these redistribute wealth from households to medical providers.
[56] See Equation (4) on page 60 of Matus, *et al.* (2012).
[57] Since pollution costs are sensitive to the relationship between health outcomes and pollution exposure, we provide lower and upper bounds in parentheses as explained in Appendix M.
[58] Average annual wage of RMB 47,132 for all employed persons in the "city area" of Beijing (see *China Urban Statistic Yearbook 2008*) converted to a daily wage assuming 250 work days per year.

worker is about RMB 417.[59] Lacking a more precise figure, we value leisure time at the average daily wage in Beijing (RMB 189). This implies a loss of RMB 228 per day for each person watching TV rather than working. We estimate an average 9.1 (16.5) thousand additional viewers each day during the restricted OneDay69 (OneDay78) periods. Given 250 work days per year, the driving restrictions cost RMB 519 (943) million annually. This could understate costs if "self-employed" generate greater output than the average worker and because we do not estimate viewership changes for all worker categories. However, our estimates include 72% of Beijing's non-government workers and most government workers have fixed hours.

This is all we can say about costs and benefits. A full welfare analysis would need to include other short-run effects. On the cost side these include implementation costs, driver compliance costs (trips not taken or taken in a non-preferred mode or route of transport), and reduced workplace agglomeration externalities (Arnott, 2007). On the benefit side these include reduced traffic congestion for drivers and pedestrians and the associated reduction in vehicle accidents (Parry, Walls, and Harrington, 2007).

8. Reasons for Effectiveness

Davis (2008) examines a similar one-day-per-week driving restriction in Mexico City and finds no short-run effect, primarily because it increased the number of vehicles in use and the proportion of high-emissions, used vehicles. Since the supply of used cars is relatively fixed, this suggests some pollution was diverted from outside Mexico City. Both of the reasons that Davis (2008) cites for the policy's failure are probably less relevant in Beijing. Although auto ownership is increasing quickly, its cost is still a significant fraction of income for most residents. In 2007, the average annual salary in Beijing was RMB 46,508 (USD 7,069) compared to USD 25,258 in Mexico City.[60] Since car sharing is difficult, purchasing a second vehicle with a different plate number to satisfy the restrictions is prohibitively expensive for most residents as is purchasing a first vehicle in response to lower congestion created by the restrictions.

Cars added in Beijing are also likely to be newer, lower-emissions vehicles. The number of vehicles in Beijing increased rapidly from 62 million in 1992 to 344

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[59] Beijing's 2007 annual per-capita GDP is RMB 60.0 thousand in the "city area" – roughly inside the 5th Ring Road (*The China Urban Statistic Yearbook 2008*). From the TAM data, 57.5% of Beijing's population is employed implying annual per-worker GDP of RMB 104.3 thousand. Dividing by 250 work days per year yields daily output of RMB 417.

[60] Beijing data from *The China Urban Statistics Yearbook 2008* and Mexico City data from http://mexico-city.co.tv/.

million in 2008.[61] This implies a younger auto stock compared to more developed countries where widespread car ownership began much earlier. Cars remain less prevalent in China than in developed countries. As of 2007, China had 24 cars per thousand people (0.065 per household) compared to 787 in the U.S. and 211 in Mexico (0.84 per household).[62] This means cheaper, higher-emissions used cars are not as readily available in China to be easily imported into Beijing from other cities.

Although the viewership results rule out Beijing's increase in public transit capacity as an explanation for the pollution reduction, it may be complementary. To the extent that the new subway lines acted as substitutes rather than complements for driving, they may have provided better commuting options thereby lowering compliance costs and limiting the labor supply decrease.

Compliance Evidence: It is uncertain whether compliance differences might explain the different outcomes in Beijing and Mexico City. Davis (2008) argues that penalties and monitoring in Mexico City are high but does not provide direct evidence. In Beijing there are about 2,215 traffic surveillance cameras (one for every 7.7 square kilometers) and about five thousand traffic police officers to detect violations. Every year, the first violation triggers a loss of approximately RMB 595 (about USD 90). Subsequent violations in the same year incur a fine of RMB 100 (about USD 15). Violators also incur time and possibly psychic costs (Appendix N provides more details on penalties and detection).

We obtained entrance records for a parking garage located within the restricted area and that attracts traffic from all parts of the city. The garage serves a mall and office tower so that parkers are a mix of shoppers and workers. The police require all Beijing garages to record the license plate number and entrance time to the minute of each entering car but they are not required to take any action against violators of the restrictions. We obtained one week of data (June 27 to July 3, 2010) chosen at random among weeks not containing holidays or government meetings that might affect

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[61] Data from "Independent Environmental Assessment: Beijing 2008 Olympic Games," United Nations Environment Programme, February 2009 (p. 42).
[62] Cars per thousand people based on "Urban Population, Development and the Environment," United Nations Department of Economic and Social Affairs, United Nations Publication #ST/ESA/SERA/274 (2008). Household size for Mexico City is based on nationwide data from "OECD Family Database," OECD (2012); household size for Beijing from China Statistic Yearbook (2008).

traffic. The garage's document retention policy prevented us from taking a sample within the time period of our main data.[63]

We divide the week's hours into three categories: restricted weekday, non-restricted weekday, and weekend (non-restricted). The week occurred during OneDay78 so we define restricted hours as weekday hours between 7:00 a.m. and 8:00 p.m. and non-restricted hours as weekday hours between 9:00 p.m. and 6:00 a.m. We avoid sampling data from 6:00 – 7:00 a.m. and 8:00 – 9:00 p.m. because commuting from the 5th Ring Road to the inner part of Beijing can take up to one hour and therefore these hours may reflect a mixture of restricted and non-restricted effects.

Since we do not know whether this garage represents Beijing traffic more generally, we only make within-garage comparisons. Weekend activity, when no drivers are restricted, should closely represent that absent restrictions. Although the station-level results in Table 4 show that weekend driving increases as drivers substitute from restricted weekdays, this is likely uniform across plate numbers. Therefore, we use the weekend distribution as the expected distribution. We compare this expected distribution to that observed during weekday restricted and weekday non-restricted periods. We discuss regular (hourly) parkers first.

Figure 7 illustrates the comparison of the expected (weekend) distribution to the observed distribution during Tuesday restricted hours when plates "2" and "7" are restricted. The expected distribution contains 5,975 observations with at least 83 for each plate number. The distribution is not uniform because drivers can pay extra to choose a plate number. The unlucky number "4" is least popular, while the lucky number "9" is most popular. The two restricted plates appear much less frequently than on the weekend and the other plates appear more frequently.[64] Appendix O analyzes data for all five weekdays and applies formal statistical tests. Overall, compliance is high. Of the ten restricted plate numbers during the week, eight are not significantly different from zero. Only plates "8," restricted on Wednesday, and "9," restricted on Friday, are significantly different from zero and only in proportions of 2.7% and 2.4% and at significance levels of 7.3% and 8.3%. A few cars entered the garage with no license plate – likely a method for avoiding detection by camera – but

[63] Therefore the sample is not necessarily representative of the plate number distribution during our sample period. In particular, over time drivers may have sought out less common plate numbers to avoid congestion.

[64] Figure 7 does not control for the fact that plates "2" and "7" should not occur under perfect compliance. Our detailed analysis in Appendix O does so.

they did not exceed 1.3% of all cars on any day. The garage serves primarily professional businesses and an upscale mall so this may understate compliance to the extent that the parkers have high incomes and are less sensitive to penalties.

There is little evidence of inter-temporal substitution across weekdays. Only four of the forty non-restricted plates during the week occur in a proportion greater than expected. We find no evidence of intra-day substitution when we compare the expected distribution to that for weekday, non-restricted hours although we have less data here. Of the fifty combinations of day/plate numbers, only five occur in greater proportion than expected and only one ("2" on Tuesday) is restricted.

The parking data separately identify monthly pass holders. The expected (weekend) distribution contains only 168 observations but the weekdays all have more than 235 observations, consistent with these being mainly workers. This group also exhibits high compliance. Of the ten restricted plates none of them are statistically different from zero. As with regular parkers, we find little evidence of inter-temporal substitution across weekdays. Of the forty non-restricted plate/day observations, only six appear in significantly greater proportion than expected. There is insufficient data on monthly pass holders during non-restricted, weekday hours to perform statistical tests for intra-day substitution.

9. Conclusion

Beijing's driving restrictions reduced air pollution but at the cost of less work time by those with discretionary labor supply. We identify the pollution reduction both inter-temporally and spatially, with larger drops at monitoring stations that are closer to major roads. This spatial test improves upon previous time series analyses by ruling out coincident policies unrelated to driving. Since most cities that monitor air pollution collect data from multiple locations to ensure representativeness, our approach can be used elsewhere to improve identification of intra-city policy changes that can be linked to identifiable pollution sources. Because it allows precise distance measures, it can disentangle the impact of concurrent and overlapping policies that affect different pollution sources differently.

We offer possible reasons for the policy's success in contrast to evidence of failure in Mexico City. Many of these reasons are shared by other rapidly-developing economies, which bear a substantial portion of the worldwide burden from urban air pollution (Cohen, *et al.*, 2005). A possible exception is high compliance to the restrictions in Beijing. To the extent that compliance is lower in other cities or countries success will be more limited.

The higher commute costs created by the restrictions reduce daily labor supply. To overcome data limitations in measuring work time, we use substitution to TV viewership. Workers with discretionary work time, often business owners and entrepreneurs who create jobs and innovations, increase their viewership during restricted hours consistent with reduced work time due to higher commute costs. Viewership by workers with fixed work time is unaffected consistent with their inability to adjust in the short run. Factors that reduce both auto and public transit congestion, such as expanded subway capacity, would increase work time for workers with discretion, eliminating these in explaining the pollution reduction.

We consider only short-run effects. As incomes in China increase, demand for driving will increase and so will the number of cars.[65] Thus, to keep auto pollution levels constant may require further increases in driving costs (*e.g.*, by restricting driving more than one day per week). To the extent that sharing vehicles is costly, this will keep average driving costs high and reduce the equilibrium number of cars. One cost of this would be further work time decreases.

Beijing's driving restrictions are not the most efficient way to reduce auto pollution. The restrictions arbitrarily reduce demand based on the last digit of a driver's license plate regardless of willingness to pay for driving. A more efficient allocation would result from pricing congestion or increasing vehicle license fees (Li (2014) quantifies the welfare loss from misallocation under Beijing's lottery system compared with a uniform price auction). We provide rough calculations of the increase in fees necessary to accomplish an equivalent pollution reduction. Beijing has moved in this direction, beginning to limit the number of new car registrations in December 2010.

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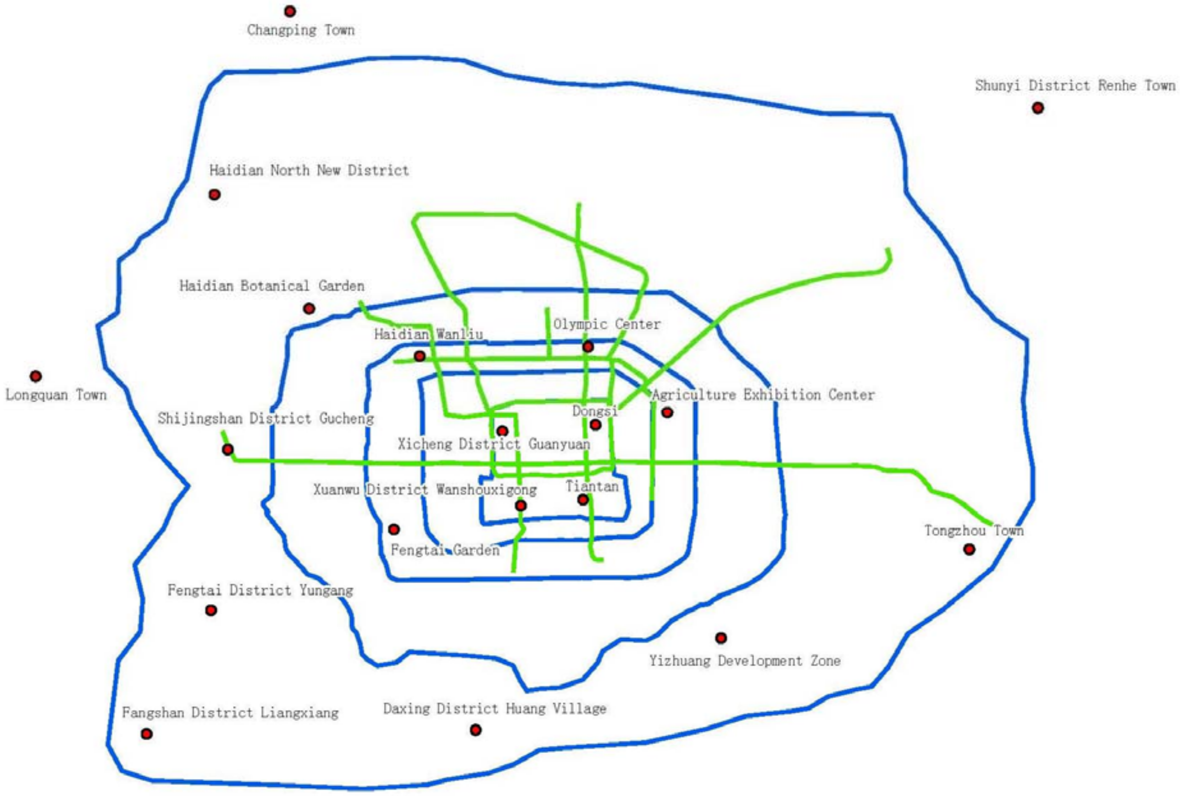
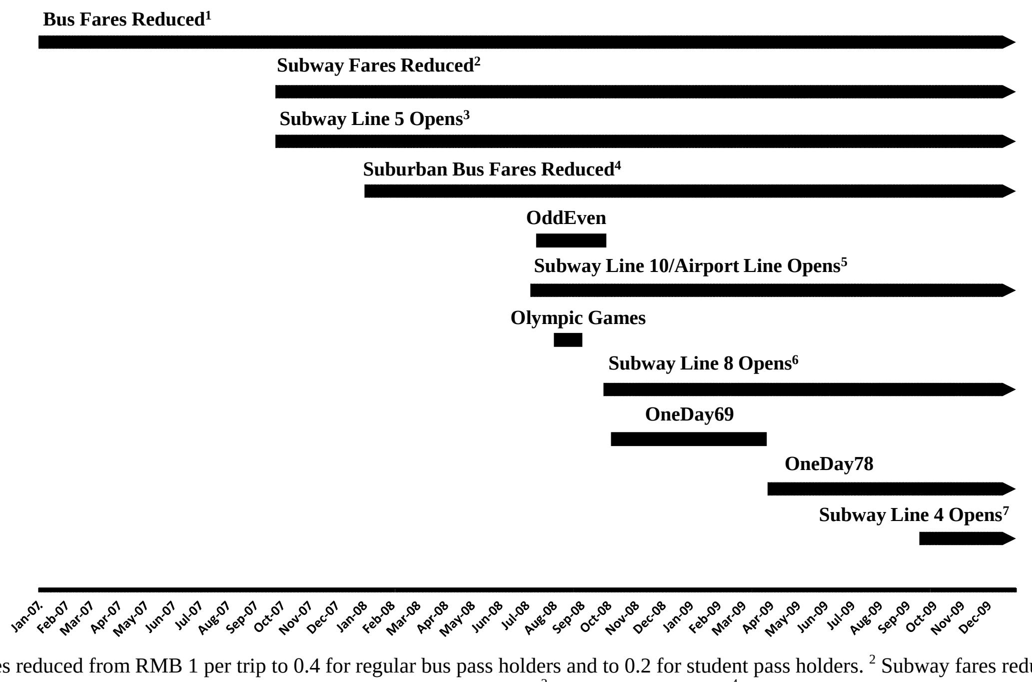
Figure 1 Map of Beijing Monitoring Station Locations in 2008 and 2009



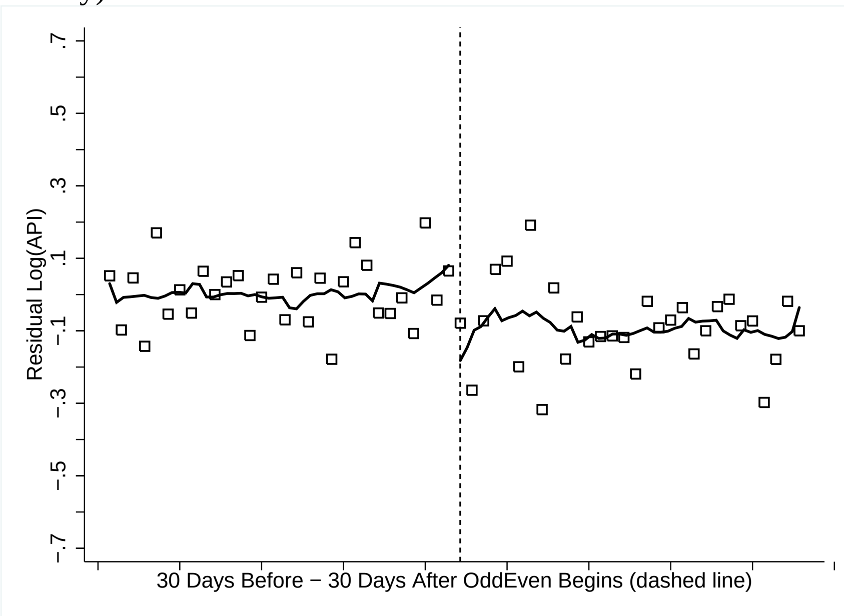
Map shows the locations of the monitoring stations (represented by dots) within or close to the 6th Ring Road (additional stations are located outside the 6th Ring Road). The green lines are subway lines. The blue lines are the Ring Roads. The inner-most blue line (which partially overlaps with a subway line) is the 2nd Ring Road and expanding out from there are the 3rd, 4th, 5th, and 6th Ring Roads.

Figure 2 Timeline of Pollution-Relevant Policies



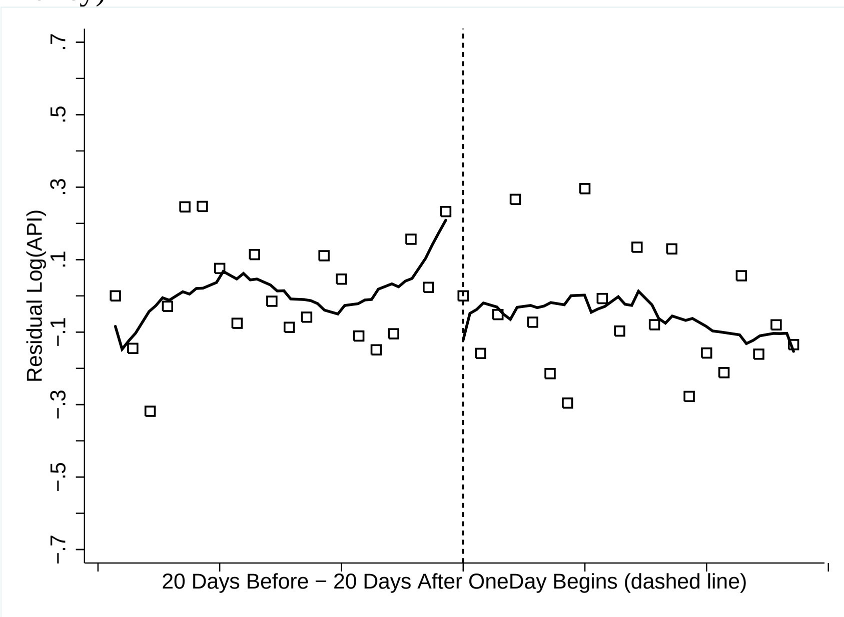
[1] Bus fares reduced from RMB 1 per trip to 0.4 for regular bus pass holders and to 0.2 for student pass holders. [2] Subway fares reduced from RMB 2 per transfer to RMB 2 per trip regardless of number of transfers. [3] Runs south to north. [4] Fares on suburban routes lowered by 60% for adults and 80% for students. "Suburban" routes connect the ten districts and counties outside the inner city with the eight city districts inside. [5] Runs southeast to northwest including the airport. [6] Serves the Olympics Park area. Opened on a more limited basis earlier to serve Olympic athletes and tourists. [7] Runs south to northwest.

Figure 3 Aggregate API Discontinuity Sample (Thirty-Day Window around OddEven Policy)



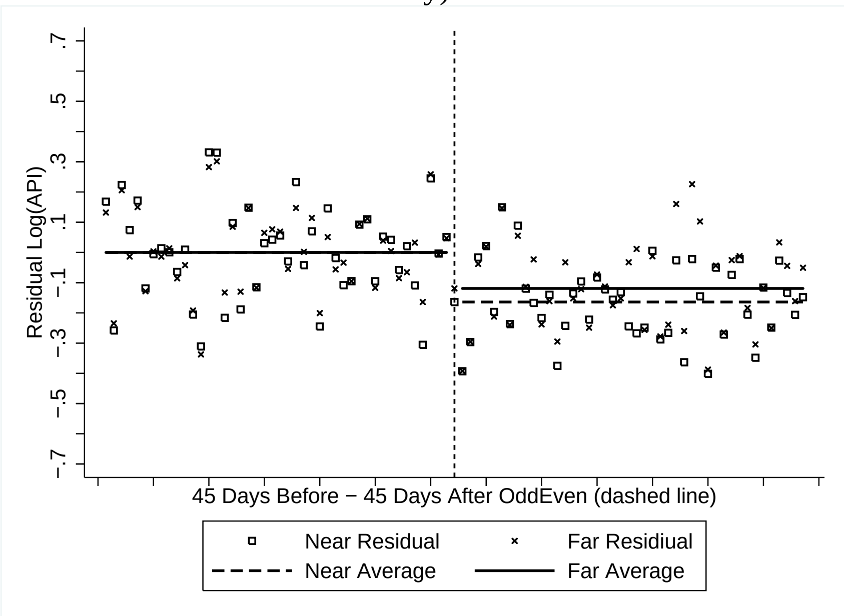
The square dots are residuals from a regression of log aggregate daily API on our standard set of controls (except time trend) in the 30 days before and after the beginning of the OddEven policy (the vertical dashed line). The solid lines are fitted values of the residuals from local linear regressions using a rectangular kernel and a bandwidth of 4.

Figure 4 Aggregate API Discontinuity Sample (Twenty-Day Window around OneDay Policy)



The square dots are residuals from a regression of log aggregate daily API on our standard set of controls (except time trend) in the 20 days before and after the beginning of the OneDay policy (the vertical dashed line). The solid lines are fitted values of the residuals from local linear regressions using a rectangular kernel and a bandwidth of 4.

Figure 5 Station-Level Differences-in-Differences Discontinuity Sample (45-Day Window around OddEven Policy)



Residuals from a regression of log daily station API on our standard set of controls (except time trend) in the 45 days before and after the beginning of the OddEven policy (the vertical dashed line). The data includes the 8 stations within the restricted area. The square dots are the average residuals for the 4 "near" stations (those below the median distance to the nearest Ring Road) and the "x" dots the average for the 4 "far" stations (those above). The horizontal dashed lines are the grand mean of the residuals for the "near" stations and the horizontal solid lines the grand mean for the "far" stations before (normalized to zero) and after the policy.

Figure 6 Coefficients on Interaction between Policy Variables and Hourly Dummies in Viewership Regression

Panel A: "Self-Employed" Percentage Difference in Viewership during OneDay69

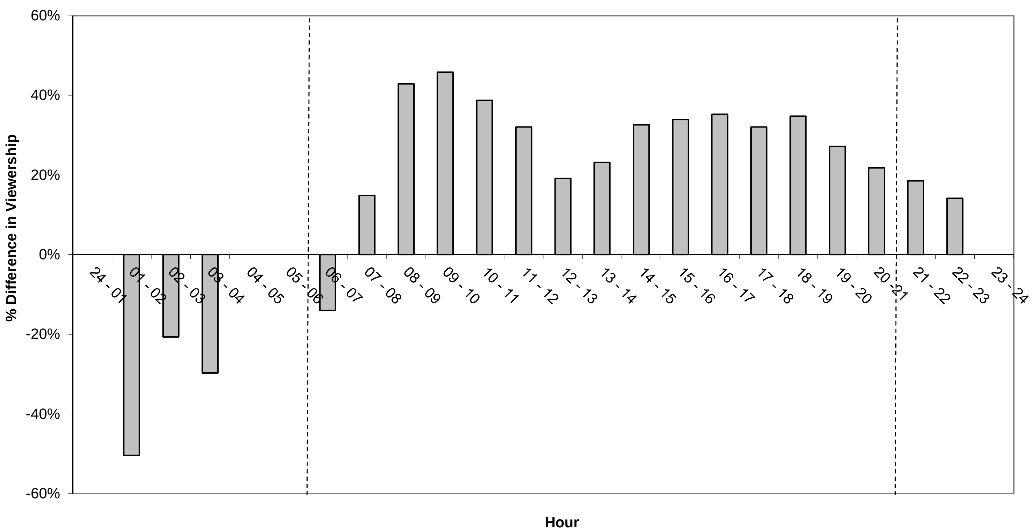


Chart shows coefficients on interactions between the OneDay69 policy variable and hourly dummies in the regression of Columns 1 and 2 of Table 7 but with OneDay69 and OneDay78 interacted with each hour separately. Coefficients are shown only if significant at the 5% level or better. The vertical dotted lines demarcate the restricted period.

Panel B: “Hourly Workers” Percentage Difference in Viewership during OneDay69

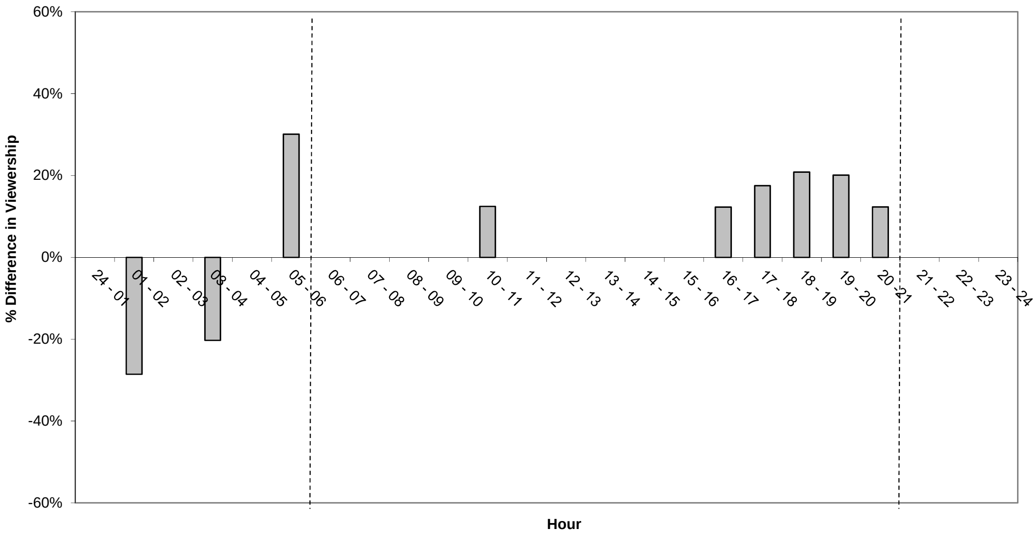
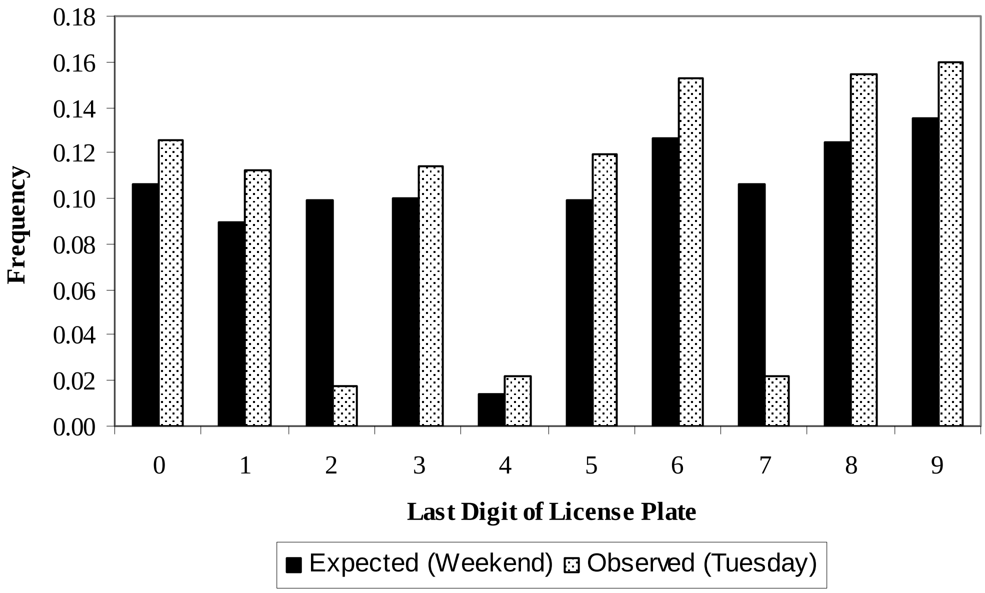


Chart shows coefficients on interactions between the OneDay69 policy variable and hourly dummies in the regression of Columns 3 and 4 of Table 7 but with OneDay69 and OneDay78 interacted with each hour separately. Coefficients are shown only if significant at the 5% level or better. The vertical dotted lines demarcate the restricted period.

Figure 7 Expected (Weekend) versus Observed (Tuesday) Distribution of License Plate Numbers



Ending license plate numbers of autos entering a Beijing parking garage inside the restricted area collected by authors. Expected distribution contains 5,975 observations and is based on June 27 (Sunday) and July 3 (Saturday), 2010. Observed distribution contains 2,848 observations and is based on Tuesday, June 29, 2010 between the hours of 7:00 a.m. and 8:00 p.m. when plates “2” and “7” were restricted.

Table 1 Descriptive Statistics

Variable	N	Mean	Standard Deviation	Min	Max
Daily Aggregate Pollution Data					
Aggregate API	1,096	90.925	49.485	12.000	500.000
Log(Aggregate API)	1,096	4.393	0.484	2.485	6.215
PM_{10}	917	146.652	79.097	18.000	600.000
Log(PM_{10})	917	4.867	0.482	2.890	6.397
OddEven	1,096	0.057	0.233	0.000	1.000
Break	1,096	0.018	0.134	0.000	1.000
OneDay	1,096	0.408	0.492	0.000	1.000
Olympics	1,096	0.016	0.124	0.000	1.000
Weekend	1,096	0.259	0.438	0.000	1.000
Holiday	1,096	0.071	0.257	0.000	1.000
Maximum Temperature	1,096	18.896	11.144	-6.900	39.600
Average Humidity	1,096	52.527	20.271	11.000	97.000
Total Rainfall	1,096	2.401	8.506	0.000	32.700
Sunshine	1,096	6.619	3.974	0.000	14.000
Daily Station-Level Pollution Data					
Station-Level API	25,482	90.227	50.751	6.000	500.000
Log(Station-Level API)	25,482	4.375	0.512	1.792	6.215
Station-Level Data (distance in kilometers)					
Distance from Ring Road	24	8.210	11.884	0.406	38.578
Distance from Ring Road (w/i Restricted Area)	8	0.831	0.264	0.406	1.280
Distance from Class I Road	24	2.216	2.630	0.073	10.040
Distance from Class I Road (w/i Restricted Area)	8	0.679	0.494	0.073	1.615
Hourly Viewership Data					
"Self-Employed" Viewership (thousands)	26,304	90.7	76.0	0.0	480.0
"Self-Employed" Log(thousands viewers)	26,304	4.042	1.179	0.000	6.176
"Hourly Workers" Viewership (thousands)	26,304	148.8	128.9	0.0	652.0
"Hourly Workers" Log(thousands viewers)	26,304	4.377	1.445	0.000	6.482
OneDay69	26,304	0.166	0.372	0.000	1.000
OneDay78	26,304	0.242	0.428	0.000	1.000
Average Temperature	26,304	13.600	10.976	-9.400	31.600
Average Wind Speed	26,304	2.212	0.915	0.500	6.700

See Appendix E for a description of the variables and their sources. Number of observations for daily station-level pollution data is slightly less than 26,304 (24 stations for 1,096 days) because not all stations present for whole sample. Number of observations for hourhly viewership data is equal to 24 hours per day for 1,096 days.

Table 2 RD Estimates using Log Aggregate Daily API and PM_{10} (2007 – 2009)

	(1)	(2)	(3)
	Log(API)		Log(PM_{10})
	Linear Trend	Quadratic Trend	Linear Trend
OddEven	-0.1771 **	-0.2570 ***	-0.3084 ***
	(0.0843)	(0.0985)	(0.1090)
OneDay	-0.2126 ***	-0.0915	-0.2684 ***
	(0.0500)	(0.0974)	(0.0681)
Weekend	-0.0563 **	-0.0600 **	-0.0757 **
	(0.0269)	(0.0266)	(0.0371)
OneDay*Weekend	0.0562	0.0567	0.0563
	(0.0404)	(0.0402)	(0.0565)
Maximum Temperature	0.0529 ***	0.0534 ***	0.0732 ***
	(0.0037)	(0.0037)	(0.0051)
Average Humidity	0.0049 ***	0.0051 ***	0.0069 ***
	(0.0010)	(0.0010)	(0.0013)
Total Rainfall	-0.0005	-0.0006	-0.0010
	(0.0010)	(0.0010)	(0.0014)
Sunshine	-0.0322 ***	-0.0320 ***	-0.0462 ***
	(0.0035)	(0.0034)	(0.0045)
R^2	0.6291	0.6632	0.4697
N	1,096	1,096	917

Dependent variable is log aggregate daily API in Models 1 and 2 and log of daily PM_{10} in Column 3. Standard errors in parentheses. Newey-West standard errors with 1-day lag used in Models 1 and 2; standard errors clustered in rolling 2-day blocks in Model 3. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include week-of-year dummies, wind direction, wind-speed quartiles, interactions between wind speed and wind direction, and dummies for Olympics, holidys and days with pollutant missing. Linear time trends are included in Models 1 and 3 and linear and quadratic time trends in Model 2. Separate time trends are allowed for the regimes Before Oddeven, During OddEven/Before Olympics, During Olympics, During OddEven/After Olympics, Break, and During OneDay.

Table 3 RD Estimates using Log Aggregate Daily API in Discontinuity Samples

	(1)	(2)	(3)	(4)	(5)
	OddEven: Residuals from 30-Day Window (N = 60) (Optimal Bandwidth 3)				
Bandwidth	3	4	5	6	7
OddEven	-0.0917	-0.3133 *	-0.3044 **	-0.2338 *	-0.2567 **
	(0.1815)	(0.1750)	(0.1431)	(0.1219)	(0.1146)
	One-Day: Residuals from 20-Day Window (N = 40) (Optimal Bandwidth 3)				
Bandwidth	3	4	5	6	7
OddEven	-0.2582	-0.4186 **	-0.3585 ***	-0.2606 **	-0.2954 ***
	(0.1813)	(0.1680)	(0.1150)	(0.1141)	(0.0908)

Estimates from local linear regressions using rectangular kernels with varying bandwidths. Dependent variable is the residual from regressing log aggregate daily API on the same control variables as in Table 2 (except time trend) in 30-day (for OddEven) and 20-day (for OneDay) windows around the policy date. Robust stranhdard errors in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. Optimal bandwidths calculated according to Imbens and Kalyanarman (2012).

Table 4 RD Estimates using Log Station-Level Daily API (2007 – 2009)

	(1) 24 Stations	(2) Balanced Panel	(3) Inside Restricted Area	(4) Outside Restricted Area
OddEven	-0.1730 ***	-0.1675 ***	-0.2095 ***	-0.1544 ***
	(0.0172)	(0.0178)	(0.0173)	(0.0234)
OneDay	-0.1970 ***	-0.1932 ***	-0.1957 ***	-0.1983 ***
	(0.0120)	(0.0122)	(0.0078)	(0.0176)
Weekend	-0.0462 ***	-0.0461 ***	-0.0534 ***	-0.0429 ***
	(0.0038)	(0.0039)	(0.0035)	(0.0052)
OneDay*Weekend	0.0639 ***	0.0634 ***	0.0632 ***	0.0651 ***
	(0.0046)	(0.0048)	(0.0056)	(0.0064)
R^2	0.6692	0.6676	0.6528	0.6767
Station Fixed Effects	Yes	Yes	Yes	Yes
Number of Stations	24	22	8	16
N	25,482	24,027	8,361	17,121

Dependent variable is log daily API at monitoring stations. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include week-of-year dummies, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind direction dummies, interactions between wind speed and wind direction, and dummies for Olympics, holidays and days with pollutant missing. Separate linear time trends are allowed for the regimes Before OddEven, During OddEven/Before Olympics, During Olympics, During OddEven/After Olympics, Break, and During OneDay and these are interacted with station fixed-effects in all regressions. The number of observations is not evenly divisable by the number of stations due to missing values.

Table 5 DD Estimates using Log Station-Level, Daily API (2007 – 2009)

	(1)	(2)	(3)	(4)
	Distance to Ring Roads			Distance to Class I Roads
	Near/Far		Quadratic Distance	
OddEven	-0.1878***	-0.1876***	-0.3449***	-0.2054***
	[-0.2918, -0.0838]	[-0.2915, -0.0837]	[-0.5359, -0.1540]	[-0.3191, -0.0917]
Near*OddEven	-0.0457**	-0.0379**		-0.0304
	[-0.0868, -0.0046]	[-0.0797, 0.0040]		[-0.1389, 0.0782]
OddEven*Distance			0.1973	
			[-0.6466, 1.0412]	
OddEven*Distance^2			-0.0378	
			[-0.1663, 0.0907]	
OneDay	-0.1819***	-0.1807***	-0.2753***	-0.2125***
	[-0.2826, -0.0812]	[-0.2807, -0.0807]	[-0.4277, -0.1229]	[-0.3301, -0.0948]
Near*OneDay	-0.0300**	-0.0222**		-0.0412*
	[-0.0604, 0.0004]	[-0.0465, 0.0022]		[-0.0994, 0.0171]
OneDay*Distance			0.1715	
			[-0.1302, 0.4733]	
OneDay*Distance^2			-0.0836	
			[-0.3791, 0.2118]	
OneDay*Weekend	0.0632***	0.0631***	0.0632***	0.0878***
	[0.0299, 0.0965]	[0.0299, 0.0964]	[0.0299, 0.0965]	[0.0415, 0.1341]
Linear Asymmetric Time Trends Interacted with Station Fixed Effects	YES	NO	YES	NO
Linear Asymmetric Time Trends Interacted with "Near"	NO	YES	NO	YES
R^2	0.6529	0.6521	0.6488	0.5041
Station Fixed Effects	Yes	Yes	Yes	Yes
Number of Stations	8	8	8	8
N	8,361	8,361	8,361	8,361

Dependent variable is log daily API at monitoring stations inside the retricted area. 95% confidence intervals based on wild bootstrap (Cameron, Gelbach, and Miller, 2008) using 10,000 iterations to control for group clustering in brackets. * = 10% significance, ** = 5% significance, *** = 1% significance based on percentile p-values. All regressions include week-of-year dummies, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind direction dummies, interactions between wind speed and wind direction, and dummies for holidays, Olympics, weekends, interaction between weekends and OneDay, and days with pollutant missing. Interaction between near dummy and Olympics dummy included in Columns 2 and 4. Separate linear time trends are allowed for the regimes Before OddEven, During OddEven/Before Olympics, During Olympics, During OddEven/After Olympics, Break, and During OneDay and these are interacted with station fixed-effects in Columns 1 and 3 and interacted with "Near" in Columns 2 and 4. The number of observations is not evenly divisable by the number of stations due to missing values.

Table 6 RD Estimates using Log Hourly Television Viewership (2007 – 2009)

	(1)	(2)	(3)	(4)
	"Self-Employed"		"Hourly Workers"	
	Coeff.	Std. Err.	Coeff.	Std. Err.
OneDay69 Viewership Effects:				
Restricted Hours	0.0893 **	(0.0378)	-0.0329	(0.0286)
Non-Restricted Morning Hours	-0.2449 ***	(0.0610)	-0.1178 **	(0.0488)
Non-Restricted Evening Hours	-0.0489	(0.0353)	-0.0278	(0.0278)
OneDay78 Viewership Effects:				
Restricted Hours	0.1685 ***	(0.0385)	-0.0833 ***	(0.0287)
Non-Restricted Morning Hours	0.0062	(0.0557)	0.0589	(0.0430)
Non-Restricted Evening Hours	-0.0050	(0.0357)	0.0115	(0.0270)
Control Variables:				
Total Rainfall	-0.0011 **	(0.0005)	-0.0010 **	(0.0004)
Average Wind Speed	0.0123 *	(0.0064)	0.0075	(0.0050)
Sunshine	-0.0049 ***	(0.0014)	-0.0027 **	(0.0011)
Average Temperature	-0.0019	(0.0020)	-0.0030 **	(0.0015)
Weekend	0.0421 ***	(0.0158)	0.1261 ***	(0.0126)
Holiday	0.1055 ***	(0.0279)	0.2307 ***	(0.0239)
Olympics	0.4479 ***	(0.0518)	0.2698 ***	(0.0986)
OddEven	0.1263 **	(0.0534)	0.0850 *	(0.0487)
R^2	0.8357		0.9128	
N	26,303		26,303	

Dependent variable is log number of thousands of individuals watching television each hour. Standard errors clustered at the daily level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. Both regressions include hour-of-day dummies, month-of-year dummies, a dummy for the break period interacted with hour-of-day dummies, interactions between each of the three policy variables (OddEven, OneDay69, OneDay78) and weekend and holiday dummies, and separate 4th-order time trends for the regimes Before Oddeven, During OddEven/Before Olympics, During Olympics, During OddEven/After Olympics, Break, and During OneDay.