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

Abstract

We evaluate the environmental benefit and economic cost of Beijing's driving restrictions. Based on daily data from multiple monitoring stations, air pollution falls 20% during every-other-day and 9% during one-day-per-week restrictions. Based on hourly television viewership data, viewership during the restrictions increases by 8.7 to 12.8% 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. Causal effects are identified from both time-series and spatial variation in air quality and intra-day variation in viewership. 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.

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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 for given compensation. There is little empirical evidence of driving restrictions' effect on pollution and none about their effect on economic activity. We examine both effects 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, we find that 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 20% and one-day-a-week restrictions by 9%. Given that motor vehicles create roughly 50% of particulate matter in Beijing through emissions and road dust; this is consistent with strong compliance with the restrictions. 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 to eliminate other explanations besides cars for the pollution reduction. Pollution drops more at stations closer to a major road.[2] This means confounding factors are related to proximity to a major road and therefore traffic flow. We consider, and rule out, changes in gasoline prices, parking rates, number of taxis, emissions standards, and government-imposed working hours.

This DD approach can be used to evaluate any intra-city policy change that can be related to identifiable pollution sources. Station-specific policy effects can be

[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 or Class I Road.

correlated with distances to pollution sources such as factories, roads, airports, or subways to disentangle the impact of concurrent and overlapping policies that affect different pollution sources differently. The approach is generally applicable to cities that monitor air pollution since city-wide air quality measures are based on multiple monitoring locations to ensure representativeness. 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 changes in airport congestion in areas downwind and upwind of airports); and Hanna and Oliva (2011) (response to a factory closure based on distance to the erstwhile factory).

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 observed consumption of a major substitute – leisure time watching television (TV). Using viewership as a proxy biases against finding an effect. 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 viewership responses of workers with discretionary work time (self-employed) to those whose days worked and daily hours are fixed conditional on their remaining employed (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 ascertain the effect on days worked but also examine viewership outside the restricted hours to determine if work day length more than compensates for effects on days worked.

Using an RD design, viewership by self-employed workers increases by 8.7 to 12.8% (1.4 to 1.9% of all self-employed workers whether watching television or not) during the restricted hours of the one-day-a-week policy, consistent with a reduction in days worked and substitution to leisure in response to higher commute costs. Viewership changes only slightly outside the restricted hours ruling out the possibility that longer daily work hours more than compensate for the fewer work days. While we cannot say with certainty that output is reduced as a result, for this not to be so would require increased efficiency during the fewer remaining work hours.

[3] TV viewing on mobile devices is extremely limited during our sample period.

Hourly employee viewership slightly decreases during restricted hours consistent with these workers 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 also 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. Besides providing evidence on the restrictions' economic cost, the viewership results further corroborate our pollution results. They preclude confounding factors that decrease both public transit and auto commute times, such as expanded subway capacity, because these would not decrease work days for those with discretionary work time.

Using back-of-the-envelope calculations, we estimate the annual benefits from reduced morbidity and fewer reduced activity days due to the OneDay driving restrictions to be around RMB 1.1 to 1.4 billion while the cost of reduced output is about RMB 0.51 to 0.72 billion.

The only other detailed economic analysis of driving restrictions is Davis (2008),[4] who finds no discernible effect on several pollutants (not including particulate matter) from a similar policy in Mexico City.[5] Our work differs in three key respects. First, we use geographic in addition to time-series variation in pollution measures 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, detailed compliance evidence. In the absence of publicly-available violations data, we gathered data from a centrally-located Beijing parking garage. All Beijing parking garages are required to record the time and license plate numbers of all entering cars but are not required to report violators of the driving restrictions. Using this minute-by-minute data, we find high compliance.

Chen, *et. al.* (2011) examine which, if any, of the policies implemented during and shortly after the Olympics had an effect on Beijing pollution. Their 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 estimates whether the policies discontinuously change aerosol

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[4] Policy papers examining driving restrictions include Osakwe (2010); Cropper, *et. al.* (2010); and Cambridge Systematics, Inc. (2007).

[5] Salas (2010) finds that the Davis (2008) results are sensitive to assumptions about time window and time trend. Eskeland and Feyzioglu (1995) 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.

optimal depth (AOD) – a satellite measure of atmospheric particulates –differently for areas with different road densities. AOD drops more precipitously in high- than in low-density areas during the every-other-day policy consistent with them having an effect.[6] The other DD approach uses nearby cities as a control group and estimates whether Beijing's air pollution index (API) changed differently during the every-other-day policy. This produces a similar estimate to ours – a 17% reduction.[7]

Our paper differs from theirs in two respects. First, our approach more conclusively identifies driving restrictions as the cause of the pollution decrease. 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 (accurate within about 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% reduction in the API during the every-other-day restrictions and no drop in the API during the one-day-per week restrictions.[8] This difference may result from their using an RD design with a single time trend throughout the sample period.

Our study also 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 to their study, 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.

[6] The paper does not explicitly test for the effects of the one-day-per-week 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 one-day-per-week 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.
[7] This is from the specification closest to ours (Column 4 of Table 11).
[8] This is from the specification closest to ours (Columns 2 to 4 of Table 14b).

2. Pollution-Relevant Policies

Air pollution and its health implications are a major concern in Beijing, which was ranked in 2004 as the thirteenth "most polluted city" in the world for suspended particulates.[9] The economic cost of suspended particulates to China is estimated at $22.4 billion in 2005 (in 1997 USD) (Matus, *et al.*, 2012). Although a particularly acute problem in developing economies (see Greenstone and Hanna, 2011), particulate matter is a major concern in cities worldwide (see Watkiss, Pye, and Holland (2005) for European Union evidence). Particulate matter is linked to cardiopulmonary diseases, respiratory infections, and lung cancer (EPA, 2004), and is found to increase infant mortality (Chay and Greenstone, 2003). Other types of 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 $\mu g/m^3$) 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.[10] Jiang (2006) finds that approximately 53% of Beijing's PM_{10} is attributable to motor vehicles – 23% due to emissions and 30% due to road dust.[11] 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.[12]

The driving restrictions began on July 20, 2008 with an odd-even ("OddEven") policy restricting cars to drive only every-other-day. The OddEven 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"). The OneDay 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 apply between 7:00 a.m. and 8:00 p.m. and remained unchanged beyond our sample period. We call this period "OneDay78" and use "OneDay" to apply to the combined OneDay69 and OneDay78 periods.

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[9] "Beijing Pollution: Facts and Figures," *BBC News*, August 11, 2008 based on 2004 World Bank data.
[10] Some governments measure $PM_{2.5}$, which includes only smaller particulates (below 2.5 μm) and does not capture road dust.
[11] 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}.
[12] 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."

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 dates. 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 small in number.[13]

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. Although the government may have had other goals for some of these policies (*e.g.*, reduced congestion or easier commutes), they all may affect air pollution.[14] Factory closures and migrant worker relocation were coincident with the Olympic Games and we include a dummy variable in our estimates to capture that period. For the other policies – bus and subway fare reductions and subway openings – we address in a variety of ways. In our RD estimates, we include flexible time trends to control for these policies and perform robustness checks to see whether these trends are sufficiently flexible to capture them. We also estimate the effect of the driving restrictions using small windows around the beginning of the driving restrictions (bus and subway fare reductions and Subway Line 4 and 5 openings are not within these windows). Finally, for those policies beginning close to the time of the driving restrictions (Subway Line 8, 10, and Airport openings), our DD results showing

[13] 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.

[14] Air travel also likely changed during this period but aircrafts produce only small amounts of PM_{10}. According to "Aviation & Emissions: A Primer" (Federal Aviation Administration Office of Environment and Energy, January 2005, page 1) particulate matter is less than 1% of aircraft engine emissions. Also, since particulate matter dissipates within a few kilometers, the small amount of PM_{10} that would be measurable by ground sensors would be produced during takeoff and landing near the Beijing airport and the airport is 10.5 kilometers from the nearest station in our sample.

viewership increasing for workers with discretionary work time but not for those without are inconsistent with an expansion of public transit.

3. Theoretical Background

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 here and discuss its main results but refer the reader 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 work times. Since most Beijing workers with 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. Within each group there is 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 over their time) considering its effect on their labor-leisure choice. Each worker's 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 one commute mode over another even if it requires more time and greater monetary cost. Examples are expending effort to commute, 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 types of days are identical. With the policy, workers suffer a penalty for driving on restricted days. The model assumes perfect compliance and that workers do not purchase a second car to comply with the restrictions; any presence of these in the empirical data would bias against finding an effect.[17] The model considers only short-run effects and therefore ignores changes in workforce

[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.
[16] After our sample period (beginning April 12, 2010) official working hours became 9 a.m. to 6 p.m.
[17] Eskeland and Feyzioglu (1995) model the latter effect. Due to the integer nature of car purchases, some households are on the margin between zero and one car while others are on the margin 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.

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 changes in congestion. Driving restrictions affect work time on both an extensive margin (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 (and therefore TV viewership) 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.

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 these first-order effects. 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 our model does not consider non-work driving and assumes all days are work days, there is no possibility of inter-temporal substitution. In a more general model, workers may drive more on their non-restricted day because they cannot on the

[18] Gibbons and Machin (2006) discuss the theoretical effect of increased commute costs on the labor participation margin. Black, Kolesnikova, and Taylor (2010) 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.

restricted day.[19] This will attenuate the pollution reduction effects 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. As a result, daily leisure time changes depending on how public transit commute times and costs compare to those by car. Those who prefer driving and 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 an aggregate and individual monitoring-station level. The second is an hourly measure of TV viewership by different categories of Beijing residents. 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

[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.
[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.

between OddEven and OneDay, 182 during OneDay69, and 265 during OneDay78. This provides a fairly symmetric window – approximately 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: Our pollution measure is the daily Beijing Air Pollution Index (API) published by the State Environmental Protection Agency and Beijing Environmental Protection Bureau.[22] The API provides specific advice on behavior (*e.g.*, not exercising or spending time outdoors) and ranges from 0 to 500 with higher values indicating stronger pollution concentrations and more harmful effects (EPA, 2009). Its value depends on concentrations of 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. To compare the relative severity of the three pollutants, the concentration of each is rescaled before choosing the maximum. 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 reported as the maximal pollutant on 917 of the 953 days with an API above 50.

The aggregate API is based on API measures at multiple monitoring stations around Beijing. Station composition varied slightly over time. In 2007 the aggregate API is based on 28 stations. Five stations are dropped and four added for a net total of 27 stations in 2008 and 2009. Figure 1 shows the 2008 through 2009 locations. The station-level API ranges from 6 to 500 and averages 90. As with the aggregate API, 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 component of leisure and therefore a big substitute for

[22] Our description of the pollution data is based on Andrews (2008). Chen, *et. al.* (2011) provide evidence on the accuracy of the aggregate Beijing API using independent satellite data.

work time.[23] Our viewership measure is CSM Media Research's "Television Audience Measurement" (TAM) database, the most comprehensive TV ratings data in China. 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 the data is individual. 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 currently 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 a small part of the outside suburban area.

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. The work time of an "hourly worker" could vary at their employer's discretion but only in the upward direction with overtime pay.[24] The two categories we use comprise over 72% of Beijing's non-government workers. CSM conducts an establishment survey estimating 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 there are an average of 91 thousand "self-employed" and 149 thousand "hourly workers" watching TV although the number varies greatly across hours.

Control Variables: Our pollution regressions include a variety of daily weather variables known to affect particulate matter (see 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

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[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 have control over their work time.
[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 of all these.

(Wiedensohler, *et al.*, 2007). To control for this flexibly, we use dummies for the four directional quadrants and interact these with the four wind speeds. We include the daily hours of sunshine to control for the amount of atmospheric solar radiation, which creates ozone and more particulate matter.

Humidity can interact with pollutants to create secondary ones so we include daily average humidity. Precipitation has opposing effects. Rain can interact with existing pollutants to create secondary ones, but can also wash particles from the air and minimize their formation. To control for either possibility, we include total daily rainfall. 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 measures of daily weather variables that might affect the desire to remain indoors watching TV. These include total rainfall, average wind speed, total hours of sunshine, 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 the aggregate API. Intuitively, our test determines if any pre-existing time trend in pollution is altered during the 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. Geographic variation allows us to relate the policy impact to each station's distance from the closest major road. The restrictions cause the local API to drop more at stations closer to a major road than at those further away and the effects dissipate at a distance consistent with PM_{10}'s atmospheric behavior.

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

$$(1) \qquad \log\left(API_t^A\right) = \alpha + \sum_{i=1}^{11} \beta_{2i} m_{t\in i} + \beta_3 WE_t + \beta_4 HO_t + \beta_5 BR_t + \sum_{k=1}^{K} \beta_{6k} Z_{tk} + Policy_t + f\left(t\right) + \varepsilon_t^A.$$

API_t^A is the aggregate API on day t, m_t are month-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. β_3 captures any differences in pollution on weekends, β_4 during holidays, and β_5 during the break relative to the period before the driving restrictions begin. Besides weather controls, Z_t includes a dummy for the Olympic Games period and dummies to distinguish days when the API is below 50 and we do not observe the maximal pollutant and when sulfur dioxide is the worst pollutant. It is important to control for the last because automobiles are not a significant contributor to sulfur dioxide emissions.[26]

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 underlying our RD estimation is that, conditional on the covariates, unobserved factors affecting the API are uncorrelated with time. If they were, 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 the long-term trend of the API. To control for this we include an L^{th}-order time trend within each of the regimes: "Before OddEven," the "Break" between the two policies, "During OneDay69," and "During OneDay78" (we attempted to include a separate time trend for the "During OddEven" regime but it was not possible to separately identify the OddEven policy dummy from it because the correlation with even a linear trend is 0.860 with a significance level below 0.01%):[27]

(3) $$f(t) = \sum_{l=1}^{L} \Big[\gamma_{1l} \mathrm{I}_{t<\bar{t}_{OE}} \left(t-\bar{t}_{OE}\right)^l + \gamma_{2l} \mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OD69}} \left(t-\bar{t}_{BR}\right)^l + \gamma_{3l} \mathrm{I}_{t>\bar{t}_{OD69}} \left(t-\bar{t}_{OD69}\right)^l + \gamma_{4l} \mathrm{I}_{t>\bar{t}_{OD78}} \left(t-\bar{t}_{OD78}\right)^l \Big],$$

where I is an indicator variable for the statement being true, $\bar{t}_{OE}$ is the first day of the OddEven policy, $\bar{t}_{BR}$ of the break period, $\bar{t}_{OD69}$ of the OneDay69 policy, and $\bar{t}_{OD78}$ of the OneDay78 policy. This allows for different time trends during the pre-treatment and policy regimes as suggested in Angrist and Pischke (2009). Formally, identification of the RD effect requires:

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

[26] According to EPA (2000, p. 2-2) on-road sources are less than two percent.
[27] The same issue arises with the short "Break" regime but we include a time trend for it since we wish to control for it but do not need to perform hypotheses tests on it.

This requires that we have correctly specified the function of time. While our specification is fairly flexible, this assumption 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 various robustness checks below.

Column 1 of Table 2 shows a regression with no time trend $(L = 0)$. In Columns 2 and 3, we introduce linear $(L = 1)$ and quadratic $(L = 2)$ time trends for each of the four regimes.[28] 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 aggregate API regressions.[29] Using a Bayesian Information Criterion of model selection the no time-trend model ranks the best, followed by the linear time-trend model, and finally the quadratic time-trend model.[30] The main difference from including a time trend is that the OneDay coefficient is larger because it is highly correlated with the "During OneDay" time trend. This highlights the importance of our station-level and viewership evidence presented later which do not rely exclusively on time-series variation. To be conservative and because of the model selection test, we focus on the no time-trend results as our baseline model.[31]

Both policy variables are highly statistically significant and show a decrease in pollution during the restricted periods. The aggregate API was 20.4% lower during the OddEven restrictions with a 95% confidence interval of 11.3 to 29.4%. 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).[32] The aggregate API was 9.3% lower with a 95% confidence interval

[28] We include only up to a linear time trend during the Break regime because its short duration creates near collinearities beyond this.
[29] 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.
[30] 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.
[31] We also re-estimated the regression in Column 1 distinguishing the OneDay69 and OneDay78 policies. The OddEven coefficient was very similar and the two OneDay coefficients were both statistically indistinguishable from the single OneDay coefficient in Column 1.
[32] Substitution effects are likely small since the restrictions applied except from midnight to 3:00 a.m. Pollution rises convexly with car density because congestion causes cars to 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 required because the OneDay restrictions do not apply in the late evening and early morning hours.

of 4.0 to 14.6% during the OneDay policy. We would expect about a 10% decline (traffic reduced by 20% each day and 50% of PM_{10} created by motor vehicles). These estimates are consistent with high compliance. The API does not significantly differ during the Break regime so that it is the same entering the OddEven period as it is exiting. The API is significantly lower on the weekends but there is no significant substitution to weekends during OneDay relative to weekends absent the policy.

As expected, the API is significantly lower on days when the API less than 50. It is also lower on days when SO_2 is the predominant pollutant although we have no prior expectation on this. The API is not significantly different during the Olympics or on holidays consistent with many days with API less than 50 occurring during these days.[33] A one-degree temperature increase is associated with a 4.0% increase in the API – consistent with greater ozone and secondary pollution creation. A one-percent increase in humidity increases the API by 0.4%, consistent with humidity creating secondary particulates. Rainfall has no significant effect, but each additional hour of sunshine decreases the API by 3.0%.

Column 4 is a partial check of the ramifications of omitting a separate time trend for the "During OddEven" regime. We estimate using the whole sample but assume that OddEven is the only policy intervention. This allows us to include separate quadratic time trends before and after the policy. The policy effect is similar to the baseline results. Column 5 uses logarithm of PM_{10} as the dependent variable using the transformation in Appendix F to convert from the API. 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. Given that the data is not contiguous, we use standard errors clustered in rolling two-day blocks rather than Newey-West. We do not include a time trend for comparison to the baseline results. The OneDay effects are very similar and the OddEven effects are larger but not significantly different than the baseline results.

An identifying assumption of our RD estimates is that our time trend is sufficiently flexible to control for the unobserved factors that they are uncorrelated with time. We perform two tests of this assumption. First, we test for discontinuities at the median of the two subsamples on either side of the policy as recommended by Imbens and

[33] 59% of Olympics days have API less than 50 and 28% of holidays during the driving restrictions have API less than 50 or $S0_2$ as the predominant pollutant.

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. The results are shown in Online Appendix G: “Pre-OddEven” in the top and “Post-OneDay” in the bottom panel. Neither panel reflects significant discontinuities with no, linear, or quadratic time trends suggesting that any of these is sufficiently flexible to capture trends due to unobserved factors.

Second, we check our results using “discontinuity samples” – small windows of observations around the cutoffs (Angrist and Levy, 1999). 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 for a thirty-day window around the OddEven policy employing the same control variables as in our full-sample regressions along with different time trends on each side of the policy. Because the time trends are not highly collinear with the policy dummies over these small windows we are able to employ higher-order trends.[34] For third- through fifth-order time trends (Columns 1 to 3), the policy effect is significant and within 1.0 standard deviation of our full-sample estimate of 20.4%. Columns 4 and 5 maintain the 5th-order time trend but widen the window to 45 and 60 days. The estimates are larger but within 1.6 standard deviations of the full-sample estimates.

Column 1 in the bottom panel estimates the OneDay effect in a twenty-day window (the longest possible to avoid overlapping with the OddEven policy) using a quadratic trend. The point estimate is significant and large although within 1.7 standard deviations of our full-sample estimate. Columns 2 to 4 in the bottom panel show estimates in thirty-day windows around the OddEven and OneDay policies together. The OddEven coefficient is only marginally significant (at the 10.5% level) with linear time trends but is significant using quadratic or cubic trends. The point estimates are all within 0.8 standard deviation of those using the full sample. The OneDay estimates are significant with linear and cubic time trends and at the 10.4% level using quadratic. The coefficients are all within 0.9 standard deviation of the full-sample estimate.

Although the API’s high volatility (coefficient of variation of 0.55) makes it difficult to identify the effect of a sharp discontinuity visually, it is useful to examine

[34] The API also exhibits more persistent serial autocorrelation over these small windows. We control for this by clustering standard errors in rolling blocks of days as in Davis (2008).

discontinuity samples graphically.[35] Figure 3 shows the residuals and a fitted cubic time trend for a thirty-day window around the beginning of the OddEven policy. The residuals appear to oscillate in short cycles. Although the residuals are quite volatile even after controlling for weather the plot shows a discontinuous drop on the first day of the policy period and, with the exception of three outliers, generally lower residuals after the policy than before. Figure 4 shows a similar plot for a twenty-day window around the beginning of the OneDay policy using a quadratic time trend. It shows a secular upward trend in the data during this period along with a discontinuous drop at the threshold.

Effect on Station-Level Pollution: The RD results depend only on time-series variation and therefore could be confounded by contemporaneous factors. To supplement this, we use geographic variation in the location of individual monitoring stations and apply a DD test.[36] These regressions test whether pollution decreased more for monitoring stations located closer to major roads than for those further away in response to the policies:

$$\log\left(API_{st}^{S}\right) = \alpha_s + \sum_{i=1}^{11}\beta_{2i} m_{t\in i} + \beta_3 WE_t + \beta_4 HO_t + \beta_5 BR_t + \beta_6 OE_t + \beta_7 OD_t + \beta_8 OD_t * WE_t + \sum_{k=1}^{K}\beta_{9k} Z_{stk} + DD_{st} + f_s\left(t\right) + \varepsilon_{st}^{S}, \tag{5}$$

where API_{st}^{S} is the daily API at station s on day t. As before, we include month-of-year dummies to capture seasonality, weekend and holiday dummies to allow for differential effects, and dummies during the break between the two policies. The control variables $\left(Z_{st}\right)$ include the same daily weather controls as before as well as station-level indicators for days when the API is below 50 or sulfur dioxide is the worst pollutant. In addition to controlling for these two factors that affect stations differently, the control variables improve precision of the estimates. Of primary interest is the treatment effect $\left(DD_{st}\right)$ which captures the effect of the driving restrictions as a function of distance $\left(Dist_s\right)$ between each station and the nearest major road. We use robust standard errors clustered at the station level to allow for general autocorrelation within stations and heteroskedasticity.

[35] Rockoff and Turner (2010) also rely on RD econometric analysis because visually their volatile data exhibits no discontinuity due to the policies (Figures 4 and 5).

[36] 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.

We include station-level fixed effects (α_s) that control for time-constant unobserved factors that cause some stations to have higher pollution levels. This includes nearby stationary pollution sources as well as the baseline effect of distance to a major road. These fixed effects prevent the estimate of 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 stations further away. The BR, OE, OD, and $OD*WE$ terms prevent the estimate of 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.

The identifying assumption for our DD estimation is that, conditional on the other 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. This might be the case if, for example, traffic patterns changed differently over time on major roads relative to smaller roads. To control for factors that affect closer and more distant stations differentially over time we include separate, station-specific time trends for the three regimes: "Before OddEven," "Break," and "During OneDay:"[37]

(6) $$f_s(t) = \sum_{s=1}^{S}\sum_{l=1}^{L}\left[\gamma_{1sl}\mathrm{I}_{t<\bar{t}_{OE}}\left(t-\bar{t}_{OE}\right)^l + \gamma_{2sl}\mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OD69}}\left(t-\bar{t}_{BR}\right)^l + \gamma_{3sl}\mathrm{I}_{t>\bar{t}_{OD69}}\left(t-\bar{t}_{OD69}\right)^l\right].$$

Formally, identification of the treatment effect requires:

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

While this specification is fairly flexible, it 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 on this below.

Before presenting our DD results we perform a few RD estimates $(DD_{st}=0)$ using the station-level data.[38] Column 1 of Table 4 uses a panel of 24 stations, 22 of which

[37] As with our RD estimates we are unable to include a separate time trend for the "During OddEven" regime because it is highly collinear with the policy dummy itself. We also consolidate the "During OneDay69" and "During OneDay78" regimes into a single regime for our DD estimates because otherwise they introduce near collinearities.
[38] We are able to include separate time trends for the "During OneDay69" and "During OneDay78" regimes in these regressions.

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 time period and are located within the restricted area which adds identifying variation to our DD estimates below. Results are similar to those at the aggregate level. The OddEven policy reduces the API by 12.5% and the OneDay policy by 16.0%. 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 very similar.

Columns 3 and 4 provide separate RD estimates for stations inside and outside the restricted area. The restricted area includes the area 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 the roads there act as a complement or substitute to those inside. The restrictions decrease traffic if, absent the restrictions, it primarily feeds into the area within the 5th Ring Road. Traffic increases if drivers use these roads more intensively to travel from one side of the city to the other while complying with the restrictions.[39] Since no stations are located on the 5th Ring itself, Column 3 includes the eight stations inside the 5th Ring Road and Column 4 the 16 stations outside of it. The results in Columns 3 and 4 are similar 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).[40] 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. 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

[39] This ambiguity also rules out using monitoring stations outside the 5th Ring Road as a control group for those inside in a DD specification.

[40] 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.

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 specific to each regime and allows for differential effects of the policies on "near" and "far" stations (defined by median distance) inside the restricted area:

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

where $\overline{Dist}$ is the median distance to the nearest Ring Road across all eight stations. Column 1 of Table 5 shows the results. The OddEven policy reduces pollution by 14.0% at "far" stations but the effect is significantly greater at "near" stations – a drop of 19.8%. For the OneDay policy, pollution drops 11.6% at the "far" stations and 14.1% at the "near" stations. The estimates for OddEven and OneDay in Column 3 of Table 4 lie between the "near" and "far" effects as expected.

The specification in Column 1 allows for different trends at different stations. To provide suggestive evidence as to whether the trends differ at "near" and "far" stations, Column 2 allows for separate time trends for "near" and "far" stations and that differ within each of the three regimes.[41] The policy effects are virtually identical to those in Column 1. Moreover, the "far" time trends are not statistically different than the "near" time trends in any of the three regimes or collectively (F-test fails to reject at 51% level). Although because of the API's high volatility we prefer relying on regression estimates, Figure 5 plots the residuals from the regression in Column 2 averaged by "near" and "far" stations in the 45 days around the start of the OddEven policy along with the fitted time trends. Prior to the policy the residual averages for "near" and "far" track each other closely (correlation of 0.966 with a significance level below 0.01%) consistent with the unobservables trending similarly. The fitted time trends nearly coincide and show a gradual downward slope. Although the volatility of the data makes it difficult to see, the residual averages still track each other closely after the policy. The fitted time trends are relatively parallel but the "near" trend lies below the "far." Also, the residual averages coincide on the days immediately before the policy. Both drop on the first day of the policy but the "near" drops more than the "far" and with the exception of a few outliers the "near" residuals lie below the "far" after the policy.

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

The results in Column 3 of Table 5 maintain the separate time trends for "near" and "far" stations but substitute a quadratic distance function:

(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 46.2% at the Ring Roads during OddEven. Both distance terms are significant and the effect diminishes with distance to a drop of 19.3% at 1.1 kilometers. Pollution drops by 24.3% at the Ring Roads during OneDay. Again, both distance terms are significant and the effect diminishes to a drop of 13.0% at 1.0 kilometers. This is consistent with PM_{10}'s dispersion radius – most PM_{10} emissions are deposited within a few kilometers of their release according to EPA (2000, p. 2-3). Although the quadratic functional form implies that pollution increases beyond these distances this is mostly outside our sample (stations inside the restricted area are within 1.3 kilometers of a Ring Road). The "near" and "far" time trends are not statistically different in this specification either.

In theory, our DD test could be applied using any size road. In practice, there is a tradeoff. With smaller roads, the average distance to the nearest road shrinks 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. Although less trafficked than Ring Roads these are still high-volume roads. Using the same specification as in Column 2, the OddEven policy reduces pollution by 19.3% at "far" stations but the effect is significantly greater at "near" stations – a drop of 24.7%. For the OD policy, pollution drops 12.5% at the "far" stations and 16.9% at the "near" stations. The time trend coefficients also confirm that the "far" and "near" stations experience similar trends before and after the policies (F-test fails to reject at 73% level). Although the magnitudes are not directly comparable – the median distance is lower for Class I than Ring Roads but emissions output is also lower – the policy effects are in the same general range except that the OddEven baseline effect is larger.[42]

[42] 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 (22.0% and 15.0%) we did not find significant differential effects for "near" and "far" stations. This is possibly because the volume of traffic on these roads is not sufficient to identify an effect given the API's high volatility.

Table 6 shows the effect of the OddEven policy on stations inside the restricted area and the differential effect on "near" stations in discontinuity samples. The requirements for identification are less demanding in these small windows – it requires only that no confounding factors change discontinuously at the cutoff differently for the "near" and "far" stations. The effects are highly significant with either a 45- and 60-day window showing declines of 12% and 19% for "far" stations and declines of 16% and 23% for "near" stations.

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 the OneDay policy (9%). 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 0.96 per gallon (16.1%) would achieve the same pollution reduction if pollution falls linearly with gas usage.[43]

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 estimate is about three times greater than ones using U.S. data, 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,[44] a license fee increase of USD 147 to 450 (RMB 965 to 2,961) would lead to a 9% pollution reduction. This compares to the current RMB 500 (USD 76) registration price in Beijing.[45]

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 but also interact the fixed effects with the policy variable. The coefficients on the interaction terms provide the station-specific changes due to the

[43] Auffhammer and Kellogg (2011) find that precisely-targeted, inflexible regulation of gasoline elements most prone to form ozone is effective in reducing ozone pollution.
[44] Unless otherwise noted, all exchange rate conversions performed at January 2011 rates (1 RMB = 0.152 USD). Most 2009 car purchasers targeted a car price 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 yields USD 15.2 thousand.
[45] See "Beijing's Plan to Steer Clear of Traffic Jams," *China Daily*, December 14, 2010.

policy. These station-specific changes should be correlated with the distance to pollution sources known to be 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 0.822 with a significance level of 1.2% while the correlations with distance to the airport and nearest subway line are insignificant (at the 58% and 99% levels).

This implies that any confounding factors are related to proximity to a major road 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 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 our baseline aggregate API regression produces very similar results.[46]

Regulated parking rates at public garages did not change during our sample period.[47] 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 under a decision by the Beijing Council of Transportation as part of the "Tenth Five-Year Plan."[48] 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 and changed once during our sample period. From the beginning of our sample through

[46] The price coefficient was insignificant in the regression. Price data taken from NDRC documents at the Beijing Development and Reform Council website (http://www.bjpc.gov.cn).
[47] 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).
[48] According to *Beijing Statistic Yearbook* (2007, 2008, 2009), China Statistics Press.

February 28, 2008 autos registered in Beijing had to conform to the Level III standard. From March 1, 2008 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 generally coincide with the OddEven and OneDay policies and our correlations above show no significant correlation between station-specific effects and distance to subway; however some of the effect that we measure 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.

The top panel of Online Appendix H, Columns 1 to 3 adds policy dummies for these to our RD specification. The OddEven and OneDay effects remain similar to the baseline results and the subway and bus policy effects are close to zero and very insignificant. This is suggestive evidence that the RD estimates do not confound the subway and bus policies with the driving restriction effects. 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 these policies occurring gradually enough that our time trends control for them. Columns 4 and 5 of the top panel implement our DD specification for the subway and bus policies while controlling for the driving restriction policies. Columns 4 and 5 of the bottom panel do the same not controlling for the driving restrictions. We find no significant evidence of monitoring stations closer to Ring Roads being differentially affected during the two policies. 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 on viewership for workers 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 additional 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 – an implication we test.

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 during the policy periods relative to any pre-existing time trend conditional on control variables. We then use a DD design to see if the policy change affects the two groups differently.

Since most workers' regular work hours 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 the restricted period, they will certainly affect viewership inside the restricted hours. 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 the daily restricted period. Given the less-than-perfect correspondence between regular work and restricted hours and since theory is ambiguous about the intensive margin effects (see 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

$$\begin{aligned}\log\left(View_{th}^{c}\right) &= \beta_1 \log\left(View_{t,h-1}^{c}\right) + \sum_{i=1}^{24}\beta_{2i}\alpha_{i=h} + \sum_{i=1}^{23}\beta_{3i}BR_t\alpha_{i=h} + \sum_{j=1}^{11}\beta_{4j}m_{t\in j} + \\ &\beta_5 WE_t + \beta_6 HO_t + \beta_7 OE_t + \left(\beta_8 OE_t + \beta_9 OD69_t + \beta_{10} OD78_t\right) * WE_t + \\ &\left(\beta_{11} OE_t + \beta_{12} OD69_t + \beta_{13} OD78_t\right) * HO_t + Policy_{th} + \sum_{k=1}^{K}\beta_{14k}Z_{tk} + g\left(t\right) + \varepsilon_{th}^{c}.\end{aligned} \tag{10}$$

$View^{c}_{th}$ is thousands of people watching TV on day t during hour h for worker category c ("self-employed" and "hourly workers"). We include lagged hourly viewership since viewing is persistent across programs (Goettler and Shachar, 2001). This hourly dependency is distinct from the daily time trend. 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_2) and during the break period (β_3). We include month-of-year dummies to capture seasonality in outdoor activity. β_5 and β_6 capture differences in weekend and holiday viewership (due to programming differences or differential appeal of outdoor options) before the policy and β_7 captures change in viewership during the OddEven policy. β_{8-10} capture difference in viewership on weekends during the different policy regimes while β_{11-13} do the same for holidays. Besides weather controls, Z_t includes a dummy for the Olympic Games period since programming differed greatly then. We cluster standard errors by day to capture intra-day correlation among the hourly unobservables.[49]

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:

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

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" $(\theta_{1-2} > 0)$ and zero for "hourly workers" $(\theta_{1-2} = 0)$. θ_{3-6} capture intensive margin effects and theory is ambiguous about these.

Identification of our RD estimates again requires controlling for unobservables affecting viewership to ensure they are uncorrelated with the error. To do so we

[49] 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 estimated standard errors are slightly larger but it does not have a significant effect on which coefficients are significant at the 10%, 5% and 1% level.

include separate daily time trends for the regimes “Before OddEven,” “Break,” and “During OneDay:”

(12) $$g(t)=\sum_{l=1}^{L}\left[\gamma_{1l}\mathrm{I}_{t<\bar{t}_{OE}}\left(t-\bar{t}_{OE}\right)^{l}+\gamma_{2l}\mathrm{I}_{\bar{t}_{OE}<t<\bar{t}_{OD69}}\left(t-\bar{t}_{BR}\right)^{l}+\gamma_{3l}\mathrm{I}_{t>\bar{t}_{OD69}}\left(t-\bar{t}_{OD69}\right)^{l}\right].$$

Viewership by Workers with Discretionary Work Time: Columns 1 and 2 of Table 7 display the “self-employed” results using a fourth-order time trend $(L=4)$ – a choice justified below. Viewership is persistent with 55% of viewers watching from the previous hour. Greater rainfall has a statistically significant but negligible effect. More sunlight hours are associated with less viewership. “Self-employed” watch more TV on weekends, holidays, and during the Olympics relative to weekdays before the restrictions began. Viewership is not differentially affected during the OddEven policy although holidays during that period have lower relative viewership. We do not have specific predictions for the OddEven period because the Olympics greatly altered regular work and leisure patterns.

Relative to weekdays before the restrictions began, viewership during the OneDay69 restricted hours is 8.7% higher with a t-statistic of 4.3 and 12.8% higher during the OneDay78 restricted hours with a t-statistic of 5.9. On the extensive margin, workers with discretionary labor supply work less and enjoy more leisure in the restricted periods. This is consistent with marginal workers who normally drive finding it too costly to do so on their restricted days. On average, there are 102.1 thousand “self-employed” viewers during the restricted hours of the OneDay69 policy, implying an increase of 8.9 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.[50] During the OneDay78 restricted hours there are an average of 98.1 thousand viewers so our estimates imply an increase of 12.5 thousand additional “self-employed” viewers or 1.9% 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 longer hours on non-restricted days; therefore, it is important to check whether intensive margin changes undo some or all of the extensive margin effects. During the

[50] 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.

OneDay69 policy, viewership decreases during the morning hours. This could reflect a shift to an earlier commute to comply with the restrictions. During the OneDay78 policy, viewership increases in both the morning and evening hours. While not the only possibilities, this could reflect decreased auto congestion or a less-than-perfect correspondence between regular work hours and restricted hours (*i.e.*, regular work hours of "self-employed" would have exceeded the restricted hours had they not stayed home on their restricted day).

The intensive margin effects do not offset those on the extensive margin and the increased commute costs under the driving restrictions raise total viewership. The OneDay69 policy increases viewing by 123.4 thousand person-hours and the OneDay78 policy by 223.4 each restricted day.[51] Work time would decrease less than this if TV viewing became more attractive relative to other leisure during the policy periods. It is more likely that we understate the effects because lower 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 7 display the results for "hourly workers" again using a fourth-order time trend. Consistent with predictions for the extensive margin (Implication 1), viewership is unaffected during the restricted hours of the OneDay69 policy relative to weekdays before the restrictions began. This is a "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 small (4.2% or 6.9 thousand-viewer) decrease during the restricted hours of the OneDay78 policy. This is consistent with these workers or their children experiencing fewer sick days from pollution. Hanna and Oliva (2011) find such an effect from sulfur dioxide pollution reduction after a Mexico City factory closure. The effects of the control variables are similar to those for "self-employed" except that viewership is less persistent, is significantly lower on warmer days, and displays a greater differential on weekends and holidays relative to weekdays prior to the restrictions consistent with "hourly workers" having less discretion over when they work.

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

[51] For OneDay69 this equals 8.9 thousand additional viewers for 15 restricted hours less 1.7 thousand viewers for 6 morning hours. For OneDay78 this equals the sum of 12.5 thousand additional viewers for 13 restricted hours, 10.0 thousand for 4 evening hours, and 2.9 thousand for 7 morning hours.

on whether public transit takes more or less time than car commuting. Viewership is unaffected during OneDay69 non-restricted hours. For OneDay78, viewership increases 7.4% in the morning. Although this is a large percentage increase, because of the small viewership in the morning hours it represents only 1.8 thousand additional viewers per hour.

Robustness and Alternative Explanations: Appendix I shows the impact of the time trend on estimates of the policy effect during restricted hours. The top panel shows that for "self-employed," the coefficients on both the OneDay69 and OneDay78 interactions are positive and highly statistically significant. The time trend affects the magnitude of the coefficients but they are reasonably stable.[52] In contrast, the bottom panel shows that the "hourly worker"-OneDay69 interaction coefficient is small, variable, and insignificant beginning with the second-order time trend. The OneDay78 interaction is small, highly variable, and insignificant above a fourth-order time trend.

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 24 hourly dummies. The results confirm our main estimates. Appendix J, Panel A plots the coefficients on the interaction terms between OneDay69 and the 24 hourly dummies for the "self-employed" category. The magnitudes of the coefficients are plotted on the y-axis only if significant at the 10% level or better. Relative to weekdays before the restrictions began, viewership is higher during eleven of the fifteen restricted hours and all ten of these are significant at the 2% level or better.[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 provides the same graph for the "hourly workers" category. The results again confirm our main estimates. Viewership is largely unaffected during the restricted period with only five of the thirteen hours showing an increase. There is also a decrease in the first hour of the restrictions (6:00 – 7:00 a.m.) similar to that for "self-

[52] More than a fourth-order time trend created collinearities between the "Break" time trend and control variables so we limit it to a maximum of fourth-order. More than a sixth-order time trend also created collinearities between the "During OddEven" time trends and other variables.

[53] The four significant effects in the early morning hours are large in percentage but small in absolute terms. The average decrease from midnight to 4:00 a.m. is 3.5 thousand viewers per hour. The effect on absolute viewership is much greater during the restricted hours. The average increase from 7:00 a.m. to 7:00 p.m. is 12.1 thousand viewers per hour. These magnitudes are similar to the average effects in the three time-segment model of Table 7.

employed” and consistent with some workers shifting their commute earlier. 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 and all are significant at the 1% level or better. For “hourly workers” viewership is not significantly different during any restricted hour.

Alternative explanations must be consistent with the differing policy effects for those with and without discretionary work time. This excludes 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 our pollution results to the extent its timing overlapped with the driving restrictions, it cannot explain our intra-day viewership results.[54] It is at odds with the “self-employed” increasing their viewership during restricted hours. Quicker auto and public transit commute times should stimulate daily labor supply. Also, shorter commute times should increase leisure time in non-restricted hours for both groups of workers (Appendix D shows this formally). It does so only during the OneDay78 policy and only for the “self-employed.”

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.

The primary benefits of the driving restrictions are reduced morbidity and mortality from lowering PM_{10} by 13.2 μg/m^3 (9% drop in an average level of 147 μg/m^3). To estimate these we rely on Matus, *et al.* (2012) which estimates pollution’s long-run effects in urban China. 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 PM_{10} exposure over a lifetime[56] so we ignore any benefits from this.

[54] The Subway Line 4 opening during the OneDay period provides an opportunity to test whether subways had a differential effect on pollution. We tried adding a policy dummy equal to one after the opening of Line 4 to the regression in Column 3 of Table 2. The coefficient was negative but not statistically significant. However, this is a low-powered test as Line 4 is a low-volume line.
[55] Medical costs are also estimated but these redistribute wealth from households to medical providers and do not affect welfare.
[56] See Equation (4) on page 60 of Matus, *et al.* (2012).

Restricted activity days occur when pollution exposure confines adults to bed because of shortness of breath from even slight exertion. Epidemiological data that relates pollution exposure to health outcomes implies 6.6 (5.8, 7.4)[57] million fewer restricted activity days annually due to the driving restrictions (Appendix K has detailed benefit calculations). Restricted activity days involve a loss of both work and leisure time. We follow Matus, *et al.* (2012) and assume that leisure is also valued at the wage rate. Beijing's average daily wage in 2007 was RMB 189,[58] implying a mean annual loss of RMB 1.24 (1.09, 1.39) billion.

Acute mortality is death resulting from current-year pollution exposure. Applying epidemiological data implies 477 (318, 637) fewer deaths annually due to the driving restrictions. Valuing the hastened mortality due to pollution is a controversial issue so we use both a lower-bound, human-capital approach 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 case of acute mortality as one-half year of foregone wages. This yields a gain from the driving restrictions of RMB 11 (8, 15) 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 70 (47, 94) 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 the leisure time they enjoy instead. Daily output per Beijing worker is about RMB 417.[59] Lacking a more precise figure, we value leisure time enjoyed by the "self-employed" 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 8.9 (12.5) thousand additional viewers each day during the restricted OneDay69 (OneDay78) periods.

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[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 K.

[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.

[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.

Given 250 work days per year, the driving restrictions cost RMB 509 (715) million annually. This could understate costs if "self-employed" generate greater profits than the average worker and because we do not estimate viewership changes for all worker categories. However, our estimates include 72% of non-government employees and most government employees have fixed work times.

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 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

The only other systematic economic evaluation of driving restrictions is Davis (2008), which examines a similar one-day-per-week driving restriction in Mexico City. The study finds no effect from the restrictions, even in the short run, 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 in Mexico City are probably less relevant in Beijing. Although auto ownership is increasing quickly in Beijing, 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 sharing cars 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 the reduced auto 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 million in 2008.[61] This implies a younger auto stock compared to more developed

[60] Beijing data from *The China Urban Statistics Yearbook 2008* and Mexico City data from http://mexico-city.co.tv/.
[61] Data from "Independent Environmental Assessment: Beijing 2008 Olympic Games," United Nations Environment Programme, February 2009 (p. 42).

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 and therefore cannot be imported easily 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 play a complementary role. 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: We find detailed evidence of high compliance in Beijing. 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 compliance 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 L provides more details on penalties and detection).

To test compliance, 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

[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).

might affect 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 contain 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 we do not find evidence in our pollution results, weekend driving may increase overall as drivers substitute from restricted weekdays. Even if this is so, we expect this to be uniform across plate numbers. Therefore, we use the weekend distribution of plate numbers as the expected distribution. We compare this expected distribution to that observed during weekday restricted and weekday non-restricted periods. We discuss the results for regular (hourly) parkers first.

Figure 6 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 observations 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 M 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

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

[64] Figure 6 does not control for the fact that plates "2" and "7" should not occur under perfect compliance. Our detailed analysis in Appendix M does so.

cars entered the garage with no license plate – likely a method for avoiding detection by camera – but 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. Drivers do not seem to compensate by driving more on non-restricted days. 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.[65]

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 this group containing mostly 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 was 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 analyses by ruling out coincident policies unrelated to driving. Since most cities that monitor air pollution collect data from multiple locations, our approach can be used elsewhere to improve identification of policy changes within a city center that can be linked to identifiable emissions locations. Because the approach allows precise distance measures, it can be used to disentangle the effects of different policies that affect separate but proximately

[65] We cannot test for substitution to weekends because we cannot measure activity "but for" the restrictions.

close areas. 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).

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 discretion over their work time increase their viewership during restricted driving 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. Since factors that reduce both auto and public transit congestion, such as expanded subway capacity, would increase work time for workers with discretion, we can also eliminate these as explaining the pollution reduction. Driving restrictions impact workers with discretion the most; these are often business owners and entrepreneurs and important sources of new jobs and innovations.

We consider only short-run effects. As incomes in China increase, demand for driving will increase and so will the number of cars.[66] 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.

Although we find that the restrictions' short-run benefits likely exceed its costs, they 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 increasing vehicle license fees or pricing congestion. 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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[66] Duranton and Turner (2009) provide empirical evidence that a fundamental law of auto congestion holds, in which a natural level of congestion is reached in the long run which equates driving demand and average cost of commuting as determined by road capacity.

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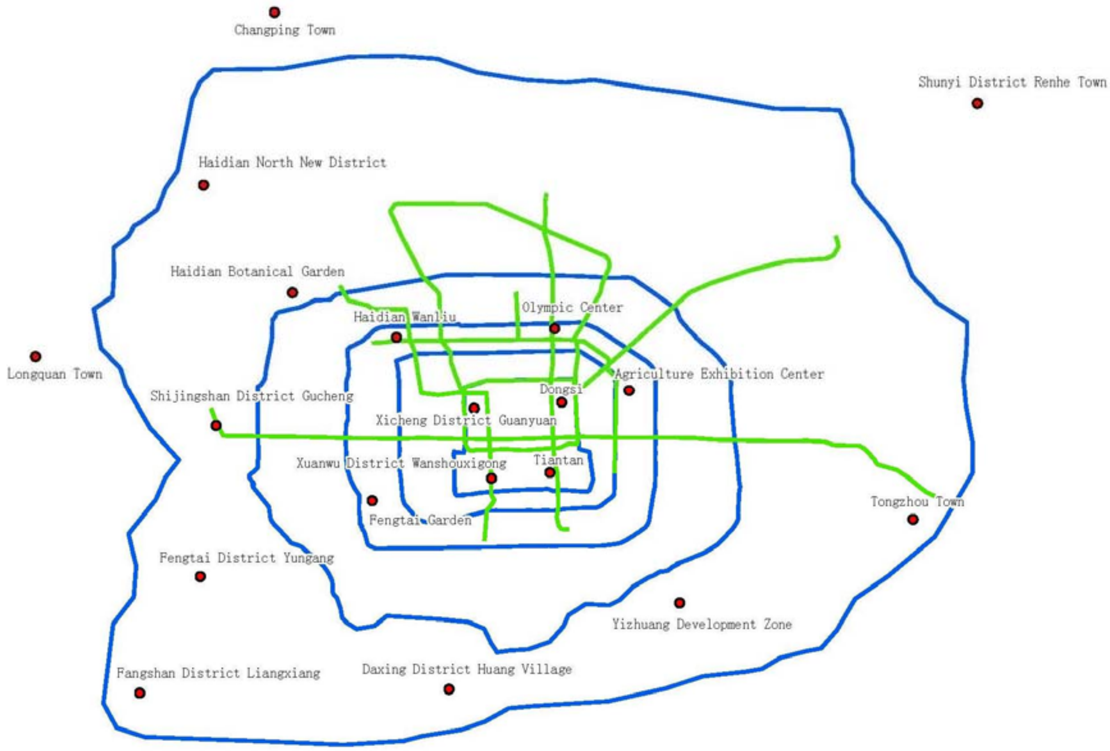
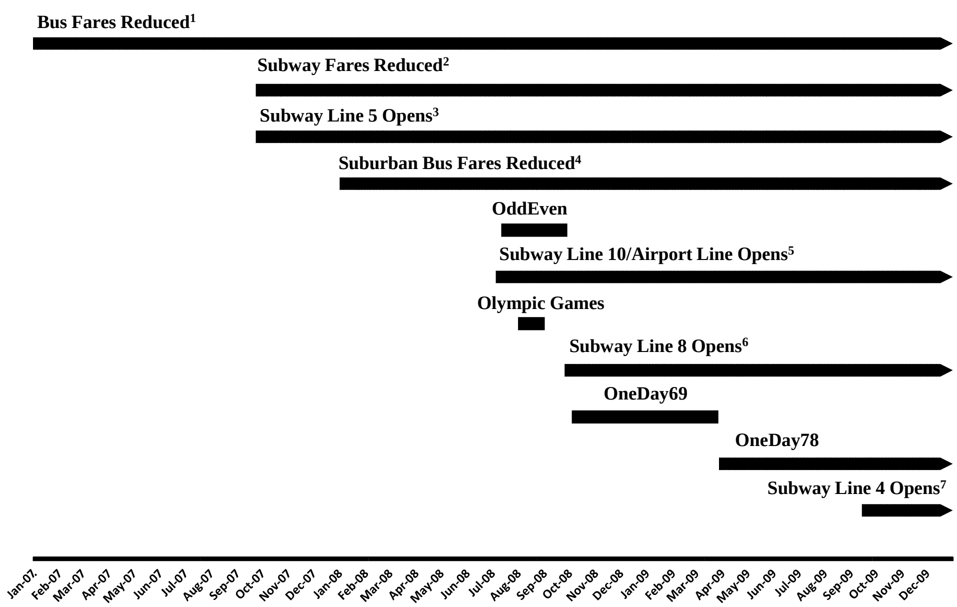
Figure 1 Map of Beijing Monitoring Station Locations in 2008 and 2009



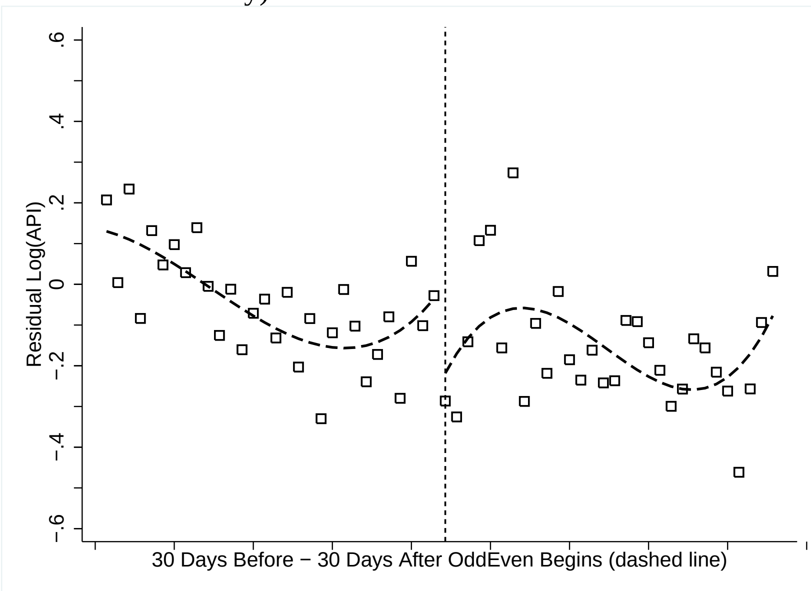
Map shows the locations of the monitoring stations (represented by dots) within or close to the 6th Ring Road (additional stations are located outside the 6th Ring Road). The green lines are subway lines. The blue lines are the Ring Roads. The inner-most blue line (which partially overlaps with a subway 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



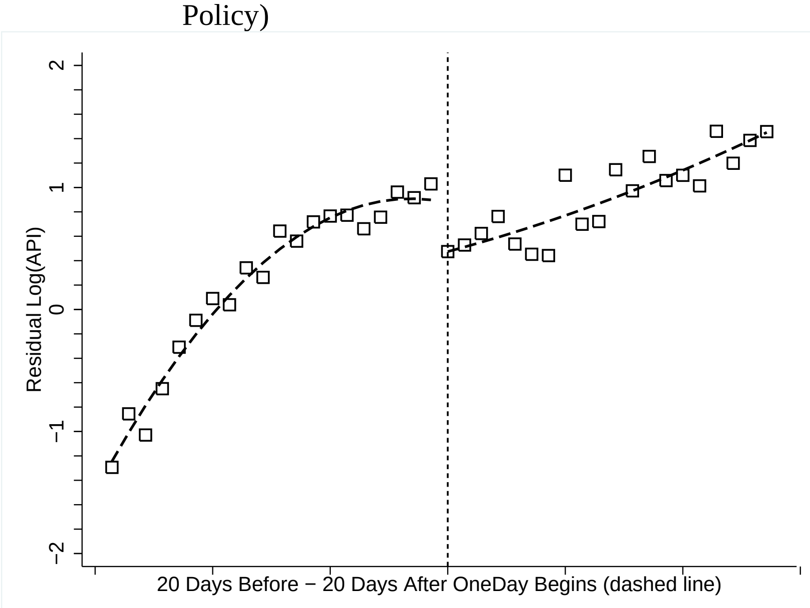
[1] Bus fares reduced from RMB 1 per trip to 0.4 for regular bus pass holders and to 0.2 for student pass holders. [2] Subway fares reduced from RMB 2 per transfer to RMB 2 per trip regardless of number of transfers. [3] Runs south to north. [4] Fares on suburban routes lowered by 60% for adults and 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)



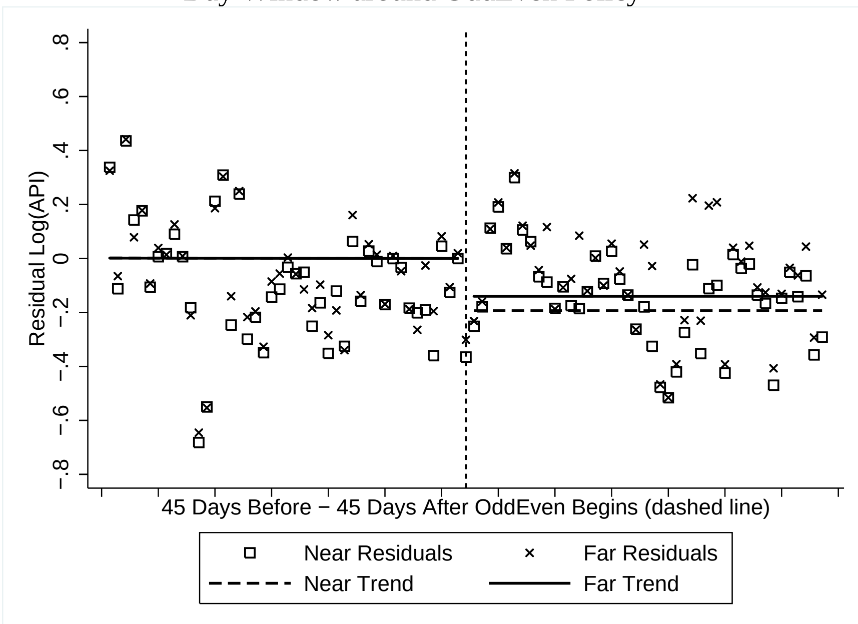
Residuals from a regression of aggregate logarithm of API in the 30 days before and after the beginning of the OddEven policy (the vertical dashed line) on Olympics dummy, holiday dummy, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind speed direction, interactions between wind speed and direction, monthly dummies, weekend dummy, a dummy for API less than 50, and a dummy for SO_2 day. The square dots are the residuals and the dashed lines are the fitted cubic time trends from the regression.

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



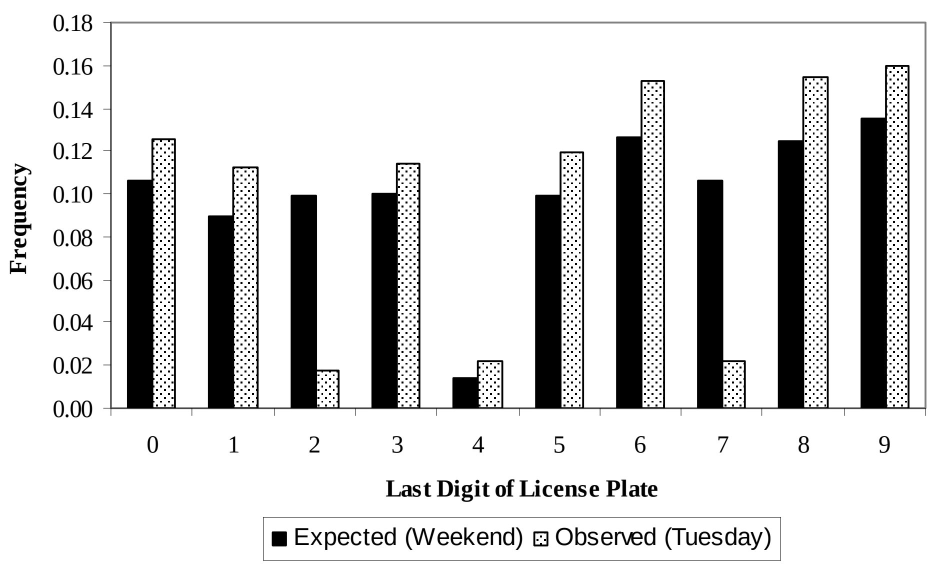
Residuals from a regression of aggregate logarithm of API in the 20 days before and after the beginning of the OneDay policy (the vertical dashed line) on Olympics dummy, holiday dummy, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind speed direction, interactions between wind speed and direction, monthly dummies, weekend dummy, a dummy for API less than 50, a dummy for SO_2 day, and an interaction between OneDay and weekend. The square dots are the residuals and the dashed lines are the fitted quadratic time trends from the regression.

Figure 5 Residuals from Station-Level Differences-in-Differences Regression in 45-Day Window around OddEven Policy



Residuals in the 45-day window before and after the beginning of the OddEven policy (the vertical dashed line) from the regression in Column 2 of Table 5. 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 dashed lines are the linear fitted time trends for the "near" residuals and the solid lines are the linear fitted time trends for the "far" stations.

Figure 6 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	90.834	49.527	12.000	500.000
Log(Aggregate API)	1,096	4.392	0.486	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	24.014	85.061	0.000	327.000
Sunshine	1,096	6.619	3.974	0.000	14.000
SO_2 Day	1,096	0.026	0.161	0.000	1.000
API < 50	1,096	0.130	0.337	0.000	1.000
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 duration. 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 (2007 – 2009)

	(1)	(2)	(3)	(4)	(5)
	Log(API)				
	No Trend	Linear Trend	Quadratic Trend	OddEven Only	Log(PM_{10})
OddEven	-0.2036 ***	-0.1882 ***	-0.1628 **	-0.1441 **	-0.3433 ***
	(0.0453)	(0.0544)	(0.0728)	(0.0583)	(0.0806)
OneDay	-0.0930 ***	-0.1934 ***	-0.1894 **		-0.1269 ***
	(0.0265)	(0.0496)	(0.0785)		(0.0359)
Weekend	-0.0514 *	-0.0505 *	-0.0509 *	-0.0403 *	-0.0900 **
	(0.0280)	(0.0281)	(0.0280)	(0.0209)	(0.0399)
OneDay*Weekend	0.0271	0.0308	0.0298		0.0431
	(0.0415)	(0.0411)	(0.0412)		(0.0601)
Olympics	-0.0142	-0.0139	-0.0129	-0.0171	-0.2023
	(0.0780)	(0.0777)	(0.0773)	(0.0766)	(0.1585)
Holiday	-0.0211	-0.0298	-0.0286	-0.0194	-0.0372
	(0.0428)	(0.0430)	(0.0425)	(0.0417)	(0.0656)
API < 50	-0.7346 ***	-0.7291 ***	-0.7292 ***	-0.7356 ***	
	(0.0367)	(0.0368)	(0.0369)	(0.0366)	
SO_2 Day	-0.3161 ***	-0.3091 ***	-0.3129 ***	-0.3138 ***	
	(0.0547)	(0.0562)	(0.0553)	(0.0556)	
Break	-0.1220	-0.1613	-0.1257		-0.1180
	(0.0937)	(0.1980)	(0.2038)		(0.1436)
Maximum Temperature	0.0395 ***	0.0414 ***	0.0419 ***	0.0408 ***	0.0593 ***
	(0.0035)	(0.0035)	(0.0036)	(0.0036)	(0.0047)
Average Humidity	0.0035 ***	0.0035 ***	0.0037 ***	0.0036 ***	0.0056 ***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0013)
Total Rainfall	-0.0001	-0.0001	-0.0001	-0.0001	-0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Sunshine	-0.0302 ***	-0.0305 ***	-0.0301 ***	-0.0301 ***	-0.0479 ***
	(0.0035)	(0.0035)	(0.0035)	(0.0035)	(0.0046)
R^2	0.6222	0.6293	0.6311	0.6272	0.3672
BIC	741.2	748.2	763.9		
N	1,096	1,096	1,096	1,096	917

Dependent variable is logarithm of aggregate, daily API in Models 1 through 4 and log of daily PM_{10} in Column 5. Standard errors in parentheses. Newey-West standard errors with one-day lag used in Models 1 through 4; standard errors clustered in rolling 2-day blocks in Model 5. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include month-of-year dummies, wind direction, wind-speed quartiles, and interactions between wind speed and wind direction. A linear time trend is included in Model 2 and a quadratic time trend in Model 3. Separate time trends are allowed for the regimes Before Oddeven, Break, During OneDay69, and During OneDay78 (except that the Break regime includes only up to a linear trend because its short duration creates near collinearities above this). Model 4 allows for separate quadratic time trends before and after OddEven. BIC is a Bayesian Information Criterion of model fit.

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

	(1)	(2)	(3)	(4)	(5)
	OddEven Only				
	30-Day Window			45-Day Window	60-Day Window
OddEven	-0.2183 **	-0.2871 **	-0.3336 ***	-0.4732 ***	-0.3499 **
	(0.1066)	(0.1251)	(0.1239)	(0.1648)	(0.1472)
Time Trend Order	3	4	5	5	5
R^2	0.9400	0.9416	0.9602	0.9019	0.8338
N	60	60	60	90	120

	(1)		(2)	(3)	(4)
	OneDay Only		OddEven & OneDay Together		
	20-Day Window		30 Days Before OddEven - 30 Days After OneDay		
OddEven			-0.2786	-0.2464 **	-0.3041 **
			(0.1702)	(0.1211)	(0.1192)
OneDay	-0.4004 **		-0.1268 **	-0.1239	-0.1680 **
	(0.1730)		(0.0567)	(0.0756)	(0.0826)
Time Trend Order	2		1	2	3
R^2	0.9477		0.8214	0.8273	0.8299
N	40		143	143	143

Dependent variable is log of aggregate, daily API. Standard errors in parentheses. All regressions cluster standard errors in rolling blocks of days: 30-Day "OddEven Only" with 10-day blocks, 45-Day "OddEven Only" with 20-day blocks, 60-Day "OddEven Only" with 20-day blocks, 20-Day "OneDay Only" with 12-day blocks, and "OddEven & OneDay Together" with 20-day blocks. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include month-of-year dummies, maximum temperature, average humidity, total rainfall, hours of sunshine, wind direction, wind-speed quartiles, interactions between wind speed and wind direction, holiday dummy (where applicable), Olympic dummy (where applicable), weekend dummy, interaction between weekend and OneDay (where applicable), and dummies for days with API less than 50 and days with SO_2 as the predominant pollutant. Separate time trends are allowed before and after the policy events in the top panel and in Column 1 of the bottom panel. Separate time trends are allowed for the regimes Before OddEven, During OddEven, Break, and During OneDay69 in Columns 2 through 4 of the bottom panel.

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

	(1)	(2)	(3)	(4)
	24 Stations	Balanced Panel	Inside Restricted Area	Outside Restricted Area
OddEven	-0.1254 ***	-0.1233 ***	-0.1725 ***	-0.1408 ***
	(0.0127)	(0.0133)	(0.0144)	(0.0184)
OneDay	-0.1600 ***	-0.1627 ***	-0.1288 ***	-0.1470 ***
	(0.0135)	(0.0139)	(0.0071)	(0.0155)
Olympics	-0.0064	-0.0060	0.0152	-0.0141
	(0.0146)	(0.0159)	(0.0181)	(0.0197)
Weekend	-0.0424 ***	-0.0425 ***	-0.0458 ***	-0.0388 ***
	(0.0037)	(0.0038)	(0.0036)	(0.0052)
OneDay*Weekend	0.0443 ***	0.0440 ***	0.0378 ***	0.0446 ***
	(0.0046)	(0.0048)	(0.0049)	(0.0066)
Holiday	-0.0571 ***	-0.0572 ***	-0.0280 ***	-0.0427 ***
	(0.0060)	(0.0063)	(0.0065)	(0.0075)
R^2	0.6399	0.6378	0.6059	0.6422
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 of daily API at monitoring stations. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include month-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 days with API less than 50 and days with SO_2 as the predominant pollutant. Separate quadratic time trends are allowed for the regimes Before OddEven, Break, During OneDay69, and During OneDay78 and these are interacted with station fixed-effects in Columns 1 and 2. Separate linear time trends are allowed for the regimes Before OddEven, Break, and During OneDay and these are interacted with station fixed-effects in Columns 3 and 4. The number of observations is not evenly divisible by the number of stations due to missing values.

Table 5 DD Estimates using Log Station-Level, Daily API (2007 – 2009), N = 8,361

	(1)	(2)	(3)	(4)
	Distance to Ring Roads			Distance to Class I Roads
	Near/Far		Quadratic Distance	
OddEven	-0.1396 ***	-0.1400 ***	-0.4617 ***	-0.1927 ***
	(0.0129)	(0.0129)	(0.0457)	(0.0221)
Near*OddEven	-0.0580 ***	-0.0536 ***		-0.0540 **
	(0.0119)	(0.0117)		(0.0255)
OddEven*Distance			0.5110 ***	
			(0.1054)	
OddEven*Distance^2			-0.2425 ***	
			(0.0588)	
OneDay	-0.1160 ***	-0.1155 ***	-0.3290 ***	-0.1254 ***
	(0.0089)	(0.0085)	(0.0350)	(0.0145)
Near*OneDay	-0.0248 **	-0.0204 **		-0.0440 *
	(0.0098)	(0.0102)		(0.0226)
OneDay*Distance			0.4150 ***	
			(0.0862)	
OneDay*Distance^2			-0.2160 ***	
			(0.0466)	
Before OddEven Trend		-0.0330	0.0100	-0.0247
		(0.0281)	(0.0320)	(0.0424)
Near*(Before OddEven Trend)		-0.0072	-0.0797	0.0063
		(0.0586)	(0.0597)	(0.0750)
Break Trend		-9.3796 ***	-6.8852 ***	-3.7289
		(0.8287)	(1.7231)	(2.4161)
Near*(Break Trend)		2.5255	-0.2934	-6.6855 **
		(2.3474)	(3.3823)	(3.0088)
During OneDay Trend		0.3334 ***	0.3817 ***	0.3463 ***
		(0.0323)	(0.0348)	(0.0534)
Near*(During OneDay Trend)		0.0027	0.0116	0.0840
		(0.0512)	(0.0779)	(0.0577)
R^2	0.6059	0.6053	0.4179	0.4173
Station Fixed Effects	Yes	Yes	Yes	Yes
Number of Stations	8	8	8	8

Dependent variable is log of daily API at monitoring stations inside the retricted area. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include month-of-year dummies, Olympics dummy, weekend dummy, holiday dummy, interaction between weekend dummy and OneDay dummy, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind direction dummies, and interactions between wind speed and wind direction. Dummies for days with API less than 50 and days with SO_2 as the predominant pollutant included in Columns 1 and 2. Separate linear time trends are allowed for the regimes Before OddEven, Break, and During OneDay and these are interacted with station fixed-effects in Column 1. The number of observations is not evenly divisable by the number of stations due to missing values.

Table 6 DD Estimates using Log Station-Level, Daily API in Discontinuity Samples

	45-Day Window	60-Day Window
OddEven	-0.1192 ***	-0.1899 ***
	(0.0153)	(0.0166)
"Near"*OddEven	-0.0447 **	-0.0414 **
	(0.0199)	(0.0205)
R^2	0.8083	0.7248
Number of Stations	8	8
Station Fixed Effects	Yes	Yes
N	718	958

Dependent variable is log of daily API at eight monitoring stations inside the restricted area. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. All regressions include an Olympics dummy, maximum temperature, average humidity, total rainfall, hours of sunshine, wind speed quartiles, wind direction dummies, interactions between wind speed and wind direction, dummy for weekends, and dummy for days with API less than 50. The number of observations is not evenly divisable by the number of stations due to missing values.

Table 7 RD Estimates using Log Hourly Television Viewership (2007 – 2009), N = 26,303

	"Self-Employed"		"Hourly Workers"	
	Coeff.	Std. Err.	Coeff.	Std. Err.
Lagged Viewership	0.5501	(0.0076) ***	0.4360	(0.0092) ***
Total Rainfall	-0.0001	(0.0000) **	-0.0001	(0.0000) **
Average Wind Speed	0.0049	(0.0029) *	0.0048	(0.0028) *
Sunshine	-0.0023	(0.0007) ***	-0.0016	(0.0006) **
Average Temperature	-0.0008	(0.0009)	-0.0015	(0.0008) *
Weekend	0.0180	(0.0074) **	0.0717	(0.0073) ***
Holiday	0.0481	(0.0128) ***	0.1305	(0.0138) ***
Olympics	0.0777	(0.0311) **	0.0577	(0.0315) *
OddEven	0.0312	(0.0396)	-0.0654	(0.0579)
OddEven*Weekend	-0.0103	(0.0181)	0.0333	(0.0228)
OddEven*Holiday	-0.0884	(0.0312) ***	-0.0541	(0.0251) **
OneDay69*Weekend	0.0031	(0.0223)	-0.0101	(0.0214)
OneDay69*Holiday	0.0781	(0.0459) *	-0.0180	(0.0486)
OneDay78*Weekend	0.0627	(0.0235) ***	0.0088	(0.0210)
OneDay78*Holiday	0.0496	(0.0462)	-0.0502	(0.0329)
OneDay69*Restr. Hours	0.0872	(0.0204) ***	-0.0106	(0.0183)
OneDay69*Non-Restr. Morning Hour	-0.0734	(0.0349) **	-0.0280	(0.0311)
OneDay69*Non-Restr. Evening Hours	-0.0153	(0.0194)	-0.0291	(0.0179)
OneDay78*Restr. Hours	0.1278	(0.0218) ***	-0.0418	(0.0187) **
OneDay78*Non-Restr. Morning Hour	0.1311	(0.0300) ***	0.0741	(0.0267) ***
OneDay78*Non-Restr. Evening Hours	0.0495	(0.0205) **	0.0215	(0.0177)
R^2	0.8849		0.9293	

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 dummies, month-of-year dummies, a dummy for the break period interacted with hour dummies, and separate 4th-order time trends for the regimes Before Oddeven, During OddEven, Break, and During OneDay.

Appendix A
Labor Supply Model with OddEven Driving Restrictions

Consider a two-stage model. In the first stage, workers choose their optimal commute mode (auto, public transit, or not working if they have discretion over their time). In stage two, they choose work time, leisure time, and goods consumption to maximize utility given their first-stage choice. Workers consider how their commute choice affects their utility so we solve the model by backward induction. For second-stage utilities, we modify a standard Cobb-Douglas labor supply function to accommodate commute mode choice and distinguish restricted from non-restricted days. We model the OddEven restrictions and consider each worker's utility over a representative two-day period: one non-restricted and one restricted day. With driving restrictions, the worker suffers a penalty for driving on the restricted day. Absent the policy, the two days are identical. We consider the OddEven policy because it is simpler to model than and generates the same intuition as the OneDay policy.[1]

There are two groups of workers: those with discretionary work time (D) and those with fixed work times (F) in proportions λ^D and $\lambda^F = 1 - \lambda^D$ respectively. The distribution of workers in each group is given by the cumulative density functions $G^D(\theta)$ and $G^F(\theta)$ where $\theta = \{w, Y, c_i, t_i, M_i\}$. w is hourly wage, Y is two-day non-wage income, and i is commute mode. Possible commute modes are auto $(i = A)$, public transit $(i = P)$, and for those with discretion, not working $(i = 0)$. For mode i, c_i is daily commute cost and t_i time (with $t_0 = c_0 = 0$). M_i is the worker's daily non-monetary disutility from commuting by mode i. Commuting by either mode is unpleasant: $M_P, M_A > M_0 = 0$. A worker's two-day utility conditional on commute choices (i for the non-restricted and j for the restricted day) is:

(A1) $$U_{ij}(\theta) = L_{Nij}^{\alpha} X_{Nij}^{1-\alpha} L_{Rij}^{\alpha} X_{Rij}^{1-\alpha} - M_i - M_j - \mathrm{I}_{Policy}\mathrm{I}_{j=A}Q;\ i, j \in \{A, P, 0\},$$

with $(0 < \alpha < 1)$. This distinguishes the restricted (R) and non-restricted (N) days. L is daily leisure hours and X daily consumption of other goods. We ignore across-day discounting and assume that utility derived from each two-day period is independent of other two-day periods. I is an indicator variable equal to one when the condition is true and zero otherwise and $Policy$ is a logical variable distinguishing the policy period. Q is expected penalty (monetary and psychic) in utility terms of driving a car while restricted.

We assume perfect compliance and full-time work absent the restrictions and focus on short-run effects:

(A) Absent the restrictions, commute times and costs are low enough that it is optimal for all workers to work both days.
(B) Compliance costs are small enough that workers do not leave the workforce or transition between jobs with discretionary and fixed work times. This ensures that the restrictions do not change these proportions.
(C) Wages and house prices do not adjust, workers do not move their residences or change their workplace (*i.e.*, commute times and costs are fixed), and workers do not purchase a second car to comply with the restrictions.
(D) The penalty is great enough that it is never optimal to drive on a restricted day.
(E) License plate numbers are uniformly distributed with half restricted each day.

After solving the model for each worker we examine the aggregate effects on pollution and work time across the distributions of workers.

Second Stage: Discretionary Work Time: Those with discretion may choose to work either "full time" (both days) or "reduced time" (one day). Assumption (A) and diminishing marginal utility of consumption ensure that the worker will at most remain home on the restricted day.[2] We consider only

[1] It is straightforward to adapt the model to the OneDay policy and the results differ only in magnitude. The commute costs it imposes are lower making "reduced time" less likely. However, declining marginal utility makes "reduced time" more likely because goods consumption suffers less from not working one day out of five rather than one day out of two. A full analysis of the OneDay model is available from the authors.
[2] Appendix B shows that it is not optimal to work on the restricted day and instead stay home on the non-restricted day under fairly general conditions.

a representative two-day period so all restricted days are identical. As a result, "reduced time" means taking every other day off from work. A more general model with random variation in daily productivity and leisure options would allow for less regular and extreme reductions. This simple model is adequate since we do not use it for calibration or direct estimation. Ignoring the penalty Q, the worker's second-stage problem conditional on mode choices i and j is:

(A2) $$\max_{\substack{\{H_{Nij},L_{Nij},X_{Nij},\\ H_{Rij},L_{Rij},X_{Rij}\}}} \quad U_{ij} = L_{Nij}^{\alpha} X_{Nij}^{1-\alpha} L_{Rij}^{\alpha} X_{Rij}^{1-\alpha} - M_i - M_j;\; i, j \in \{A, P, 0\} \quad st:$$

(A3) $$Y + w\left(H_{Nij} + H_{Rij}\right) - \left(X_{Nij} + c_i\right) - \left(X_{Rij} + c_j\right) = 0,$$

(A4a) $$T - \left(H_{Nij} + t_i\right) - L_{Nij} = 0,$$

(A4b) $$T - \left(H_{Rij} + t_j\right) - L_{Rij} = 0,$$

(A5a) $$H_{Nij} \geq 0 \leftarrow \kappa_N,$$

(A5b) $$H_{Rij} \geq 0 \leftarrow \kappa_R;$$

where T is total available hours per day, H is daily working hours, and the κ's are Kuhn-Tucker multipliers. Equation (A3) is the resident's two-day budget constraint with the price of X normalized to one. Equations (A4a) and (A4b) are the resident's day-by-day time constraints. We assume that the budget and time constraints bind but that the constraints on positive working hours may not. Substituting (A3) and (A4) the problem becomes:

(A6) $$\max_{\{H_{Nij},X_{Nij},H_{Rij}\}} \quad U_{ij} = \left(T - H_{Nij} - t_i\right)^{\alpha} X_{Nij}^{(1-\alpha)} \left(T - H_{Rij} - t_j\right)^{\alpha} \left(Y + wH_{Nij} + wH_{Rij} - X_{Nij} - c_i - c_j\right)^{1-\alpha} - M_i - M_j$$

The first-order conditions for the worker's problem are:

(A7a) $$\left[H_{Nij}\right]: \frac{\alpha\left(U_{ij} + M_i + M_j\right)}{T - H_{Nij} - t_i} = \frac{(1-\alpha)\, w\left(U_{ij} + M_i + M_j\right)}{Y + wH_{Nij} + wH_{Rij} - X_{Nij} - c_i - c_j},$$

(A7b) $$\left[H_{Rij}\right]: \frac{\alpha\left(U_{ij} + M_i + M_j\right)}{T - H_{Rij} - t_j} - \kappa = \frac{(1-\alpha)\, w\left(U_{ij} + M_i + M_j\right)}{Y + wH_{Nij} + wH_{Rij} - X_{Nij} - c_i - c_j},$$

(A8) $$\left[X_{Nij}\right]: \frac{(1-\alpha)\left(U_{ij} + M_i + M_j\right)}{X_{Nij}} = \frac{(1-\alpha)\left(U_{ij} + M_i + M_j\right)}{Y + wH_{Nij} + wH_{Rij} - X_{Nij} - c_i - c_j},$$

(A9a) $$\left[\kappa_R\right]: H_{Rij}\kappa_R = 0,$$

(A9b) $$\left[\kappa_N\right]: H_{Nij}\kappa_N = 0.$$

There are two cases to solve: "full time" $\left(H_{Nij}, H_{Rij} > 0;\, i, j \in \{A, P\}\right)$ and "reduced time" ($H_{Ni0} > 0$, $i \in \{A, P\}$; but $H_{Ri0} = 0$ or vice versa). Conditional on the commute mode choices i and j, define:

(A10a) $$NT_{Ni} = T - t_i \text{ and } NT_{Rj} = T - t_j,$$

(A10b) $$NI_{ij} = \frac{Y - c_i - c_j}{w};$$

(A10c) $$\Delta t_{ji} = t_j - t_i,$$

(A10d) $$\Delta c_{ji} = \left(c_j - ct_i\right)/w.$$

NT_{Ni} and NT_{Rj} are the time available net of commuting on restricted and non-restricted days while NI_{ij} is the two-day, non-wage income net of commute costs. Δt_{ji} and Δc_{ji} are the difference in commute times and costs respectively on the restricted versus non-restricted days. Both NI_{ij} and Δc_{ji} are converted to hours based on the opportunity cost of time.

Case 1): "Full Time" $\left(H_{Nij}, H_{Rij} > 0;\, i, j \in \{A, P\}\right)$. Solving the model (the Optional Appendix contains a detailed derivation), the results are:

(A11a) $$H_{Nij} = (1-\alpha)\, NT_{Ni} - \frac{\alpha}{2}\left[NI_{ij} - \Delta t_{ji}\right],$$

(A11b) $$H_{Rij} = (1-\alpha)\, NT_{Rj} - \frac{\alpha}{2}\left[NI_{ij} + \Delta t_{ji}\right];$$

(A12) $L_{Nij} = L_{Rij} = \alpha\left[NT_{Ni} + \frac{1}{2}\left(NI_{ij} - \Delta t_{ji}\right)\right]$,

(A13) $X_{Nij} = X_{Rij} = \left(1-\alpha\right)w\left[NT_{Ni} + \frac{1}{2}\left(NI_{ij} - \Delta t_{ji}\right)\right]$.

Two-day indirect utility is, where we re-introduce the penalty Q:

(A14) $U_{ij} = \left(kw^{(1-\alpha)}\right)^2\left(NT_{Ni} + \frac{NI_{ij}}{2} - \frac{\Delta t_{ji}}{2}\right)^2 - M_i - M_j - \mathrm{I}_{Policy}\mathrm{I}_{j=A}Q; i, j \neq 0$ where $\left(k = \alpha^{\alpha}\left(1-\alpha\right)^{(1-\alpha)}\right)$.

Leisure time is equated across the days. For workers who prefer public transit the work day lengths are the same: $H_{RPP} - H_{NPP} = 0$. For those who prefer driving, their restricted work day will be shorter or longer than their non-restricted depending on whether their public transit commute is longer or shorter than by car ($H_{RAP} - H_{NAP} = \left(\alpha - 1\right)\Delta t_{PA}$).

Case 2): "Reduced Time" *(* $H_{Ni0} > 0, i \in \{A, P\}$ *but* $H_{Rij} = 0$ *)*. We solve the model assuming zero hours on the restricted day. In this case $t_R = c_R = 0$. The results for instead working zero hours on the non-restricted days are symmetric but Appendix B shows that this is not optimal under fairly general conditions. Solving (the Optional Appendix contains a detailed derivation), the results are:

(A15a) $H_{Ni0} = \frac{2}{1+\left(1-\alpha\right)}\left[\left(1-\alpha\right)NT_{Ni} - \frac{\alpha}{2}NI_{i0}\right]$, (A15b) $H_{Ri0} = 0$;

(A16a) $L_{Ni0} = \frac{\alpha}{1+\left(1-\alpha\right)}\left[NT_{Ni} + NI_{i0}\right]$, (A16b) $L_{Ri0} = T$;

(A17a) $X_{Ni0} = \frac{\left(1-\alpha\right)w}{1+\left(1-\alpha\right)}\left[NT_i + NI_{i0}\right]$, (A17b) $X_{Ri0} = \frac{\left(1-\alpha\right)w}{1+\left(1-\alpha\right)}\left[NT_i + NI_{i0}\right]$.

Two-day indirect utility is:

(A18) $U_{i0} = \frac{\left(kw^{(1-\alpha)}\right)^2}{\left(1+\left(1-\alpha\right)\right)^{1+(1-\alpha)}\alpha^{\alpha}}\left(NT_{Ni} + NI_{i0}\right)^{1+(1-\alpha)}T^{\alpha} - M_i$, where $\left(k = \alpha^{\alpha}\left(1-\alpha\right)^{(1-\alpha)}\right)$.

The worker cannot balance leisure or work time across restricted and non-restricted days. The results for $H_{Nij} = 0$ but $H_{Rij} > 0$ are obtained by replacing N with R, i with 0, and 0 with j.

Second Stage: Fixed Work Times: Since daily work hours are fixed $\left(H_{Nij} = H_{Rij} = \overline{H} > 0; i, j \in \{A, P\}\right)$, the worker chooses only L_{Nij}, L_{Rij}, X_{Nij}, and X_{Rij}. Solving, (the Optional Appendix contains a detailed derivation), the results are:

(A19a) $L_{Nij} = T - \overline{H} - t_i$, (A19b) $L_{Rij} = T - \overline{H} - t_j$;

(A20) $X_{Nij} = X_{Rij} = w\left[\overline{H} + \frac{1}{2}NI_{ij}\right]$.

Two-day indirect utility is, where we re-introduce the penalty Q:

(A21) $U_{ij} = w^{2(1-\alpha)}\left[\left(T - \overline{H} - t_i\right)\left(T - \overline{H} - t_j\right)\right]^{\alpha}\left(\overline{H} + \frac{NI_{ij}}{2}\right)^{2(1-\alpha)} - M_i - M_j - \mathrm{I}_{Policy}\mathrm{I}_{j=A}Q; i, j \neq 0$.

The difference in leisure time on restricted versus non-restricted days depends on relative commute times for the chosen modes $\left(L_{Rij} - L_{Nij} = \Delta t_{ji}\right)$ but the difference is not shared across the two days.

This completes the second-stage solution for type θ. We now consider the first stage when workers choose their commute mode. Using the distributions of the θ's we can specify the share of each commute mode for both categories of workers: $s^k_{ij}, k \in \{D,F\}; i,j \in \{A,P,0\}$. We solve the first stage with and without the restrictions.

First Stage – Without Restrictions: Without the restrictions, the two days are identical and the worker makes the same choice across days ($i = j$). The shares of each mode are $(k = D,F)$:

(A22a) $s^k_{AA} = \int \{\theta \mid U_{AA}(\theta) > U_{ii}(\theta); i = P,0\} dG^k(\theta) d\theta$, (A22b) $s^k_{PP} = \int \{\theta \mid U_{PP}(\theta) > U_{ii}(\theta); i = A,0\} dG^k(\theta) d\theta$,

where U_{ij} is given by (A14) and Assumption (A) implies $s^k_{00} = 0$ so that $s^k_{AA} + s^k_{PP} = 1$.

First Stage – With Restrictions: Assumption (D) ensures that Q is great enough that no workers drive on their restricted day so that $i \in \{A,P\}$ and $j \in \{P,0\}$. Regardless of whether they have discretion or not, commuters who prefer public transit absent the restrictions will take public transit both days under the restrictions so that $\hat{s}^k_{PP} = s^k_{PP}; k \in \{D,F\}$ where we use hats to denote outcomes under the restrictions. This follows because $U_{PP}(\theta) > U_{AA}(\theta)$ implies $U_{PP}(\theta) > U_{AP}(\theta)$ in both Equations (A14) and (A21).

Workers who prefer to drive absent the restrictions will continue to drive on the non-restricted day. On the restricted day, those with fixed work times must take public transit on the restricted day so that $\hat{s}^F_{A0} = 0$ and $\hat{s}^F_{AP} = s^F_{AA}$. On the restricted day, those with discretion can either take public transit or not work. The shares doing each are:

(A23a) $\hat{s}^D_{AP} = \int \{\theta \mid U_{AP}(\theta) > U_{A0}(\theta)\} dG^D(\theta) d\theta$, (A23b) $\hat{s}^D_{A0} = \int \{\theta \mid U_{A0}(\theta) > U_{AP}(\theta)\} dG^D(\theta) d\theta$.

Given Assumption (B), we know that $\hat{s}^D_{AP} + \hat{s}^D_{A0} = s^D_{AA}$ and if some commuters find it optimal to stay home when restricted $\left(\hat{s}^D_{A0} > 0\right)$ then $\hat{s}^D_{AP} < s^D_{AA}$.

Extensive Margin Effects: For those with fixed work times, there is no effect on the extensive margin since they have no control over work time (*i.e.*, $\hat{s}^F_{AP} = s^F_{AA}$ and $\hat{s}^F_{PP} = s^F_{PP}$). This yields **Implication 1** in the main text.

Assumption (A) implies that absent the restrictions no workers with discretionary work time stay home on the restricted day.[3] With the restrictions, this increases to $\lambda^D \hat{s}^D_{A0} / 2$ – the density of workers choosing "reduced time." This yields **Implication 2** in the main text.

Under the restrictions, daily car density and pollution on Beijing roads decreases by $\frac{1}{2}\left(\lambda^D s^D_{AA} + \lambda^F s^F_{AA}\right)$. That is, half the drivers cannot drive on a given day. This yields **Implication 3** in the main text.

Intensive Margin Effects – Workers with Fixed Work Times: Those who took public transit absent the restrictions will still do so and their leisure time is unaffected $\left(\hat{L}_{NPP} - L_{NPP} = \hat{L}_{RPP} - L_{RPP} = 0\right)$ by Equation (A19). Those who prefer to drive, with density $\lambda^F s^F_{AA} / 2$, are forced to take public transit and leisure is unaffected on non-restricted $\left(\hat{L}_{NAP} - L_{NAA} = 0\right)$ but affected on restricted days $\left(\hat{L}_{RAP} - L_{RAA} = -\Delta t_{PA}\right)$ by Equation (A19). Since intensive margin effects are zero for those who

[3] In our data, this is not literally zero due to multiple daily work shifts, vacations, and sick days.

normally take public transit and ambiguous for those who normally drive, the total effect $\left(-\lambda^F s^F_{AA} \Delta t_{PA}/2\right)$ could be positive or negative. This yields **Implication 4** in the main text.

Intensive Margin Effects – Workers with Discretionary Work Time: Workers who prefer public transit absent the restrictions choose to work "full time" and there is no effect on leisure time: $\hat{L}_{NPP} - L_{NPP} = \hat{L}_{RPP} - L_{RPP} = 0$ by Equation (A12). Those who prefer driving absent the restrictions and choose to work "full time" must commute by public transit on the restricted day and their leisure time could increase or decrease depending on whether public transit commute times and costs are less than those by car or not: $\hat{L}_{NAP} - L_{NAA} = \hat{L}_{RAP} - L_{RAA} = -\alpha/2\left(\Delta c_{PA} + \Delta t_{PA}\right)$ by Equation (A12). Unlike those with fixed work times, commute costs also matter because daily labor supply is discretionary. Due to diminishing marginal utility, the worker equalizes leisure time across the work days and shares the difference in commute times and costs across the restricted and non-restricted days.

For workers who work "reduced time," leisure time most likely decreases on the non-restricted day. Equations (A12) and (A16a) imply:

$$\text{(A24)} \qquad \hat{L}_{NA0} - L_{NAA} = \frac{\alpha}{1+(1-\alpha)}\left[(\alpha-1)(T-t_A) + \frac{\alpha}{2}\frac{Y}{w} + (1-\alpha)\frac{c_A}{w}\right] \equiv \Upsilon \cdot$$

That the expression in Equation (A24) can be positive (negative) is most easily seen by setting α close to one (zero). This expression is more likely positive the greater Y, c_A, or t_A. The total effect across all workers with discretionary work time is $\lambda^D\left[\hat{s}^D_{A0}\Upsilon - \hat{s}^D_{AP}\alpha/2\left(\Delta c_{PA} + \Delta t_{PA}\right)\right]$, which could be positive or negative. This yields **Implication 5** in the main text.

Appendix B
Non-Optimality of Staying Home on Non-Restricted Day

Working on the restricted day but not on the non-restricted is not optimal under at least two general cases:

Case 1: $M_A = M_P$ and $c_A > c_P$. For a worker who prefers to commute by auto, $U_{AA} > U_{PP}$ which by Equation (A14) implies:

$$\text{(B1)} \qquad \left(NT_{NA} + \frac{NI_{AA}}{2}\right)^2 > \left(NT_{NP} + \frac{NI_{PP}}{2}\right)^2 \Rightarrow (t_P - t_A) > \frac{2}{w}(c_A - c_P)\text{. Now:}$$

$$\text{(B2)} \qquad c_A > c_P \Rightarrow \frac{1}{w}(c_A - c_P) < \frac{2}{w}(c_A - c_P) \Rightarrow (t_P - t_A) > \frac{1}{w}(c_A - c_P) \text{ which implies:}$$

$$\text{(B3)} \qquad (NT_{NA} + NI_{A0}) > (NT_{NP} + NI_{P0}) \Rightarrow (NT_{NA} + NI_{A0})^{1+(1-\alpha)} > (NT_{NP} + NI_{P0})^{1+(1-\alpha)}\text{. This implies}$$

$U_{A0} > U_{P0}$ using Equation (A18).

Case 2: $t_A = t_P$ and $c_A = c_P$ but $M_A \neq M_P$. By Equation (A14) $U_{AA} > U_{PP} \Rightarrow M_P > M_A$. This implies $U_{A0} > U_{P0}$ using Equation (A18).

Assumption (A) ensures that the worker will remain home on at most the restricted day since the non-restricted day is unaffected and extra leisure is already enjoyed on the restricted day under "reduced-time" work.

Appendix C
Conditions for "Reduced-Time" Work for Discretionary Workers

We consider two cases:

Case 1: $M_A = M_P = 0$. Comparing Equations (A14) and (A18), $U_{A0} > U_{AP}$ when:

(C1) $$\frac{\left(NT_{NA}+NI_{A0}\right)^{1+(1-\alpha)}T^{\alpha}}{\left(NT_{NA}+NI_{A0}-\frac{c_P}{2}-\frac{\Delta t_{PA}}{2}\right)^2}>\left(1+\left(1-\alpha\right)\right)^{1+(1-\alpha)}\alpha^{\alpha}.$$

It follows immediately that this is more likely the greater c_P or Δt_{PA}.

Case2: $\left(M_P \gg 0\right)$. Since U_{A0} in Equation (A18) does not depend on M_P and U_{AP} in Equation (A14) is decreasing in M_P it follows directly that $U_{A0} > U_{AP}$ when M_P is sufficiently large $\left(M_P \gg 0\right)$.

Appendix D
Effect of Expanded Subway Capacity on Leisure Time

Expanded subway capacity reduces both public transit and auto commute times: $\tilde{t}_A < t_A$ and $\tilde{t}_P < t_P$, where tildes indicate outcomes after the expansion. Assume that the expansion has no effect on commute costs ($\tilde{c}_A = c_A$ and $\tilde{c}_P = c_P$) and does not change workers' optimal commute modes. Assuming all workers obey the restrictions and continue to work "full time" (*i.e.*, there is no extensive margin effect), compute the change in leisure time due to the subway expansion for each category of worker and commute mode. For those with discretionary work time who prefer driving and public transit respectively (by Equation (A12)):

(D1) $$\tilde{L}_{NAP}-L_{NAA}=\tilde{L}_{RAP}-L_{RAA}=\alpha\left[\left(t_A-\tilde{t}_A\right)+\frac{1}{2}\frac{c_A-c_P}{w}-\frac{1}{2}\left(t_P-\tilde{t}_A\right)\right],$$

(D2) $$\tilde{L}_{NPP}-L_{NPP}=\tilde{L}_{RPP}-L_{RPP}=\alpha\left(t_P-\tilde{t}_P\right).$$

For those with fixed work times who prefer driving and public transit respectively (by Equation (A19)):

(D3a) $$\tilde{L}_{NAP}-L_{NAA}=\left(t_A-\tilde{t}_A\right),$$

(D3b) $$\tilde{L}_{RAP}-L_{RAA}=\left(t_A-\tilde{t}_P\right);$$

(D4) $$\tilde{L}_{NPP}-L_{NPP}=\tilde{L}_{RPP}-L_{RPP}=\left(t_P-\tilde{t}_P\right).$$

All of the expressions on the right-hand sides of Equations (D1) through (D4) are weakly decreasing in both $\tilde{t}_A$ and $\tilde{t}_P$ and are strictly decreasing in one of them for at least one commute mode within each group of workers. This implies that leisure time increases for both groups due to the expansion.

Appendix E
Variable Descriptions and Data Sources

Variable	Description	Frequency/ Availability	Data Source
Aggregate API	Aggregate Air Pollution Index; see text for detailed description.	Daily	SEPA and BJEPA
Station-Level API	Air Pollution Index from 24 monitoring stations.	Daily	Andrews (2008) and BJEPA
Maximum Temperature	Maximum daily temperature in celcius.	Daily	CMDSSS
Average Humidity	Average percent humidity over the day.	Daily	CMDSSS
Total Rainfall	Total rainfall over the day in centimeters.	Daily	CMDSSS
Sunshine	Number of total hours of sunlight during the day.	Daily	CMDSSS
Wind Direction	Predominant direction of wind during the day divided into four quadrants (Northeast, Southeast, Southwest, Northwest).	Daily	CMDSSS
Max. Wind Speed	Maximum of the average wind speed over 15-minute increments across the day in meters per second.	Daily	CMDSSS
Distance from Ring Road	Distance in kilometers of monitoring station from nearest Ring Road.	Once	Geographic Information System calculations
Distance from Class I Road	Distance in kilometers of monitoring station from nearest Class I Road.	Once	Geographic Information System calculations
Television Viewership	Number of people in thousands watching television.	Hourly	CSM Media Research Television Audience Measurement (TAM)
Average Temperature	Average daily temperature in celsius.	Daily	CMDSSS
Average Wind Speed	Average daily wind speed in meters per second.	Daily	CMDSSS

CMDSSS refers to China Meteorological Data Sharing Service System, SEPA to State Environmental Protection Agency, and BJEPA to Beijing Environmental Protection Agency.

Appendix F
Construction of API Indices

A daily measure of particulate matter, sulfur dioxide, and nitrogen dioxide at each station s on day t is based on the average of 24 hourly (indexed by h) readings: $PM10_{st} = \frac{1}{24}\sum_{h=1}^{24} PM10_{sth}$, $SO2_{st} = \frac{1}{24}\sum_{h=1}^{24} S02_{sth}$, and $NO2_{st} = \frac{1}{24}\sum_{h=1}^{24} N02_{sth}$. The three measures are then scaled to reflect comparable severity $\left(\overline{PM10}_{st}, \overline{SO2}_{st}, \overline{NO2}_{st}\right)$. The piece-wise linear conversion formula for particulate matter is given in the table below – similar conversions are used for sulfur dioxide and nitrogen dioxide. Station-level API is:

(F1) $$API_{st}^{S} = \max\left\{\overline{PM10}_{st}, \overline{SO2}_{st}, \overline{NO2}_{st}\right\}.$$

The aggregate API is calculated as:

(F2) $$API_{t} = \max\left\{\overline{PM10}_{t}, \overline{SO2}_{t}, \overline{NO2}_{t}\right\},$$

where $\left(\overline{PM10}_t, \overline{SO2}_t, \overline{NO2}_t\right)$ are the scaled versions of the average daily measures across all stations:
$PM10_t = \frac{1}{S}\sum_{s=1}^{S} PM10_{st}$, $SO2_t = \frac{1}{S}\sum_{s=1}^{S} SO2_{st}$, and $NO2_t = \frac{1}{S}\sum_{s=1}^{S} NO2_{st}$.

We observe only API_{st}^{s}, API_t, and the identity of the "major pollutant" for each if the value exceeds 50. We do not observe daily data for all three pollutants for each station – we observe only the "major pollutant" for each station on each day – and we do not observe the underlying hourly data. The percentage of days that PM_{10} is the "major pollutant" at a station ranges from 68% to 91% across stations. At the aggregate level, PM_{10} is the "major pollutant" on 83% of the days.

What we observe about these indices limits their use. First, since the "major pollutant" at a station may differ from that at the aggregate level, we are unable to fully verify the construction of the aggregate API from the station-level APIs. Second, since the "major pollutant" for each station can vary day-by-day we cannot construct station-level PM_{10} measures over time. Finally, since the "major pollutant" varies across stations within a day and across days within a station, we cannot construct an alternative, aggregate pollution measure.

Conversion of PM_{10} to API

API	PM_{10}	Conversion Formula
0 – 50	0 – 50	API = PM_{10}
50 – 200	50 – 350	API = (1/2)*PM_{10} + 25
200 – 300	350 – 420	API = (10/7)*PM_{10} – 300
300 – 400	420 – 500	API = (5/4)*PM_{10} – 225
400 – 500	500 – 600	API = PM_{10} – 100

Based on Andrews (2008).

Appendix G
Placebo Tests of RD Design

	Pre-OddEven - Mid-Point			Pre-OddEven - 3/4-Point		
	No Trend	Linear Trend	Quadratic Trend	No Trend	Linear Trend	Quadratic Trend
"OddEven" Placebo	0.1174	0.1152	0.1007	-0.0195	-0.0681	-0.0661
	(0.1433)	(0.1435)	(0.1364)	(0.0686)	(0.0899)	(0.1319)
R^2	0.5528	0.5539	0.5590	0.5515	0.5540	0.5596
N	566	566	566	566	566	566
	Post-OneDay - Mid-Point			**Post-OneDay - 1/4-Point**		
	No Trend	Linear Trend	Quadratic Trend	No Trend	Linear Trend	Quadratic Trend
"OneDay" Placebo	0.0748	-0.1238	0.1444	0.0960	-0.2749	-0.1996
	(0.0525)	(0.0954)	(0.1007)	(0.0593)	(0.3568)	(0.2484)
R^2	0.6637	0.6693	0.6975	0.6646	0.6666	0.6950
N	446	446	446	446	446	446

Dependent variable is log of aggregate, daily API. Standard errors in parentheses. Newey-West standard errors with one-day lag used in all regressions. * = 10% significance, ** = 5% significance, *** = 1% significance. Month-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 days with API less than 50 and days with SO_2 as the predominant pollutant included in all regressions. The top panel regressons include all days before the OddEven policy begins on July 20, 2008. The bottom panel regressions include all days after the OneDay policy begins on October 11, 2008. Separate time trends are allowed before and after the placebo policies.

Appendix H
Effect of Subway Line 5/Subway Fare and Bus Fare Reduction Policies

	(1)	(2)	(3)	(4)	(5)
	RD			DD	
	Line 5/ Subway Fare	Bus Fare Reduction	Both	Line 5/ Subway Fare	Bus Fare Reduction
OddEven	-0.1808 ***	-0.1948 ***	-0.1898 ***	-0.1684 ***	-0.1718 ***
	(0.0547)	(0.0550)	(0.0556)	(0.0145)	(0.0134)
OneDay	-0.1730 ***	-0.1724 ***	-0.1538 ***	-0.1312 ***	-0.1242 ***
	(0.0543)	(0.0494)	(0.0540)	(0.0116)	(0.0072)
Line 5 Opening/Subway Fare Reduction	0.0887		0.1130	-0.0900	
	(0.0899)		(0.0891)	(0.0685)	
Bus Fare Reduction		0.0482	0.0794		0.0018
		(0.0807)	(0.0801)		(0.0994)
Line 5 Opening/Subway Fare Reduction*Distance				0.0781	
				(0.1626)	
Line 5 Opening/Subway Fare Reduction*Distance^2				0.0166	
				(0.0947)	
Subway Fare Reduction*Distance					0.1521
					(0.2911)
Subway Fare Reduction*Distance^2					-0.1076
					(0.1718)
R^2	0.6299	0.6295	0.6303	0.6059	0.6060

	(1)	(2)	(3)	(4)	(5)
	RD			DD	
	Line 5/ Subway Fare	Bus Fare Reduction	Both	Line 5/ Subway Fare	Bus Fare Reduction
Line 5 Opening/Subway Fare Reduction	0.0940		0.0959	-0.0979 *	
	(0.0662)		(0.0683)	(0.0507)	
Bus Fare Reduction		-0.0002	-0.0123		-0.0947
		(0.0630)	(0.0647)		(0.0701)
Line 5 Opening/Subway Fare Reduction*Distance				0.0525	
				(0.1095)	
Line 5 Opening/Subway Fare Reduction*Distance^2				0.0062	
				(0.0598)	
Subway Fare Reduction*Distance					0.0905
					(0.1788)
Subway Fare Reduction*Distance^2					-0.0558
					(0.1002)
R^2	0.6188	0.6179	0.6188	0.5974	0.5975
Number of Stations				8	8
N	1,096	1,096	1,096	8,361	8,361

RDD regressions: Dependent variable is log of aggregate, daily API. Standard errors in parentheses. Newey-West standard errors with one-day lag. * = 10% significance, ** = 5% significance, *** = 1% significance. Regressions include all control variables used in Column 1 of Table 2 as well as separate linear time trends for the regimes Before Oddeven, Break, During OneDay69, and During OneDay78. DD regressions: Dependent variable is log of daily API at monitoring stations inside the restricted area. Robust standard errors clustered at the station level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. Regressions include all control variables used in Column 3 of Table 5. In the top panel, separate linear time trends are allowed for the regimes Before OddEven, Break, and During OneDay and these are interacted with station fixed effects. In the bottom panel, linear and quadratic time trends are included in Columns 1 to 3; and linear time trends are interacted with station fixed effects in Columns 4 and 5. The number of observations in the DD regressions is not evenly divisable by the number of stations due to missing values.

Appendix I
Sensitivity of Policy Coefficients to Order of Polynomial Daily Time Trend in Regression of Log Hourly Television Viewership, N = 26,303

	0-Order	1-Order	2-Order	3-Order	4-Order[1]	5-Order[1]	6-Order[1]
"Self-Employed"							
OneDay69*Restricted Hours	0.1883 ***	0.1161 ***	0.1219 ***	0.0832 ***	0.0872 ***	0.1203 ***	0.1684 ***
	(0.0092)	(0.0107)	(0.0168)	(0.0196)	(0.0204)	(0.0215)	(0.0214)
OneDay78*Restricted Hours	0.2659 ***	0.1162 ***	0.1363 ***	0.1308 ***	0.1278 ***	0.1779 ***	0.2450 ***
	(0.0096)	(0.0181)	(0.0216)	(0.0216)	(0.0218)	(0.0228)	(0.0231)
Prob > F (Time Trend)		0.0001	0.0000	0.0000	0.0000	0.0000	0.0000
"Hourly Workers"							
OneDay69*Restricted Hours	0.1049 ***	0.0747 ***	0.0084	-0.0130	-0.0106	0.0153	0.0296
	(0.0080)	(0.0095)	(0.0141)	(0.0182)	(0.0183)	(0.0195)	(0.0203)
OneDay78*Restricted Hours	0.1378 ***	0.0283 *	-0.0452 **	-0.0459 **	-0.0418 **	-0.0138	0.0201
	(0.0067)	(0.0165)	(0.0187)	(0.0185)	(0.0187)	(0.0194)	(0.0203)
Prob > F (Time Trend)		0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Coefficents on selected policy variables in regression of log viewership on control variables and a polynomial time trend as in Table 7. Dependent variable is log number of thousands of individuals watching television each hour. All regressions include the control variables shown in Table 7 as well as hour dummies, month-of-year dummies, and a dummy for the break period interacted with hour dummies. Standard errors clustered at the daily level in parentheses. * = 10% significance, ** = 5% significance, *** = 1% significance. Separate time trends are allowed for the regimes: Before Oddeven, During OddEven, Break, and During OneDay. The F-test is the p-value for the joint significance level of the time trend variables. [1] Time trend during the break period becomes collinear above a 4th-order trend and are omitted.

Appendix J
Coefficients on Interaction between Policy Variables and Hourly Dummies

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

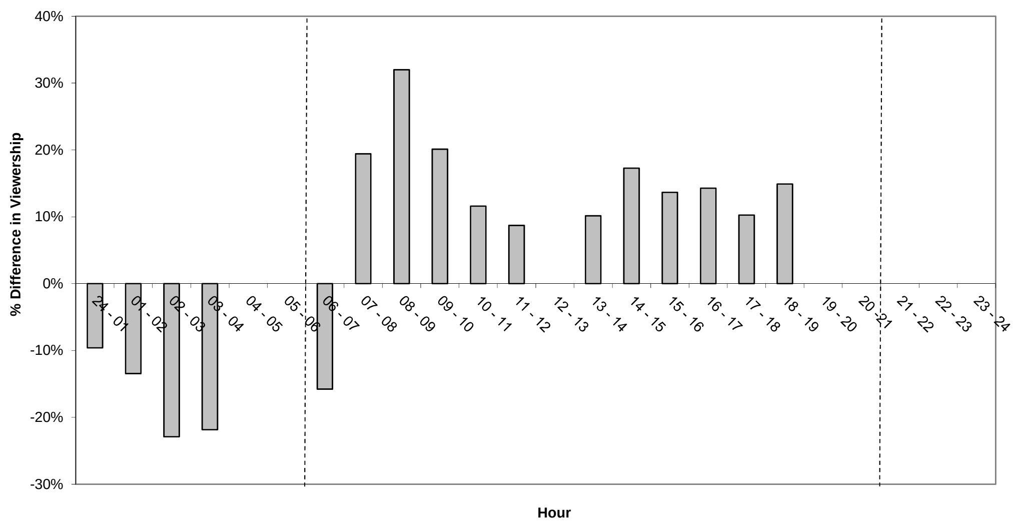


Chart shows coefficients on interactions between the OneDay69 policy variable and hourly dummies in the regression of Columns 1 and 2 of Table 5 but with OneDay69 and OneDay78 interacted with each hour separately. Coefficients are shown only if significant at the 10% level or better. The vertical dotted lines demarcate the restricted period.

Panel B – "Hourly Workers" Percentage Difference in Viewership during OneDay69 Period

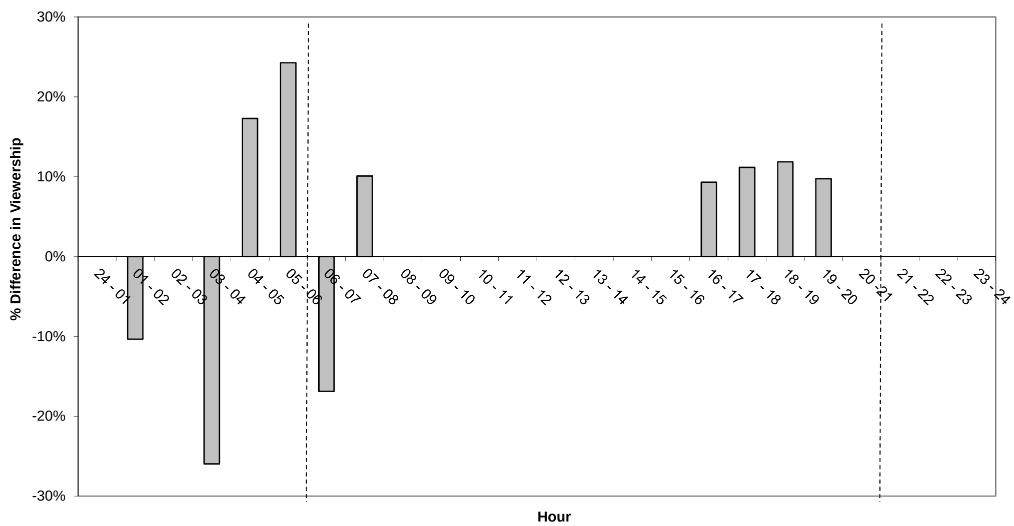


Chart shows coefficients on interactions between the OneDay69 policy variable and hourly dummies in the regression of Columns 3 and 4 of Table 5 but with OneDay69 and OneDay78 interacted with each hour separately. Coefficients are shown only if significant at the 10% level or better. The vertical dotted lines demarcate the restricted period.

Appendix K
Detailed Welfare Benefit Estimates

Number of Restricted Activity Days: Matus, *et al.* (2012) provide an exposure-response (ER) function of 0.0541 (0.0475, 0.0608)[4] additional restricted activity days per year-adult-$\mu g/m^3$ increase in PM_{10} concentration. A 13.2 $\mu g/m^3$ decrease in PM_{10} concentration due to the driving restrictions and a Beijing adult population of 9.2 million implies 6.6 (5.8, 7.4) million fewer restricted activity days.

Number of Acute Mortality Cases: Matus, *et al.* (2012) provide an ER function of a 0.06% (0.04%, 0.08%) increase in the mortality rate per $\mu g/m^3$ increase in PM_{10} concentration. Given total Beijing population of 10.9 million and a mortality rate of 0.55% (2007 data from *Beijing Health Yearbook 2008*) this implies 477 (318, 637) fewer deaths per year from the pollution reduction under the driving restrictions.

Acute Mortality Value –Lower Bound: Death from acute exposure normally hastens death by about 0.5 years (Matus, *et al.*, 2008). Therefore, a human-capital estimate of the value of lost life is one-half year's wages or RMB 23,566 (average daily wage of RMB 189 for 125 work days per half-year). Applying this to the number of cases yields annual welfare gains of RMB 11 (8, 15) million.

Acute Mortality Value –Upper Bound: Hammitt and Zhou (2006) use a contingent valuation method to estimate a mean value-of-statistical-life (VOSL) in Beijing of RMB 147.3 thousand.[5] Applying the VOSL to the number of cases yields benefits of RMB 70 (47, 94) million.

Appendix L
Penalties for and Detection of Driving Restrictions Violations

Violation penalties include monetary and time costs and depend on the detection method. Violators are immediately fined RMB 100 and incur a time cost because payment requires going to the relevant police station for documentation and then to a bank to pay. The latter step can be done online but only if the recipient has an account at the Industrial and Commercial Bank of China. The driver can delegate these tasks to someone with a lower cost of time by loaning them their national identity card. If a police officer detects the violation, it must be paid within fifteen days or interest is accrued at RMB 3 per day. For violations detected by cameras there is no immediate deadline. Regardless of how detected, the fine must be paid before renewal of the vehicle's bi-annual registration. During our sample period, only one penalty could be issued per day.[6]

A first-time violation would also trigger the loss of several fee waivers. Those complying with the OddEven restrictions received a waiver of three months' vehicle taxes (about RMB 100)[7] and highway maintenance fees (about RMB 330).[8] During the OneDay period the waiver equaled one month's fees. During both the OddEven and OneDay periods, a driver received a discount on auto insurance equal to the number of days their car was restricted. Although the precise amount depended on individual premiums, the average reduction was RMB 65 during the OneDay69 period.[9]

Beijing had 1,958 traffic surveillance cameras as of March 31, 2009 and the number increased to 2,215 by the end of 2009. This equals 0.13 cameras per square kilometer if equally spaced.[10] As of October, 2010 Beijing had about five thousand police officers to direct traffic.[11]

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[4] We provide lower and upper bounds in parentheses.
[5] The authors estimate a value of USD 16,000 (in 1999 terms). We convert to RMB as of July 1, 1999 (www.xe.com) and adjust for inflation using "Beijing by Data: 30 Years since Reform and Opening" (China Statistic Press, 2008). The authors' survey methodology may understate VOSL by up to ten times (page 415). To be conservative, we use their main estimate.
[6] As of December 24, 2010 the law was changed to allow multiple citations to be issued per day.
[7] Annual vehicle taxes ranged from RMB 300 to 600 depending on vehicle size according to Beijing Local Taxation Bureau Document Nos. 329 (2004) and 339 (2007).
[8] Until December 31, 2008, monthly highway maintenance fees for passenger vehicles were RMB 22 for each seat of capacity according to the Beijing Highway Bureau (http://www.ylfzhj.bj.cn). For a common passenger vehicle with five seats the monthly fees would therefore be RMB 110. After December 31, 2008, the fees were absorbed into fuel taxes and not affected by a violation.
[9] According to China Insurance Regulatory Commission Beijing Bureau (http://www.china-insurance.com/newscenter/newslist.asp?id=132329).
[10] Data from Beijing Traffic Management Bureau, accessed at http://www.bjjtgl.gov.cn. Density calculated based on Beijing's land area of 16,411 square kilometers.
[11] According to http://www.chinanews.com/gn/2010/10-11/2579335.shtml.

Appendix M
Detailed Compliance Results

Panel A: Comparison of Expected (Weekend) and Observed (Weekday) Distributions of Ending License Plate Numbers Entering a Beijing Parking Garage during Restricted Hours (7:00 am - 8:00 pm) from June 27 to July 3, 2010 - Regular Parkers

The top panel shows the expected distribution from the two weekend days (June 27 and July 3). The second panel shows data for the Monday (June 28) restricted hours, when plate numbers “1” and “6” were banned:

- The first two rows show the observed distribution of plate numbers.
- The third row tests whether each plate’s proportion during the restricted hours is significantly greater than zero using a one-tailed test. Plain text indicates that the proportion is not significantly greater than zero (plates “1,” “4,” and “6”) and bold indicates that it is statistically greater than zero (all other plates).
- The fourth row tests whether the observed proportion of each non-restricted plate differs from the expected proportion using a two-tailed test. In doing so, we adjust the expected distribution for the fact that there should be no “1” and “6” plates (*i.e.*, we compute the expected proportion assuming only the presence of the eight other plates). Bold significance levels indicate that the plate appears in statistically greater proportion than expected (none), those in bold italics indicate that it appears in significantly lower proportion than expected (plates “2” and “3”) and those in plain text that it is not significantly different (all others).

The data for the other weekdays is in the same format. Restricted numbers are shown in boxes.

Distribution	0	1	2	3	4	5	6	7	8	9	Total	No Plate
Expected Distribution (Weekend)												
Number	635	534	594	597	83	593	753	636	743	807	5,975	96
Percentage	10.6%	8.9%	9.9%	10.0%	1.4%	9.9%	12.6%	10.6%	12.4%	13.5%	100.0%	1.6%
Observed Distributions												
Monday (1, 6 Restricted)												
Number	398	45	312	315	54	380	67	400	486	490	2,947	28
Percentage	13.5%	1.5%	10.6%	10.7%	1.8%	12.9%	2.3%	13.6%	16.5%	16.6%	100.0%	1.0%
Different from Zero (SL)[1]	**0.0%**	20.2%	**0.0%**	**0.0%**	15.8%	**0.0%**	10.6%	**0.0%**	**0.0%**	**0.0%**		
Different from Expected (SL)[2]	54.7%		***3.2%***	***3.7%***	67.3%	34.5%		50.8%	14.1%	93.8%		
Tuesday (2, 7 Restricted)												
Number	357	319	50	325	63	339	436	63	440	456	2,848	26
Percentage	12.5%	11.2%	1.8%	11.4%	2.2%	11.9%	15.3%	2.2%	15.4%	16.0%	100.0%	0.9%
Different from Zero (SL)[1]	**0.0%**	**0.0%**	17.2%	**0.0%**	11.6%	**0.0%**	**0.0%**	11.6%	**0.0%**	**0.0%**		
Different from Expected (SL)[2]	34.3%	29.6%		18.8%	**4.8%**	44.9%	46.7%		31.2%	35.5%		
Wednesday (3, 8 Restricted)												
Number	353	270	327	31	43	351	453	393	75	447	2,743	29
Percentage	12.9%	9.8%	11.9%	1.1%	1.6%	12.8%	16.5%	14.3%	2.7%	16.3%	100.0%	1.1%
Different from Zero (SL)[1]	**0.0%**	**0.0%**	**0.0%**	27.6%	20.4%	**0.0%**	**0.0%**	**0.0%**	**7.3%**	**0.0%**		
Different from Expected (SL)[2]	35.4%	***4.7%***	30.4%		30.7%	26.4%	15.2%	**8.2%**		30.9%		
Thursday (4, 9 Restricted)												
Number	382	375	333	369	0	409	492	372	526	79	3,337	29
Percentage	11.4%	11.2%	10.0%	11.1%	0.0%	12.3%	14.7%	11.1%	15.8%	2.4%	100.0%	0.9%
Different from Zero (SL)[1]	**0.0%**	**0.0%**	**0.0%**	**0.0%**	N/A[4]	**0.0%**	**0.0%**	**0.0%**	**0.0%**	**8.3%**		
Different from Expected (SL)[2]	29.9%	14.9%	***3.8%***	56.4%		22.1%	71.4%	13.6%	**5.7%**			
Friday (0, 5 Restricted)												
Number	69	349	340	373	46	68	402	348	497	533	3,025	39
Percentage	2.3%	11.5%	11.2%	12.3%	1.5%	2.2%	13.3%	11.5%	16.4%	17.6%	100.0%	1.3%
Different from Zero (SL)[1]	10.2%	**0.0%**	**0.0%**	**0.0%**	20.0%	10.6%	**0.0%**	**0.0%**	**0.0%**	**0.0%**		
Different from Expected (SL)[2]		26.8%	33.8%	66.6%	60.9%		***2.2%***	***8.8%***	**7.4%**	10.5%		

Ending license plate numbers of autos entering a Beijing parking garage inside the 4th Ring Road collected by authors. [1] SL = significance level. Bold indicates significantly greater than zero (at the 10% level or better) using a one-tailed equality of proportions test. [2] SL = significance level. Bold indicates significantly greater (at the 10% level or better) than expected proportion (assuming restricted plates occur in proportion zero) using a two-tailed equality of proportions test and bold, italics significantly lower. [3] No observations - significance level is undefined. Boxes indicate restricted plate numbers on that day.

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Appendix M
Detailed Compliance Results

Panel B: Comparison of Expected (Weekend) and Observed (Weekday) Distributions of Ending License Plate Numbers Entering a Beijing Parking Garage during Non-Restricted Weekday Hours (9:00 pm - 6:00 am) from June 27 to July 3, 2010 - Regular Parkers

The top panel shows the expected distribution from the two weekend days (June 27 and July 3). The second panel shows data for the Monday (June 28) non-restricted hours:

- The first two rows show the observed distribution of plate numbers.
- The third row provides test statistics comparing the observed proportion of each plate to the expected based on a two-tailed test. Bold font indicates that the observed proportion is significantly greater than expected (none), bold italics lower (none), and plain text not significantly different (all plates).

The data for the other weekdays is in the same format. Restricted numbers are shown in boxes.

Distribution	0	1	2	3	4	5	6	7	8	9	Total	No Plate
Expected Distribution (Weekend)												
Number	635	534	594	597	83	593	753	636	743	807	5,975	96
Percentage	10.6%	8.9%	9.9%	10.0%	1.4%	9.9%	12.6%	10.6%	12.4%	13.5%	100.0%	1.6%
Observed Distributions												
Monday (1, 6 Restricted)												
Number	7	3	4	2	1	3	7	4	7	4	42	2
Percentage	16.7%	7.1%	9.5%	4.8%	2.4%	7.1%	16.7%	9.5%	16.7%	9.5%	100.0%	4.8%
Different from Expected (SL)1	20.6%	68.4%	92.8%	25.9%	58.5%	54.8%	42.9%	81.4%	40.8%	45.1%		
Tuesday (2, 7 Restricted)												
Number	13	9	2	9	1	4	11	6	14	7	76	2
Percentage	17.1%	11.8%	2.6%	11.8%	1.3%	5.3%	14.5%	7.9%	18.4%	9.2%	100.0%	2.6%
Different from Expected (SL)1	**7.0%**	37.9%	**3.4%**	59.3%	95.7%	17.6%	62.6%	43.9%	11.7%	27.5%		
Wednesday (3, 8 Restricted)												
Number	7	4	2	6	2	5	9	5	5	5	50	2
Percentage	14.0%	8.0%	4.0%	12.0%	4.0%	10.0%	18.0%	10.0%	10.0%	10.0%	100.0%	4.0%
Different from Expected (SL)1	44.2%	81.7%	16.1%	63.7%	11.9%	98.6%	25.3%	88.3%	60.3%	47.0%		
Thursday (4, 9 Restricted)												
Number	1	2	4	0	0	2	8	1	1	0	19	0
Percentage	5.3%	10.5%	21.1%	0.0%	0.0%	10.5%	42.1%	5.3%	5.3%	0.0%	100.0%	0.0%
Different from Expected (SL)1	44.8%	80.9%	10.7%	14.6%	60.5%	93.0%	**0.0%**	44.7%	34.4%	***8.5%***		
Friday (0, 5 Restricted)												
Number	6	9	13	10	3	3	14	9	11	5	83	0
Percentage	7.2%	10.8%	15.7%	12.0%	3.6%	3.6%	16.9%	10.8%	13.3%	6.0%	100.0%	0.0%
Different from Expected (SL)1	31.7%	54.6%	**8.5%**	53.5%	**8.9%**	***5.5%***	24.6%	95.3%	82.3%	***4.7%***		

Ending license plate numbers of autos entering a Beijing parking garage inside the restricted area collected by authors.[1] SL = significance level. Bold indicates significantly greater (at the 10% level or better) than expected proportion using a one-tailed equality of proportions test, bold italics indicates significantly less (at the 10% level or better) than expected proportion using a two-tailed equality of proportions test. Boxes indicate restricted plate numbers on that day.

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Appendix M
Detailed Compliance Results

Panel C: Comparison of Expected (Weekend) and Observed (Weekday) Distributions of Ending License Plate Numbers Entering a Beijing Parking Garage during Restricted Hours (7:00 am - 8:00 pm) from June 27 to July 3, 2010 - Monthly Parkers

The top panel shows the expected distribution from the two weekend days (June 27 and July 3). The second panel shows data for the Monday (June 28) restricted hours, when plate numbers "1" and "6" were banned:

- The first two rows show the observed distribution of plate numbers.
- The third row tests whether each plate's proportion during the restricted hours is significantly greater than zero using a one-tailed test. Plain text indicates that the proportion is not significantly greater than zero (plates "1," "4", and "6") and bold indicates that it is statistically greater than zero (all other plates).
- The fourth row tests whether the observed proportion of each non-restricted plate differs from the expected proportion using a two-tailed test. In doing so, we adjust the expected distribution for the fact that there should be no "1" and "6" plates (*i.e.*, we compute the expected proportion assuming only the presence of the eight other plates). Bold significance levels indicate that the plate appears in statistically greater proportion than expected (plates "3" and "5"), those in bold italics indicate that it appears in significantly lower proportion than expected (plate "8") and those in plain text that it is not significantly different (all others).

The data for the other weekdays is in the same format. Restricted numbers are shown in boxes.

Distribution	0	1	2	3	4	5	6	7	8	9	Total	No Plate
Expected Distribution (Weekend)												
Number	14	20	15	7	1	9	27	20	29	26	168	3
Percentage	8.3%	11.9%	8.9%	4.2%	0.6%	5.4%	16.1%	11.9%	17.3%	15.5%	100.0%	1.8%
Observed Distributions												
Monday (1, 6 Restricted)												
Number	46	3	46	56	6	60	6	60	58	70	411	1
Percentage	11.2%	0.7%	11.2%	13.6%	1.5%	14.6%	1.5%	14.6%	14.1%	17.0%	100.0%	0.2%
Different from Zero (SL)[1]	**0.8%**	44.1%	**0.8%**	**0.1%**	38.3%	**0.1%**	38.3%	**0.1%**	**0.1%**	**0.0%**		
Different from Expected (SL)[2]	96.9%		77.4%	**1.6%**	57.6%	**3.3%**		66.7%	***1.3%***	31.0%		
Tuesday (2, 7 Restricted)												
Number	26	27	3	21	3	28	36	5	44	42	235	3
Percentage	11.1%	11.5%	1.3%	8.9%	1.3%	11.9%	15.3%	2.1%	18.7%	17.9%	100.0%	1.3%
Different from Zero (SL)[1]	**3.6%**	**3.1%**	42.2%	**7.6%**	42.2%	**2.6%**	**0.5%**	37.1%	**0.1%**	**0.1%**		
Different from Expected (SL)[2]	78.7%	39.3%		17.3%	61.9%	***9.3%***	28.4%		58.1%	80.7%		
Wednesday (3, 8 Restricted)												
Number	36	29	51	3	3	43	36	49	11	51	312	2
Percentage	11.5%	9.3%	16.3%	1.0%	1.0%	13.8%	11.5%	15.7%	3.5%	16.3%	100.0%	0.6%
Different from Zero (SL)[1]	**1.5%**	**4.2%**	**0.1%**	43.2%	43.2%	**0.4%**	**1.5%**	**0.1%**	26.3%	**0.1%**		
Different from Expected (SL)[2]	66.0%	10.3%	12.7%		80.4%	**2.6%**	***2.4%***	73.6%		51.9%		
Thursday (4, 9 Restricted)												
Number	25	23	21	27	0	34	38	26	31	11	236	2
Percentage	10.6%	9.7%	8.9%	11.4%	0.0%	14.4%	16.1%	11.0%	13.1%	4.7%	100.0%	0.8%
Different from Zero (SL)[1]	**4.3%**	**5.8%**	**7.6%**	**3.1%**	N/A[4]	**0.8%**	**0.3%**	**3.6%**	**1.5%**	23.2%		
Different from Expected (SL)[2]	72.1%	25.2%	68.3%	**2.4%**		**1.2%**	58.2%	46.0%	***8.8%***			
Friday (0, 5 Restricted)												
Number	1	47	41	54	3	9	66	61	59	66	407	4
Percentage	0.2%	11.5%	10.1%	13.3%	0.7%	2.2%	16.2%	15.0%	14.5%	16.2%	100.0%	1.0%
Different from Zero (SL)[1]	48.0%	**0.7%**	**1.6%**	**0.2%**	44.1%	32.6%	**0.0%**	**0.1%**	**0.1%**	**0.0%**		
Different from Expected (SL)[2]		54.1%	99.5%	**0.4%**	93.7%		58.5%	65.0%	15.1%	72.0%		

Ending license plate numbers of autos entering a Beijing parking garage inside the 4th Ring Road collected by authors. [1] SL = significance level. Bold indicates significantly greater than zero (at the 10% level or better) using a one-tailed test. [2] SL = significance level. Bold indicates significantly greater (at the 10% level or better) than expected proportion (assuming restricted plates occur in proportion zero) using a two-tailed test and bold, italics significantly lower. 3 No observations - significance level is undefined. Boxes indicate restricted plate numbers on that day.