Pollution and Human Capital Accumulation: Explicitly Causal Evidence from the Barack Obama Sanctions on Iran

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Abstract

We use granular data from standardized assessments in Tehran to evidence a substantial impact of pollution in the vicinity of a school on how much children, especially boys, achieve academically. Causal identification exploits that the 2010 US sanctions preventing the sale of refined petroleum products to Iran differentially impacted air quality at schools in the city, depending on the location of each with respect to the road network. Relative performance dropped at more road-exposed (variously-measured) schools. Road mass upwind matters four times more than downwind, consistent with a prevailing wind 80% from the west, discouraging competing interpretations. **Keywords: Air pollution - social costs - academic achievement.**

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1 Introduction

In this paper we provide evidence of a substantial negative causal impact of traffic-related air pollution (TRAP) on student learning.

To do this we exploit the sudden change in air quality in Tehran, the capital city of Iran, that followed the instigation of sanctions by the government of the United States in July 2010. The essence of our approach is as follows. In Tehran vehicles contribute between 70 and 85% of the total emissions of common air pollutants (Heger & Sarraf (2018), Naddafi et al. (2012)) making major roads the main pollution 'sources' in the city. The sanctions cut, within a few weeks and almost completely, the supply of refined petroleum products from outside the country. While Iran is a major producer of crude oil it had (and has to this day) limited refinery capacity. As locally-held stocks of imported fuel were exhausted the country returned to the use of dirtier, more coarsely refined fuel in its vehicles. Emissions of major pollutants from roads increased sharply and citywide air quality deteriorated with particularly marked deterioration close to major roads. We collect data on standardized test performance in the universe of elementary schools in Tehran and apply a dose-response differences-in-differences design to the performance of children before and after 2010 at schools varying in their degree of "road exposure".

We show the following: (1) The imposition of sanctions had a sudden and substantial impact on air pollution in the city, with the size of that effect increasing with proximity to major roads. (2) The negative impact of sanctions on a child's learning was increasing in the extent to which that child's school was road_exposed. We use two alternative metrics for road_exposure of a school, one a simple distance to closest major road, the other a count of lane-meters of major road within a circle of chosen radius. The effect is substantial. In our preferred specification, based on the density measure and a circle of 500m radius, performance of children at schools in the top quartile declined by 4.10 percentage points (0.31 of a standard deviation) compared to those studying in schools in the bottom quartile. (3) Road density in the semi-circle to the west (upwind) of a school has around four times more effect than density in the semi-circle to the east, closely consistent with the city being subject to a pronounced prevailing wind that blows from the west 80% of the time. This assuages concerns that there may be impacts of sanctions other than air pollution that are correlated with road_exposure and affect school performance. (4) While we are not able definitively to isolate the impact of individual pollutants the effects appear *not* to be driven by (coarse or fine) particulate matter.

The results point to a benefit of emissions control unaccounted for in previous assessments of air quality improvement programs. They can also motivate and inform policies specific to schools, such as location with respect to roads, building specification and management.

The literature to which we contribute is sparse. A number of studies confirm that prevailing levels of air pollution in the vicinity of a child's home and/or school are negatively correlated with academic performance (for examples Suglia et al. (2008), Wang et al. (2009)).[1] To the best of our knowledge there are three papers using methods that provide for causal inference.[2] First, Austin et al. (2019) uses variation in the timing of diesel school bus retrofits in the US to show that retrofitting significantly improved satellite-measured air quality and was also associated with local increases in standardized test scores. While an important study for understanding the benefits of the retrofitting program inference about the effect of changes in air quality more generally is difficult. Many of the children (around 55% of all K-12 school students) whose performance contribute to the test outcome measure travel by school bus where pollutant levels in the cabin are 4 to 12 times higher than directly outside (Beatty & Shimshack (2011)). Further, even children not traveling on the school buses are likely to disproportionately pollute in the micro-vicinity of schools and particularly at times when children are arriving and departing the school site on foot. Second, Heissel et al. (2021) compare achievement of students moving to schools upwind and downwind of major highways in Florida. They find that attending school downwind more than 60% of the time causes a 0.04 standard deviation reduction in performance. Third, Gilraine (2019) uses a regression discontinuity design exploiting that classrooms in the 11 schools within 5 km of the Aliso Canyon gas leak in Los Angeles were retrofitted with plug-in air purifiers, those beyond 5 km were not. Fourth, Hollingsworth et al. (2020) show an impact on the performance of children at schools close to NASCAR motor racing circuits when participants switched from use of leaded to unleaded fuel in its races in 2007.

The analysis here complements this nascent line of research. As well as offering

[1]Studies vary in the extent to which they make causal conjectures based on correlational analyses. Wang et al. (2009), for example, is transparent: "This is a cross-sectional, epidemiologic study with an ecologic design ... therefore inferences of causality are not possible" (page 1618). In their study linking TRAP to attention in a large sample of Spanish children Sunyer et al. (2017) conclude: "... the association of TRAP with attention adds to the evidence that air pollution may have harmful effects on neurodevelopment. Our study suggests that, *if shown to be a causal relationship*, traffic pollution, and in particular diesel emissions addressed here by measuring NO_2 and EC, could affect the cognitive development of children while at school" (page 188). Panel methods can go only some way to addressing cross-sectional concerns. Period to period variations in, for example, economic conditions or civic leadership plausibly cause changes in both environmental conditions and school performance at a particular location.

[2]There are also a small number of papers that focus on daily variation in air quality and how that affects student performance in standardized tests. The pioneering paper is Ebenstein et al. (2016) using Israeli data. Marcotte (2017) explore the role of both pollution and pollen. Zivin et al. (2020) show a reduced form association between exam scores and agricultural fires in the vicinity of exam venues in China.

alternative identification, the pollution levels in our setting are much above those in the US and other highly-developed countries. In 2016, ambient PM_{10} levels in Tehran averaged 77 μg/m3, almost four times the WHO recommended threshold of 20. As such the analysis here is the first to explore impacts at parts of the support relevant for most school-children in the large urban centres in India, China, Indonesia, Mexico and other poorer and middle-income countries. Our effect sizes prove much bigger than those found in US-based studies.

There are several mechanisms that plausibly link TRAP to reduced learning. Physical impacts such as cough (Neidell (2004)), watering eyes (Wiwatanadate (2014)), headaches (Dales et al. (2009)) plausibly influence the efficiency of learning of a child who is present at school on a particular day (or, equally, the efficiency of instruction by a teacher).[3] A substantial health science literature explores the impact of TRAP on the brain, of which De Prado Bert et al. (2018) provides a systematic review based on 234 studies identified in PubMed. These include evidence from both human and animal studies relating to exposure over various timescales to effects on development of white matter, functional integration in the brain and other short and long term damage (see in particular the extended Table 1 in De Prado Bert et al. (2018) and surrounding discussion).[4] They conclude that "Tremendous work has been done and is still ongoing to determine the mechanisms through which are pollutants reach the brain and induce damages at the cellular and molecular level. Current findings converge towards the hypothesis that TRAP neurotoxicity leads to neuroinflammation, oxidative stress, and/or neurovascular unit dysfunction, which in turn result in oligodendrocytes and/or myelin damage and loss of neurons or alterations in their morphology" (page 361). Regardless of the primitive channel exposure has also been credibly linked to a number of intermediate outcomes that plausibly inhibit learning including increased school absence (Romieu et al. (1992), Currie et al. (2009)), reduced sleep quality (Heyes & Zhu (2019)), memory (Margai & Henry (2003)), depressive mood (Zhang et al. (2017)) attentiveness (Sunyer et al. (2017)), and inability to concentrate (Chen & Schwartz (2009)).

In reality exposure seems likely to work simultaneously through a mixture of channels, making pinning down 'a mechanism' doomed to fail. Furthermore the most

[3]More generally there is a very large literature exploring the negative health consequences of pollution exposure over various time scales. A survey of that is beyond the scope of this paper. Two papers that explicitly examine the impact of TRAP on health (and also include insightful pointers to related research) are Knittel et al. (2016) and Currie & Walker (2011).

[4]For example, Pujol et al. (n.d.) conducted MRI and other testing of 8 to 12 year old Spanish children finding that: "Vehicle exhaust-related air pollution exposure was associated with brain changes of a functional nature...Children from schools with higher traffic-related pollution showed lower functional integration and segregation in key brain networks. Age and performance (*i.e.* motor response speed) both showed the opposite effect to that of pollution to brain function, thus indicating that high exposure is associated with slower brain maturation" (page 183).

obvious policy responses - namely reducing TRAP and/or protecting children from TRAP by moving schools or other interventions - do not require knowing which subset of channels are the most important. We return to policy implications later in paper.

2 Setting

Tehran is a major metropolis, with a daytime population of around 12.5 million (Heger & Sarraf (2018)). Seventeen million vehicular trips are made per day in Tehran (Hosseini & Shahbazi (2016)) and mobile sources account for about 70% of total emissions of the major pollutants (Heger & Sarraf (2018)). As such major roads are critical 'linear sources' of pollution in Tehran, as in many other major cities.

Emissions from roads disperse in ways that are sensitive to considerations such as wind conditions and urban design, and contribute to a "sleeve" of elevated pollution levels in neighborhoods close to those roads. A large engineering literature measures the role of distance in decay and a reading of two careful literature reviews (Karner et al. (2010) and Pasquier & André (2017)) points to ambient levels of the main TRAP pollutants decline with distance from roadside, returning to background levels within about 150 - 750m. The exceptions to this are fine and ultra-fine particulate matter that show either no trend with distance, or decay much more gradually. The Appendix provides a more detailed summary of this literature.

In June 2010 US Congress passed and President Obama signed the Comprehensive Iran Sanctions, Accountability and Divestment Act (CISADA). Iran had at that time (and still today) limited capability to refine its crude oil, and the main target of the sanctions was the supply of refined petroleum product to Iran. The sanctions prohibited US or non-US entities from (1) selling to Iran refined petroleum products; (2) selling or leasing to Iran, equipment or technology that could directly and significantly facilitate the maintenance or expansion of Iran's refining capability or (3) supplying any services that might facilitate such maintenance or expansion (for examples insurance, brokerage, shipping) (HFW-London (2010)).

The CISADA sanctions are widely regarded as being highly and swiftly effective. Imports of refined products to Iran fell from 120,000 barrels per day to 30,000 barrels per day within a few weeks (Katzman (2013)). As supply dwindled it was replaced by dirtier, more coarsely refined and more polluting alternatives.

Th impact on air quality in Tehran was substantial. Figure 1 provides two summary measures (we will present some analysis of the impact on individual pollutants later). (1) The upper panel shows that the number of days when air pollution reached unhealthy levels - that is days when the Air Quality Index (AQI) exceeded 100, the international standard - rose from around 50 per annum before sanctions to 200. (2)

Even though TRAP is a multi-pollutant mix, NO_2 is the usual marker used by researchers as a single-dimensional proxy (Hamra et al. (2015), Lavy et al. (2014)).[5] The lower panel plots the ambient concentration of NO_2 by quarter, averaged across the 22 pollution monitoring stations for which we have data. As a subsequent article in *The Atlantic* magazine put it: "It's not really geography's fault that Tehran's air is so filthy. Thanks to strict sanctions on refined gasoline imposed by the United States in 2010, all of Iran has struggled to come up with enough fuel for its cars, so the country has been improvising and mixing its own - call it bathtub gas. Its dirty stuff, too." (Estes (2013)).

2.1 Data

Our analysis requires two main things. First, a school by year panel of standardized test results. Second, a vector of school level measures of the degree of road exposure. These will capture treatment and outcome in a differences-in-differences framework. We also require air quality measures for the period studied.

Standardized test score Nahayee is the standard final test taken by all children in Tehran at the end of primary school. For the purpose of this study we focus on mathematics element of the exam, the component for which we have data. It is taken in June at the end of school year grade 5, when the child is aged 12 years. The setting and marking of tests is common across schools and the responsibility of he Ministry of Education, Directorate of Education for the Province of Tehran.

A panel of mean test score data by school, by year was obtained by e-mail application from June 2005 through June 2015 inclusive. The dataset provides schools name and sex (all schools in Tehran are single-sex). We then google each school to derive its exact geo-coordinates. The school by year mean is computed over those students that attend the exam. Those not attending, due to illness or other reasons, are dropped from the calculation of average. The panel on which we estimate contains 6,083 school/year means covering tests taken by approximately 640,000 children.

Road_exposure To calculate the extent to which a school location is exposed to major roads. For most of our analysis we will define a major road to be one with three or more lanes. These roads and the schools in our sample are represented in the map in Appendix Figure B.1 and an example of each type of road is presented in Appendix Figure B.2. In robustness tests we will consider two alternative maps that embed different definitions of major roads. For each school we calculated the distance and angle (orientation) to the nearest major road and the number of lane-meters of major

[5] A major report into social impacts of TRAP in Ontario, Canada (SOCAAR (2019)) use NO_2 as a TRAP marker noting that "Nitrogen oxides such as nitrogen monoxide and nitrogen dioxide are emitted in vehicle exhaust and are a good indicator of traffic pollution." (page 6).

road carriageway within 250, 500 and 750 full and half buffers (circles and semi-circles) using ArcGIS software. The interpretation of these measures is included in the text below and technical detail in the Appendix.

Pollution data was obtained from the Tehran Air Quality Company.[6] We collected pollution data from the 22 pollution monitoring stations in Tehran that were active during our period of study. The locations of the monitoring stations are mapped in Appendix Figure B.3. These provide daily mean measures of ambient concentrations of carbon monoxide (CO), particulate matter (PM_{10}), Nitrogen Dioxide (NO_2) and AQI.

Summary statistics for key variables are presented in Appendix Table A.1.

3 Empirical strategy

Our identification strategy is based on the observation that if pollution is detrimental to learning then, when major roads became sharply more prolific sources of pollution at the introduction of sanctions, we would expect to see decrements to student performance that were largest at those school that were the most 'road exposed'.[7] The lower the road exposure of a school, the smaller should be any detrimental impact of sanctions on students studying at that school.

This makes the question ideal for causal exploration using dose-response differences-in-differences. Our main estimating equation is

$$y_{idt} = \beta_0 + \beta_1 \text{road\_exposure}_{id} \times \text{sanction}_t + \Gamma_{dt} + \Phi_i + e_{idt}. \tag{1}$$

The outcome variable is the standard test performance of children in school i located in district d in year t (so for example $t = 2006$ means exams taken in June 2006, at the end of academic year 2005-06). Road_exposure$_{id}$ is a time-invariant measure for how 'exposed' or proximate school i located in district d is to major roads. Sanction is a dummy that takes value 1 for years 2011 and onward, zero otherwise. Tehran is divided into 22 districts. District-year fixed effects Γ_{dt} absorb yearly shocks that are common across schools within a district, such as district-level policy changes. School fixed effects Φ_i account for time-invariant characteristics of schools, including road_exposure but also building construction, micro-geography and urban design.

The coefficient of interest throughout the paper is β_1 which is the differences-in-

[6]The data is obtained from https://airnow.tehran.ir/home/DaiyAQIArchive.aspx.

[7]In principle the interpretation of our results could be confounded by the selection into and out of treatment, for example a parent moving a child between schools in response to sanctions. However in practice there are strong institutional and circumstantial reasons to think any such effect would be negligible in our setting.

differences parameter. However we will use two alternative metrics for road exposure.

Distance. This is simple straight-line measure from the school to the closest major road. It is a 'quick and dirty' measure, but replicates that used as a metric for road exposure by researchers in other contexts (for example Acemoglu et al. (2015), Fafchamps & Gubert (2007)). To 'set the scene' we will present some basic linear and nonlinear results using this measure. Here a value of the differences-in-differences parameter $\beta_1 > 0$ would be consistent with pollution from roads damaging learning (any decrement to performance being *larger* for children studying at schools closer to the closest major road).

Density. Our second and preferred road_exposure metric is a count of the number of lane-meters of major road within a circle or buffer of the school of radius 500m. The choice of 500m is informed by our reading of the engineering and atmospheric science literature on dispersion of pollution from roads (see Appendix), but for the purposes of robustness we will also report the main results derived using 250m and 750m buffer radii. In ancillary specifications we will allow for differential effects of road density in the upwind and downwind portions of the buffer (semicircles), and for road density in a series of concentric 'donuts' centred on the school. Here a value of the differences-in-differences parameter $\beta_1 < 0$ would be consistent with pollution from roads damaging learning.

The identifying assumption is that the only way in which sanctions could influence student performance in a manner correlated with proximity of a school to major roads was through differential effects on air quality.[8]

3.1 Road_exposure and air quality

Data on air quality is not available at a level granular enough to observe variations in air quality at the level of individual schools (the number of monitoring stations is not as great as the number of schools, nor are they co-located).

However we can provide two types of evidence to corroborate that impact of sanctions on pollution levels was biggest closest to roads. (1) The well-developed engineering and atmospheric science literature, summarized in the Appendix, that both models and measures air pollution and its dispersion in the vicinity of major roads. (2) An analysis of measured pollution data using monitoring stations as 'surrogate' school.

[8]It is difficult to think of confounders, even contrived, that are not dealt with by our design. If any effects working through confounding channels seem likely to work *against* the direction of our results. For example, if textbooks or other school supplies became rationed (and there is no evidence that they did) then it is plausible that first deliveries would be to schools close to roads, with supply then running out to more peripheral schools. Even if deliveries went to schools close to major roads *last,* the results from the wind direction analysis would require that the order of delivery was also sensitive to the school being on the downwind (east) versus upwind (west) side.

To do this we treat each monitoring station as if it were a school, compute for each the distance and density road_exposure metric, then estimate Equation 1 but with the student performance dependent variable y_{idt} replaced by p_{jdt} which is the pollution measured at monitoring station j located in district d on a date in academic year t.

The results of doing this are reported in Appendix Table A.2. The coefficients for both CO and NO_2 have signs consistent with the engineering literature at a high level of statistical significance using either road_exposure metric. That is sanctions had a negative effect on air quality that was increasing with road_exposure. The coefficient for PM_{10} does not come close to statistical significance at conventional levels (t statistics in columns 1 and 5 are 0.21 and 0.01).

4 Results 1: Distance

First we present results using the distance measure of road_exposure. Identification here relies on comparing performance before and after sanctions at schools like A in the upper panel in Figure 2 (close to a major road) with those such as B. The distribution of schools by distance is summarized in Appendix Figure B.4.

4.1 Linear

Main results are presented in Table 1. The dependent variable in each column is the mean score of a child at school i in the standardized test. The sparsest specification contains only year and school FEs and is reported in column 1. School FEs are excluded in column 2. Column 3 also includes district-year fixed effects, is our preferred specification in this table and corresponds to Equation 1. There are several things to note here. In each column the estimated coefficient of interest, that on the distance*sanction interaction, is positive and significant at a level much higher than 1%, with t-statistics in most cases around 10. The inclusion of school and district-year controls has little impact on point estimates.

The estimated coefficient in column 3 of 2.003*** has a differences-in-differences interpretation and implies that for each additional 100 meters distance from the nearest major road the decrement of performance associated with sanctions is reduced by 2.003 percentage points. With a mean exam score of 59.01/100 across the whole sample this implies a 'protective' benefit of being an extra 100m distance from a major road as 3.39% of score.

4.2 Non-linear

In addition to the linear model we also estimate a more flexible specification. To do this the distance*sanction regressor in equation 1 was replaced by a series of distance_bin*sanction regressors where for school i the dummy variable distance_bin took the value 1 if the distance of school i from the nearest major road fell into the interval of values covered by that bin, zero otherwise. The bins were defined by the distances 0 - 100m, 100m - 200m, 200m - 300m, and > 300m.

The results are in Table 2 with 100 - 200m the excluded category. As before, the inclusion of school and/or district-year fixed effects, make relatively little difference to the main coefficient estimates. The main specification is again that in column 3. The effect of sanctions on a school in the 200m to 300 bin was 3.281* percentage points smaller than on a school in the 100 - 200m reference category, and so on.

These results, plotted in Figure 3, also points to a roughly linear relationship between distance and effect size consistent with the linear specification reported above.

5 Results 2: Density

While the distance measure of road_exposure is easy to understand and conforms with that used in some existing research it is crude. Schools C and D in the lower panel of Figure 2 are each the same distance from their closest major road, so score equally on the distance metric, yet it is natural to think of D as having greater road_exposure because it has a greater 'amount' of road in its environs. To address this we develop a density metric, which computes the number of lane-meters of major road within (in the preferred case) a 500m buffer of the the school. On such a measure D scores much higher than C. It is difference-in-differences results based on this metric that are the focus of this section. The distribution of schools by this measure of road_exposure can be seen in Appendix Figure B.5.

5.1 Suggestive graphical exercise

Before turning to regressions we conduct a simple graphical exercise.

First, we identify the quartile of schools ranked most road_exposed by our preferred density measure with preferred 500m buffer radius, and the quartile of schools least road_exposed by the same metric. The schools in each quartile are dispersed widely across the city and its disparate neighborhoods. This can be seen visually from the map in Appendix Figure B.6 and in Appendix Figure B.7 that plots the count of schools from each quartile in each of the 22 districts into which Tehran is divided.

Second, for each year in our study period we take the mean score obtained by a student at a school in the first set and subtract the mean score obtained by a student studying at a school in the second.

The resulting differences, by year, are plotted in Figure 4. Prior to sanctions students in the low-road-intensity quartile of schools outperformed those in the high road_exposed by 1 to 2 percentage points on the standardized test. After sanctions that quickly settled to a deficit of 7 to 8 points.

These numbers are descriptive statistics not estimates. That they represent the difference between the mean performance in the two subsets of schools within-year implies that any year effects common across schools are differenced out, but no other factors are controlled for. Nonetheless it suggests that any negative impact of sanctions on performance was particularly marked at more road_exposed schools.

5.2 Linear

Table 3 presents the main linear results using the density measure for road_exposure. Column 3 again reports our preferred specification (corresponds to Equation 1).

The calculation of density requires choice of the radius of the circle or buffer around a school within which lane-meters are counted. For reasons already noted our preferred choice of radius is 500m and the middle panel of the table reports results with density calculated on that basis. The coefficient estimates in that middle panel are relatively stable across columns. The coefficient from the preferred specification, containing the full set of controls, is -0.043*** and is significant at a level much higher than 1%. The coefficient implies that each additional 1000 lane-meters of major road within 500m of school i increases the decrement associated with sanctions to the mark of a student at that school by 0.43 percentage points. Given the distribution of density across schools (Appendix Figure B.5) a one standard deviation increase in the number of lane-meters increases the decrement by 2.89% or 0.22 of performance standard deviation.

The top and bottom panels in Table 3 repeat this exercise for alternative buffer sizes, namely buffers of radius 250m and 750m. The relative size of effects across the three panels is as expected with the addition of extra road density generating a larger decrement when within a tighter buffer of the school (250m). However caution is needed in comparing the estimates between panels. As the buffer size is increased we not only expect the true potency of the additional road_exposure to be reduced, but also expect road_exposure to become an increasingly noisy proxy for air quality at the school. Insofar as measurement error is a problem we would expect attenuation of estimates to be greatest in the wider buffers.

5.3 Non-linear

We also estimate a more flexible specification. To do this the density*sanction regressor in Equation 1 was replaced by a series of density_bin*sanction regressors where for school i the dummy variable density_bin takes the value 1 if the density of road within the 500m of school i from the nearest major road fell into the interval of values covered by that bin, zero otherwise.[9] The bins were defined by the the count of lane-meters of main road within the buffer being 0 - 5,000m, 5,000 - 10,000m, 10,000 - 15,000m, 15,000m - 20,000m, and $>$ 20,000m.

The results are in Table 4 and the associated Figure 5. The coefficients reported are those on the density_bin*sanction regressors. The excluded or reference category is the 0 - 5,000m bin and the specifications across columns again repeat those in previous results tables. Compared to the bin containing the least road_exposed schools (those in the excluded bin) the decrement to performance is significantly larger for children studying in schools in each of the higher density bins, with the relationship roughly linear. Relative to a school in the least road_exposed reference category, performance at a school in the 15,000 - 20,000 bin (which includes, for example, the school at the 75th percentile) was reduced by 3.77* percentage points, and so on.

5.4 Donuts

In Table 4 the coefficient of interest was that associated with the density*sanction regressor. The three horizontal panels were differentiated by the radius of buffer used to calculate the density measure, such that each coefficient estimate in the table was derived from a separate regression.

In this section we conduct an alternative specification in which we include simultaneously in the regression the count of lane-meters of major road within each of three concentric 'donuts' delineated by our three buffer radii. In other words within 250m of the school, between 250 and 500m of the school and between 500 and 750m of the school.

This serves two purposes. First, it sheds light on how rapidly the impact of additional road_exposure declines with how far away that road is from the school. Second, it speaks to robustness of our results in the face of potential omitted variables bias in the results using a single buffer (Table 4). For example, suppose the density of roads within 250m of a school is positively correlated with density between 250 and 500m.[10] In that case the top right hand coefficient in Table 4 would be biased upwards,

[9]To prevent proliferation of cases we only report this exercise for the preferred buffer size of 500m. Results using the 250m and 750m buffer sizes give consistent results.

[10]It is. Appendix Table A.3 reports the school-level correlations between road density in each of the donuts. All are positive.

overstating the true impact of additional lane located in that smaller buffer, and so on.

The results of this exercise are reported in Appendix Table A.4 and (in the main exhibits) Figure 6. Note that in this specification the figure presents estimates from a *single* regression, namely that reported in column 5 of the table. The table and figure make clear that any total effect is likely 'driven' by road density within the tightest, 0 - 250m zone. The coefficient on density in that bin is 5 to 10 times larger in absolute value than that on the others with an associated t-statistic of over 19. In terms of the extent of bias to the single-buffer model it is reassuring that the coefficient in the top-right cell here (-0.195***) is not very different to that in the top-right cell in Table 3 (-0.168***). The other coefficients in this table are not directly comparable with their counterparts in Table A.4 as they relate to donuts not circles.

5.5 Upwind/downwind

The specifications reported so far assume pollution at school i to be increasing in the density of roads within either a circular buffer or else a donut centred on the school, regardless of the spatial arrangement within that buffer or donut.

In fact Tehran is subject to a pronounced prevailing wind from the west. The wind roses relating to wind directions during the months that comprise the school (September through May) are in Appendix Figure B.9. During these nine months wind blows from the West slightly over 80% of the time. As such, if our hypothesis that wind-borne pollution from roads is inhibiting student learning is correct, it should be the case that roadway in that part of the buffer upwind of the school should play a more important role than the equivalent amount of roadway downwind.

To investigate this we refine the buffer data and for each school calculate separately the lane-meters of major road in the upwind and downwind semi-circles or half-buffers formed by cutting the circle with a north to south line through the centre. Road in that part of the buffer west of the school is upwind, east is downwind. Appendix Figure B.10 shows the variation in upwind and downwind road_exposure so computed.

Table 5 presents the results of running regressions using these refined measures. Results are again reported for three alternative buffer radii (250, 500 and 750m). Our preferred radius remains 500m.

The coefficient estimates in columns 4 and 5 in Table 5 relate to the impact of additional lane-meters of major road located in the upwind and downwind half-buffers respectively, implied by regressions that include each measure separately. Taken at face value the estimates suggest that road in the upwind zone is around twice as potent as that in the downwind (-0.75*** compared to -0.042***).

However, upwind and downwind density is mildly positively correlated (the correlation coefficient is 0.22, see Appendix Figure B.11 for a scatterplot). As such the estimates in both columns 5 and 6 are subject to omitted variables bias that lead the coefficient estimates to be inflated in absolute value. Including both upwind and downwind density measures in the same regression delivers the estimates in column 6. Additional road density in either half-buffer has a negative effect on our differences-in-differences estimate. However, the coefficient on upwind density is now 3.8 times the magnitude of that on downwind (-0.069*** vs. -0.018). This is consistent with the observation that wind direction in the city is from the Western versus Eastern semi-circle approximately 80% of the time (recall the school-year wind roses in Appendix Figure B.9).

Columns 1 through 3, and 7 through 8, repeat these exercises for the alternative buffer radii. All results are consistent with expectation.

As well as allowing investigation of the relative impact of upwind and downwind emission this exercise represents an additional falsification exercise. Consider the possibility of factors other than air pollution but correlated with road_exposure that impacted school performance post-sanction. That upwind and downwind road_exposure half-buffers are found to having substantially different effects implies any confounding mechanism would have to be correlated not only with the location of a school with respect to roads, but also with respect to school-year wind patterns.

5.6 Cohort cross-sections (density)

As a final approach we return to our preferred road_exposure metric, namely the single buffer of radius 500m, but instead of differences-in-differences estimate a series of cohort cross-sections. That is we regress mean performance of a student at school i on road density of school i in each annual cross section in turn.

The results of this exercise are reported in Table 6 and the associated Figure 7. Each dot in the figure comes from a separate cross-sectional regression. While performance was negatively associated with road_exposure in each year for which we have data, the coefficient on that association was much smaller in value and only sometime statistically significant before 2011.

Figure 7 also provides compelling visual evidence of the absence of any pre-sanction trend.

Figure 7 points to a 'half' step down in 2011, then a further step in 2012, with the treatment effect thereafter close to constant. Since school cohorts taking exams in 2012 through 2015 had been exposed to two or more years of treatment, compared to the 2011 cohort which received only one, it is tempting to think that damage to learning might be cumulative, with accumulation extended over a period of more than a year (but no more than two). While the results are consistent with that we are careful not to over-interpret given that the link from sanctions to reduced air quality did not happen sharply at the start of the 2010/11 school year, such that the sample of students taking the test in June 2011 may have been only partially treated.[11]

6 Heterogeneity and robustness

Collectively we believe that the results of the various exercises reported provide persuasive evidence that the introduction of sanctions had an effect on performance that was sharply increasing in road_exposure. In this section we present the results of a series of additional heterogeneity and robustness exercises. Most of these are reported in Table 7. Each is conducted for both the distance and density road_exposure metrics, though we focus on the latter which is our preferred.

Sex Junior schools in Tehran are single-sex. School fixed effects included in our main specifications absorb any time-invariant characteristic of a school that might contribute to pupil performance, including the sex of the pupils it educates. However it is possible that the size of treatment effect would vary between male and female children. Prior literature that has explored the longer term effect of pollution on children has tended to find boys more sensitive on various measures for a given intensity of exposure (for example early life lead exposure, Aizer & Currie (2019), Grönqvist et al. (2019)). In addition systematic differences in how male versus female schools are run might drive differences in exposure. For example boys in Iranian schools plausibly spend more time playing outdoors than their female counterparts. In columns 2 and 3 the main specification is re-estimated on the sample of male and female schools separately. Consistent with priors the effect size is more pronounced among males (-0.049*** vs. -0.033***).

Affluence Despite all schools in the sample being state-operated and so in principle funded to similar levels, the financial status of the neighborhood in which a school is located might impact the sensitivity of the school population to worsened external environment. To probe this we conduct two exercises. First, in columns 4 and 5 of Table 7 we report estimates of the preferred specification on schools in 'rich' and 'poor' neighborhoods as defined in Movahhed et al. (2016). The estimated coefficient is somewhat smaller in the rich sample. Estimates in each subsample remain positive and significant at conventional levels (-0.042*** and -0.031*) though precision of estimates is eroded in these smaller samples, and we would not want to over-interpret these

[11]Evidence points to full impact of sanctions on air quality conditions being achieved by the end of calendar year 2010, approximately four months into the 2010/11 school year.

differences. Second, the southern part of Tehran is commonly regarded by those who know the city as the poorer half, with the richer neighborhoods more concentrated in the north. In columns 6 and 7 of Table 7 respectively we report the outcome of estimating the preferred specification on schools in 'north' and 'south' neighborhoods. The estimated coefficient of interest is similar across the subsamples (-0.046*** versus -0.041***).

Districts Tehran is divided into 22 districts. Some elements of school policy are controlled at district level as are some other areas of policy that might affect a child's academic development, such as social housing. Districts also vary in socioeconomic profile. While our main specifications contain district-year fixed effects the specification reported in column 8 of Table 7 additionally incorporates a vector of road_exposure*sanction*district_FE interaction controls. These allow for the impact of treatment to vary by districts. The sign and significance of the estimated coefficient sustains while the magnitude is larger. For ease of interpretation we do not include these interactions in our preferred version but this column suggest that the exclusion makes our central results conservative.

2011 As already detailed the sanctions were signed into law in mid-June 2010. However it took time for them to take full effect. Individuals and organizations (such as petrol retailers) in Iran took time to deplete stocks, shipments already en route were allowed to complete their deliveries, etc.. As such air quality was not fully deteriorated until some time in the Fall of 2010. While we categorized 2010 as pre-treatment, 2011 as treated since the onset of treatment was neither sharp nor did it coincide precisely with the start of a school-year implies some "fuzziness" in the definition of the treated period. In particular children taking tests in June 2011 could be regarded as only partially-treated (treated for around half the school-year). In column 9 we re-estimate the preferred specification but excluding 2011 altogether. This disturbs results only slightly (with the main estimate now -0.047*** vs. -0.043***).

Major road definition The road_exposure measure for each school was based on a lane-based definition of major roads. We generated two alternative major road maps for the city based on maximum speed limit being greater than 50 km/h ('Alternative map 1') and roads with more than 2 lanes ('Alternative map 2'). Columns 10 and 11 of Table 7 report the results of re-estimating the preferred specification using these alternative maps. Estimates coincide in sign and magnitude with our main estimates and in both cases are significant at a level much higher than 1%.

Outliers To investigate whether our central results are driven by outliers we conduct three exercises. First, in their work exploring health impacts of wind-borne pollution from roads Anderson (2019) and Barreca et al. (2011) identify the risk of measurement error attenuating estimates of near-highway treatment effects (see Anderson (2019) pages 11 to 12 for discussion). While schools within that 50m corridor are not excluded from our main analysis we report in column 12 the result obtained with such exclusion. Second, in columns 13 and 14 we report the results under when the outcome variable and distance or density variable winsorized at 5%. In each of columns 12 through 14 results are little disturbed.

Alternative standard errors The standard errors reported as part of our main results were clustered on school. In Table A.5 we report five alternative approaches to calculation of standard errors that we might plausibly have adopted. In each case significance of the main result is sustained at a level much higher than 1%. Further the preferred standard error is the largest among those in this table, suggesting main results are conservative.

Placebos Variation in treatment in our analysis has been based on school location with respect to major roads, so as a test of design we conduct placebo exercises based on falsely-assigned location. To do this we randomize locations within the sample of school, such that each school i is falsely associated with the location (of some other school j). We then re-estimate our preferred specification, that in column 3 of Table 3, middle panel (density measure, 500m buffer), and record the t-statistic from the differences-in-differences parameter estimate. We then randomize and re-estimate repeatedly, in each case harvesting a t-statistic, until we have 1000. The bar-chart of t-statistics collected is plotted in Appendix Figure B.12. The pattern of results is as would be expected if running a series of regression using an irrelevant regressor. The t-statistics vary roughly symmetrically around zero, occasionally (spuriously) reaching significance against the conventional benchmarks of 1.96 and 2.58. In none of the 1000 repetitions does the t-statistic approach that from the version with the correctly assigned location (-5.81).

7 Some additional considerations

7.1 Exposure

We do not know where children live or how/where they spend their time when not in school. The inability to observe actual exposure is shared with almost all research on the health and non-health impacts of pollution, and we acknowledge it as a limitation of the study.[12] What we do know is where the child typically resides from approximately 8 AM to 4 PM for around 200 days during the school-year, and we have been explicit that what we identify is the effect of changing the ambient level of TRAP in the

[12] There are a tiny number of pollution and health studies that have exposure measured at the level of individual subjects via pollution monitors worn on the person.

vicinity of the school at which the child is enrolled. Insofar as exposure that the child receives during leisure (non-school) time is orthogonal to TRAP at the school location our inability to observe and control for the non-school exposure would not bias results. Given that we are considering micro-geography - locations within a a few down meters of each other, and in some specifications immediately upwind versus downwind of a road - orthogonality is possible. However we cannot verify it and as such main results should be interpreted in that light.

The importance of this limitation is mitigated by observing three things that make pollution most pronounced at times when school is in session, making it likely that a large portion of a typical child's total exposure will indeed be received while at school. (1) Air quality varies substantially *between months* in Tehran, at its worst in October through January, with July and August being by far the cleanest months (Heger & Sarraf, 2018, Figure 3). (2) Traffic volume is greater on school (work) than other days. (3) The most polluted hours within a day are those associated with the morning peak of 8 AM for private cars, buses and taxis, 11 AM - 1 PM for pickups and trucks, 11 AM to 1 PM for motorcycles (Hosseini & Shahbazi (2016)).

Also related, we know little about the physical characteristics of individual schools. Anecdotal evidence from those with local experience is that air filtering technology - which might isolate children inside from ambient conditions outside - would be extremely rare to non-existent in our sample. The inclusion of school fixed effects in all specifications implies that insofar as a subset of schools did have such technology, with that technology invariant across that period, such that our estimates can be interpreted as incorporating that margin of adjustment.

7.2 Disentangling individual pollutants

Taken collectively we believe that the results presented to here provide persuasive evidence of the negative impact of TRAP on academic development.

However, TRAP embodies a cocktail of different pollutants, and for some purposes understanding the role of individual constituents is important.[13] To our reading the most common single pollutant marker or proxy for TRAP in the health and epidemiology literature is NO_2 (Hamra et al. (2015), Lavy et al. (2014)). The problem of isolating the role of individual pollutants out of the cocktail of pollution to which people are exposed on a 'bad air' day is a challenge throughout this literature, not least because variation in pollutant levels are often highly correlated. In *all* cases involving individual pollutants, researchers study at most a subset of pollutants, with

[13] In many cases such additional knowledge will not be important. For example if the policy under consideration is relocating schools away from major roads what matters is the causal link from TRAP to outcomes.

that subset typically dictated by data availability.[14] An alternative and increasingly popular approach (see for example Gendron-Carrier et al. (2018), Aldeco et al. (2019)) exploits data from NASA satellites that measure Aerosol Optical Depth (AOD). AOD measures how 'thick' or generically 'dirty' the air is at a particular GIS point, and while remote-sensing means geographical coverage is good, none of the research using such a measure allows for pollutant-by-pollutant inference.

Though neither our design nor data context allow us to isolate effects definitively, our results point to the effects that we find *not* being driven by larger particulates ($PM_{2.5}$ and PM_{10}). A mature engineering literature, some of which is summarized in the Appendix, points to the gradient in concentrations of these pollutants with distance from major roads being either very slight or non-existent over the distance ranges that we study, while the gradient for other common TRAP constituents (e.g. CO and Nitrogen Oxides) is very pronounced. This was consistent with our own analysis of data from the pollution monitoring network in Tehran, where the implementation of sanctions affected ambient levels of CO and NO_2 in a manner increasing in both of our road_exposure metrics, but for PM_{10} - the only particulate measure recorded by the Tehran monitoring system - there was no such sensitivity (recall Appendix Table B.2). The binned variants of our regression exercises also showed continued decline of effect outside very-close-to-road range (less than 100 m) in which some engineering studies have identified a decay in fine and coarse particulate concentrations.

However, it is very important to understand that this is *not* to say $PM_{2.5}$ and PM_{10} do not affect student learning. Rather that because the impact of sanctions on levels of these pollutants was common across schools (insensitive to our road_exposure measures) any such effects would be stripped out by the inclusion of district-year fixed effects in our main specifications. As such any effects through these pollutants would be additional to those that we uncover.[15]

[14]To take one example, in their excellent recent study on health outcomes Schlenker & Walker (2016) deploy only data on CO, NO_2 and Ozone. Indeed their central results, and all but one table in the paper, relate to exercises in which each of these pollutants is used as an explanatory variable separately, absent controls for the other two. However, they are explicit in "... acknowledging that we may be picking up the health effects of other pollutants" (page 787).

[15]Similar logic has been applied to health impacts of TRAP. "Elevated health risks associated with living in close proximity to roads is unlikely to be explained by $PM_{2.5}$ mass since this is only slightly elevated near roads. In contrast, levels of such pollutants as ultra-fine particles (UFP), carbon monoxide, NO_2, black carbon .... are elevated near roads. Individually or in combination, these are likely to be responsible for the adverse health effects. Current available evidence does not allow discernment of the pollutants or pollutant combinations that are related to different health outcomes." (WHO (2013)), response to Question C.1).

8 Conclusions

The paper presents evidence of a substantial negative causal impact of TRAP in the vicinity of a primary school on the academic development of children in that school.

As such we contribute to the developing knowledge-base regarding how air pollution - particularly in major urban centres - damages human well-being.

We will not repeat the results here, except to say the treatment effects are large, and prove robust to a range of alternative modelling variations and robustness exercises.

The policy implications of the results and related literature can be organized into two.

First, they lead us to re-evaluate upwards the social costs of TRAP, and with additional assumptions urban air pollution more generally. Monetary values of TRAP that feature in cost-benefit assessments of infrastructure projects, automobile regulations etc., focus more or less exclusively on health impacts. The work here points to an additional externality. Monetizing the dis-benefits of increased TRAP would require identifying the social cost of reduced primary educational attainment in Iran (or other setting), which is outside of the current scope of this paper.

The second set of implications has regard to the case for interventions at school level. In 2003 California prohibited the location of new elementary and secondary schools within 500 feet (153m) of "... a freeway or other heavy traffic corridor" (Senate of California Bill 352, chapter 668). Our results would point to an academic performance in addition to a health rationale for such a policy. Other recent evidence points to the benefits on academic attainment of the installation of air filtration equipment, even at ambient pollutant levels much lower than those observed in Tehran and most major metropolises (Stafford (2015), Gilraine (2019)). While such technology may be out of reach for many schools in most of the world's largest cities there are a range of simpler adjustments that can be effective in screening school-children from pollution, including planting of trees, closing windows and avoiding outdoor play.

We have identified a number of limitations of the study as we have progressed. In an ideal setting we would have individual measures of pollutant exposure (say from body-worn pollution monitors) and be able to track the development of particular children through time. Despite this not being available to us we believe that our design and the natural experiment that it exploits offer credible causal evidence. Future research could usefully address; (1) the external validity of results to other highly-populated cities in which TRAP is a major contributor to pollution (many cities in Asia, Latin America) and (2) the extent to which alternative policy interventions, including simple and cheap behavioral interventions, could partially- or wholly mitigate the effects identified.

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Table 1: Main (distance)

	(1) District-year exclusion	(2) School exclusion	(3) Preferred
distance*sanction	1.899***	2.109***	2.003***
	(0.418)	(0.401)	(0.415)
Observations	6,083	6,083	6,083
Year FEs	Y	N	N
School FEs	Y	N	Y
District-year FEs	N	Y	Y

Note: School level data. The dependent variable is test score. The coefficients are reported per 100 meters of distance. Standard errors clustered on school in parentheses. Each specification contains controls as indicated. * significant at 10% ** significant at 5% *** significant at 1%.

Table 2: Nonlinear (distance)

	(1) District-year controls	(2) School exclusion	(3) Preferred
(0 < distance <100) * sanction	-1.559	-1.527	-1.630
	(1.469)	(1.588)	(1.487)
(100 < distance <200) * sanction	-	-	-
	-	-	-
(300 < distance <200) * sanction	3.044	1.978	3.281*
	(1.946)	(2.155)	(1.988)
(300 < distance <963) * sanction	7.011***	9.256***	7.559***
	(1.587)	(1.740)	(1.597)
Observations	6,083	6,083	6,083
Year FEs	Y	N	N
School FEs	Y	N	Y
District-year FEs	N	Y	Y

Note: School level data. The dependent variable is test score. These regressions are estimated in the same manner as those in Table 1 but we replace distance with dummies for distance in each of the four bins. Reference category is the 100 to 200m bin. Standard errors clustered on school in parentheses. Each specification contains controls as indicated. * significant at 10% ** significant at 5% *** significant at 1%.

Table 3: Main (density)

	(1) District-year controls	(2) School exclusion	(3) Preferred
		250m buffer	
density*sanction	-0.166***	-0.177***	-0.168***
	(0.018)	(0.019)	(0.018)
		500m buffer (Preferred)	
density*sanction	-0.044***	-0.054***	-0.043***
	(0.007)	(0.007)	(0.007)
		750m buffer	
density*sanction	-0.009**	-0.016***	-0.008*
	(0.005)	(0.005)	(0.005)
Observations	6,083	6,083	6,083
Year FEs	Y	N	N
School FEs	Y	N	Y
District-year FEs	N	Y	Y

Note: School level data. The dependent variable is test score. The coefficients are reported per 100 meters of major road density. Standard errors clustered on school in parentheses. Each specification contains controls as indicated. * significant at 10% ** significant at 5% *** significant at 1%.

Table 4: Nonlinear (density)

	(1) District-year controls	(2) School exclusion	(3) Preferred
(0 < density <5,000) * sanction	-	-	-
	-	-	-
(5,000 < density <10,000) * sanction	-4.831**	-1.887	-2.025
	(2.249)	(2.131)	(2.143)
(10,000 < density <15,000) * sanction	-4.731**	-2.786	-3.082
	(2.246)	(2.079)	(2.093)
(15,000 < density <20,000) * sanction	-6.746***	-3.992*	-3.773*
	(2.264)	(2.156)	(2.186)
(20,000 < density <34,527) * sanction	-13.350***	-9.442***	-9.434***
	(2.113)	(2.044)	(2.020)
Observations	6,083	6,083	6,083
Year FEs	Y	N	N
School FEs	Y	N	Y
District-year FEs	N	Y	Y

Note: School level data. The dependent variable is test score. These regressions are estimated in the same manner as those in preferred specification in Table 3 but we replace density measure with dummies for each density bins. The reference category is null to 5000m bin. Standard errors clustered on school in parentheses. Each specification contains controls as indicated. * significant at 10% ** significant at 5% *** significant at 1%.

Table 5: Upwind/downwind density

	250m buffer			500m buffer (preferred)			750m buffer		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(upwind-density) * sanction	-0.254***	-	-0.209***	-0.075***	-	-0.069***	-0.018**	-	-0.019**
	(0.030)	-	(0.034)	(0.012)	-	(0.013)	(0.005)	-	(0.008)
(downwind-density) * sanction	-	-0.201***	-0.126***	-	-0.042***	-0.018	-	-0.006	0.001
	-	(0.034)	(0.039)	-	(0.013)	(0.014)	-	(0.008)	(0.008)
Observations	6,083	6,083	6,083	6,083	6,083	6,083	6,083	6,083	6,083

Note: School level data. The dependent variable is test score. Column 1, 4 and 7 estimate the preferred specification from Table 3 but replacing density measure with upwind density. Column 2, 5 and 8 estimate the preferred specification from Table 3 but replacing density measure with downwind density. Column 3, 6 and 9 estimate the preferred specification from Table 3 with upwind and downwind density at the same time. The coefficients are reported per 100 meters of upwind/downwind density. Standard errors clustered on school in parentheses. All regressions include school and district-year fixed effects. * significant at 10% ** significant at 5% *** significant at 1%.

Table 6: Cohort cross sections (density, 500m buffer)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Density	-0.012**	-0.001	-0.009	-0.010	-0.006	-0.002	-0.036***	-0.054***	-0.052***	-0.055***	-0.055***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	553	553	553	553	553	553	553	553	553	553	553

Note: School level data. Each column presents point estimates from our density preferred specification (including only district fixed effect) for each single year. Standard errors clustered on school in parentheses. * significant at 10% ** significant at 5% *** significant at 1%.

Table 7: Heterogeneity and robustness (distance and density)

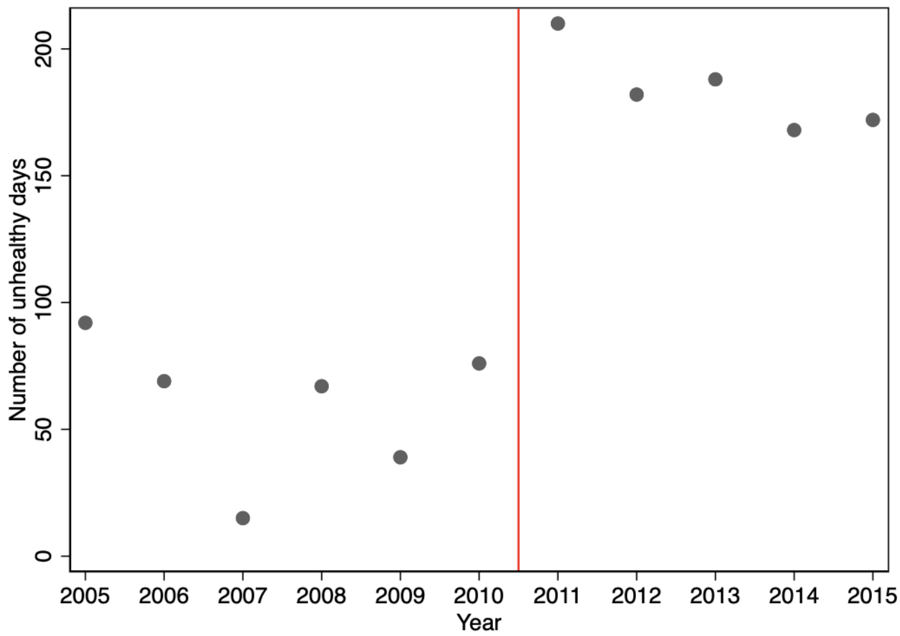
	(1) Preferred	(2) Male	(3) Female	(4) Poor	(5) Rich	(6) North	(7) South	(8) District interaction	(9) Exclude 2011	(10) Alternative Map 1	(11) Alternative Map 2	(12) Outside 50m	(13) Winsorize score 5%	(14) Winsorize road-exposure 5%
distance*sanction	2.003***	2.730***	1.328**	1.854***	1.114	1.969***	2.026***	2.505*	2.171***	2.334***	2.217***	2.542***	2.035***	2.431***
	(0.209)	(0.623)	(0.610)	(0.642)	(1.270)	(0.655)	(0.541)	(1.516)	(0.472)	(0.815)	(0.210)	(0.564)	(0.403)	(0.543)
density*sanction	-0.043***	-0.049***	-0.033***	-0.042***	-0.031*	-0.046***	-0.041***	-0.073***	-0.047***	-0.061***	-0.043***	-0.042***	-0.044***	-0.048***
	(0.007)	(0.009)	(0.012)	(0.011)	(0.016)	(0.011)	(0.009)	(0.023)	(0.008)	(0.007)	(0.007)	(0.010)	(0.007)	(0.009)
Observations	6,083	3,080	3,003	2,288	1,375	2,651	3,432	6,083	5,530	6,083	6,083	3,993	6,083	6,083

Note: School level data. The dependent variable is test score. The coefficients are reported per 100 meters of density. Column 1 repeats column 5 from Table 3, the preferred specification. Columns 2 and 3 estimate the preferred specification on male and female subsamples. Columns 4 and 5 estimate the preferred specification on the subsamples of poor and rich districts. Columns 6 and 7 estimate the preferred specification on the subsample of schools located in the north and south part of Tehran. Column 8 includes district interactions. Column 9 excludes 2011. Columns 10 and 11 use alternative map definitions for major roads to calculate density. Column 12 excludes schools that are located somewhere less than 50 meters from a major road. In columns 13 and 14 we winsorize 5% of test score and our measure of road_exposure. Standard errors clustered on school in parentheses. All regressions include school and district-year fixed effects. * significant at 10% ** significant at 5% *** significant at 1%.

Figures

Figure 1: Air pollution in Tehran

a) Annual number of days in each calendar year that the Air Quality Index for Tehran exceeded 100



b) Quarterly mean of Nitrogen Dioxide (NO_2) concentration in Tehran (averaged across all monitoring stations)

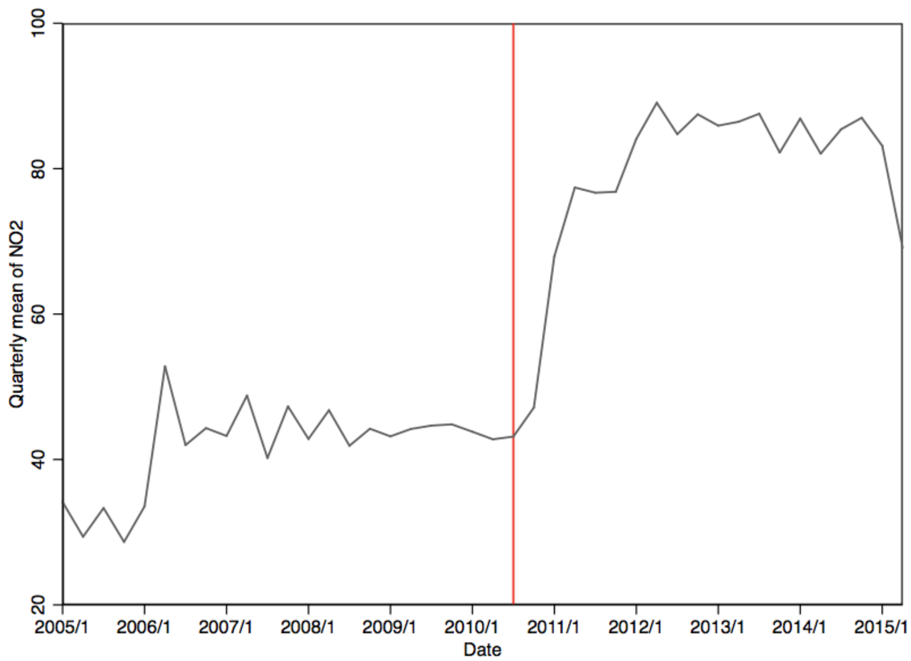


Figure 2: Illustrative schools

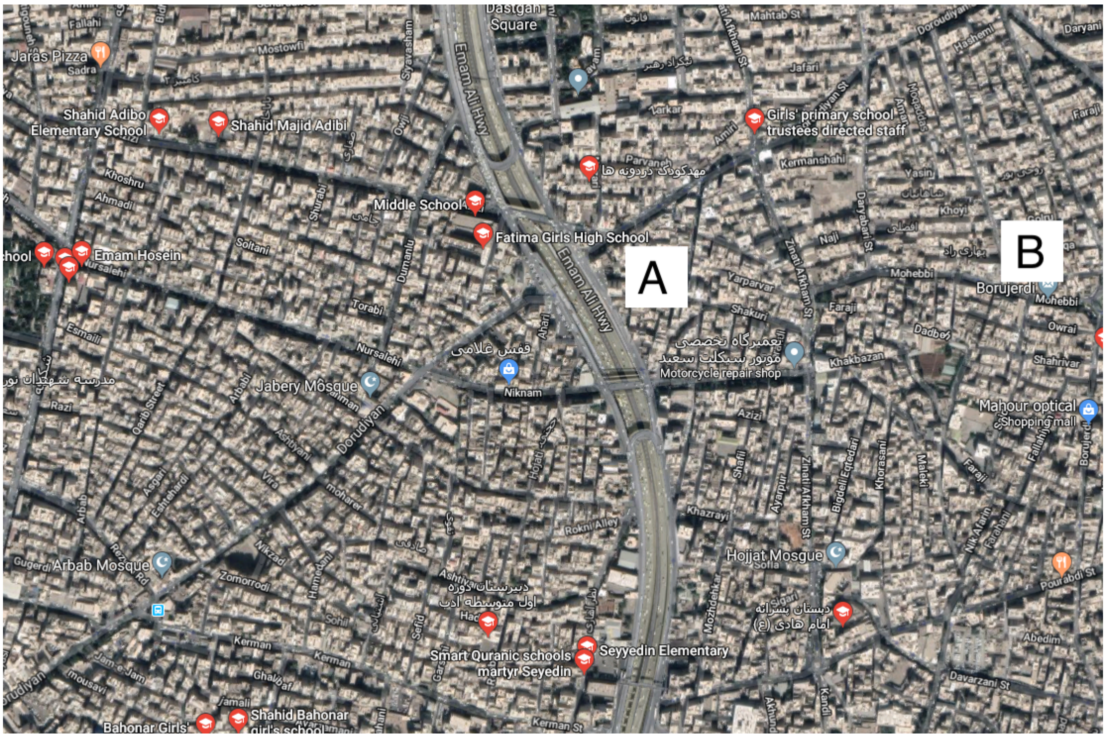
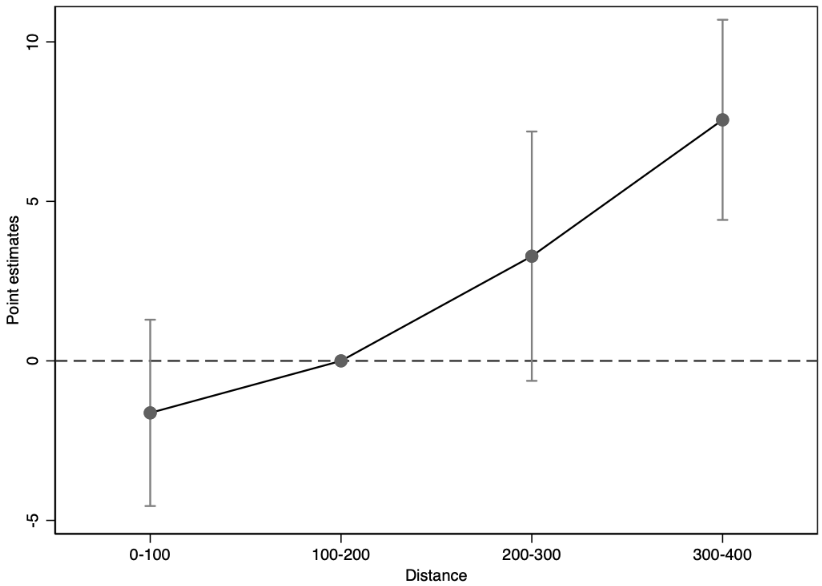
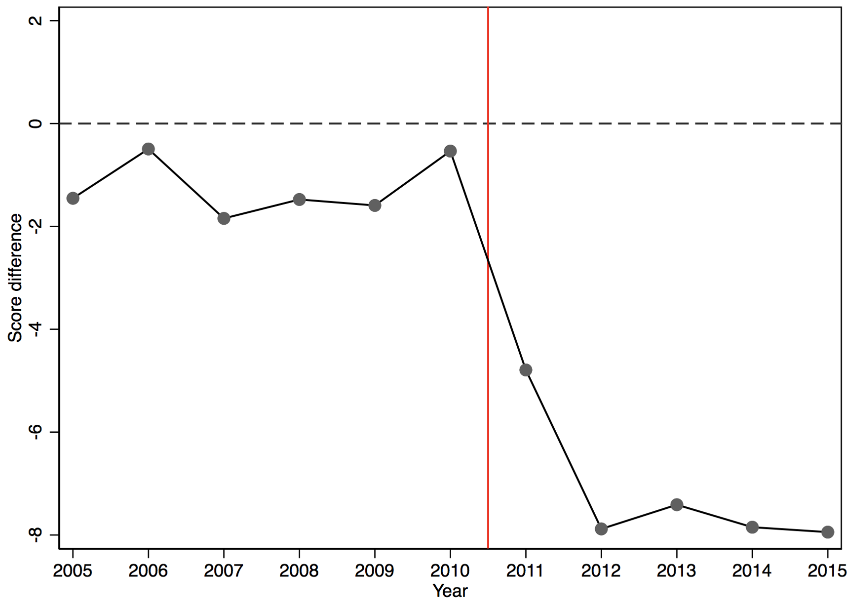


Figure 3: Nonlinear (distance)



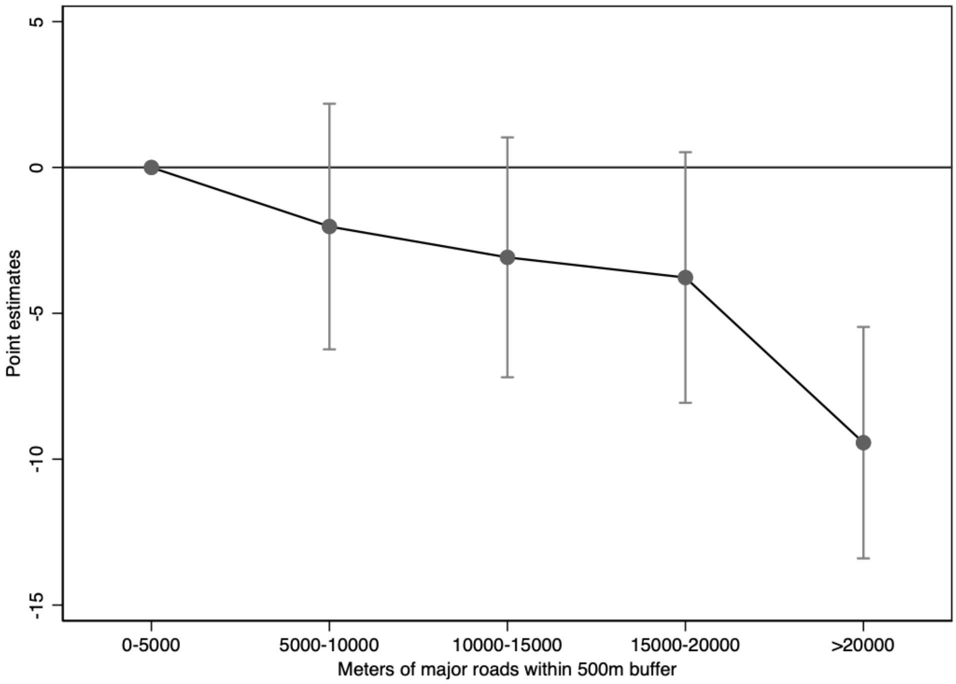
Note: This graph shows estimated impact of sanctions on test score by distance from our preferred specification. The reference category is schools located between 100 to 200m from a major road. Whiskers represent 95% confidence intervals based on standard errors clustered on school. Regression includes school and district-year FEs.

Figure 4: Test score difference between top quartile and bottom quartile road_exposed schools by year (density, 500m buffer)



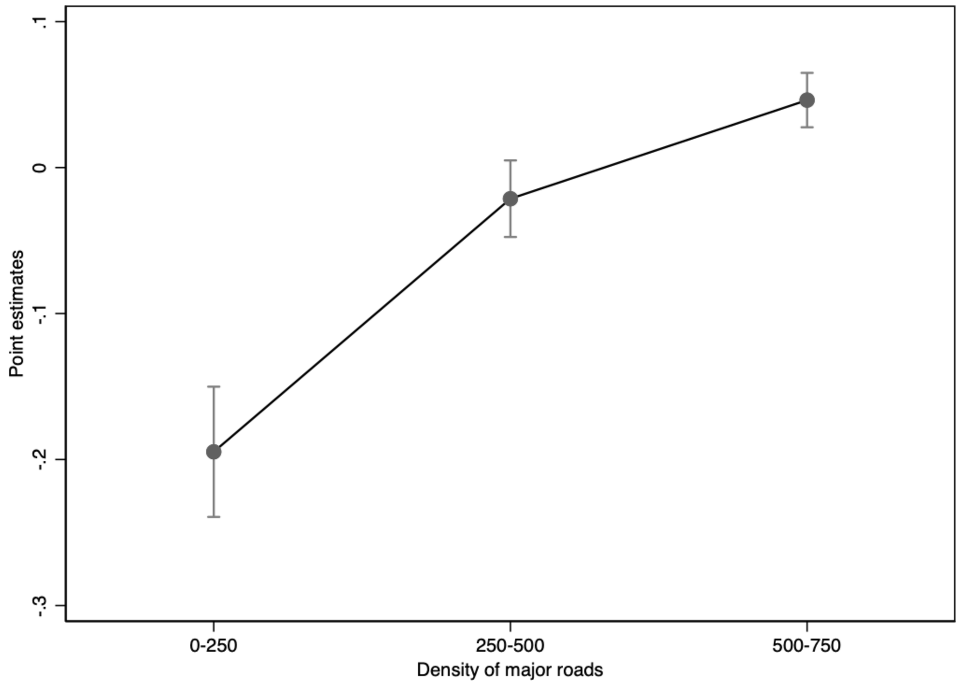
Note: Difference in test score between schools in the top and bottom quartile by road density in the 500m buffer.

Figure 5: Nonlinear (density, 500m buffer)



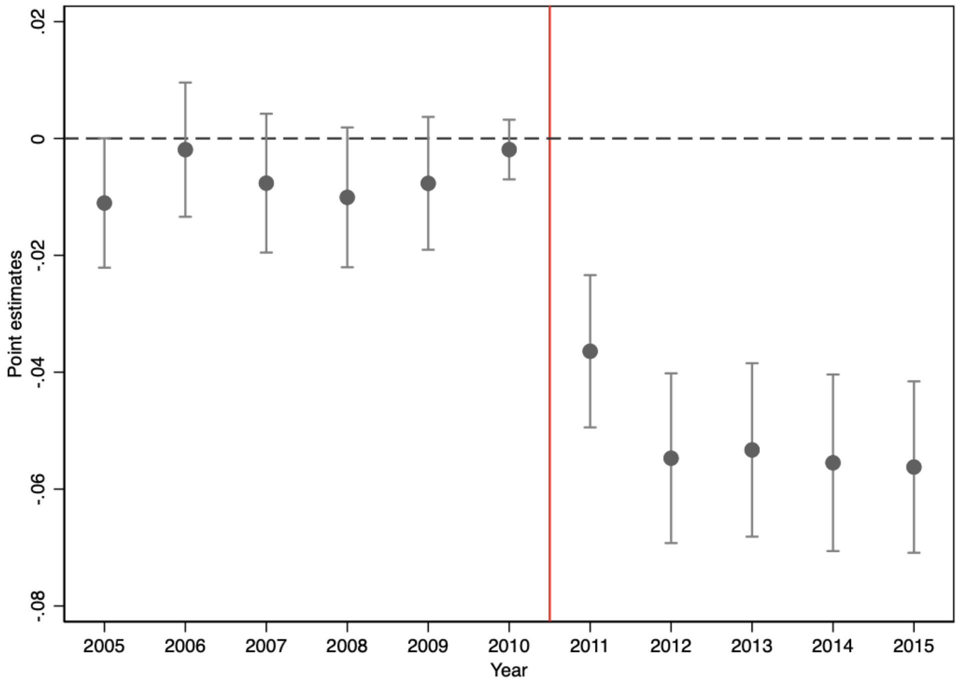
Note: This graph shows estimated impact of sanctions on test score by density from our preferred specification. The reference category is schools located in areas where there is no to 5000m of major road density within their 500m buffer. Whiskers represent 95% confidence interval based on standard errors clustered on school. Regression includes school and district-year FEs.

Figure 6: Donut regression



Note: This graph shows point estimates from a single regression same as our preferred specification where we replace road density within 500m buffer with 3 separate measures for road density within 250m, between 250 and 500m and between 250 and 750m buffer. Whiskers represent 95% confidence interval based on standard errors clustered on school. Regression includes school and district-year FEs.

Figure 7: Cohort cross sections (density, 500m buffer)



Note: This graph shows point estimates from our preferred specification for each year. Whiskers represent 95% confidence interval based on standard errors clustered on school. Regressions include district fixed effect.