# The effect of leaded gasoline on elderly mortality: Evidence from regulatory exemptions

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# Online Appendix

## A Appendix Table of Contents

## A.1 Automotive racing and the switch to unleaded fuel

NASCAR is a racing organization that operates three national series with races across the U.S. Its most well-known national series is the Monster Energy Cup. The other two national series are a minor league series called the Xfinity Series and a modified pickup truck series called the Gander Outdoors Truck Series. NASCAR also runs several regional racing series that concentrate in particular areas of the U.S., such as the K&N Pro Series East and West, and the Whelen Modified Tour. In total, NASCAR operates hundreds of races a year. In its national series, which are the races included in our main analysis, NASCAR enforces strict rules on vehicle dimensions and mandates that all vehicles use the exact same gasoline, which is provided by NASCAR and their fuel sponsor Sunoco.

In 2006, NASCAR announced that its three national racing series would switch to unleaded fuel.[30] NASCAR set a tentative goal to switch at the beginning of 2008, but also stated that their ultimate target was to make the switch in 2007 ahead of the Daytona 500 (Bernstein, 2006; Bay Area News Group, 2006). Prior to the official switch, NASCAR performed a four-week experiment with unleaded fuel: first in the Xfinity Series starting on July 29, 2006 at Gateway International Speedway, and then a two race test in the truck series in August 2006 at the O'Reilly Raceway Park in Indianapolis and at the Nashville Superspeedway. Both of these series then permanently switched to unleaded fuel on September 23, 2006 for the remainder of the 2006 season (Fryer, 2006). After the first tests at Gateway were successful, NASCAR announced a plan to switch in early 2007 (Fryer, 2006). NASCAR officially switched from leaded Sunoco Supreme to unleaded Sunoco 260 GTX across all three national series on the weekend of February 25, 2007, two weeks after the Daytona 500.[31] Sunoco Supreme is a 112 octane fuel while Sunoco GTX is 98 octane; the difference between the two octane ratings can be fully explained by the removal of lead (Sunoco, 2018).

NASCAR operates several other regional racing series, such as the K&N Pro Series. NASCAR still allows teams to use leaded fuel in these regional series. We do not include these regional series in our main analysis because it is unclear whether leaded, unleaded, or both types of fuel are used at individual races. For our main analysis we will focus on races

[30] The EPA had been pushing NASCAR to make the switch since at least 1998 (Howard, 2005). NASCAR originally partnered with Unocal to find a workable unleaded gasoline, however Unocal abandoned the partnership in 2003 (Associated Press, 2006).

[31] One other change that occurred during 2007 and 2008 was the adoption of the "Car of Tomorrow." The Car of Tomorrow was a new generation of cars aimed at improving safety in light of recent deaths during races. This model was used in some races during the 2007 Monster Energy Cup season before being fully phased in during the 2008 season. The Xfinity Series adopted the Car of Tomorrow for the 2011 season. Results—not shown due to space constraints, but available upon request—from an analysis of pole speeds (proxying for pure vehicle performance) and average race speeds and demonstrates that there have been only had minor fluctuations since 1999. This indicates that the new cars and deleading are unlikely to have affected vehicle performance in a way that confounds our results.

from the national series, since we are certain about the leaded status of each race.

ARCA is a racing organization that was operated separately from NASCAR over our sample period. ARCA runs several different series and has a similar national/regional hierarchical structure to NASCAR. ARCA races are either run on the same racetrack as NASCAR races but several days earlier, or on smaller tracks not used by the national NASCAR series. ARCA racers have also historically used older model NASCAR vehicles (Stock Car Racing, 2009).

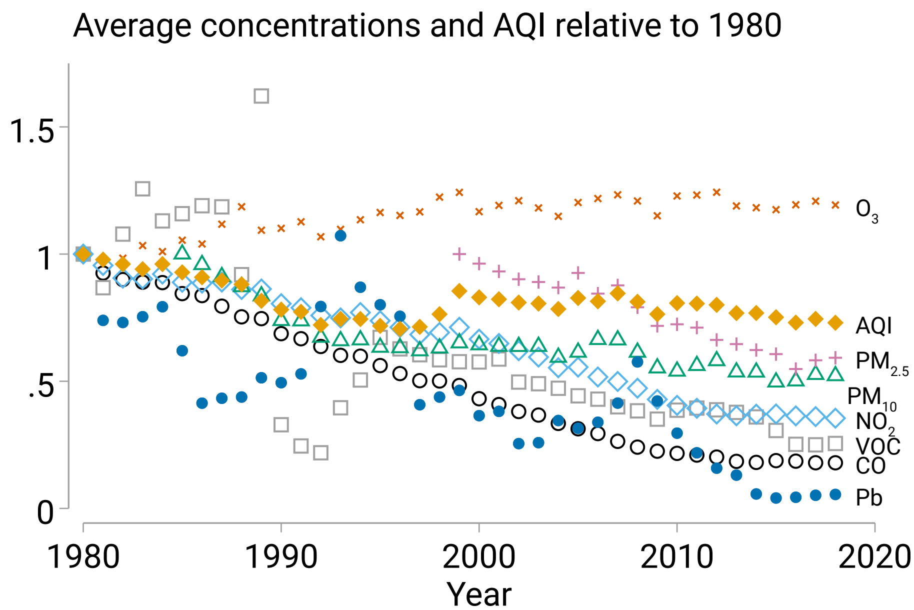
In 2006, ARCA announced plans for their top racing series, currently called the ARCA Menards Series, to switch to unleaded fuel the following year. Prior to the official switch, ARCA performed an unleaded fuel test during the Food World 250 at Talladega Superspeedway on October 6, 2006. Teams had the option of using leaded or unleaded fuel for the remainder of races in the 2006 season before permanently switching at beginning of the 2007 season (ARCA Racing, 2006).

In 2006, across the four national race series, leaded races accounted for 86% of all race miles, while unleaded races accounted for the remaining 14%. For ease of exposition, throughout the paper we will refer to deleading as having occurred in 2007, since this is when the change was made permanent. When we report estimated effects in terms of leaded or unleaded miles driven, as in our ambient lead outcomes, we correctly account for the unleaded races in 2006. However, when we use an event study approach, as we do in our blood lead and mortality specifications, we report the average effect of all races in each county-year. Therefore the reported estimates from any event study estimate are slightly attenuated for 2006.

## A.2 Correlated criteria pollutants since 1980

Figure A1 shows the trends in criteria pollutants and the Air Quality Index (AQI) in the United States since 1980. Average concentrations in the initial year of reporting for each pollutant are normalized to 1. AQI, and all criteria pollutants except for ozone, display correlated downward trends between 1980 and 2018. The correlation coefficients for each pair of non-ozone pollutants are all greater than 0.6.

Figure A1: Criteria pollution concentrations and AQI since 1980.

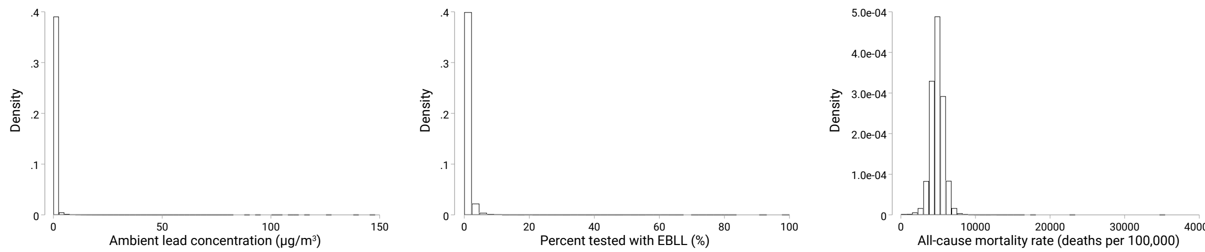



*Note:* AQI is the Air Quality Index, which is a function of all reported pollutant concentrations. Each point represents the annual mean relative to the mean from the first year reported for each pollutant/measure. Data come from the EPA AirData pre-generated daily datasets, which begin in 1980 for all pollutants/measures except for $PM_{10}$ and $PM_{2.5}$. All monitor-day readings are used to calculate the mean.

## A.3 Outcome distributions

Figure A2 plots the distributions of our outcome variables. The left panel plots the ambient lead concentration distribution. It is highly right-skewed and approximately 10% of the sample consists of zeroes. The middle panel plots the distribution of the percent of children tested with elevated blood lead. Similar to the air data, the distribution is right-skewed and about a third of the data are zero-valued. The right panel plots the distribution of the all-cause elderly mortality rate. These data appear normally distributed although with a longer right tail as the distribution is bounded below by zero.

Figure A2: Distributions of ambient lead concentrations (left), percent of children tested with elevated blood lead (middle), and the all-cause elderly mortality rate (right).




*Note:* Each subfigure plots the distributions of the untransformed outcome variables. The left panel plots the mean lead concentration readings ($\mu g/m^3$), the middle panel plots the percent of children tested with elevated blood lead (%), and the right panel plots the all-cause elderly mortality rate (deaths per 100,000 in the elderly population).

## A.4 Summary statistics

Tables A1-A4 display the summary statistics for the variables used in each of our main regressions.

Table A1: Summary statistics for the ambient lead dataset.

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Pb concentration ($\mu$ g/m$^3$) | 0.34 | 1.47 | 0.00 | 147.90 |
| Leaded miles past 7 days within 50 miles (100,000s) | 0.00 | 0.01 | 0.00 | 0.41 |
| Unleaded miles past 7 days within 50 miles (100,000s) | 0.00 | 0.01 | 0.00 | 0.37 |
| Air temperature (° C) | 13.03 | 10.42 | –37.54 | 37.07 |
| Preciptable water (kg/m$^2$) | 19.26 | 11.89 | –0.47 | 66.92 |
| Surface pressure (kPa) | 97.10 | 5.31 | 74.22 | 104.48 |
| Relative humidity (%) | 74.38 | 18.73 | 0.00 | 100.01 |
| Wind speed (m/s) | 4.43 | 2.40 | 0.01 | 22.19 |
| Observations | 875034 | | | |

*Note:* These data range over our entire lead pollution dataset, 1957 to 2018. Relative humidity can be above 100% when the air is supersaturated. Ambient lead estimates are robust to forcing total precipitable water to be non-negative.

Table A2: Summary statistics for the ambient lead data used in regressions from Table 1.

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| Pb concentration ($\mu$ g/m$^3$) | 0.14 | 0.76 | 0.00 | 57.47 |
| Leaded miles past 7 days within 50 miles (100,000s) | 0.00 | 0.01 | 0.00 | 0.41 |
| Unleaded miles past 7 days within 50 miles (100,000s) | 0.00 | 0.02 | 0.00 | 0.37 |
| Air temperature (° C) | 13.24 | 10.24 | –36.38 | 37.07 |
| Preciptable water (kg/m$^2$) | 19.73 | 12.07 | –0.25 | 66.92 |
| Surface pressure (kPa) | 97.65 | 4.45 | 74.36 | 104.48 |
| Relative humidity (%) | 74.54 | 17.67 | 1.25 | 100.01 |
| Wind speed (m/s) | 4.27 | 2.33 | 0.03 | 19.79 |
| Observations | 312277 | | | |

*Note:* These data range from 1996 to 2018. Relative humidity can be above 100% when the air is super-saturated. Ambient lead estimates are robust to forcing total preciptable water to be non-negative.

Table A3: Summary statistics for the blood lead dataset.

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| EBLL rate (%) | 0.92 | 3.92 | 0.00 | 100.00 |
| Race county | 0.01 | 0.12 | 0.00 | 1.00 |
| Border county | 0.08 | 0.27 | 0.00 | 1.00 |
| Unemployment rate (%) | 0.07 | 0.03 | 0.02 | 0.29 |
| Median income (1,000 USD) | 43.48 | 11.45 | 17.84 | 119.53 |
| Percent non-white (%) | 0.14 | 0.16 | 0.00 | 0.89 |
| TRI facility lead emissions (Metric tons) | 17.69 | 210.27 | 0.00 | 11275.01 |
| Manufacturing payroll (1,000 USD) | 208.51 | 591.96 | 0.00 | 12935.51 |
| Observations | 22887 | | | |

Table A4: Summary statistics for the mortality dataset.

| | Mean | S.D. | Min. | Max. |
|---|---|---|---|---|
| General cardiovascular (deaths per 100,000) | 1845.19 | 586.15 | 0.00 | 35424.34 |
| Ischemic heart disease (deaths per 100,000) | 871.85 | 423.16 | 0.00 | 35424.34 |
| Respiratory (deaths per 100,000) | 588.21 | 235.42 | 0.00 | 8856.08 |
| All-cause (deaths per 100,000) | 4873.42 | 915.49 | 0.00 | 35424.34 |
| Diabetes (deaths per 100,000) | 149.75 | 119.03 | 0.00 | 8291.19 |
| Deaths of despair (deaths per 100,000) | 47.83 | 54.06 | 0.00 | 3248.70 |
| Race county | 0.01 | 0.11 | 0.00 | 1.00 |
| Border county | 0.07 | 0.25 | 0.00 | 1.00 |
| Unemployment rate (%) | 0.06 | 0.03 | 0.01 | 0.30 |
| Median income (1,000 USD) | 41.65 | 11.62 | 0.00 | 134.61 |
| Percent non-white (%) | 0.13 | 0.16 | 0.00 | 0.97 |
| TRI facility lead emissions (Metric tons) | 79.06 | 2516.61 | 0.00 | 275971.86 |
| Manufacturing payroll (1,000 USD) | 175.06 | 578.43 | 0.00 | 19471.85 |
| Observations | 58063 | | | |

*Note:* The maximum mortality rates for cardiovascular, IHD, and all-cause mortality are all the same because they are all from the same observation of a small county with a single IHD death. IHD is a subset of cardiovascular and all-cause mortality.

## A.5 Racetracks: locations, other events, statistics, and distance to lead monitors

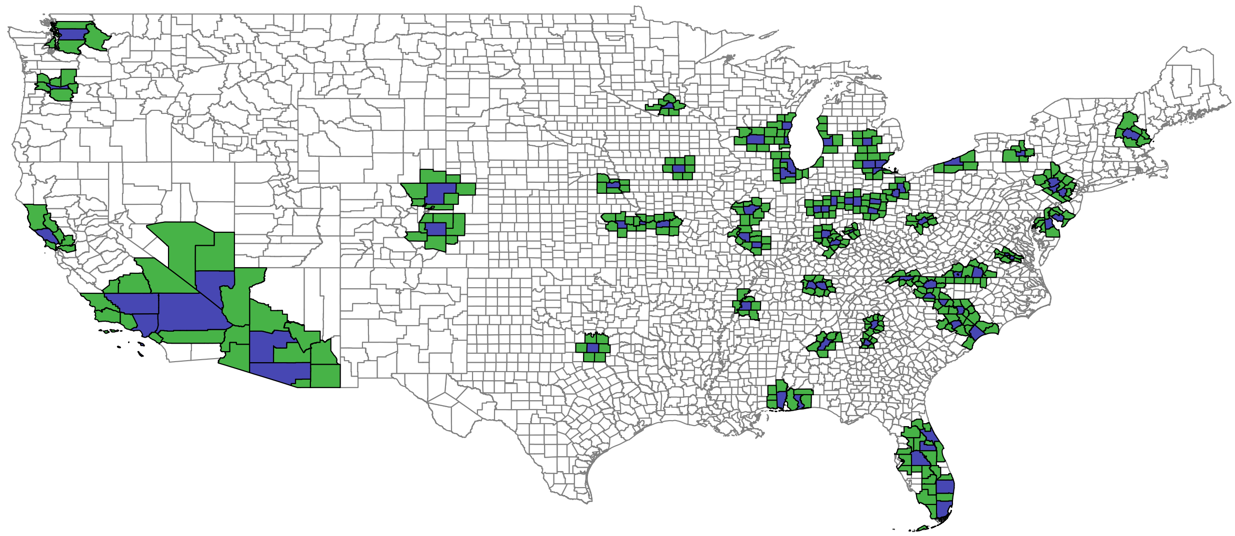
### i. Racetrack counties and border counties

Figure A3 plots the county types for the EBLL and mortality analysis. The 75 counties in blue are those that had at least one NASCAR or ARCA race since 1995; the 365 counties in green are those that border race counties; the 2,793 counties in white are the control group. If a race county did not have a race in a particular year, it might be a control or border county for that year.

### ii. Other events at racetracks

Other events besides NASCAR and ARCA races occur at the racetracks in our sample. Here we document the set of events that occurred in 2017 at the tracks used for the national

Figure A3: Map displaying counties with at least one race (blue) or that border a race county (green) since 1995.



*Note:* White counties are control counties, blue counties are those that had at least one race since 1995, and green counties are those that have not had a race since 1995 but border a county that did.

NASCAR series. Data were obtained from cached versions of schedules on the racetrack websites. In total we were able to obtain information on 20 racetracks, including major ones like Auto Club Speedway, Daytona International Speedway, and Indianapolis Motor Speedway. We have documented that in 2017, these tracks had a total of 200 events. 84 of these events were individual NASCAR or ARCA races. 20 were non-automotive events like concerts, festivals, or marathons. The remaining 96 were automotive events. About a fifth of the 96 were days for a supercar (e.g. Ferrari, Lamborghini) driving school at Auto Club Speedway. Other common events at racetracks include drag racing, NHRA events, sprint car racing, IndyCar, and monster trucks.

### iii. Distribution of monitor-racetrack distances

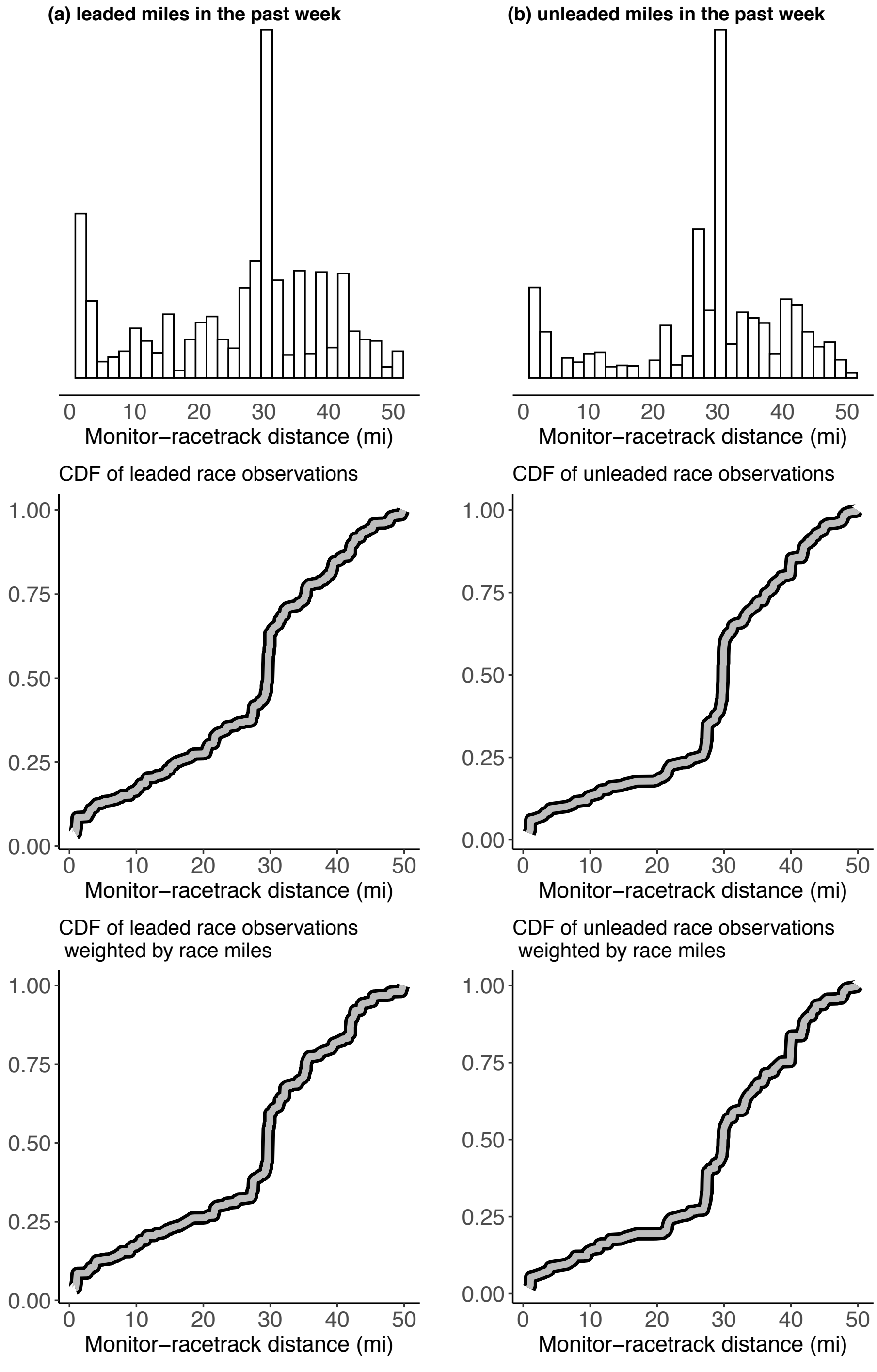
Figure A4 displays the distributions of the distances of ambient lead monitor readings from the nearest racetrack by the status of the observation in our preferred specification. The first column shows the distributions for observations that had a leaded race in the last 7 days within 50 miles; the second column is for unleaded races in the last 7 days within 50 miles; and the third column is for observations at monitors that have never had a race within 50 miles. For each column, the top row shows a histogram of the data, the middle row shows the cumulative density function, and the bottom row scales the cumulative density function by the number of race miles per observation.

The distributions across the first two columns show similar patterns. There are a moderate number of race miles driven within 3 miles of a monitor, however data in the 3-25 mile range is sparse. The modes of the distributions are at 30 miles and approximately 50% of observations occur 25-35 miles from a racetrack. The third column shows that the distribution of observations for monitors that never had a race within 50 miles is relatively smooth.

### iv. Racetrack statistics

Table A5 details statistics for each racetrack in our dataset for the ambient lead specifications, including the lap length, mean distance to the nearest monitor reading, and the fraction of treated observations accounted for by the racetrack. We limit the data to monitors within 50 miles for computing the statistics to preserve direct comparison with our preferred specification. Monitors tend to be located a substantial distance from racetracks. Averaging over the fourth column shows that the mean distance from a racetrack to a monitor is 25 miles. 40% of racetracks have a mean distance to a monitor of over 30 miles. The minimum distance column indicates that two thirds of racetracks do not have a monitor within 10 miles, and about 40% of racetracks do not have a monitor within 20 miles. The racetracks that account for larger fractions of our leaded miles in our sample also tend to be located further from racetracks as shown in the eighth and ninth columns. Only 1 of the 14 racetracks that contributes more than 1% of the leaded miles (column 9) in the sample has an average distance to a monitor of less than 20 miles. These 14 racetracks account for over 90% of our observations. The final two columns show that about a quarter of racetracks are, on average 25-35 miles from a monitor, and that 40% of racetracks are on average 30-50 miles from a monitor. The 25-35 mile range accounts for over half of the race miles driven in our sample. These miles come from 13 distinct racetracks and 89 unique monitor-racetrack pairs out of a total of 47 racetracks and 261 monitor-stadium pairs.

Figure A4: Distribution of distance from monitor to the nearest racetrack a with race in the past week by leaded and unleaded race status.




*Note:* Each column is differentiated by the status of the lead monitor reading in our preferred specification. The first column displays the distance distribution for daily lead monitor readings that occur within one week of a leaded race; the second column is the distribution of lead monitor readings that occur within one week of an unleaded race; and the third column displays the distribution of lead monitor readings whose monitors never had observations that occur within one week of a nearby race. The top row shows a series of histograms that display the distance between each daily lead monitor reading and the distance to the nearest race within the past week. The middle row shows the empirical cumulative density functions of the race observations, weighting each race equally. The bottom row displays the empirical cumulative density functions of the race data by distance, where we weigh each race by miles driven. 50% of our treated observations (column a) occur between 25 and 35 miles of a racetrack.

Table A5: NASCAR/ARCA track locations, monitor distance, and % of treatment contribution for ambient lead specifications.

| Track | Location | Lap Length (mi.) | Mean Monitor Dist. (mi) | S.D. | Min. | Max. | % of treated obs | % of treated obs accounting for miles driven at track | Mean dist between 25 and 35 mi | Mean dist between 30 and 50 mi |
|---|---|---|---|---|---|---|---|---|---|---|
| Alabama International Motor Speedway | Talladega, AL | 2.66 | 43.47 | 1.62 | 42.04 | 45.23 | 0.12 | 0.26 | 0 | 1 |
| ISM Raceway | Avondale, AZ | 1.00 | 14.86 | 1.38 | 13.75 | 16.33 | 0.27 | 0.25 | 0 | 0 |
| Tucson Raceway Park | Tucson, AZ | 0.38 | 16.35 | 4.23 | 12.69 | 20.01 | 0.15 | 0.03 | 0 | 0 |
| Auto Club Speedway | Fontana, CA | 2.00 | 26.74 | 15.96 | 0.91 | 47.09 | 4.66 | 6.23 | 1 | 0 |
| Infineon Raceway | Sonoma, CA | 1.99 | 25.06 | 6.10 | 15.00 | 31.90 | 0.46 | 0.35 | 1 | 0 |
| Colorado National Speedway | Erie, CO | 0.38 | 20.08 | 3.29 | 16.33 | 25.19 | 0.54 | 0.13 | 0 | 0 |
| Pikes Peak International Raceway | Fountain, CO | 1.00 | 21.29 | 3.34 | 18.28 | 24.85 | 0.93 | 0.63 | 0 | 0 |
| Dover Downs International Speedway | Dover, DE | 1.00 | 37.14 | 9.45 | 14.11 | 45.40 | 2.04 | 3.96 | 0 | 1 |
| Daytona International Speedway | Daytona Beach, FL | 2.50 | 44.35 | 0.00 | 44.35 | 44.35 | 0.12 | 0.16 | 0 | 1 |
| Homestead-Miami Speedway | Homestead, FL | 1.50 | 21.78 | | 21.78 | 21.78 | 0.04 | 0.03 | 0 | 0 |
| Lanier National Speedway | Braselton, GA | 0.40 | 15.48 | 7.64 | 10.09 | 20.88 | 0.08 | 0.02 | 0 | 0 |
| Atlanta International Raceway | Hampton, GA | 1.50 | 31.60 | 6.64 | 28.01 | 42.36 | 0.31 | 0.64 | 1 | 1 |
| Chicago Motor Speedway | Cicero, IL | 1.00 | 13.11 | 6.39 | 4.41 | 21.77 | 1.31 | 0.59 | 0 | 0 |
| Gateway International Raceway | Madison, IL | 1.25 | 22.87 | 10.26 | 3.00 | 31.17 | 18.62 | 12.58 | 0 | 0 |
| Illinois State Fairgrounds | Springfield, IL | 1.00 | 33.07 | 2.65 | 31.74 | 38.08 | 0.73 | 0.19 | 1 | 1 |
| Anderson Speedway | Anderson, IN | 0.25 | 23.10 | 10.09 | 15.32 | 39.07 | 0.69 | 0.10 | 0 | 0 |
| Indianapolis Raceway Park | Clermont, IN | 0.69 | 10.04 | 5.19 | 4.86 | 16.47 | 0.50 | 0.16 | 0 | 0 |
| Indiana State Fairgrounds | Indianapolis, IN | 1.00 | 44.26 | 0.14 | 44.04 | 44.33 | 0.15 | 0.04 | 0 | 1 |
| Salem Speedway | Salem, IN | 0.50 | 34.78 | 4.74 | 29.87 | 39.00 | 0.50 | 0.13 | 1 | 1 |
| Indianapolis Motor Speedway | Speedway, IN | 2.50 | 25.47 | 22.05 | 3.83 | 49.87 | 3.12 | 5.17 | 1 | 0 |
| Winchester Speedway | Winchester, IN | 0.50 | 20.70 | 0.16 | 20.52 | 20.84 | 2.27 | 0.50 | 0 | 0 |
| Heartland Park Topeka | Topeka, KS | 1.80 | 12.35 | 4.77 | 8.39 | 20.28 | 0.19 | 0.07 | 0 | 0 |
| Michigan Speedway | Brooklyn, MI | 2.00 | 35.04 | 0.00 | 35.04 | 35.04 | 0.08 | 0.04 | 0 | 1 |
| Michigan International Speedway | Brooklyn, MI | 2.00 | 35.04 | 0.00 | 35.04 | 35.04 | 0.42 | 0.63 | 0 | 1 |
| Flat Rock Speedway | Flat Rock, MI | 0.25 | 17.38 | 1.45 | 15.62 | 18.58 | 0.31 | 0.02 | 0 | 0 |
| Berlin Raceway | Marne, MI | 0.44 | 15.50 | 6.16 | 9.01 | 22.10 | 0.35 | 0.06 | 0 | 0 |
| I-70 Speedway | Odessa, MO | 0.50 | 39.90 | 1.93 | 38.12 | 43.67 | 0.35 | 0.09 | 0 | 1 |
| Charlotte Motor Speedway | Concord, NC | 1.50 | 32.89 | 3.10 | 32.19 | 46.23 | 1.54 | 2.93 | 1 | 1 |
| Hickory Motor Speedway | Hickory, NC | 0.36 | 5.87 | | 5.87 | 5.87 | 0.04 | 0.01 | 0 | 0 |
| North Carolina Motor Speedway | Rockingham, NC | 1.00 | 40.88 | 0.77 | 38.93 | 41.58 | 0.39 | 0.75 | 0 | 1 |
| Flemington Speedway | Flemington, NJ | 0.62 | 41.59 | 6.72 | 20.40 | 49.58 | 4.74 | 1.33 | 0 | 1 |
| Toledo Speedway | Toledo, OH | 0.50 | 42.51 | 3.19 | 36.42 | 45.46 | 2.20 | 0.52 | 0 | 1 |
| Cloverleaf Speedway | Valley View, OH | 0.25 | 32.39 | 5.90 | 21.57 | 47.83 | 6.44 | 8.65 | 1 | 1 |
| Pocono Raceway | Long Pond, PA | 2.50 | 36.40 | 9.61 | 18.09 | 43.56 | 5.63 | 11.17 | 0 | 1 |
| Nazareth Speedway | Nazareth, PA | 1.00 | 34.99 | 8.85 | 7.66 | 47.91 | 5.51 | 3.24 | 1 | 1 |
| Darlington Raceway | Darlington, SC | 1.25 | 25.44 | 13.40 | 9.17 | 40.32 | 2.35 | 3.95 | 1 | 0 |
| Myrtle Beach Speedway | Myrtle Beach, SC | 0.54 | 27.08 | 11.61 | 5.23 | 33.99 | 0.73 | 0.29 | 1 | 0 |
| Bristol International Raceway | Bristol, TN | 0.53 | 1.19 | 2.80 | 0.88 | 42.73 | 8.56 | 8.55 | 0 | 0 |
| Memphis Motorsports Park | Memphis, TN | 0.75 | 14.88 | 0.48 | 13.99 | 15.15 | 1.23 | 0.56 | 0 | 0 |
| Nashville Speedway | Nashville, TN | 0.50 | 20.92 | 7.07 | 2.63 | 32.92 | 2.35 | 1.08 | 0 | 0 |
| Nashville Superspeedway | Nashville, TN | 1.33 | 21.72 | 0.35 | 20.37 | 22.04 | 2.20 | 2.15 | 0 | 0 |
| Texas Motor Speedway | Fort Worth, TX | 1.50 | 30.21 | 2.89 | 27.27 | 42.19 | 15.73 | 20.55 | 1 | 1 |
| Martinsville Speedway | Martinsville, VA | 0.50 | 38.71 | 0.00 | 38.71 | 38.71 | 0.08 | 0.07 | 0 | 1 |
| Richmond Fairgrounds | Richmond, VA | 0.50 | 4.54 | 2.22 | 2.68 | 7.01 | 0.54 | 0.84 | 0 | 0 |
| Evergreen Speedway | Monroe, WA | 0.65 | 26.08 | 0.00 | 26.08 | 26.08 | 0.19 | 0.06 | 1 | 0 |
| Milwaukee Mile | West Allis, WI | 1.00 | 12.73 | 14.69 | 4.85 | 38.64 | 0.19 | 0.23 | 0 | 0 |
| West Virginia Motor Speedway | Mineral Wells, WV | 0.62 | 5.51 | | 5.51 | 5.51 | 0.04 | 0.01 | 0 | 0 |

## A.6 Lead concentrations

### i. Specification and sample robustness

Table A6 shows estimates from a set of alternative specifications for equation (1). Column 1 replaces asinh(Pb) with just the untransformed mean reading; column 2 replaces asinh(Pb) with ln(Pb + 1); column 3 clusters at the monitor level instead of county level; column 4 includes monitor-by-year-by-month fixed effects; column 5 controls for baseline ambient Pb levels prior to the race; column 6 includes data back to 1957; column 7 only uses miles from the national NASCAR series; column 8 includes regional NASCAR series; column 9 restricts the data to be within 50 days of a race; column 10 replaces the miles treatment variables with indicator variables for if there was a race in the past week to closer match the EBLL and mortality regressions; column 11 clusters at the state level; and column 12 uses Conley standard errors where the distance cutoff is 150 miles from the monitor and the time cutoff is 100 years. The estimates are robust across all specifications.

Table A6: The effect of 100,000 race miles within 50 miles using alternative Pb functional forms, using race indicators instead of miles, alternative treatments of standard errors, controlling for baseline Pb levels, or using alternative samples.

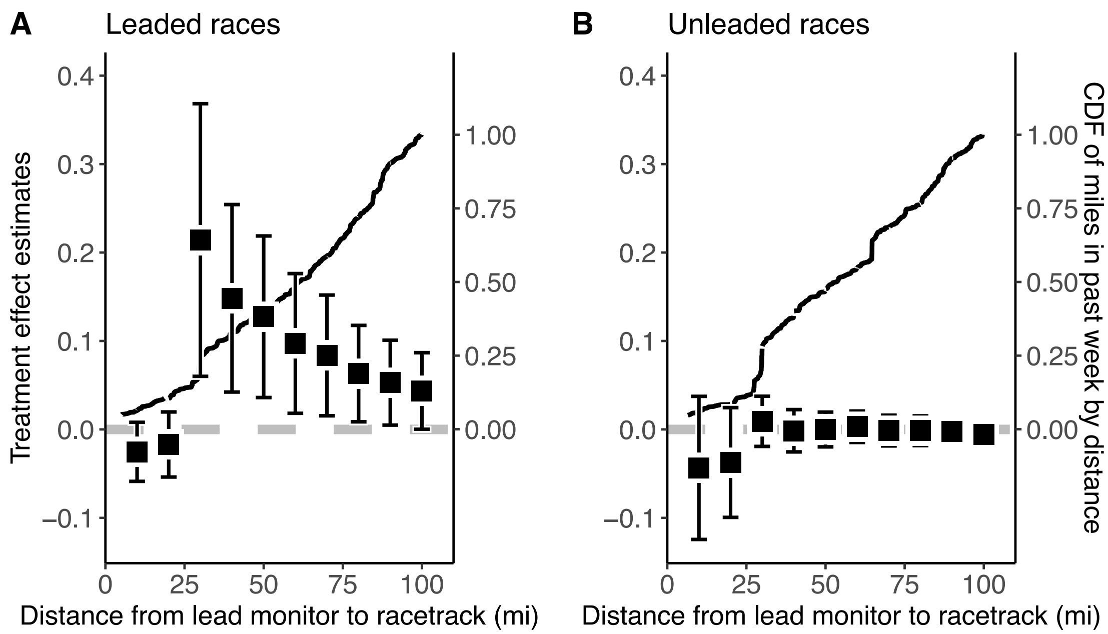
| | (1)<br>mean Pb | (2)<br>ln(Pb +1) | (3)<br>asinh(Pb) | (4)<br>asinh(Pb) | (5)<br>asinh(Pb) | (6)<br>asinh(Pb) | (7)<br>asinh(Pb) | (8)<br>asinh(Pb) | (9)<br>asinh(Pb) | (10)<br>asinh(Pb) | (11)<br>asinh(Pb) | (12)<br>asinh(Pb) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Leaded race miles in past week (100k) | 0.25* | 0.10*** | 0.13** | 0.12*** | 0.13*** | 0.10** | 0.13** | 0.08* | 0.11*** | | 0.13* | 0.13** |
| | (0.13) | (0.04) | (0.06) | (0.03) | (0.05) | (0.04) | (0.06) | (0.05) | (0.03) | | (0.06) | (0.06) |
| Unleaded race miles in past week (100k) | –0.02 | –0.00 | –0.00 | 0.02 | –0.00 | –0.00 | –0.01 | 0.00 | –0.02 | | –0.00 | –0.00 |
| | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) | (0.02) | | (0.01) | (0.02) |
| Mean lead two weeks before race | | | | | 0.02*** | | | | | | | |
| | | | | | (0.01) | | | | | | | |
| 1(Leaded race in past week) | | | | | | | | | | 0.02*** | | |
| | | | | | | | | | | (0.01) | | |
| 1(Unleaded race in past week) | | | | | | | | | | –0.00 | | |
| | | | | | | | | | | (0.00) | | |
| Daily Weather Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Monitor-by-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mon-by-Year-by-Month FE | No | No | No | Yes | No | No | No | No | No | No | No | No |
| Week-by-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | Monitor | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Years Included | 1996-2018 | 1996-2018 | 1996-2018 | 1996-2018 | 1996-2018 | 1957-2018 | 1996-2018 | 1996-2018 | 1996-2018 | 1996-2018 | 1996-2018 | 1996-2018 |
| Races Included | Main Sample | Main Sample | Main Sample | Main Sample | Main Sample | Main Sample | NASCAR Only | Main Sample + Regional | Main Sample | Main Sample | Main Sample | Main Sample |
| Restriction Around Race | None | None | None | None | None | None | None | None | ± 50 days | None | None | None |
| Adjusted $R^2$ | 0.29 | 0.47 | 0.45 | 0.44 | 0.45 | 0.64 | 0.45 | 0.45 | 0.45 | 0.45 | 0.45 | .45 |
| Observations | 312277 | 312277 | 312277 | 311301 | 311207 | 872880 | 311056 | 312842 | 59968 | 312277 | 312277 | 312277 |

*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors clustered at the county level in parentheses unless otherwise noted. Monitor-specific daily weather variables include air temperature, pressure, relative humidity, wind speed, and daily precipitable water.


A14

Figure A5 shows how the size of the estimates in column 7 of Table 1 vary with the chosen treatment distance cutoff. The thin black line corresponds to the cumulative density function of monitor readings. Estimated effects of leaded miles under 10 and 20 mile cutoffs are approximately zero which is consistent with the small number of monitors within 20 miles as shown in Figure A4 and Table A5. Table A5 showed that a substantial number of leaded miles occur in the 25-35 mile range. This is when we are sufficiently powered to estimate an effect and at the 30 mile cutoff we estimate an effect of 0.22 for leaded miles and an effect of approximately zero for unleaded miles. The leaded miles effect attenuates as the distance cutoff is expanded outward up to 100 miles.

Figure A5: Treatment effect by distance cutoff and the distribution of distance from monitor to nearest racetrack by leaded or unleaded race status.




*Note:* This figure shows the effect of 100,000 leaded and unleaded race miles on ambient lead concentrations across increasing distances between the stadium and nearby racetrack. The left panel (A) plots the estimated effect of leaded race miles from a set of increasing distance cutoffs. The right panel (B) plots the estimated effect of unleaded race miles. Each regression is similar to Equation 1, but with a different distance bin. Each regression includes both leaded and unleaded miles in the past week within the given distance cutoff and controls for week-by-year fixed effects, monitor-by-year fixed effects, and daily monitor-specific weather controls. The dependent variable is asinh(Pb). Brackets denote 95% confidence intervals, calculated from robust standard errors clustered at the county level. The estimates for 50 miles depicted in the figure correspond to the results shown in column 7 of Table 1. The black line behind the coefficient estimates depicts the cumulative density function of all leaded race miles (A) and unleaded race miles (B) that occur within 100 miles of a stadium and occur in the week before a lead monitor reading. Over 50% of past week race miles between 0 and 50 miles of a race track occur between 25 and 35 miles. There are 261 unique monitor-racetrack pairs from 0 to 50 miles, 89 of these unique pairs occur between 25 and 35 miles.

### ii. Comparison tests and confounding pollutants

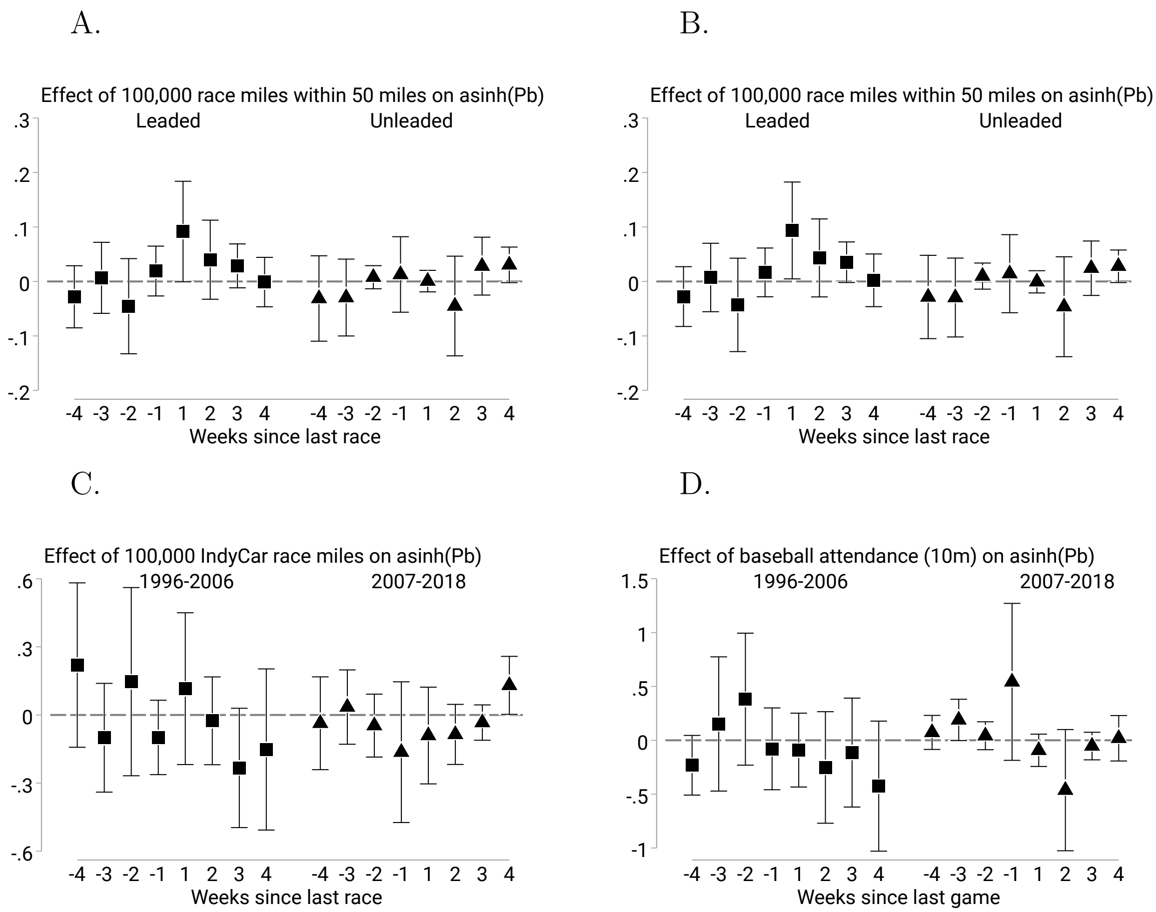
Figures A6 and A7 display event study estimates when including treatment by a pair of placebo events and a set of potentially confounding pollutants using the same specification as the left panel of Figure 3. Figure A6 displays the results using the two placebo events: baseball games and IndyCar races. The estimates come specifications corresponding to the left panel Figure 3, but where we include identical treatment variables for IndyCar miles and baseball attendance in the regression. This tests whether similar large events are associated with changes in lead concentrations that may occur due to spurious lead trends, resuspension of soil lead, or increased leaded air travel. The top row shows the effect of NASCAR miles when now additionally controlling for IndyCar miles or baseball attendance. The event study estimates are nearly identical to Figure 3. The bottom row shows the effects of IndyCar race miles and baseball attendance on lead concentrations. Note that IndyCar did not use leaded fuel over our sample period. Both sets of estimates are centered around zero with no clear spike in lead concentrations following a race as was observed following leaded NASCAR and ARCA races.

Figure A7 displays results from the second set of placebo tests. These tests examine whether the switch to unleaded racing fuel affected concentrations of other automotive pollutants that could confound estimated effects on mortality (U.S. Environmental Protection Agency, 1994).[32] For our estimates to be confounded, leaded and unleaded races would need to differentially affect levels of other pollutants. Similar to the left panel of Figure 3, we would need to find evidence that the levels of other pollutants increase following leaded or unleaded races, and that there is no change or a significantly smaller increase following the other type of race. Moreover, we would also need to demonstrate parallel pre-trends in the weeks preceding both leaded and unleaded races to provide the usual suggestive evidence that we are estimating a causal effect of races rather than background trends. We do not find convincing evidence that any of these pollutant levels were altered as a result of the deleading of NASCAR and ARCA races.

The first row of Figure A7 shows no effect of races on ambient CO or VOCs before or after deleading. CO levels at monitors within 4 weeks of a leaded or unleaded race tend to be the same as CO levels at control monitors. VOC concentrations tend to be higher at monitors nearby a race, but there is no clear effect of the race itself on VOC concentrations. The second row does not show strong evidence that either leaded or unleaded races affect ambient $PM_{10}$ and $PM_{2.5}$, although unleaded races tend to occur when $PM_{2.5}$ concentrations are lower. There is an increase in $PM_{2.5}$ two weeks after a leaded race, however this is likely

[32] If gasoline is not perfectly combusted inside the engine, a vehicle may emit hydrocarbons (a VOC), $NO_x$, PM, or CO. $NO_x$ and VOCs can react in the air to form $O_3$.

Figure A6: The effect of other large events on ambient lead concentrations within 50 miles.

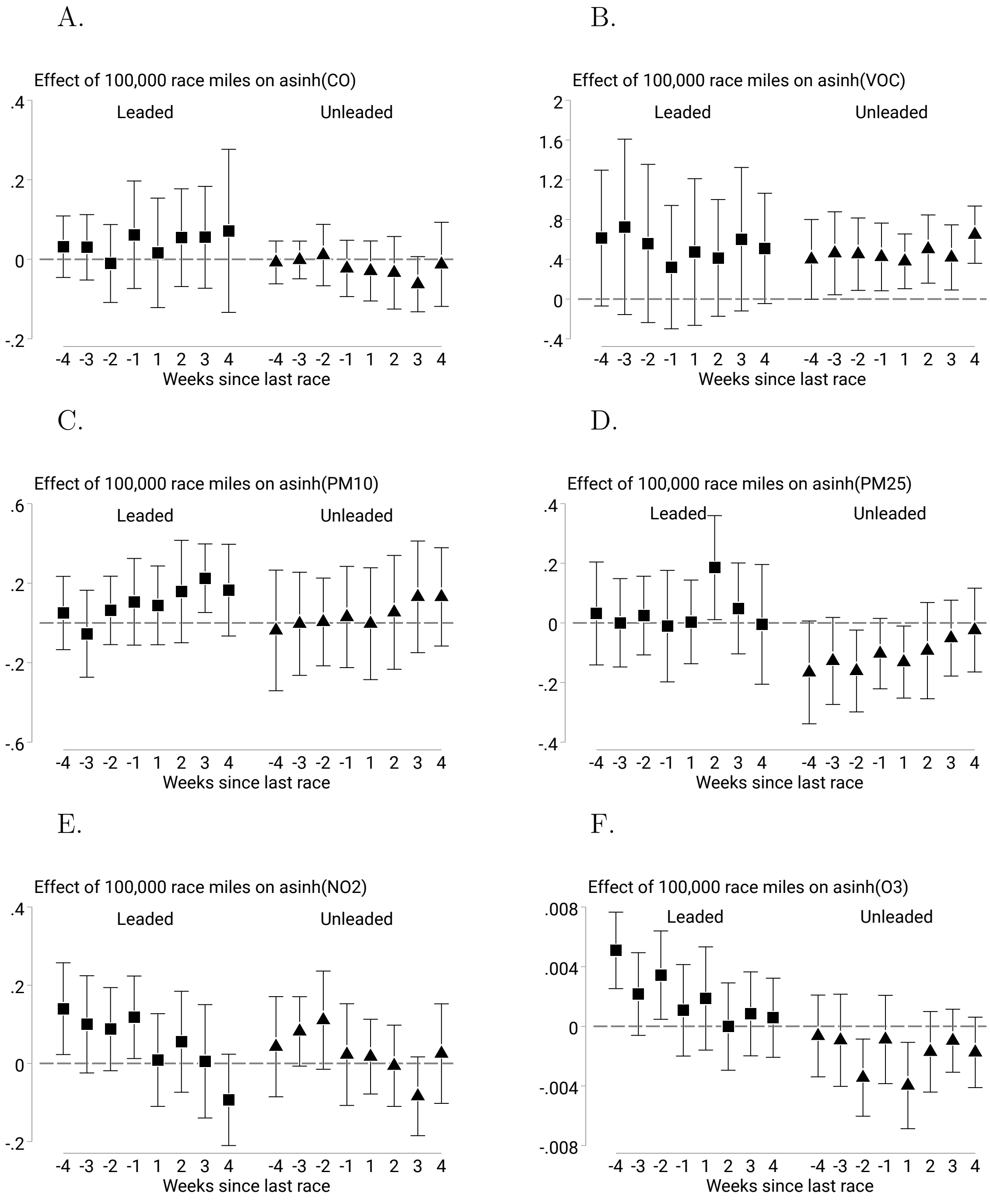



*Note:* The top left panel (A) shows the effect of 100,000 NASCAR/ARCA miles on ambient lead concentrations when controlling for IndyCar race miles. The top right panel (B) shows the effect of 100,000 NASCAR/ARCA miles on ambient lead concentrations when controlling for baseball attendance. The bottom left panel (C) shows the effect of 100,000 IndyCar race miles on ambient lead concentrations. The bottom right (D) panel shows the effect of 10,000,000 baseball attendees on ambient lead concentrations. For each panel, all coefficients come from the same regression. Each regression controls for week-by-year fixed effects, monitor-by-year fixed effects, and daily monitor-specific weather controls. The dependent variable is asinh(Pb). We estimate effects separately before and after 2007 to approximately match the timing of deleading for NASCAR and ARCA. Brackets represent 95% confidence intervals, calculated from robust standard errors clustered at the county level. Negative values on the x-axis indicate monitor readings that occurred in the weeks prior to a race, and positive values on the x-axis indicate monitor readings that occurred in the weeks after a race. For all outcomes only race miles (or attendance) within 50 miles are included.

spurious. There is no increase in $PM_{10}$ which contains $PM_{2.5}$ as a subset, and the positive estimated effect is delayed until two weeks after a race and does not show the same decay pattern one would expect as in Figure 3.

The final row shows no effect of races on $NO_2$ and $O_3$ concentrations. $NO_2$ concentrations tend to be lower in the weeks following both leaded and unleaded races, so although it tends to change after a race, there is no differential effect of leaded versus unleaded races that would confound identification of the effects of lead on mortality outcomes. For ozone, we cannot distinguish any of the leaded miles estimates from zero except for the estimates for a race four and two weeks in the future. Similar to the $PM_{2.5}$ specification, $O_3$ levels appear to be slightly lower in the weeks surrounding unleaded races, but there is no clear, unique effect immediately following a race. Finding no evidence of increases in concentrations in non-Pb pollutants is not entirely surprising. NASCAR and ARCA use high-quality fuel and have extremely efficient engines, making it more likely for gasoline to combust into $CO_2$ and water rather than leaving uncombusted byproducts like CO. Unlike the hydrocarbons in gasoline, lead is not combusted into other compounds and thus either remains in a vehicle or is emitted from its tailpipe.

Figure A7: The effect of race miles within 50 miles on other pollutant concentrations.




*Note:* Each panel shows the effect of 100,000 leaded and unleaded race miles on ambient concentrations of a non-lead pollutant (CO, VOCs, $PM_{10}$, $PM_{2.5}$, $NO_2$, and $O_3$). For each panel, all coefficients come from the same regression. Each regression controls for week-by-year fixed effects, monitor-by-year fixed effects, and daily monitor-specific weather controls. The dependent variable is asinh(Pb). We estimate effects separately before and after 2007 to approximately match the timing of deleading for NASCAR and ARCA. Brackets represent 95% confidence intervals, calculated from robust standard errors clustered at the county level. Negative values on the x-axis indicate monitor readings that occurred in the weeks prior to a race, and positive values on the x-axis indicate monitor readings that occurred in the weeks after a race. For all outcomes only race miles (or attendance) within 50 miles are included.

**Estimated effects in terms of grams of lead** Table A7 translates effects in miles into effects in grams of lead using the race mile fuel efficiency estimates from Section A.10. The estimates indicate that a metric ton of lead emitted from racing approximately doubles lead concentrations the following week.

**Distributions of wind direction and the initial bearing between the track and monitor** Figure A8 explores wind direction in the ambient lead dataset. The hexagonal heatmap shows the empirical joint distribution of the absolute value of the angle difference between the initial bearing (direction from track to monitor) and the prevailing wind direction at the monitor, and of the absolute value of the angle difference between the initial bearing (direction from track to monitor) and the prevailing wind direction at the track. The smoothed distributions are the marginal distributions. Observations to the left are those where the wind at the track is blowing toward the monitor, i.e. the monitor is downwind of the track, and observations to the bottom are those where the wind at the monitor is also blowing downwind. Wind at the monitor matters because the cells in the wind reanalysis dataset are relatively large. We define downwind to be if the prevailing wind direction is within 22.5° of the initial bearing, a wedge of 45° in total. Lighter colors indicate more observations in that hexagonal cell.

If the wind angle difference was uniformly distributed from 0 to 180°, we would expect 12.5% of observations to be downwind for each marginal distribution. This is not the case, only 9.7% percent of observations have the wind at the track blowing downwind, while 8.2% have wind at the monitor blowing downwind. In fact, most of our observations are upwind, and the mode of both marginal distributions is such that the prevailing direction blows in the opposite direction of the monitor. This is consistent with monitors tending to be located in populated areas, and large facilities like racetracks being located downwind of these areas to avoid noise and air pollution.

In total, our data contain fewer than 200 observations where the monitor is downwind of the track, significantly hampering our ability to estimate a model exploring the effect of wind direction.

### iii. Excluding observations by monitor-racetrack distance deciles

Figure A9 displays estimates using the same specification as column 7 of Table 1, but where each decile of monitor-racetrack is excluded, cutting our sample by 10%. This test is to determine which distances are driving most of the effect while preserving statistical power. The estimates are insensitive to excluding deciles.

### iv. Alternative sample periods

Figure A10 plots estimated effects of 100,000 leaded and unleaded miles on concentrations as a function of the years in our dataset using the same specification as column 7 of Table 1. The top graph shows estimates as we expand the dataset year by year back to 1957. Expanding the panel back in time decreases the effect of leaded miles down to 0.1, however the effect is still statistically distinguishable from that of unleaded miles.

The bottom graph restricts the dataset to the two years around deleading for the left-most pair of estimates, 2006-2007, and then expands in both directions year by year until we arrive at the 1996-2018 dataset used in the main ambient lead analysis in the right-most pair of estimates. With panel lengths under 4 years, the estimates are very noisy and in fact have the opposite of expected signs when there are only two years in the panel. As the panel length grows, the effects become more precise, particularly for unleaded miles, and the signs are correct.

### v. Raw data example of our fixed effects strategy

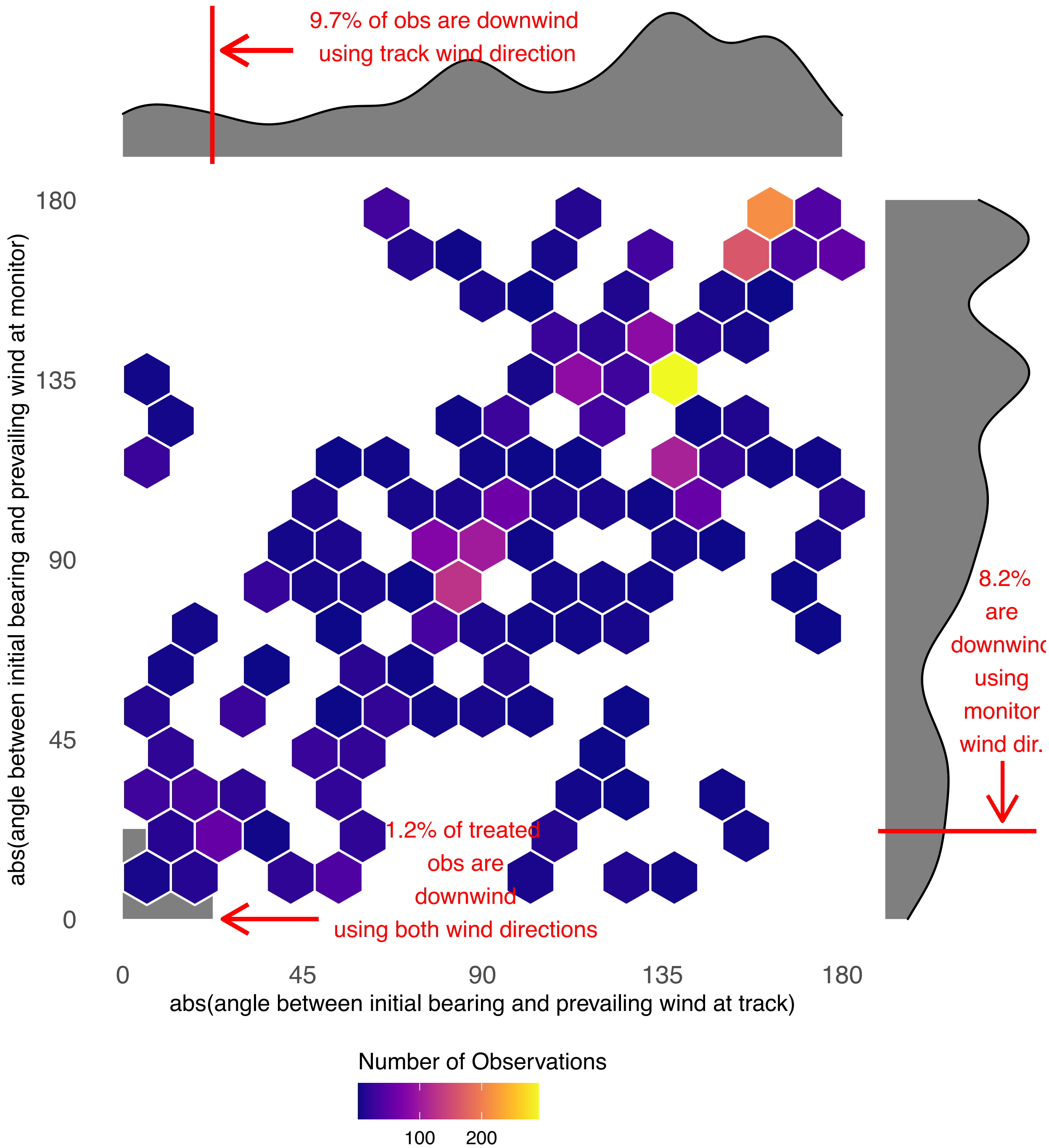
Figure A11 plots an example of the raw data for the ambient lead concentrations. We selected Bristol Motor Speedway since it has three nearby lead monitors in operation in 1998 which facilitates a clear graph. The solid markers show the amount of leaded race miles in the past week, while the hollow markers show lead concentration readings at the nearby monitors. The vertical dashed lines denote the dates of the two races at Bristol in 1998. Prior to the races, lead concentrations fluctuate but are generally low. In the week after the races, concentrations spike at all three monitors to their highest or second-highest levels in the two-month window before falling back to normal levels.

Table A7: The effect of estimated grams of lead emitted from racing in the past week on ambient lead concentrations.

| | (1)<br>asinh(Pb) | (2)<br>mean Pb |
|---|---|---|
| Lead emitted from racing in past week, metric tons | 0.89*** | 1.69** |
| | (0.31) | (0.83) |
| Daily weather controls | Yes | Yes |
| Monitor-by-year fixed-effects | Yes | Yes |
| Week-by-year fixed-effects | Yes | Yes |
| Standard errors clustered by | County | County |
| Observations | 312277 | 312277 |

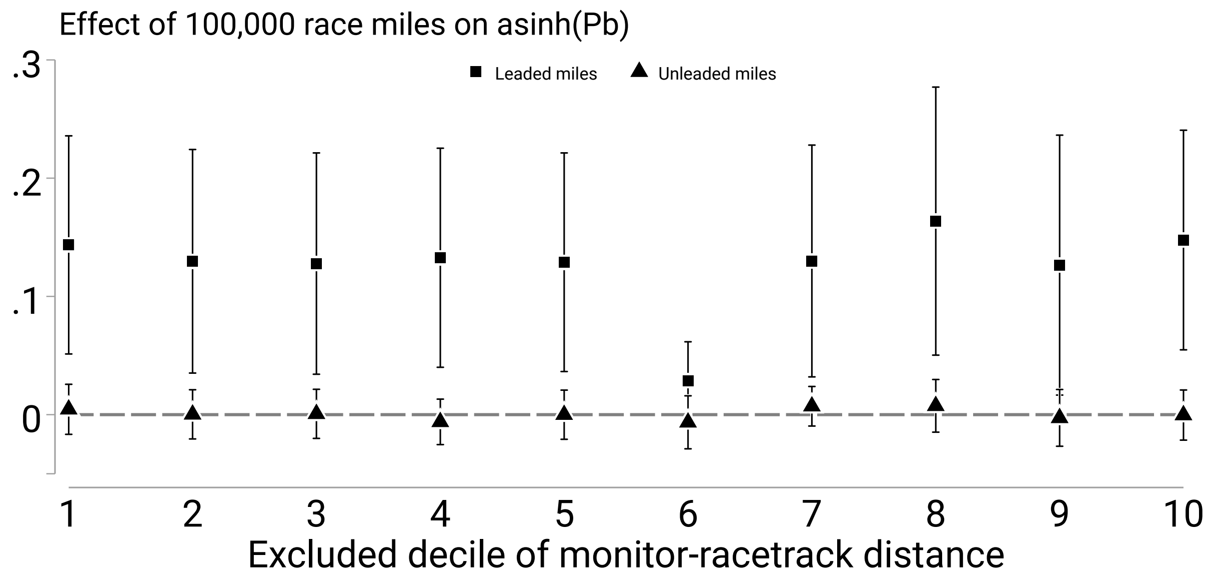
*Note:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the county level in parentheses. The procedure for estimating grams of lead emitted can be found in Section A.10. Monitor-specific daily weather variables include air temperature, pressure, relative humidity, wind speed, and daily precipitable water.

Figure A8: Joint distribution of the difference between wind direction at the monitor and initial bearing from the racetrack to the monitor, and the difference between wind direction at the racetrack and initial bearing from the racetrack to the monitor.




*Note:* Differences between initial bearing (direction from track to monitor) and wind direction at the monitor (y axis) and wind direction at the racetrack (x axis). Smoothed marginal distributions are shown in gray, and the joint distribution is plotted as a heatmap. Lighter colors indicate more observations.

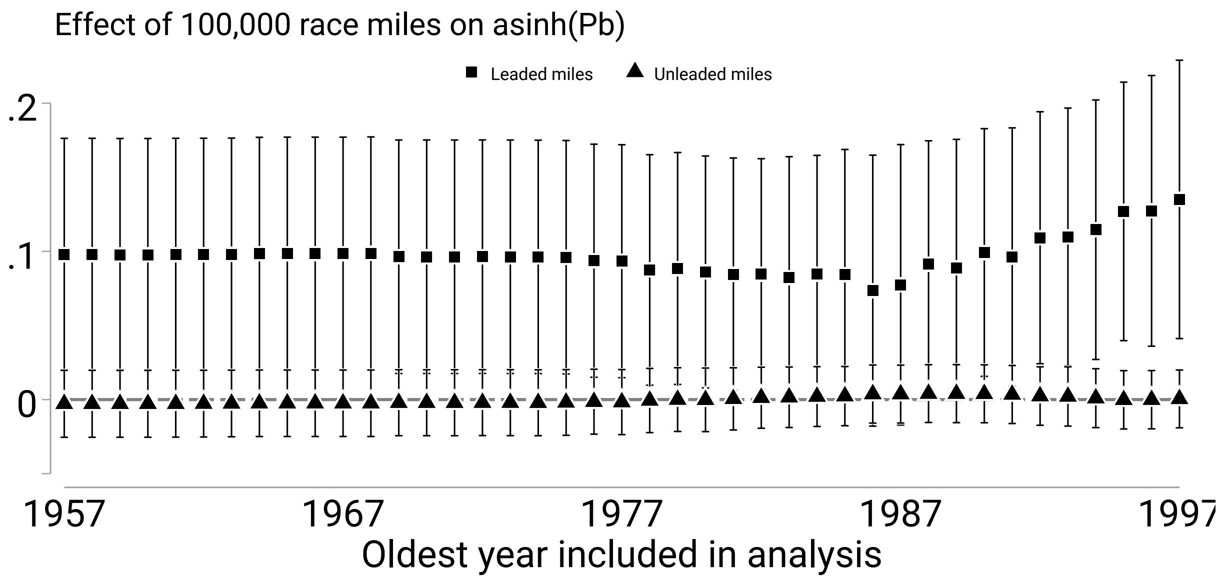
Figure A9: The effect of race miles within 50 miles on ambient lead excluding deciles of distance between monitor and racetrack.



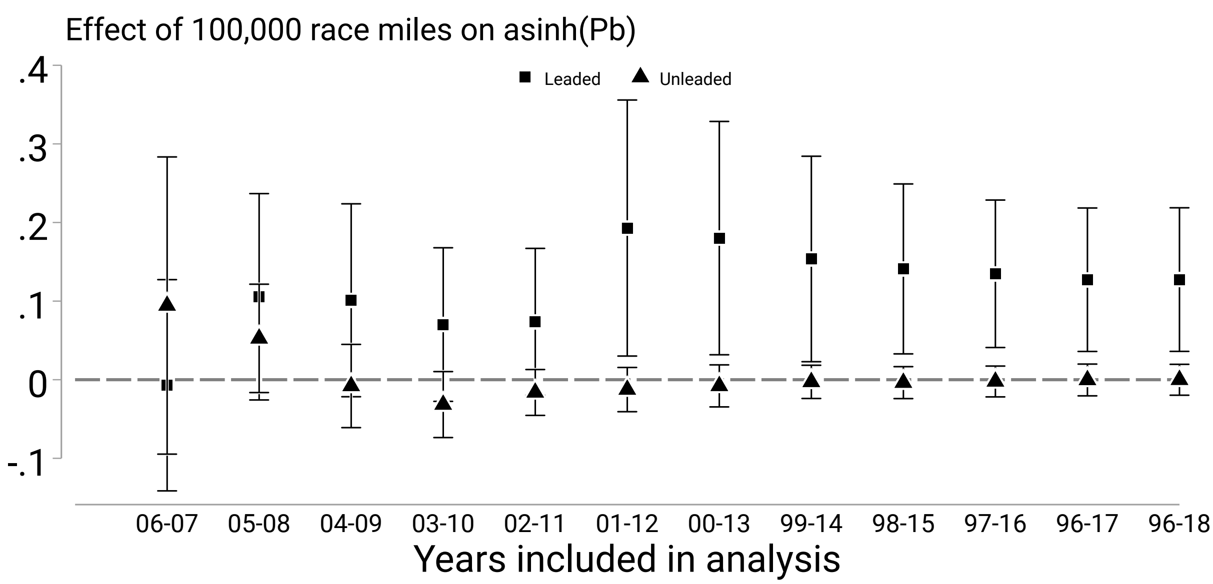

*Note:* This figure plots estimates corresponding to column 7 of Table 1 when excluding deciles of observations by monitor-racetrack distance within 50 miles. Each regression controls for week-by-year fixed effects, monitor-by-year fixed effects, and daily monitor-specific weather controls. The dependent variable is asinh(Pb). Only race miles within 50 miles in the past week are included.

Figure A10: The effect of race miles within 50 miles on ambient lead by weeks since most recent race (left) and by distance from track (right).

A.

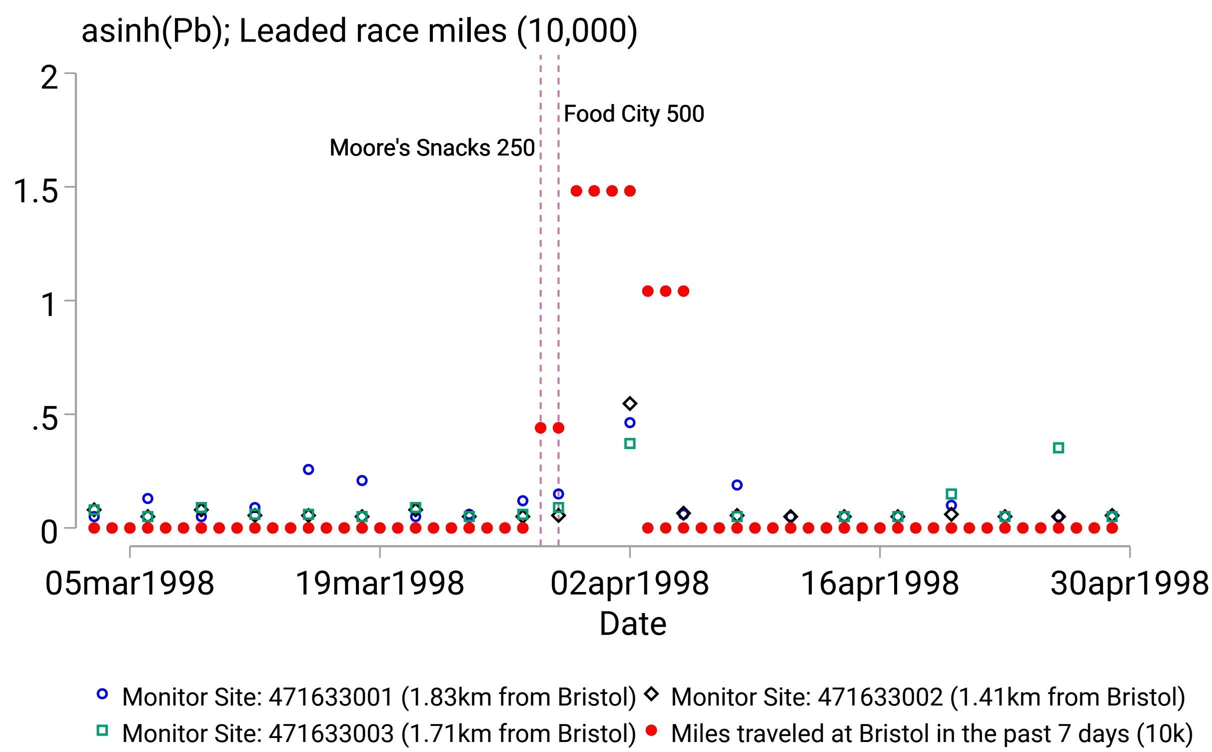



B.




*Note:* This figure plots estimates corresponding to column 7 of Table 1 when varying the panel length. The top panel shows estimates when expanding the panel back to 1957, year by year. The bottom panel shows estimates when expanding the panel from just 2006-2007 to our full sample from 1996-2018. Each regression controls for week-by-year fixed effects, monitor-by-year fixed effects, and daily monitor-specific weather controls. The dependent variable is asinh(Pb). Only race miles within 50 miles in the past week are included.

Figure A11: Raw data from three monitors around Bristol Motor Speedway for ambient lead concentration regressions.




*Note:* The red markers are our variable of interest in equation (1), the number of leaded race miles in the past week within 50 miles. The hollow markers are the inverse hyperbolic sine lead readings at 3 nearby monitors.

## A.7 Blood lead

### i. Difference-in-differences

Table A8 reports results from difference-in-differences regressions estimating the relative effect of having leaded races in a county or in a bordering county. Column 1 reports estimates only including county and year fixed effects, column 2 adds in the set of controls, column 3 adds in state-specific linear time trends, column 4 replaces the linear trends with state-by-year effects. The next four columns repeat the first four but use leaded miles instead of dummy variables. Column 9 is the same as column 4 but where the the indicator variable is now a continuous measure of the number of races. Columns 10 and 11 cluster standard errors at the state level and using the Conley approach with a 150 mile distance cutoff from the county centroid. The estimated effects are robust across all specifications, however estimates for the effect in per-mile terms are noisy.

### ii. Untransformed outcome variable

Figure A12 plots estimates corresponding to the same regression as the one used for Figure 5 but where the outcome variable is the untransformed percent of children tested with elevated blood lead. Estimates for race counties are similar to our main results with the inverse hyperbolic sine transformation.

### iii. Targeted testing in high risk areas

The data we use for the blood lead analysis comes from monitoring efforts that are targeted at high risk areas in ways we do not observe. This may confound our analysis in two ways. The first is if the the number of children tested in a county is changing differentially across county types. For example, if a county added more children to the testing pool, it may be because of a newly discovered lead hot spot, or additional funding which allows for testing in more marginal areas. Both of these effects would bias estimates but in opposite directions. The second way is if the targeting scheme within counties were changed differentially over time even though the number of children tested is constant. For example, if race counties spuriously changed their blood lead sampling population to even higher risk areas once NASCAR and ARCA deleaded, then our results would be biased toward zero.

To test whether these phenomena are occurring, we first estimate an unweighted version of equation (2) with the inverse hyperbolic sine of the number of children tested as the dependent variable.[33] The estimates for the event study are shown in Figure A13. There is

[33] It is unweighted since the weights we previously used were the number of children tested.

no clear differential trend across county types in the number of children tested except for a slightly increasing, but small, pre-trend in border counties. This suggests that differential changes in the number of children tested is not a concern for identification.

Differential trends in targeting schemes within counties is more difficult to directly test. Figure 5 provides supporting evidence in a similar way to pre-trends tests. Given that the number of children tested does not exhibit differential trends, if targeting is changing differentially in the post-period (which comprises most of our panel), we would expect EBLL rates to show differential trends across county types in the post-period. This is not the case. The trends are flat, providing supporting evidence that differential changes in targeting is not occurring.

Table A8: Effect of races and race miles on EBLL rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 0.22** | 0.21** | 0.21** | 0.16*** | | | | | | 0.16** | 0.16*** |
| | (0.10) | (0.10) | (0.09) | (0.06) | | | | | | (0.06) | (0.05) |
| 1(Leaded Race in Border County) | 0.09* | 0.08* | 0.07* | 0.05 | | | | | | 0.05 | 0.05 |
| | (0.05) | (0.05) | (0.04) | (0.04) | | | | | | (0.05) | (0.04) |
| Leaded Miles in County (100k) | | | | | 0.50 | 0.50 | 0.53* | 0.23 | | | |
| | | | | | (0.38) | (0.38) | (0.32) | (0.16) | | | |
| Leaded Miles in Border County (100k) | | | | | 0.15 | 0.12 | 0.15 | 0.04 | | | |
| | | | | | (0.13) | (0.12) | (0.10) | (0.07) | | | |
| Leaded Races in County | | | | | | | | | 0.04* | | |
| | | | | | | | | | (0.02) | | |
| Leaded Races in Border County | | | | | | | | | 0.01* | | |
| | | | | | | | | | (0.01) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 22832 | 22832 | 22832 | 22831 | 22832 | 22832 | 22832 | 22831 | 22831 | 22831 | 22831 |

*Note:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on asinh(EBLL). Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on asinh(EBLL). Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Counties are weighted by the square root of the number of children tested.

## A.8 Mortality

### i. Causes of death

In our mortality analysis we aggregate causes of death into six categories: cardiovascular, ischemic heart disease, respiratory, diabetes, deaths of despair, and all-cause. We categorize each death using the primary cause of death reported on the death certificate and the CDC 113 cause of death recode. Each CDC 113 cause recode is an aggregate of ICD-10 causes of death. For each cause of death we use in the paper, we list below the category, the set of CDC 113 recodes composing the category, the associated ICD-10 codes for the category, and one example cause of death included in the category. For deaths of despair, we use a set of ICD-10 codes reported in Case and Deaton (2015) and thus do not report a 113 recode.

- Cardiovascular: CDC 113: 53–75; ICD-10: I00–I78; Hypertensive heart and renal disease
- Ischemic heart disease: CDC 113: 58–63; ICD-10: I20–I25; Acute myocardial infarction
- Respiratory: CDC 113: 76–89; ICD-10: J10-J18, J20–J22,J40–J47, J60–J66, J68, J69, J00–J06, J30–J39, J67, J70–J98; Acute bronchitis
- Diabetes: CDC 113: 46 ; ICD-10: E10-E14; Diabetes mellitus
- Deaths of despair (drugs, suicide, liver), as defined in Case and Deaton (2015):
  - ICD-10 X40–X45; Accidental poisoning by and exposure to narcotics and psychodysleptics [hallucinogens], not elsewhere classified
  - ICD10 Y10–Y15; Poisoning by and exposure to nonopioid analgesics, antipyretics and antirheumatics, undetermined intent
  - ICD10 Y45, Y47, Y49; Adverse effects in therapeutic use: analgesics, antipyretics and anti-inflammatory drugs
  - ICD10 X60–84, Y87.0; Intentional self-harm
  - ICD10 K70, K73, K74; Alcoholic liver disease
- All-cause: All codes and causes.

### ii. Raw data

Figure A14 plots the data for cardiovascular, IHD, respiratory, and deaths of despair mortality. Cardiovascular mortality rates show a similar pattern to all-cause mortality rates.

IHD and respiratory mortality rates were generally higher in race counties relative to control counties prior to deleading, but then were about the same or lower afterwards. Deaths of despair mortality rates are decreasing everywhere prior to about 2009 before starting to increase. Prior to 2007, deaths of despair mortality rates were higher in race counties than border counties, and higher in border counties than control counties. Beginning in 2007 mortality rates across the three types of counties are similar.

### iii. Difference-in-differences

Tables A10-A13 display estimates for our alternative specifications for the mortality results. The specifications for the mortality tables are the same as the blood lead specifications in Table A8. Results are consistent with estimates in the main text and robust to different choices of fixed effects, treatment variables, and clustering.

### iv. Mortality analysis using publicly available data

Figure A15 reports results from the same analysis as reported in Figure 7, but using publicly available data instead of restricted access data. The publicly available data are provided by CDC Wonder and report the all-cause age-standardized mortality rate for those age 65 and above in each county-year. These data differ from the restricted access data in that they suppress death rates from any county-year that contain fewer than ten deaths. Over 96% of all county-year observations are included in the public data, accounting for over 99.5% of all deaths. Results are similar when using this alternative dataset.

### v. Elderly mortality placebo

Figure A16 displays event study estimates for our placebo cause of death: diabetes. We find little evidence of placebo effects. There is no trend over the full time-frame, nor any clear change in 2007.

### vi. Split-sample instrumental variables

Here we perform a split-sample IV similar to our mortality regressions to identify the effect of changes in ambient lead on mortality. Recognizing the fact that lead concentrations spike only temporarily after a race, we do the following for the first stage. At the county-year level, we estimate the effect of leaded races on ambient lead averaged across all weeks immediately following a race. Our first stage regression is

$$\text{Pb in week after races}_{scy} = \beta_l 1(\text{leaded race})_{scy} + \gamma \mathbf{X}_{scy} + \Theta_c + \Omega_{sy} + \varepsilon_{scy}. \tag{3}$$

$1(\text{leaded race})_{scy}$ is an indicator variable equal to 1 if county $c$ in state $s$ had a leaded race in year $y$. The remaining variables are identical to the mortality regressions. For counties with lead monitors and racetracks, Pb in week after $\text{races}_{scy}$ is the average across the lead concentrations in the 7 days immediately following races in county $c$ in year $y$. As stated above, we take this approach because Figure 3 indicates that lead concentrations spike in the first week after a race before declining back to baseline levels. Races also occur at the same time each year (e.g. the Daytona 500 is always in mid-February), so there are generally no changes in the season of when these lead readings occur. For counties with lead monitors but without racetracks (i.e. our control counties), $\text{Pb}_{scy}$ is the average lead concentration in these counties in weeks of the year that immediately followed races other counties. In other words, we average across all lead readings in control county $c$ when $1(\text{leaded race in past week})_{sdy}$ for any $d \neq c$. This lets us compare concentrations in race counties versus control counties on the same days of the year.

Our second stage regression is then

$$\text{mortality rate}_{scy} = \beta \text{Pb } \widehat{\text{after race}}_{scy} + \gamma \mathbf{X}_{scy} + \Theta_c + \Omega_{sy} + \varepsilon_{scy}. \tag{4}$$

where $Pb\ \widehat{after\ race}_{scy}$ is the first stage prediction. We calculate non-parametric confidence intervals and p-values using a bootstrap procedure. We generate 500 randomly drawn bootstrap samples and estimate the model on each sample. The 95% confidence interval is the $2.5^{th}$ and $97.5^{th}$ percentiles from the distribution of bootstrap sample estimates. Because of this, the confidence interval may not be symmetric about the point estimate. The p-value is the percent of times a bootstrap sample estimate is larger in absolute value than the absolute value of our point estimate.

Results are reported in Panel A of Table A15. The instrument is well powered and operates in the expected direction with a Kleibergen-Paap F-stat of 14.76 and a first stage coefficient of .04 ($se = .01$, $p < .01$). After using the inverse hyperbolic sine elasticity formula, the estimates show a 1 percent increase in the ambient lead level increases elderly mortality by roughly 9%, with about 50% of these deaths coming from Ischemic Heart Disease (IHD).

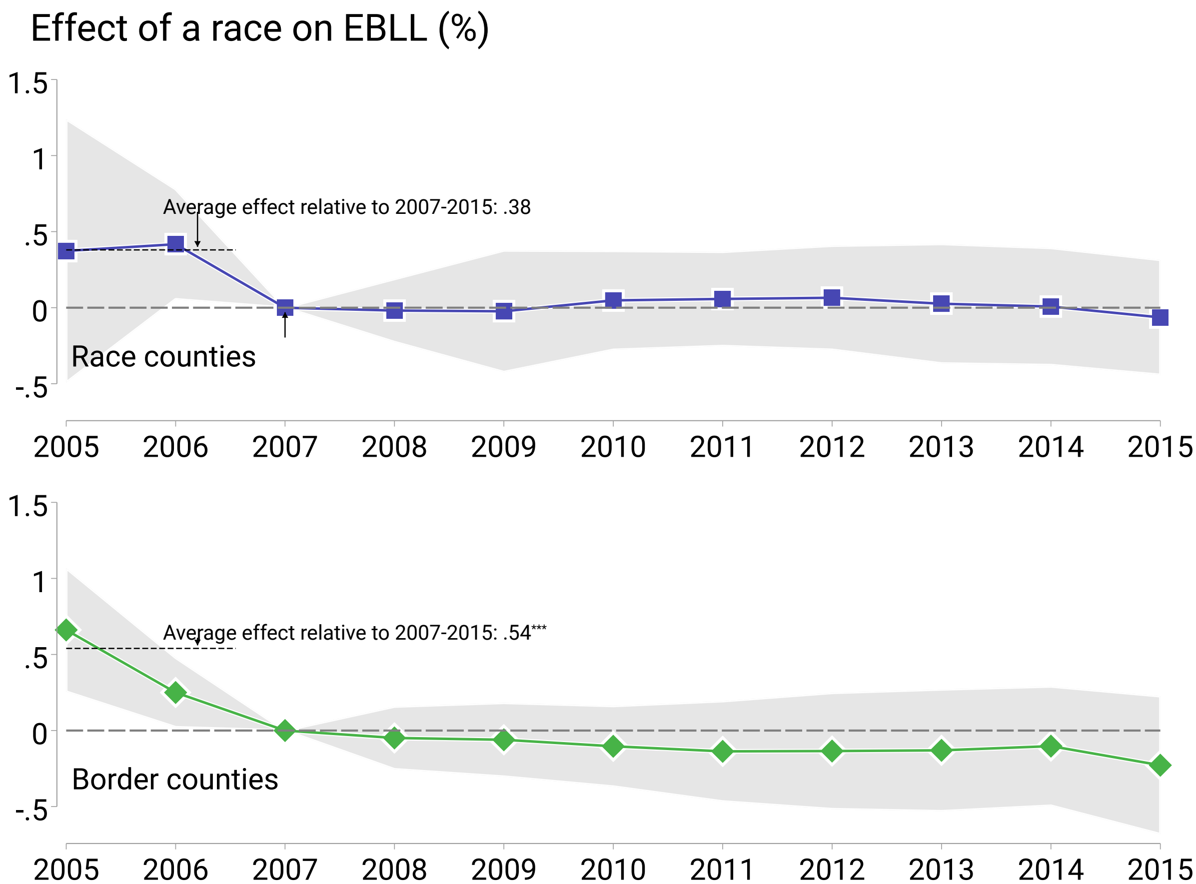
We obtain a nearly identical result when we use the untransformed lead level as our endogenous predictor instead of the asinh(Pb). This is displayed in column 7 of table A15. We find no statistically significant effect on deaths of despair or diabetes outcomes.

In addition to the split sample IV strategy, we also consider a more traditional instrumental variable strategy. Where we only use the subset of observations that have data on pollution, race miles, and mortality. This is the same subset of observations that is used to construct the first stage for results in panel A. Despite this analysis only having 3% of the observations as the split-sample analysis, we find consistent, albeit less precise, point estimates.

### vii. Infant mortality event studies

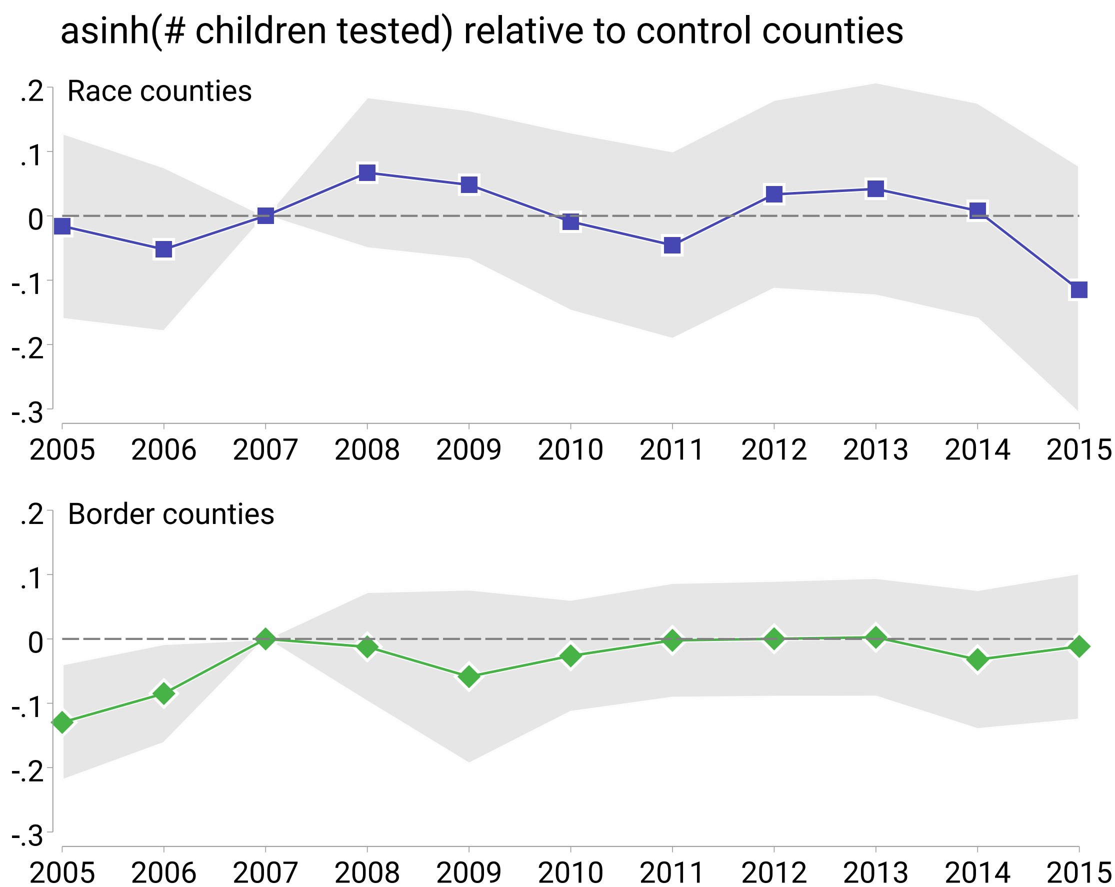
Figures A17 and A18 display event studies for infant mortality. We find no effect for our outcomes of interest or the placebos in either race counties or border counties.

Figure A12: The effect of leaded races on the percent of children with elevated blood lead levels in race and border counties.



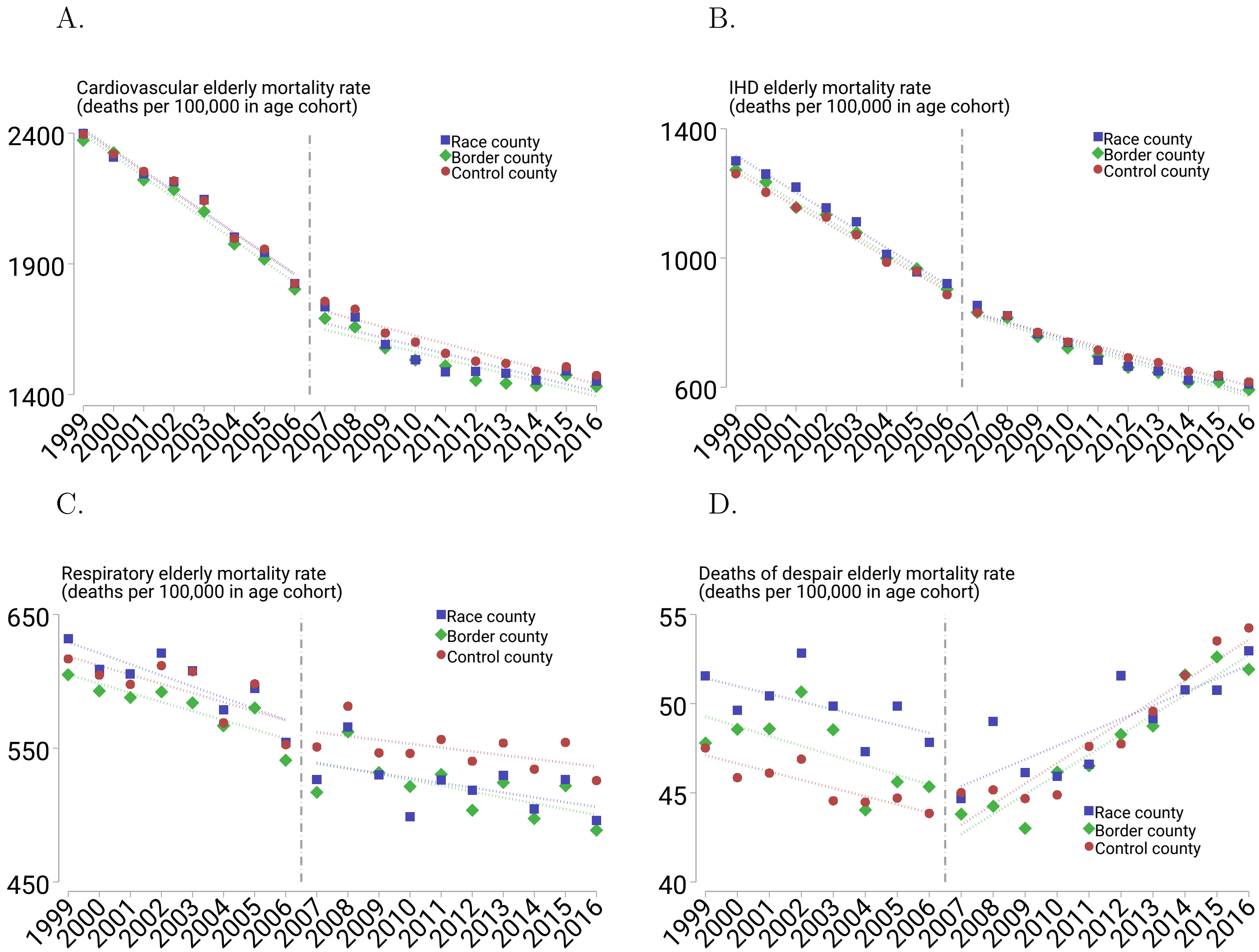

*Note:* The outcome variable is the untransformed percent of children tested with elevated blood lead. The top panel reports coefficients (blue squares) for children living in race counties. The bottom panel reports coefficients (green diamonds) for children living in border counties. Each coefficient represents the effect of being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients come from the same regression. The regression is weighted by the square root of the number of children tested. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level. The dashed line is the average effect from our preferred difference-in-differences regression, where the race county treated group consists of those counties that had at least one leaded race prior to 2007, and the border county treated group consists of those counties that did not have a leaded race but bordered a county with a leaded race prior to 2007. This regression defines the post-period as 2007 and after.

Figure A13: The association between the inverse hyperbolic sine of the number of children tested for elevated blood lead and county type.




*Note:* The top panel reports coefficients (blue squares) for children living in race counties. The bottom panel reports coefficients (green diamonds) for children living in border counties. Each coefficient represents the effect of being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients come from the same regression. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level.

Figure A14: Mean elderly mortality rates by cause of death, year, and county type.




*Note:* Each panel presents the mean age-standardized elderly death rate by county type weighted by the square root of elderly population for each cause of death. The elderly population of the entire U.S. in the year 2000 was used as the reference population for standardization. Panel A shows cardiovascular death rate, panel B the death rate from ischemic heart disease (IHD), panel C shows the respiratory mortality death rate, and panel D shows the deaths of despair death rate. Exact ICD-10 codes for each of these causes of death are reported in Section i.. County type refers to if there was a NASCAR or ARCA race in that county (blue squares) or in a border county (green diamond) in that year. All other counties are considered control counties (red circle). For this figure and in our regression estimates we use a balanced panel of counties.

## Table A9: Effect of races and race miles on all-cause elderly mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 122.4*** | 104.0*** | 99.0*** | 91.4*** | | | | | | 91.4*** | 91.4*** |
| | (30.5) | (27.6) | (28.1) | (28.9) | | | | | | (27.3) | (28.1) |
| 1(Leaded Race in Border County) | 69.3*** | 56.5*** | 42.2** | 37.9** | | | | | | 37.9* | 37.9** |
| | (17.1) | (16.1) | (17.0) | (18.0) | | | | | | (21.0) | (18.2) |
| Leaded Miles in County (100k) | | | | | 279.9*** | 224.9** | 185.3* | 161.5 | | | |
| | | | | | (100.5) | (96.7) | (103.1) | (103.2) | | | |
| Leaded Miles in Border County (100k) | | | | | 176.8*** | 138.2*** | 70.5* | 54.4 | | | |
| | | | | | (42.4) | (40.0) | (42.8) | (44.5) | | | |
| Leaded Races in County | | | | | | | | | 21.4** | | |
| | | | | | | | | | (9.1) | | |
| Leaded Races in Border County | | | | | | | | | 6.5* | | |
| | | | | | | | | | (3.9) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly all-cause mortality rate. Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly all-cause mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

## Table A10: Effect of races and race miles on elderly cardiovascular mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 30.7** | 25.5* | 41.5*** | 36.6** | | | | | | 36.6** | 36.6** |
| | (14.8) | (14.1) | (15.0) | (15.5) | | | | | | (14.5) | (15.1) |
| 1(Leaded Race in Border County) | 22.3** | 18.1* | 14.0 | 12.4 | | | | | | 12.4 | 12.4 |
| | (10.5) | (10.4) | (9.9) | (10.5) | | | | | | (11.1) | (10.4) |
| Leaded Miles in County (100k) | | | | | 73.4 | 62.9 | 92.8** | 76.3* | | | |
| | | | | | (47.7) | (44.6) | (45.6) | (46.2) | | | |
| Leaded Miles in Border County (100k) | | | | | 62.1** | 48.3** | 38.3* | 28.6 | | | |
| | | | | | (24.2) | (24.2) | (23.0) | (24.2) | | | |
| Leaded Races in County | | | | | | | | | 10.2** | | |
| | | | | | | | | | (4.3) | | |
| Leaded Races in Border County | | | | | | | | | 2.5 | | |
| | | | | | | | | | (2.2) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly cardiovascular mortality rate. Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly cardiovascular mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

## Table A11: Effect of races and race miles on ischemic heart disease elderly mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 48.5** | 41.9* | 55.0*** | 53.6** | | | | | | 53.6*** | 53.6*** |
| | (23.3) | (23.5) | (21.3) | (21.7) | | | | | | (18.9) | (20.8) |
| 1(Leaded Race in Border County) | 33.5*** | 28.7*** | 19.6** | 19.8** | | | | | | 19.8* | 19.8** |
| | (10.6) | (10.2) | (8.9) | (9.2) | | | | | | (11.1) | (8.8) |
| Leaded Miles in County (100k) | | | | | 114.1* | 99.3 | 134.9** | 129.9** | | | |
| | | | | | (66.7) | (66.0) | (58.9) | (60.1) | | | |
| Leaded Miles in Border County (100k) | | | | | 94.3*** | 82.2*** | 70.1*** | 67.9*** | | | |
| | | | | | (25.1) | (24.0) | (19.5) | (20.5) | | | |
| Leaded Races in County | | | | | | | | | 15.0*** | | |
| | | | | | | | | | (5.8) | | |
| Leaded Races in Border County | | | | | | | | | 6.5*** | | |
| | | | | | | | | | (2.0) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly ischemic heart disease mortality rate. Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly ischemic heart disease mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

## Table A12: Effect of races and race miles on respiratory elderly mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 34.0*** | 28.0*** | 22.4*** | 20.5*** | | | | | | 20.5*** | 20.5*** |
| | (10.2) | (9.2) | (6.9) | (7.0) | | | | | | (6.4) | (6.9) |
| 1(Leaded Race in Border County) | 13.1*** | 9.2** | 6.1 | 4.8 | | | | | | 4.8 | 4.8 |
| | (4.9) | (4.6) | (4.6) | (4.9) | | | | | | (7.1) | (5.1) |
| Leaded Miles in County (100k) | | | | | 90.0*** | 73.2** | 50.8** | 44.0* | | | |
| | | | | | (31.9) | (29.8) | (25.5) | (24.2) | | | |
| Leaded Miles in Border County (100k) | | | | | 27.4** | 18.2 | 1.6 | –1.9 | | | |
| | | | | | (12.2) | (11.4) | (11.2) | (11.8) | | | |
| Leaded Races in County | | | | | | | | | 5.8*** | | |
| | | | | | | | | | (2.1) | | |
| Leaded Races in Border County | | | | | | | | | 0.5 | | |
| | | | | | | | | | (1.1) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly respiratory mortality rates. Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly respiratory mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

Table A13: Effect of races and race miles on deaths of despair elderly mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | 4.1*** | 4.1*** | 4.6*** | 4.3*** | | | | | | 4.3*** | 4.3*** |
| | (1.2) | (1.3) | (1.2) | (1.3) | | | | | | (1.5) | (1.2) |
| 1(Leaded Race in Border County) | 1.6** | 1.6** | 1.6** | 1.6** | | | | | | 1.6** | 1.6** |
| | (0.7) | (0.7) | (0.7) | (0.7) | | | | | | (0.7) | (0.7) |
| Leaded Miles in County (100k) | | | | | 7.6** | 6.6* | 7.6** | 6.7* | | | |
| | | | | | (3.3) | (3.5) | (3.5) | (3.5) | | | |
| Leaded Miles in Border County (100k) | | | | | 3.2* | 3.4** | 3.3** | 2.9* | | | |
| | | | | | (1.7) | (1.6) | (1.6) | (1.6) | | | |
| Leaded Races in County | | | | | | | | | 0.9** | | |
| | | | | | | | | | (0.4) | | |
| Leaded Races in Border County | | | | | | | | | 0.4** | | |
| | | | | | | | | | (0.2) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

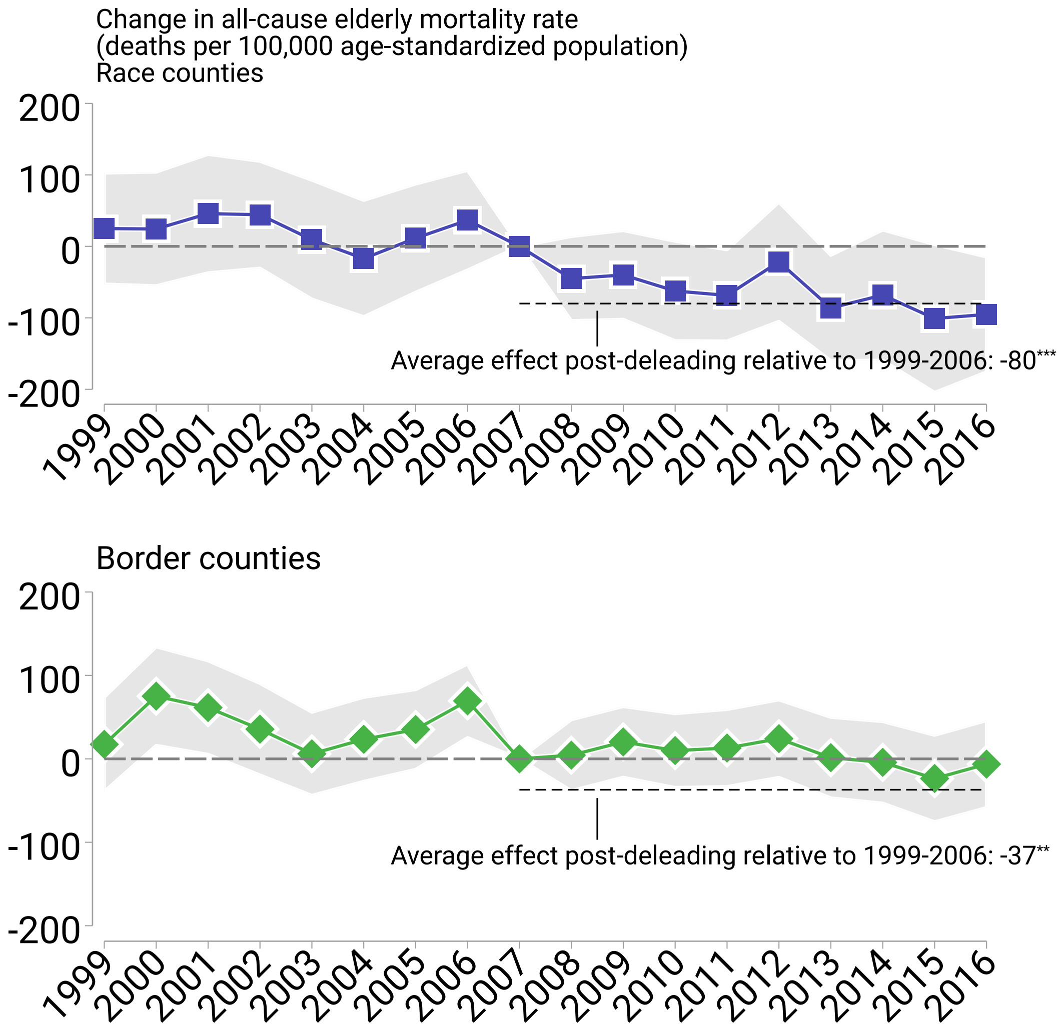
*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly deaths of despair mortality rates Case and Deaton (2015). Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly deaths of despair mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

Table A14: Effect of races and race miles on diabetes elderly mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1(Leaded Race in County) | –6.7 | –4.0 | –3.2 | –2.6 | | | | | | –2.6 | –2.6 |
| | (4.1) | (4.1) | (3.2) | (3.3) | | | | | | (3.4) | (3.2) |
| 1(Leaded Race in Border County) | –1.3 | –0.3 | –0.9 | –0.9 | | | | | | –0.9 | –0.9 |
| | (2.2) | (2.2) | (2.0) | (2.1) | | | | | | (2.2) | (2.1) |
| Leaded Miles in County (100k) | | | | | –21.3** | –14.6 | –12.1 | –10.8 | | | |
| | | | | | (10.1) | (10.4) | (7.5) | (8.0) | | | |
| Leaded Miles in Border County (100k) | | | | | 0.2 | 3.5 | 0.5 | –0.0 | | | |
| | | | | | (5.0) | (4.9) | (4.7) | (4.9) | | | |
| Leaded Races in County | | | | | | | | | –0.9 | | |
| | | | | | | | | | (0.9) | | |
| Leaded Races in Border County | | | | | | | | | 0.0 | | |
| | | | | | | | | | (0.5) | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | No | Yes | Yes | Yes | No | No | No | No |
| State-Specific Linear Time Trends | No | No | Yes | No | No | No | Yes | No | No | No | No |
| State-by-Year FE | No | No | No | Yes | No | No | No | Yes | Yes | Yes | Yes |
| Controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered Standard Errors | County | County | County | County | County | County | County | County | County | State | Conley (150 miles) |
| Observations | 58063 | 56202 | 56202 | 56184 | 58063 | 56202 | 56202 | 56184 | 56184 | 56184 | 56184 |

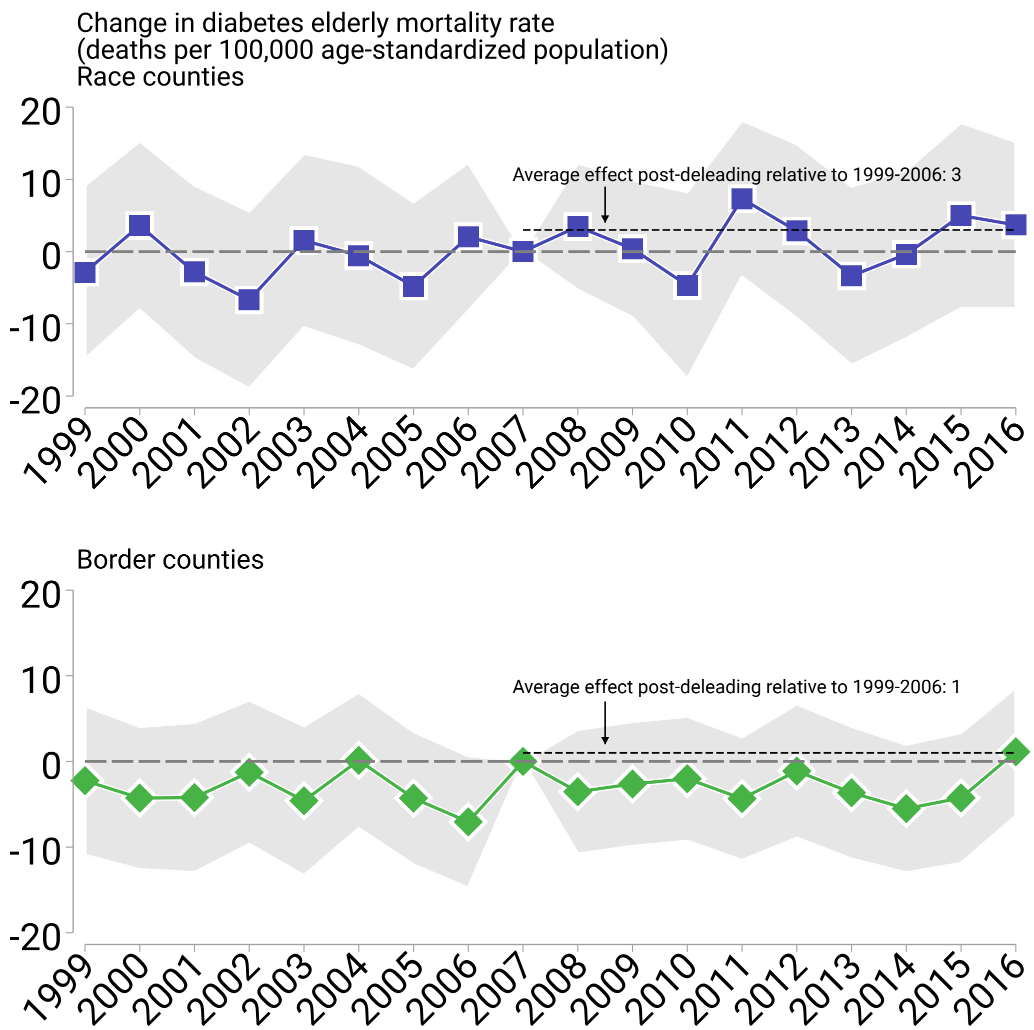
*Note:* * p < 0.1, ** p < 0.05, *** p < 0.01. Columns 1-4: Estimated effects of having a NASCAR or ARCA race in county, or in a bordering county, on the elderly diabetes mortality rates. Columns 5-8: Estimated effects of 100,000 leaded race miles in county, or in a bordering county, on the elderly diabetes mortality rate. Column 9: Same as column 4 but where the indicator for having a race is replaced with a continuous measure of the number of races. Columns 10-11: same as column 4 but with state clusters or Conley standard errors. Robust standard errors clustered at the county level in parentheses for columns 1-9. Weights are given by the square root of the elderly population.

Figure A15: The effect of leaded races on all-cause elderly mortality rates in race and border counties using publicly available mortality data.




*Note:* These data use publicly available data obtained from CDC Wonder. These data differ from the restricted access data in that they suppress death rates from any county-year that contain fewer than ten deaths. The top panel reports coefficients (blue squares) for race counties, and the bottom panel reports coefficients (green diamonds) for border counties. Each coefficient represents the effect on the age-standardized, all-cause elderly morality rate from being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients come from the same regression. The elderly population of the entire U.S. in the year 2000 was used as the reference population for age standardization. The regression is weighted by the square root of the elderly population. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level. The dashed line is the average effect of deleading from our preferred difference-in-differences regression, where the race county treated group consists of those counties that had at least one leaded race prior to 2007, and the border county treated group consists of those counties that did not have a leaded race but bordered a county with a leaded race prior to 2007. This regression defines the post-period as 2007 and after.

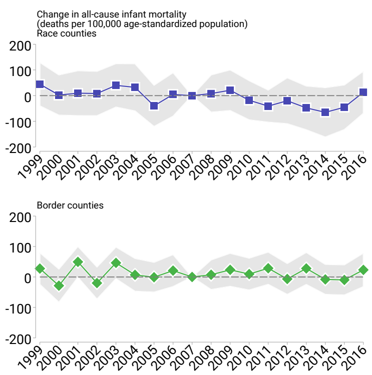
Figure A16: The effect of leaded races on placebo elderly mortality rates in race and border counties.



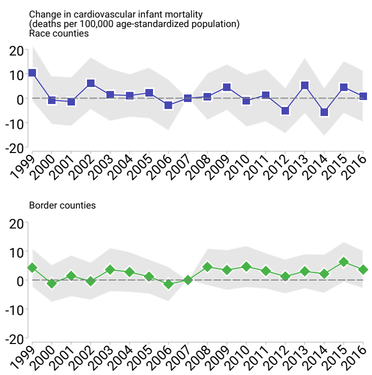

*Note:* The figure shows estimate for elderly diabetes mortality. The top panel reports coefficients (blue squares) for race counties, and the bottom panel reports coefficients (green diamonds) for border counties. Each coefficient represents the effect on the age-standardized, elderly morality rate for that cause of death from being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients come from the same regression. The elderly population of the entire U.S. in the year 2000 was used as the reference population for age standardization. Each regression is weighted by the square root of the elderly population. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level. The dashed line is the average effect of deleading from our preferred difference-in-differences regression where, the race county treated group consists of those counties that had at least one leaded race prior to 2007, and the border county treated group consists of those counties that did not have a leaded race but bordered a county with a leaded race prior to 2007. This regression defines the post-period as 2007 and after.

Figure A17: The effect of leaded races on infant mortality rates in race and border counties.

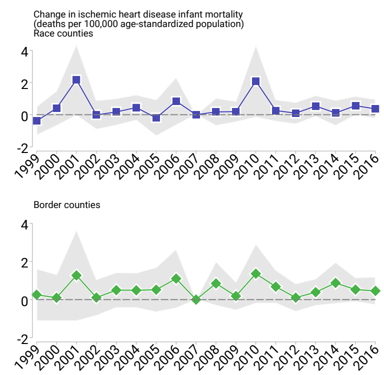
A. All-cause



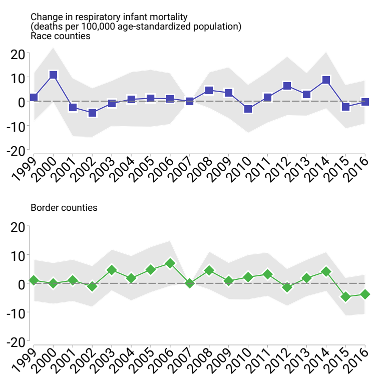

B. Cardiovascular



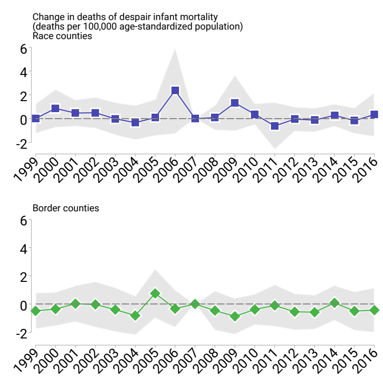

C. IHD




D. Respiratory

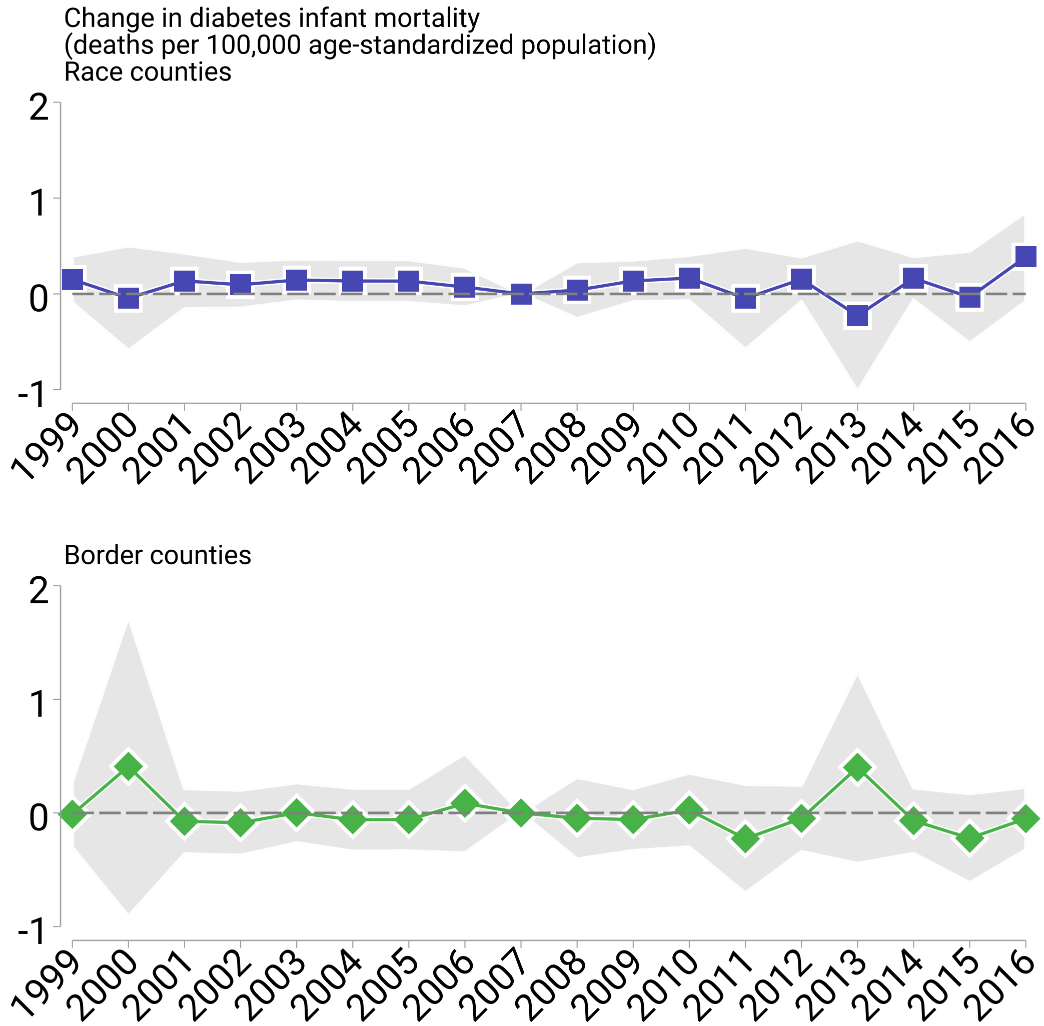



E. Deaths of despair




*Note:* Subfigure A shows estimate for infant all-cause mortality, Subfigure B shows estimates for infant cardiovascular mortality, Subfigure C shows estimates for infant ischemic heart disease (IHD) mortality, Subfigure D shows estimates for infant respiratory mortality, and Subfigure E shows estimates for infant deaths of despair mortality. The top panel of each subfigure reports coefficients (blue squares) for race counties, and the bottom panel reports coefficients (green diamonds) for border counties. Each coefficient represents the effect on the age-standardized, infant morality rate for that cause of death from being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients in each subfigure come from the same regression. The infant population of the entire U.S. in the year 2000 was used as the reference population for age standardization. Each regression is weighted by the square root of the infant population. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level. The dashed line is the average effect of deleading from our preferred difference-in-differences regression, where the race county treated group consists of those counties that had at least one leaded race prior to 2007, and the border county treated group consists of those counties that did not have a leaded race but bordered a county with a leaded race prior to 2007. This regression defines the post-period as 2007 and after.

Figure A18: The effect of leaded races on placebo infant mortality rates in race and border counties.




*Note:* The figure shows estimate for infant diabetes mortality. The top panel reports coefficients (blue squares) for race counties, and the bottom panel reports coefficients (green diamonds) for border counties. Each coefficient represents the effect on the age-standardized, infant morality rate for that cause of death from being in a particular county type relative to 2007, which is omitted. The regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. All coefficients come from the same regression. The infant population of the entire U.S. in the year 2000 was used as the reference population for age standardization. Each regression is weighted by the square root of the infant population. The shaded gray areas denote the 95% confidence interval calculated from robust standard errors clustered at the county level. The dashed line is the average effect of deleading from our preferred difference-in-differences regression, where the race county treated group consists of those counties that had at least one leaded race prior to 2007, and the border county treated group consists of those counties that did not have a leaded race but bordered a county with a leaded race prior to 2007. This regression defines the post-period as 2007 and after.

Table A15: Split-sample instrumental variables estimates of the effect of ambient lead concentrations in the week after a race on mortality rates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | All cause | Cardiovascular | Respiratory | IHD | Deaths of despair | Diabetes | All cause |
| *A. 2SLS Split Sample* | | | | | | | |
| $\widehat{\text{Lead level}}$ | 3601* | 1278* | 604* | 1736* | 69 | 7 | 3402* |
| | [1886 to 10387] | [401 to 4063] | [250 to 1839] | [ 754 to 5013] | [ 16 to 201] | [-222 to 225] | [1759 to 10335] |
| Observations | 56183 | 56183 | 56183 | 56183 | 56183 | 56183 | 56183 |
| *B. 2SLS* | | | | | | | |
| $\widehat{\text{Lead level}}$ | 1246 | 629 | 100 | 1554** | 43 | -217 | 1177 |
| | [-777 to 3269] | [-495 to 1753] | [-401 to 601] | [115 to 2994] | [-50 to 136] | [-490 to 56] | [-752 to 3106] |
| Observations | 1731 | 1731 | 1731 | 1731 | 1731 | 1731 | 1731 |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| State-by-year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Potentially endogenous measure of lead level | asinh(Pb) | asinh(Pb) | asinh(Pb) | asinh(Pb) | asinh(Pb) | asinh(Pb) | mean(Pb) |
| F-Stat | 14.79 | 14.79 | 14.79 | 14.79 | 14.79 | 14.79 | 14.00 |

*Note:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 95% confidence intervals reported in brackets. Both p-values and 95% confidence intervals are bootstrapped and based upon a non-parametric randomization inference procedure. 95% confidence intervals may not be symmetric. Panel A reports results from a split-sample strategy. The observations in first stage contain data on average race miles per race and average lead concentration in the week following a race. The observations in the second stage contain data on average race miles per race and mortality. We do this because there are not lead monitors in every county-year. Panel B shows results from a traditional IV strategy where we restrict our data to counties that have information on lead concentrations, race miles, and mortality.

## A.9 Other robustness checks, sensitivity checks, and analyses

### i. Soil lead cross-section robustness check

To provide supporting evidence that leaded races emit substantial quantities of lead, we estimate the conditional correlation between soil lead concentrations and distance from a NASCAR/ARCA track. We obtain data on county urban-rural status from the USDA and data on soil lead from the U.S. Geological Survey (USGS). USGS sampled over 5,000 sites across the U.S. over 2007-2010. The samples were taken so that there was approximately one for every 1,600 square kilometers. We estimate a model of the following form:

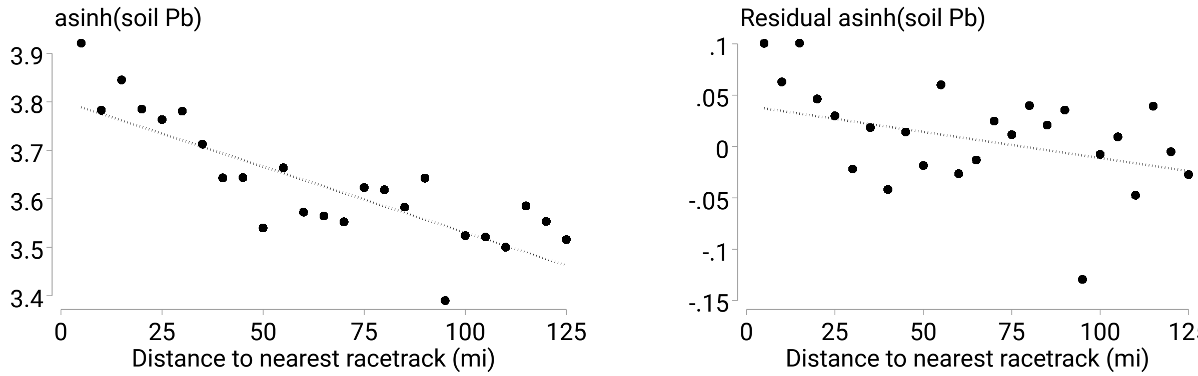
$$\text{asinh}(\text{soil Pb})_{sy} = \beta \text{distance to racetrack}_s + \gamma \mathbf{X}_{sy} + \Omega_{sy} + \varepsilon_{sy}. \tag{5}$$

$\text{asinh}(\text{soil Pb})_{sy}$ is the inverse hyperbolic sine of soil lead concentrations at site $s$, sampled in year $y$; $\text{distance to racetrack}_s$ is the distance from site $s$ to the nearest NASCAR/ARCA racetrack. $\mathbf{X}_{sy}$ is same set of controls in the EBLL results. $\Omega_{sy}$ is a state-by-year of sample effect. $\varepsilon_{sy}$ is the error term which is robust to heteroskedasticity and clustered at the county level.

The left panel of Figure A19 plots the raw data. There is a strong negative correlation between soil lead concentrations and distance from a racetrack. The right panel of Figure A19 plots the residuals from estimating equation (5) without the distance to racetrack variable. After removing variation from potential confounders, we still find a negative relationship between soil lead concentrations and distance from a racetrack.

Table A16 shows the estimates from equation (5) as we add fixed effects and split the estimates into whether the samples were taken in urban or rural counties. We find a strong negative correlation between distance to the nearest racetrack and soil concentrations across all specifications. Inclusion of both state-by-year-of-sample fixed effects and controls attenuate the size of the negative correlation. We also find a slightly stronger negative correlation in urban areas.

Figure A19: Inverse hyperbolic sine of soil lead concentrations as a function of distance to the nearest racetrack.




*Note:* The left panel shows the raw data correlation between the inverse hyperbolic sine of soil lead concentrations (mg/kg) and distance from the measurement site to the nearest NASCAR/ARCA track. The right panel is the same but uses residuals from a regression of asinh(soil Pb) on the set of controls and fixed effects in equation (5). Data are averaged within 5 mile bins.

Table A16: Correlation between asinh(soil Pb) and distance to the nearest NASCAR/ARCA racetrack.

| | (1) | (2) | (3) | (4) Urban | (5) Rural |
|---|---|---|---|---|---|
| Distance to Nearest Racetrack (1,000 miles) | –1.26** | –0.94*** | –0.72*** | –1.01** | –0.51** |
| | (0.48) | (0.17) | (0.17) | (0.43) | (0.21) |
| State-by-Year of Sample FE | No | Yes | Yes | Yes | Yes |
| Controls | No | No | Yes | Yes | Yes |
| Observations | 4839 | 4834 | 4683 | 1546 | 1751 |

*Note:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the county level in parentheses. Control variables are unemployment rate, % non-white, median income, lead emissions reported in Toxic Release Inventory, and total manufacturing payroll. Urban and rural indicates that the specifications are limited to urban or rural counties only. County urban or rural status is taken from the 2013 Rural-Urban Continuum Codes produced by the United States Department of Agriculture with a USDA continuum code of 1, 2, or 3, indicating an urban county and USDA continuum code of 7, 8, or 9, indicating a rural county.

### ii. Balance table

Table A17 displays results from a series of regressions with different dependent variables, where each is a socio-economic measure that is not included as a control variable in our other analyses. Each column reports results from a separate regression. For each regression, the independent variables of interest are dummy variables that indicate whether or not there was a leaded race in a given county or border county in each year.

Each regression is analogous to our preferred difference-in-differences specifications for elevated blood lead levels and for elderly mortality; the results from these are reported in column 4 of Tables A9 to A13 (for mortality) and in column 4 of Table A8 (for blood lead). The key difference between our preferred specifications and results presented here is that in this robustness check we swap our dependent variable of interest for a socio-economic variable. Each regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. Standard errors are clustered at the county level.

This procedure follows Pei et al. (2019) and serves as a balance test. This balance test helps to verify the identification assumption that the variation in our independent variables of interest is unrelated to other omitted variables after conditioning our preferred set of controls and fixed effects. We find no clear, statistically significant or economically meaningful relationship between any of our independent variables of interest and these socio-economic variables. The null findings here mitigate concerns that the estimated relationships in our specifications of interest between exposure to leaded races and either blood lead levels or mortality are driven by omitted variables or differential trends that are not otherwise captured by our preferred covariates and fixed effects.

Table A17: Balance table examining conditional relationship between county-race status and socioeconomic variables not included in regression of interest.

| | (1) Adjusted gross income per return | (2) Manufacturing employment per 1000 | (3) % in poverty | (4) % children | (5) % elderly |
|---|---|---|---|---|---|
| 1(Leaded Race in County) | 508.617 | –0.607 | 0.000 | 0.002*** | 0.003* |
| | (410.444) | (1.912) | (0.002) | (0.001) | (0.002) |
| 1(Leaded Race in Border County) | 143.879 | 0.491 | 0.000 | 0.002*** | –0.001 |
| | (290.099) | (1.219) | (0.001) | (0.000) | (0.001) |
| Mean of dependent variable | 49761.29 | 45.07 | .14 | .06 | .14 |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | No | No |
| State-Specific Linear Time Trends | No | No | No | No | No |
| State-by-Year FE | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 56170 | 56173 | 56173 | 56150 | 56157 |

*Note:* Regressions in this table correspond to specifications in column 4 of the mortality regressions in Tables A9 to A13 and to the specification column 4 of the blood lead level regressions in Table A8. However, here each dependent variable is a socio-economic variable that has not been included in other analyses. Each dependent variable is the at the county-year level. The independent variables of interest are whether or not there was a leaded race in a given county or border county in a given year. Each regression includes state-by-year fixed effects, county fixed effects, and controls for the unemployment rate, median income, % non-white, tons of lead emitted from TRI facilities, and total manufacturing payroll. Robust standard errors clustered at the county level in parentheses and each regression is weighted by the square root of total population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

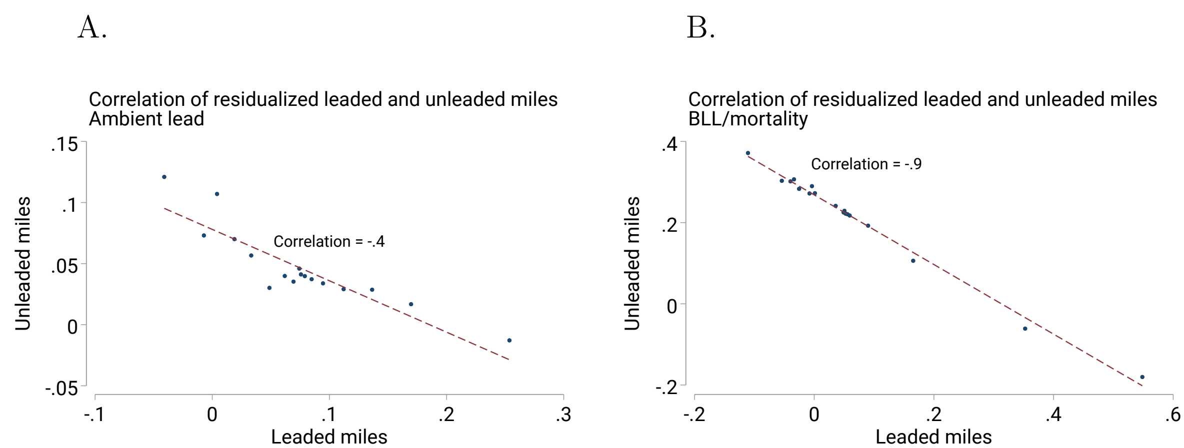
### iii. Collinearity of leaded and unleaded miles in annual regressions

Here we show the high degree of collinearity between leaded and unleaded miles in our samples with annual variation. After conditioning on observational unit fixed effects, e.g. county fixed effects, there is effectively no variation in the sum of leaded and unleaded miles. Since their sum is approximately constant, it is difficult to separately identify them or identify the effect of race miles in an event study framework. We show this in Figure A20 by plotting the residualized unleaded miles against residualized leaded miles after taking out the observational unit fixed effects.

The left panel of Figure A20 shows the low degree of collinearity between the two types of miles in our ambient lead concentrations sample, which has daily variation. When regressing unleaded miles on leaded miles, we only obtain a correlation of -0.4. This allows us to separately identify the effect of leaded miles from unleaded miles. Collinearity is not an issue for the ambient lead setting because the data are in terms of days: the sum of the types of miles can take on the value of the actual miles driven over a racing weekend, a fraction of the actual miles driven, or zero if the monitor reading was taken more than a week after any race within the treatment radius.

The right panel of Figure A20 shows the near-perfect collinearity between the two types of miles in our county-year BLL and mortality samples. Regressing unleaded miles on leaded miles yields a correlation of -0.9. If there is an increase in unleaded miles in a given county by 1, it is offset by a decrease in leaded miles by 0.9. There is very little within-observational unit variation in the sum of leaded and unleaded miles. This hinders our ability to do event studies like Figure 5 for BLLs or mortality. Since there is not much inter-annual variation in total miles, it is effectively equivalent to a dummy variable. For the difference-in-differences specifications in Section A.7 we use leaded miles instead of indicator variables for leaded races and find similar results.

Figure A20: Collinearity between leaded and unleaded miles.




*Note:* Correlation between leaded and unleaded miles after conditioning on monitor effects in the ambient lead sample (left panel), or conditioning on county effects in the BLL and mortality samples (right panel).

## iv. Environmental justice

One concern about the effect of lead emissions from racing is that it may have negative environmental justice implications. Here we analyze associations between total county level exposure to leaded race miles and the average of five demographic variables over the last 5 years with leaded races. The five variables are the percent of the population that are children, non-white, male, or elderly, and the county's median income. To estimate the association, we run a cross-sectional county level regression with the the average of the variable on the left-hand side, and total leaded miles on the right-hand side. We estimate the associations separately for urban and non-urban counties, where urban counties have USDA continuum codes of 1, 2 or 3, while non-urban counties have continuum codes of 4, 5, or 6. There are no leaded races in our sample with higher continuum codes.

Table A18 reports coefficient estimates on leaded miles. Here we find that in urban counties, areas with more leaded miles driven tend to have more children, more non-whites, more women, fewer elderly, and higher incomes. Non-urban areas generally show the same pattern except for children. Pooling the urban and non-urban samples together yields results almost identical to the urban sample. The associations suggest that races may be disproportionately harming certain subgroups of the overall population, based on demographic, but not necessarily socioeconomic variables.

Table A18: Associations between average county socioeconomic and demographic characteristics and total leaded race miles within county from 2002-2006.

| | Urban | | | | | Non-Urban | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | % Children | % Non-White | % Male | % Elderly | Median Income | % Children | % Non-White | % Male | % Elderly | Median Income |
| Total Leaded Miles 2002-2006 | 0.002** | 0.029* | –0.002 | –0.006* | 1261.361 | –0.002 | 0.023 | –0.002 | –0.007 | 2145.133 |
| | (0.001) | (0.015) | (0.002) | (0.004) | (1155.302) | (0.002) | (0.035) | (0.004) | (0.006) | (1423.185) |
| Observations | 1157 | 1228 | 1158 | 1158 | 1159 | 895 | 902 | 895 | 895 | 895 |

*Note:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Regressions have no controls or fixed effects.

### v. Survey on beliefs about NASCAR fuel

To determine whether the general public had knowledge about NASCAR fuels that may have caused behavior changes following the fuel switch, we conducted a survey on Amazon's Mechanical Turk. We surveyed 100 respondents and paid each $0.10 for completing the survey. Respondents were restricted to those who live in the United States. We asked the respondents for their county of residence, state of residence, how far away they believe they live from the nearest NASCAR track, and two multiple choice questions:

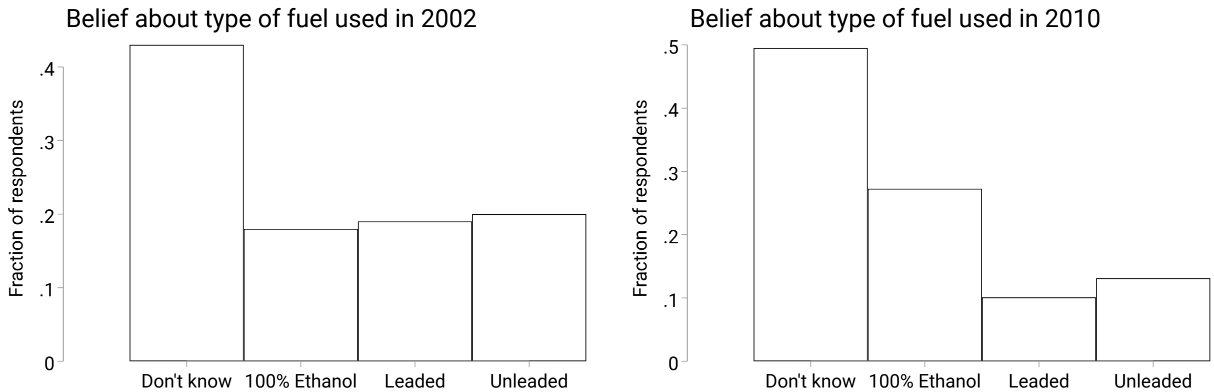
1. What type of fuel did NASCAR use in 2002?
   (a) Don't know
   (b) 100% Ethanol
   (c) Leaded gasoline
   (d) Unleaded gasoline
2. What type of fuel did NASCAR use in 2010?
   (a) Don't know
   (b) 100% Ethanol
   (c) Leaded gasoline
   (d) Unleaded gasoline

Figure A21 shows results for the responses to the multiple choice questions. About half the respondents said they didn't know the fuel used either before or after the fuel switch. Conditional on answering with a specific fuel type, the 2002 responses are evenly spread across unleaded gasoline, leaded gasoline, and 100% ethanol. Respondents tended to believe that NASCAR used pure ethanol in 2010 conditional on answering with a specific fuel type.

Regressing a binary variable for a correct fuel type response on believed distance from the track,[34] we do not find any correlation between distance to a track and knowing which fuel type NASCAR used in either year.

[34] We drop responses from the non-contiguous U.S. or where they believe they were more than 1000 miles from a NASCAR track, which is effectively impossible.

Figure A21: Histograms of responses to our survey asking what respondents believed was the type of fuel used in NASCAR in 2002 (left) and 2010 (right).




## A.10 City lead emissions counterfactuals

In this section we use our quasi-experimental estimates to both predict out-of-sample historical lead concentrations and estimate the counterfactual ambient lead concentration if lead were still added to gasoline. Our estimates predict out-of-sample historical levels well, which provides credibility to our quasi-experimental estimates. Moreover, the counterfactual estimates give some insight into what ambient lead levels would have been in the absence of EPA abatement efforts and the Clean Air Act, which is an essential component for any attempt to value the benefits provided by these regulatory efforts.

Our out-of-sample historical prediction is performed in three steps. First we estimate the average grams of lead emitted from daily vehicle miles traveled (VMT) for U.S. cities from 1971 to 1995. Second, we convert our ambient lead estimates in Section A. from changes in concentration *per race mile* to changes in concentration *per gram of lead per gallon*. This involves making assumptions about the fuel efficiency of the racing vehicles. Third, we combine these estimates to form our historical prediction. We perform our counterfactual prediction by a similar procedure, except where we assume that the lead content of gasoline remained at 1971 levels.

### i. Estimating historical lead emissions from daily VMT

To estimate grams of lead emitted from daily VMT, we collect data from several sources that account for increases in VMT, changing fuel economy, and the decreasing use of lead additives across time. First, we obtain estimates of daily VMT for both highway and non-highway roads for 101 cities from 1982 to 2014 from the Texas A&M Transportation Institute's Urban Mobility Scorecard. These estimates are based upon the Federal Highway Administration's

Highway Performance Monitoring System Data. We estimate city VMT prior to 1982 by assuming each city's VMT growth followed the U.S. national trend, which is provided the U.S. Department of Energy's Alternative Fuels Data Center. National average annual fuel economy data are provided by the U.S. Energy Information Administration. Finally, the share of unleaded gasoline and the lead content in leaded gasoline by year come from Newell and Rogers (2003). We combine these measures as follows:

$$\begin{aligned}\text{Daily grams TEL from routine VMT}_{cy} = \big(&\text{Daily VMT estimate}_{cy}\times \\ &\text{Mean U.S. MPG}_{y}\times \\ &\text{\% of all gasoline leaded}_{y}\times \\ &\text{Mean grams TEL per leaded gallon}_{y}\big)\end{aligned} \tag{6}$$

### ii. Converting race miles to grams of lead used per race

Next we use our ambient lead estimates to translate grams of TEL emitted to changes in ambient lead concentrations. In Tables 1 and A6, we provide multiple estimates of the relationship between miles driven in a leaded race and the impact on ambient lead concentration in the following week. Our race data report the number of miles driven during each race, but do not report the number of gallons consumed at each race. One strategy to estimate fuel consumption would be to multiply the average fuel economy of a racecar by the total miles driven. In-race fuel economy has been cited to be between four and five miles per gallon (Belson, 2011). However, even if we knew the exact in-race fuel economy of each car in every race, using this measure exclusively would still cause us to under-count total fuel consumption because we would be neglecting additional fuel consumed during idling, practice, and qualifying sessions. The number of miles driven in addition to the in-race miles is unknown because practice and qualifying laps are not usually reported and will differ by race and series. We consider two estimates of the importance of these miles relative to in-race miles.

The first estimate uses data for a race that reported both in-race miles and practice miles, the 2019 Ticket Guardian 500. This race had 10,766 race miles and 3,053 practice miles.[35] The total number of qualifying laps were not tracked since the qualifying procedure allows drivers to drive as many laps as they wish across a qualifying period that totals up to 25 minutes in length.[36] Under the assumptions that the average driver travels 5 laps during a

[35] `https://www.nascar.com/results/race_center/2019/monster-energy-nascar-cup-series/ticketguardian-500/stn/practice1/`

[36] Qualifying for the 2019 Ticket Guardian 500 occurred in three rounds. Drivers are ranked by their fastest single lap performed during each round. In the first round, 36 drivers drove for up to 10 minutes each. In the second round, 24 drivers drove for up to ten minutes each. In the third round, 12 drivers drove for up to five minutes each.

ten-minute period, 330 miles were driven as a part of qualifying. Thus the 10,766 in-race miles represent only 76% of the total miles driven as a part of the entire event. Accounting for these additional non-race miles would mean adjusting in-race fuel economy to be between 3 and 3.8.

This simple deflator does not account for gasoline combustion due to idling or testing that may also occur as a part of the race. To address this last issue, we provide another fuel economy estimate that comes from reports that the top series in NASCAR used 175,000 gallons of fuel in 2008 (Fryer, 2008). In the previous season, the top series ran 566,130 in-race miles; combining these two estimates suggests that there are 3.24 in-race miles traveled per gallon used for all race activities. This estimate is within the bounds of the first one, so we will use 3.24 as our measure of total gallons used per race mile.

To obtain the total grams of lead emitted per race, we first multiply the number of in-race miles traveled by 3.24 to calculate the number of gallons of fuel used for all race-related activities.[37] We then multiply by our year-specific estimate of the grams of TEL added to each each gallon to get the grams of lead emitted per race. Next., we estimate the same specifications displayed in Tables 1 and A6, but with the estimated metric tons of lead emitted during each race as our treatment variable rather than the number of race miles traveled. This gives us the relationship between an additional metric ton of lead emitted at a race and ambient lead concentrations. The resulting coefficients are displayed in Table A7.

### iii. Comparison of historic lead emitted from daily VMT to automotive racing sample

Figure A22 compares how both historical ambient airborne lead concentrations and historical lead emissions from daily motor vehicle traffic compare to the analogous measures used in our automotive racing analysis. Panels A and B compare the distributions of estimated quantities of lead emissions caused by NASCAR and ARCA races to average daily traffic in 101 major U.S. cities across different decades. Below the densities, the blue bar shows the range of emissions that share common support with our automotive racing analysis. Panel A shows the density of average lead emissions from a day's worth of traffic in the 1970s and 1980s, while panel B shows the distribution of estimated lead emissions from daily traffic from 1990 to 1995 and from NASCAR/ARCA races from 1996 to 2018. A single race generally emits more lead than all daily traffic in a major city in the 1990s and is on the lower end of daily emissions from major cities in the 1970s and 1980s. Panels C and D present the

[37] We have data on the grams of TEL added to each gallon of fuel used in NASCAR from 1951 to 2006 (Wusz, 1994). From 2003 to 2006, NASCAR used Sunoco Supreme fuel, which contains 5.2 grams of TEL per gallon. This was confirmed by e-mail with Sunoco Race Fuels.

distribution of mean ambient lead concentrations for these same 101 cities across time. Mean daily lead pollution levels for each city and year are calculated using EPA lead monitoring data; see Section II.. Panel C shows that lead concentrations in the 1970s were generally higher than in the 1980s, reflecting the effect of the EPA's policies. Panel D zooms in on the support of the distribution of lead concentrations during the period of our automotive racing analysis (1996-2018) and shows again that lead concentrations have continued to decrease relative to ambient concentrations from the early 1990s.

### iv. Out-of-sample historical prediction

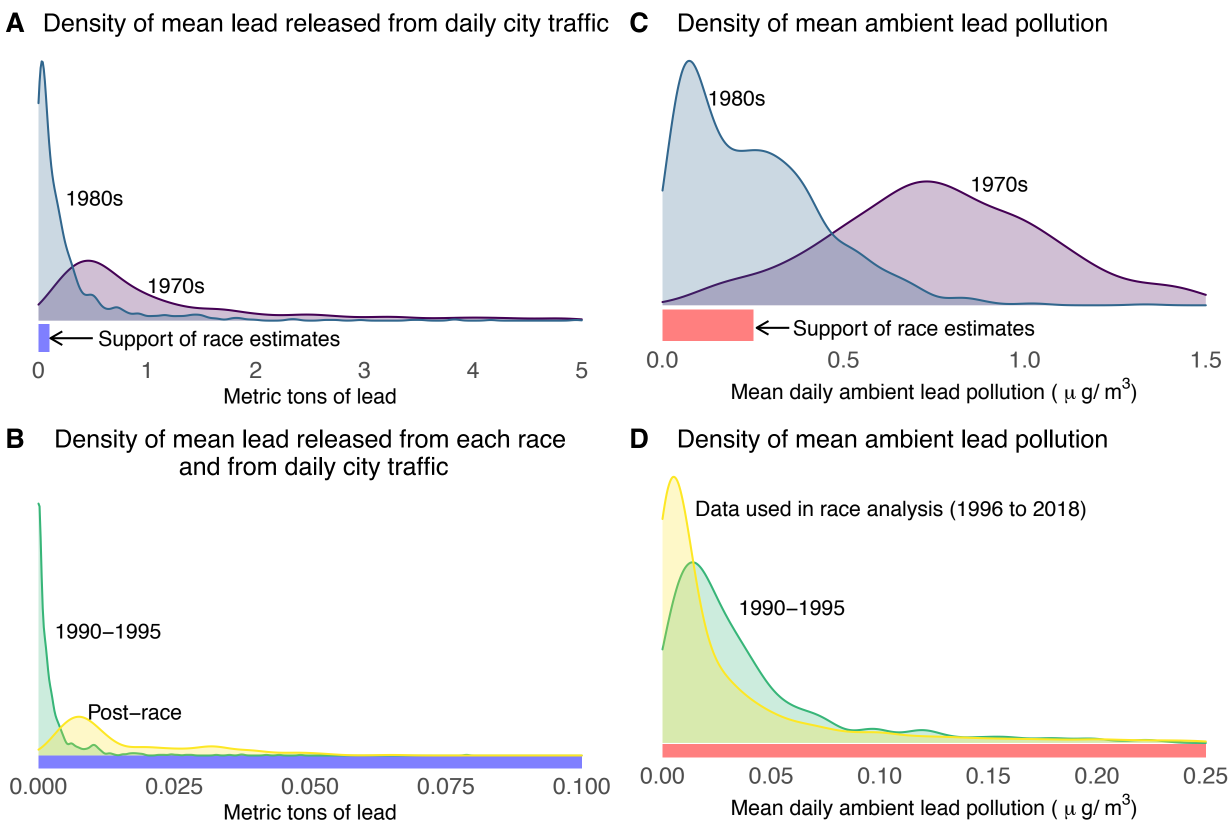
Figure A23 presents results that directly compare a non-parametric estimate of the historic relationship between lead emitted from daily VMT and mean ambient lead levels to an estimate of the relationship that is based upon our quasi-experimental automotive racing analysis. Panel A presents a binscatter showing the non-parametric relationship between estimated lead emitted from daily vehicle traffic and average daily ambient lead concentrations in 101 U.S. cities. The binscatter is created by dividing the x-axis into ten equally sized bins, computing the mean of the x and y variable for each bin, and then showing a scatterplot of the mean pairs for each bin. Our x-axis is the average daily metric tons of lead emitted due to vehicle travel in 101 U.S. cities from 1971 to 1995. The y-axis is the mean average daily ambient lead concentration for 101 U.S. cities in each year from 1971 to 1995. The blue and red bars at the bottom of the panel denote the range of both lead emissions and mean ambient lead that overlap with the data we use in our automotive racing analysis. Panel B zooms in on the support of Panel A that overlaps with the range of each axis used in our automotive racing analysis. Thus only the first four deciles of the binscatter are shown. Panel B also shows predicted ambient lead concentrations using our estimates from Table A7. The prediction based on asinh(Pb) and its associated 95% confidence interval are displayed in red (on the right), and the prediction using unadjusted (i.e., linear) Pb as the dependent variable is reported in blue (on the left). We form the prediction by multiplying each coefficient by an amount of lead released across an evenly spaced grid from 0 to .11 metric tons. For the asinh(Pb), we perform the correct transformation of the estimate and calculate the standard errors and resulting 95% confidence interval using the delta method.

Both our estimates do an excellent job of predicting the out-of-sample historic mean lead concentrations. However these predictions are so far limited to be within the support of our automotive racing analysis. In Figure A24 we extend this comparison back to 1971, which is far off of the support of both our dependent and independent variables from our preferred analysis. We report the mean of the prediction for the 101 cities in each year. The prediction based on the asinh regression is depicted with the solid orange line, while predictions using the linear regression are depicted with the dashed orange line. The actual average historical ambient lead concentration for these 101 U.S. cities is depicted by the solid black line. Our quasi-experimental estimates predict historical ambient lead levels quite well, suggesting that our estimates are capable of predicting out-of-sample ambient lead levels with a good degree of accuracy.

### v. Counterfactual ambient lead levels

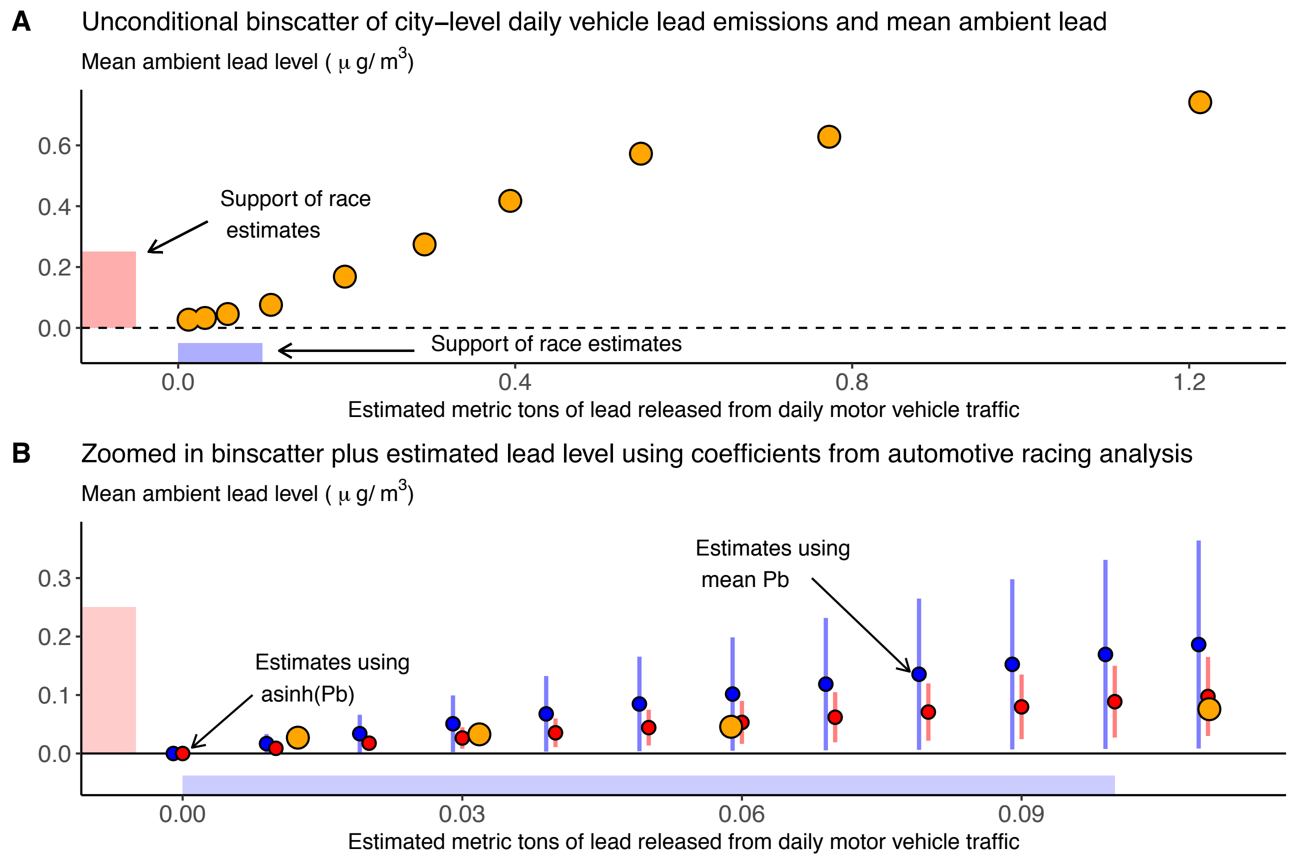
We next estimate counterfactuals that attempt to predict ambient lead concentrations if leaded gasoline use was still widespread. The counterfactuals assume that TEL additives remained constant at the 1970 level of 2.1 g/gallon, that all cars used leaded gasoline, and that there were no counterfactual changes in VMT or fuel economy. Figure A24 reports the results from two counterfactuals. The solid gray line represents the counterfactual estimate using our asinh regression, and the dashed gray line is the counterfactual from our linear specification. Our most conservative counterfactual estimates suggest that if tetraethyl lead was still added to gasoline, ambient lead levels would be 6.34 $\mu$g/m$^3$—4 times larger than the 1971 mean lead concentration of 1.45 $\mu$g/m$^3$ and 500 times higher than the actual 2014 concentration.

Figure A22: Comparison of lead emissions and lead pollution for data used in our quasi-experimental specification with estimates for 101 U.S. cities by decade.




*Note:* Data plotted in yellow represent the data used in our main analysis outlined in Section A.. The post-race density in panel B only displays the subset of data in the week following a race. The analogous density in panel D includes all observations, both before and after a race. The range of support for these data are indicated by the blue and red bars at the bottom of each panel. The data sources and procedure used to estimate city VMT across time are outlined in Section A.10. Mean daily lead pollution levels for each city and year are calculated using EPA lead monitoring data; see Section II.. Overall this panel shows that the amount of lead released after a typical race is greater than the average amount of lead released from all of a city's daily VMT in the early 1990s. A large NASCAR race (about 3 hours long) emits about as much lead as was emitted from all vehicle miles traveled on an average day in Buffalo, NY in 1987.

Figure A23: Predicted out-of-sample lead levels using our ambient lead estimates.




*Note:* Panel A presents a binscatter showing the non-parametric relationship between estimated lead emitted from daily vehicle traffic and average daily ambient lead concentrations in 101 U.S. cities. The binscatter is created by dividing the x-axis into ten equally sized bins, computing the mean of the x and y variable for each bin, and then showing a scatterplot of the mean pairs for each bin. Our x-axis is the average daily metric tons of lead emitted due to vehicle travel in 101 U.S. cities from 1971 to 1995. The y-axis is the mean average daily ambient lead concentration for 101 U.S. cities in each year from 1971 to 1995. The data sources and procedure used to estimate daily city lead emissions from VMT across time are outlined in Section A.10. Mean daily lead pollution levels for each city and year are calculated using EPA lead monitoring data; see Section II.. The blue and red bars at the bottom of the panel denote the range of both lead emissions and mean ambient lead that overlap with the data we use in our automotive racing analysis. Panel B zooms in on the support of Panel A that overlaps with the range of each axis used in our automotive racing analysis. Thus only the first four deciles of the binscatter are shown. Panel B also shows predicted ambient lead concentrations using our estimates from Table A7. The prediction based on asinh(Pb) and associated 95% confidence interval are displayed in red (on the right), and the prediction using unadjusted (i.e., linear) Pb as the dependent variable is reported in blue (on the left). We form the prediction by multiplying each coefficient by an amount of lead released across an evenly spaced grid from 0 to .11 metric tons. For the asinh(Pb), we perform the correct transformation of the estimate and calculate the standard errors and resulting 95% confidence interval using the delta method.


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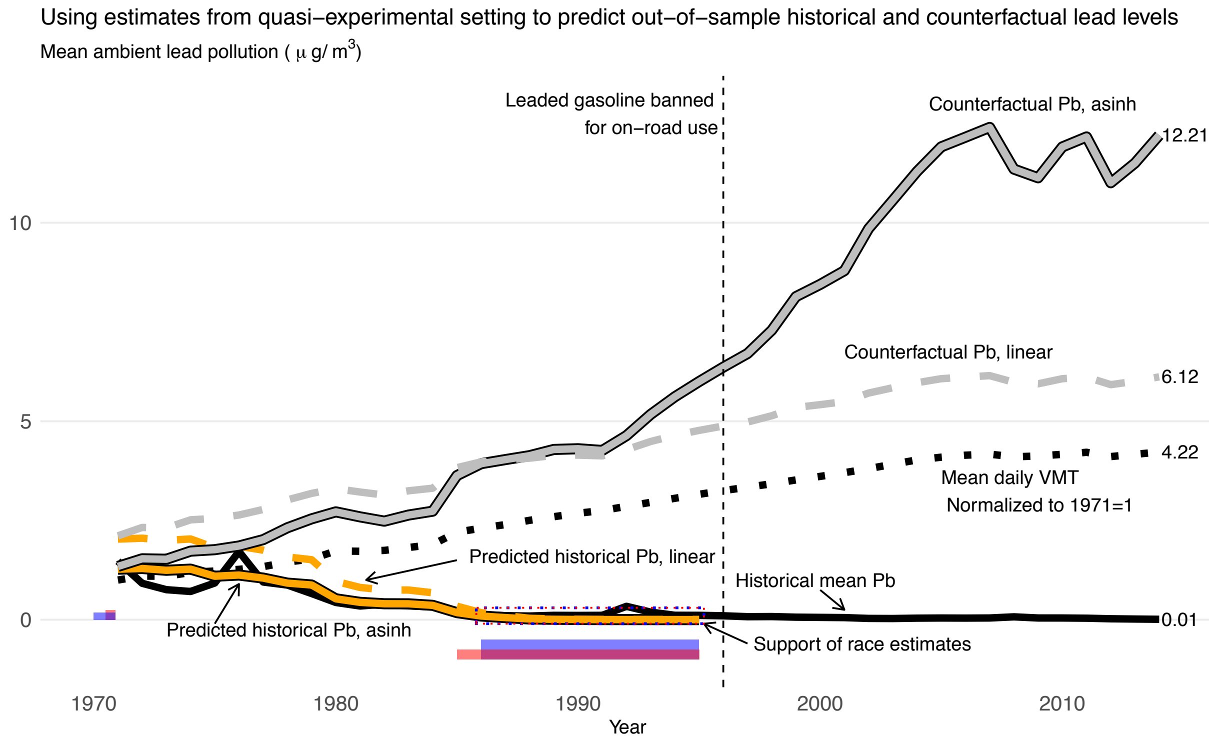
Figure A24: Predicted counterfactual lead levels using our ambient lead estimates.




*Note:* The average historical ambient lead concentration in U.S. cities is depicted by the solid black line. We construct a city-year estimates of daily grams of lead emitted due to VMT from 1971 to 1995. We multiply this estimate by the coefficient estimate of our ambient lead regression in Table A7, which assumes automotive racing uses 3.24 gallons of fuel per mile raced. This provides us with predictions of historical ambient lead concentrations due to daily leaded traffic in each city and year. Prediction based upon the asinh regression are depicted with the solid orange line, while predictions using the linear regression are depicted with the dashed orange line. Counterfactuals assume that TEL additives in gasoline remained constant at the 1970 level of 2.1 g/gallon and that there were no changes in VMT or fuel economy. The solid gray line represents the counterfactual estimate using our asinh regression, and the dashed gray line is the counterfactual from our linear specification. Average daily VMT relative to 1970 is plotted by the dashed black line.

## A.11 Calculations used in the main text

### i. Deleading is equivalent to a 10 $\mu$g/m$^3$ reduction in PM$_{2.5}$ concentrations

We compare our finding to a result from Deryugina et al. (2019) that reports that a 1 $\mu$g/m$^3$ increase in daily PM$_{2.5}$ exposure results in an additional 0.69 deaths per million elderly in the subsequent 3 days.

We first use our difference-in-differences estimate that living in a race county results in a 1.7% increase in the all-cause mortality rate. We use this to compute the number of additional deaths per million elderly in each county-year caused by in-county races. We compute this by multiplying the change in the mortality rate by the county-year elderly mortality rate and elderly population per 100,000 for each county-year

$$\begin{aligned}\text{county-year } \widehat{\text{deaths}} \text{ per million} =& \% \text{ change } \widehat{\text{in mortality}} \text{ rate} \\ & \times \text{deaths per 100,000 elderly} \\ & \times \frac{10 \times 100{,}000}{1 \text{ million}}\end{aligned}$$

where hats indicate estimated values and the other terms are data or scalars. To put this in PM$_{2.5}$ terms, we first translate our annual estimate into a 3-day number of deaths, and use the Deryugina et al. (2019) estimate of the mortality effect of PM$_{2.5}$ to get an estimated equivalent effect of PM$_{2.5}$

$$\begin{aligned}\text{equivalent } \widehat{\text{county}} \text{ PM}_{2.5} \text{ change} =& \text{county-year } \widehat{\text{deaths}} \text{ per million} \\ & \times \frac{1 \text{ year}}{365 \text{ days}} \\ & \times 3 \text{ days} \\ & \times \frac{1 \ \widehat{\mu\text{g/m}}^3}{0.69 \text{ deaths per million elderly over 3 days}}.\end{aligned}$$

We then take the average of this value over race counties in 2006 resulting in an average of an 8.7 $\mu$g/m$^3$ reduction in PM$_{2.5}$ concentrations.

### ii. NASCAR and ARCA used an estimated 2 million grams of TEL

In 2005, our data give us that NASCAR and ARCA drove approximately 1.2 million race miles. Using our estimate of 3.2 race miles per gallon from Section A.10, this gives us that there were 370 thousand gallons used during that season. Lead content of the fuel prior to deleading was 5.2 grams of TEL per gallon, yielding approximately 2 million grams.

$$1,146,547 \text{ miles} \times \frac{1 \text{ gallon}}{3.2 \text{ miles}} \times \frac{5.2 \text{ grams}}{1 \text{ gallon}} = 1.9 \text{ million grams}$$