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ABSTRACT

We study how ambient lead exposure impacts learning in elementary school by leveraging a natural experiment where a large national automotive racing organization switched from leaded to unleaded fuel. We find increased levels and duration of exposure to lead negatively affect academic performance, shift the entire academic performance distribution, and negatively impact both younger and older children. A single average race emitted more than 10 kilograms of lead — a quantity similar to the total annual emissions of an airport or the median lead-emitting industrial facility in the United States. The average treated student in our setting has an expected income reduction of $5,200 in present value terms. Avoiding said treatment has an effect size similar to improving teacher value added by one-fourth of a standard deviation, reducing class size by 3 students, or increasing school spending per pupil by $750. The marginal impacts of lead are larger in impoverished, non-white counties, and among students with greater duration of exposure, even after controlling for the total quantity of exposure.

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Education drives future income, productivity, and upward mobility. The United States devotes a large and growing share of resources to the promotion of education through formal schooling, with public school spending in the United States exceeding $12,000 per student in 2017, and accounting for 30% of state and 8% of national expenditures (U.S. Census Bureau, 2017). A large body of work examines the education production function, showing how a wide range of inputs drive student achievement (Hanushek, 2020). Some inputs directly relate to formal schooling, such as teaching quality, pedagogy, and class size (Krueger, 1999; Chetty et al., 2011, 2014b); while others are primarily determined outside the bounds of school, such as socioeconomic status, sleep, the environment, and nutrition (Ladd, 2012; Frisvold, 2015; Ebenstein et al., 2016; Anderson et al., 2018; Jagnani, Forthcoming; Park et al., 2020; Park, 2022).

Using a unique natural experiment that changed airborne lead exposure, we examine the effect of an important educational input, environmental quality. We show that exposure to airborne lead—which causes neurological damage, increases impulsiveness, and hinders learning—has a negative and cumulative effect on student performance. Separately identifying the role of lead—or any education input—is challenging, as many inputs are co-determined or endogenous. Education inputs also display complementarities where changes in one affect the marginal benefit of others. For example, improvements in teacher quality and reductions in class size have differential effects across income groups and race (Krueger, 1999; Chetty et al., 2014b), and socioeconomically disadvantaged students are generally more costly to educate (Duncombe and Yinger, 2005).[1] These interdependencies provide a rationale for policies that improve life outside of the classroom, enabling the argument that improving educational outcomes requires addressing core disparities (Ladd, 2012).

We estimate the causal effect of lead on test scores using the natural experiment of the National Association for Stock Car Auto Racing (NASCAR) switch from leaded to unleaded fuel in 2007. This generated an exogenous decrease in lead exposure for areas near racetracks.[2] The sudden deleading meant a permanent drop in the annual flow of lead emissions in areas near racetracks, reducing nearby ambient lead concentrations, children's lead poisoning rates, and elderly mortality (Hollingsworth and Rudik, 2021), but did not impact lead exposure in locations further from tracks. After 2007, each cohort attending schools proximate to NASCAR emissions experienced less lifetime lead exposure than the preceding cohort, allowing us to compare test scores both within and across schools as well

[1] There are substantial gaps in educational attainment by race and income (Fryer and Levitt, 2004; Reardon, 2018), which persist even when comparing students within the same school (Fryer and Levitt, 2006).

[2] Despite a Clean Air Act ban for on-road leaded fuel, both automotive racing and aviation have exemptions allowing its use. Prior to the unleaded fuel switch, NASCAR was one of the largest lead emitters in the US (Hollingsworth and Rudik, 2021), providing ample power for statistical analyses.

as between cohorts with differential levels and duration of exposure to lead emissions. Our identification uses data on the location, timing, and quantity of emissions to disentangle the effects of lead from both persistent socioeconomic confounders (e.g., household income) and time-varying confounders (e.g., co-emitted pollutants).

We document several new and important facts. First, we provide quasi-experimental evidence linking lead emissions, rather than blood lead levels, to decreased test scores.[3] Most research focuses on blood lead levels, and while blood lead is a direct measure of current health conditions, it is net of several other factors, including any mitigating behavior taken in response to blood lead information and selection into screening.[4] Our findings are less subject to such mitigating factors or selection, because proximity to NASCAR races was largely an unknown source of exposure.[5]

We directly link quantities of lead emissions to outcomes, which informs policies addressing ambient lead and lead emission sources. Previous work using detailed microdata shows that elevated blood lead in *early* life is strongly associated with negative *future* school outcomes (Reyes, 2015; Aizer et al., 2018; Aizer and Currie, 2019; Gazze et al., Forthcoming). By studying an abrupt change in lead emissions, we avoid confounding from unobservable socioeconomic factors correlated with early life blood lead that also affect student achievement.[6]

Second, we study exposure to airborne lead in a modern setting where average lead exposure is low; other important work on deleading, such as Clay et al. (2021), focuses on the large-scale removal of lead from commercial gasoline during a time when ambient lead levels were much higher. Our analysis is on children in Florida, which—unlike many of the other populous U.S. states—has comparatively low lead contamination. Florida has the lowest soil lead concentrations of any state in the contiguous U.S. (Smith et al., 2014), and one of the lowest lead poisoning rates (U.S. Centers for Disease Control, 2019). Given that lead exposure continues to decline, future lead policy must be informed by research on impacts when ambient levels are low.

[3]The economics literature has found that lead negatively impacts many outcomes including lifetime earnings (Grönqvist et al., 2020), fertility (Grossman and Slusky, 2019; Clay et al., 2014), and adult IQ test performance (Ferrie et al., 2012). There is an extensive public health literature on lead impacts, but these studies tend to be associational (e.g Canfield et al., 2003; Lanphear et al., 2005, 2018).

[4]Blood lead testing and associated regulations are typically targeted at those at the highest risk for lead exposure. Thus, blood lead data, even when using the universe of blood lead tests, is often from a selected population. See Gazze (2022) for more information regarding selection and screening in blood lead tests for children.

[5]Our results still include any negative within-classroom spillover effects from exposure (Gazze et al., Forthcoming), as well as mitigating behavior by parents or educators in response to observed poor academic performance, such as hiring tutors to help struggling students.

[6]Identifying the causal effects of lead exposure is timely, as the US EPA is currently reviewing the lead National Ambient Air Quality Standards and is interested in causal effects on cognitive outcomes in children (U.S. Environmental Protection Agency, 2020).

Finally, we document new evidence on how duration, quantity, and distributional impacts of lead exposure impact test scores. The variation in our data is such that we can compare students exposed to the same lifetime total quantity of gasoline lead, with said total spread across different numbers of years. Our results indicate that exposure both in very early years and up to at least age 8 can have negative effects, and that the same quantity of exposure causes more harm when spread over a longer timeframe; "death by a thousand cuts" may be worse than a single large exposure dose.[7] We also find no evidence that the marginal effects of additional lead exposure change with total exposure, suggesting effects are largely linear in our observed range. Lead exposure has negative effects on students across the entire achievement distribution, and disproportionately affects those in school districts with larger shares of Black and low-income populations.

We find exposure to an additional distance-adjusted 10kg of lifetime lead emissions by the third grade — equivalent to growing up near the average airport or a 42nd percentile lead-emitting Toxics Release Inventory (TRI) facility — decreases standardized test scores by 0.08 standard deviations.[8] Removing this exposure would generate returns similar in magnitude to decreasing class size by 3 students, having a more experienced teacher, or increasing school spending per student by $750 (Jepsen and Rivkin, 2009; Jackson et al., 2021). The estimate is also of a similar magnitude to documented gender and racial gaps observed in test scores (Fryer and Levitt, 2006, 2010).

Combining our results with estimates of how test scores affect future income (Chetty et al., 2014b), we calculate the present value of lost future income for the average 3rd grader growing up exposed to emissions in our sample is approximately $5,200 in 2020 dollars. For a 90th percentile exposure student in our data, discounted lifetime income losses are closer to $8,500.

While many education inputs are often effectively fixed or costly to change, lead may be simpler to target.[9] Legacy lead can be mitigated by one-time expenditures such as home remediation, and other sources of lead emissions can be reduced by the removal of the exemptions from the Clean Air Act leaded fuel ban for off-road racing and aviation.[10]

[7]Importantly, our findings do not indicate that later life exposure is more harmful than early life exposure, but that additional lead exposure later in life, on top of early life exposure, causes additional harm.

[8]10kg of lead emissions is approximately one-third of the emissions caused by a 500 mile race, such as the Daytona 500. Prior research suggests this amount of lead emissions within a county in a single year increases county-level lead poisoning rates that year by approximately 1 percent (Hollingsworth and Rudik, 2021).

[9]For example, parental income or education is nearly impossible to change, and meaningfully decreasing class size requires regular salary payments to newly hired teachers. Hiring more teachers may also decrease average teaching quality, offsetting some of the gains from smaller class size.

[10]Prior research has found that remediation of lead-contaminated homes improves test scores (Billings and Schnepel, 2018; Sorensen et al., 2019).

1 Data

1.1 Test Scores

We obtain data on educational achievement from the Florida Department of Education. Each year, the Florida Department of Education records school-level outcomes from the Florida Comprehensive Assessment Test (FCAT), the standardized test used in Florida public schools in grades 3–10. The FCAT data we use span 2003–2014.[11]

FCAT data report test score outcomes at the school-grade-subject-year level, averaged across all students. We standardize school-grade-subject-year average scores within each grade, year, and subject to be mean zero and standard deviation one. The z-score gives us how many standard deviations a school cohort is above the state-wide subject-specific test average in that year. This means that our treatment effect estimates will be based on *school-level* rather than individual-level standard deviations. Following Ost et al. (2017) we adjust our school-level estimates to be comparable with estimates from individual-level data whenever such comparisons are made.[12]

The FCAT data also report the percentage of students in each of five achievement levels. Level 5 contains the highest-achieving students, while level 1 contains the lowest-achieving students. Achievement levels proxy for the distribution of scores within a school. We define students in level 3 and above to be *proficient*, as level 3 is the threshold for "passing" the test. For example, a level 3 score in grade 10 reading is required for graduation.

In this paper we focus on mathematics and reading tests for grades 3–5. Focusing on the earliest grades provides the most variation in lead exposure. These grades also correspond to elementary schools, which have smaller catchment areas than middle or high schools. Since we lack data on where students live, we assign lead exposure based on school location, and smaller catchment areas reduce measurement error. Our estimates will be biased toward zero from any remaining measurement error in exposure from this source or if students move to different districts. We weight observations by the number of students in each school-grade-year.

[11] After 2014 Florida changed its standardized test to the Florida Standards Assessment. Prior to 2003 FCATs were not taken in each grade.

[12] Ost et al. (2017) demonstrate that treatment effect estimates reported in standard deviation units will reflect the level of aggregation used in the analysis. Even when studying the same test, 1) a more aggregate group will tend to have a smaller standard deviation than the a less aggregated one and 2) this difference must be accounted for when comparing treatment effects across the studies. In Table A1, we use data from the Florida Department of Education on a subset of data used in our paper to demonstrate that—for the same test—a school-level standard deviation is around 37% the size of a student-level standard deviation. Following Ost et al. (2017), we use this FCAT-specific deflator whenever we compare school-level results to results from individual-level data. Thus includes our estimates of monetary damage since the studies linking test scores to future income are based on individual-level data.

1.2 Lead, NASCAR, and the TRI

Race data come from Racing-Reference, an archive maintained by NASCAR.[13] The data detail the number of drivers, the number of laps completed by each driver, track length, and day and time for each race. Hollingsworth and Rudik (2021) describe the leaded fuel status of each NASCAR race and its evolution over time. Section 2 describes our main treatment variable, which we base on our NASCAR lead measure.

Data on industrial lead emissions come from the Toxics Release Inventory, which reports emissions from facilities that use, manufacture, or process more than 100 pounds of lead and have 10 or more employees.

1.3 Income and Demographic Data

Data on county median income come from the U.S. Census Small Area Income Poverty Estimates, and data on county unemployment rates come from the Bureau of Labor Statistics Local Area Unemployment Statistics. Data on the percent of individuals in a county who are Black or Hispanic, and the percent of homes built before 1940, come from the American Community Survey. County median household income and poverty rates come from the U.S. Census Small Area Income Poverty Estimates. Fast food establishments per capita come from the USDA Food Atlas.

2 Theoretical Framework, Summary Data, and Methods

To frame our contribution and model the relationship between lead and test scores, we first briefly consider a stylized static education production function:

$$\text{test scores} = f(S, C, N, E, b(Pb)),$$

where test scores — averaged at the school-grade-year level, like our data — are a function of vectors of socioeconomic characteristics S, school/classroom characteristics C, nutrition variables N, environmental characteristics E, and blood lead concentrations $b(Pb)$, which are a function of lead exposure Pb. $Pb = x + l$ is the sum of exposure from NASCAR x, and from other lead sources l.[14]

[13] The data also include races from the Automobile Racing Club of America, a related organization that deleaded at the same time as NASCAR; we refer to both as NASCAR.

[14] In non-randomized settings, other inputs may also be a function of Pb. For example, due to residential sorting or pollutant co-emission, changes in Pb may correlate with changes in other inputs into f, yielding

We are interested in the marginal effect of Pb on test scores, holding other factors constant:

$$\frac{\partial f(S, C, N, E, b(Pb))}{\partial b(Pb)} \frac{\partial b(Pb)}{\partial Pb} = f_b b'.$$

Regressing learning outcomes on measures of total lead exposure will confound its impact through correlated inputs: in general, lead is likely to covary with socioeconomic status and other pollutants. To identify $f_b b'$, we need to isolate independent variation in Pb. The deleading of NASCAR satisfies this requirement. Hollingsworth and Rudik (2021) show deleading did not change ambient concentrations of other automotive pollutants captured by E, thus $\frac{dE}{dx} = 0$. There is no evidence that the deleading of NASCAR fuel associates with trends in socioeconomic variables S, nutrition choices N, or school characteristics C, indicating that $\frac{dS}{dx} = \frac{dN}{dx} = \frac{dC}{dx} = 0$. The robustness of our estimates to a wide range of fixed effects and socioeconomic controls supports this as well. This gives us:

$$\frac{d\,\text{test scores}}{dx} = f_b b',$$

and variation in lead exposure from NASCAR identifies the effect of gasoline lead on test scores.[15]

The cross partial derivatives show how lead matters for educational policy. Consider some policy that affects a variable $c \in C$. The policy's marginal effect depends on lead exposure Pb if the cross-derivative is non-zero:

$$\frac{d^2\,\text{test scores}}{dx dc} = f_{bC} b'.$$

If lead-poisoned students obtain smaller marginal benefits from better teachers or smaller

direct and indirect effects of lead on test scores. The total effect of lead on test scores is:

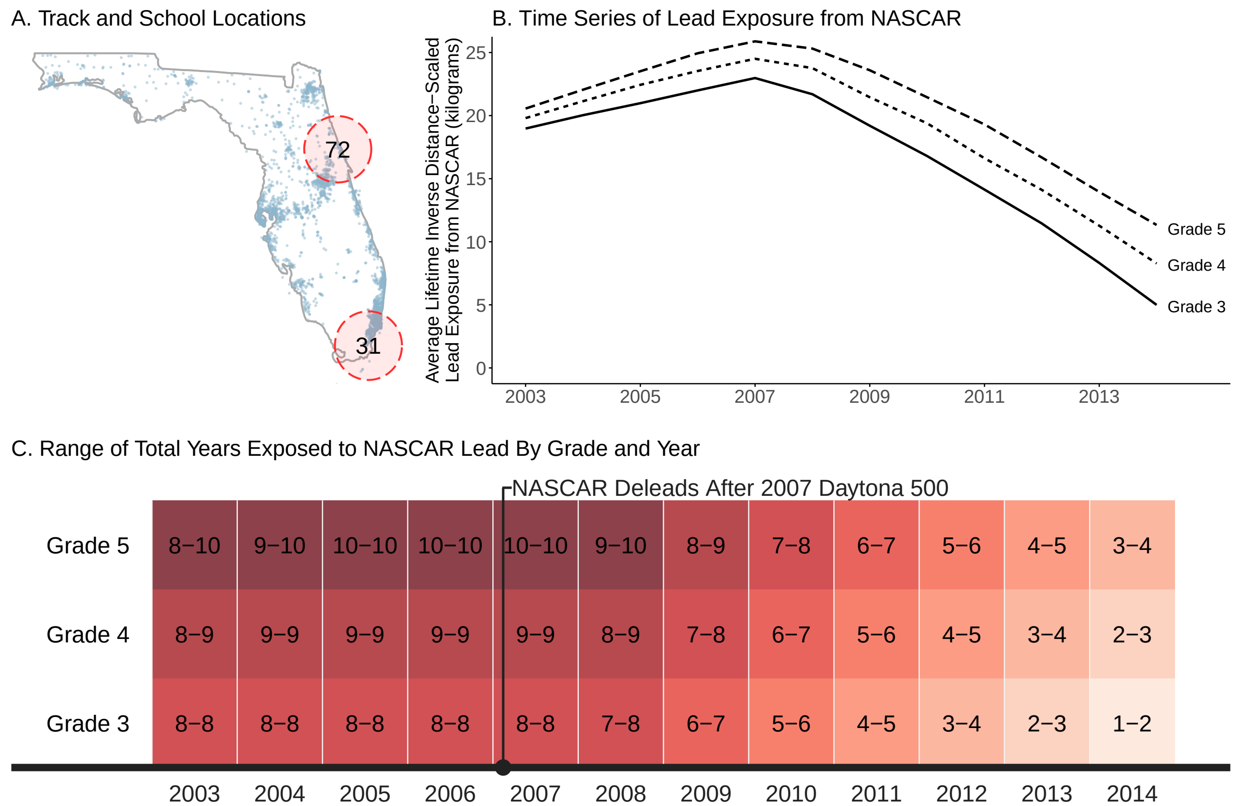
$$\frac{d\,\text{test scores}}{dPb} = f_b b' + f_C \frac{dS}{dPb} + f_S \frac{dC}{dPb} + f_N \frac{dN}{dPb} + f_E \frac{dE}{dPb}. \tag{1}$$

The first term on the right hand side, $f_b b'$, is the direct effect of lead on test scores, holding other inputs fixed. Our goal is to estimate this direct effect. The last three terms capture common potential confounders for $f_b b'$ due to indirect effects of lead. For example, large, persistent changes in ambient lead—like those induced by deleading standard on-road gasoline in the 1970s or the opening or closing of a TRI plant (Currie et al., 2015)—may induce residential sorting, which will affect inputs in S, altering test scores by changing the student composition.

[15]Equation (1) illustrates one of our key contributions. Research focuses largely on the relationship between outcomes and blood lead at a point in time, f_b, instead of the relationship between outcomes and exposure to emissions $f_b b'$. This is often due to a lack of data or a lack of quasi-experimental variation in measurable exposure. We can measure lead emissions using observed leaded race miles and the lead content of the fuel used. This helps identify the effect of lead exposure across distance, as well as the role of length of exposure conditional on total exposure.

classrooms, lead mitigation is complementary and increases returns to educational investments. We leave this question for future research.

Figure 1: Locations of tracks, schools, and number of races at each track, time series of lifetime inverse distance-scaled lead exposure by year, and timeline of years of life exposed to leaded races.



Panel A: Schools in our dataset are blue points. The shaded red area shows all places within 50 miles of a racetrack. The number in the center of the red circle is the location and number of leaded races that occurred at that racetrack between the birth year of the oldest students in our sample and the final year of our sample.
Panel B: The inverse distance-scaled lead exposure in kilograms for each grade's cohort, averaged across all schools within 50 miles of a racetrack. Lead exposure is estimated assuming an average race fuel efficiency of 3.2 race miles per gallon and 5.2 grams of tetraethyl lead per gallon.
Panel C: For each year's cohort, the minimum and maximum years of life exposed to leaded NASCAR races, ignoring exposure during the year of the cohort's test, for those schools within 50 miles of a racetrack. The left number is the minimum number of years exposed by a school cohort, the right number is the maximum number of years. The 2007 Daytona 500 was leaded, so maximum exposure for 2008 does not drop despite the fact that NASCAR deleaded in 2007.

We next give an overview of the cross-sectional and longitudinal variation in our data. Our data contain variation across space, time, age at exposure, duration of exposure, and intensity of exposure. This enables us to explore a variety of fixed effects to reduce concerns of omitted confounders. Figure 1 Panel A shows the locations of the two largest racetracks featuring leaded races, the location of schools, and the number of leaded races at each of the two racetracks over the lifetime of all children in our sample.[16] Shaded red areas depict the treatment radius around each track, with a cutoff of 50 miles. We use the cutoff of 50 miles due to both a "first stage" and reduced form association between distance and detectable effects.[17] We show cubic spline estimates on detectable correlations between distance from racetracks and student outcomes with effects approaching zero after a distance of 50 miles. This aligns with prior work showing NASCAR race effects on ambient airborne lead concentrations out to 50 miles in (Hollingsworth and Rudik, 2021). We use schools outside these areas as controls and assign them zero lead exposure from NASCAR.

For the treated schools, we inverse distance-scale exposure by the distance between the school and racetrack using equation (3) which we describe in more detail below. This scaling allows us to better capture how exposure to NASCAR lead declines in distance from the racetrack, though scaling does not affect the magnitude of our estimated effects for the average student. Because some schools are very close to racetracks, using inverse distance-scaling to calculate exposure causes values for the 100th percentile of observations to be an extreme outlier, with scaled exposure at 10 times the 99th percentile and 100 times the 90th percentile. In our primary results, we winsorize to top-code the top 5% of treated observations (approximately 1% of our full data sample) in terms of inverse distance-scaled lead emissions in the treatment group.[18] Results are robust to alternate distance scaling metrics and changing the threshold. Panel B shows that, prior to deleading, average lifetime lead exposure is slightly increasing for all grades as race volume increases over time. After deleading, contemporaneous exposure drops to zero in a each year, and lifetime exposure for each ever-treated cohort steadily declines to about 10 kilograms at the end of our sample in 2014.

Panel C shows the range of possible years exposed to leaded races for the treatment group of schools by grade and cohort.[19] For example, the 2003 grade 5 cohort was exposed to races between 8–10 years of life, as some years had no races. Cohorts that took the test in 2008 or earlier, within 50 miles of a racetrack holding annual races, have the largest possible

[16]There are two additional tracks that hosted very limited NASCAR races across our time period — four at one track, and two at another. An earlier version of this paper included students around these races as treated as well, though their exposure was minimal compared to the currently included tracks, and we now omit those tracks for simplicity of demonstration.

[17]Although we take a data driven approach to the choice of cutoff, the scientific literature has detected European lead isotopes in Greenland (McConnell et al., 2018), as well as lead up to 250km away from the actual source (Widory et al., 2018), and benchmark atmospheric dispersion models indicate that particulates can travel hundreds of miles (Morehouse and Rubin, 2021).

[18]Figure A1 shows how the distribution is highly right skewed and Table A2 shows how winsorizing affects our results.

[19]We ignore exposure during the year of the test to avoid capturing short-term effects (Ebenstein et al., 2016; Marcotte, 2017). Our results are robust to including this year.

number of exposure years.[20] As races usually occur annually, the maximum exposure of each subsequent treated test cohort declines by 1 as they phase out of leaded exposure years, until the end of our sample. Note that later treated cohorts receive reduced exposure largely through fewer race-years later in life (e.g., an 8-year old with 6 years of exposure got them from ages 0-6, with the two most recent years free of exposure). Minimum race exposure within each cohort is generally either one less or equal to the maximum exposure. Additional within-cohort variation comes from the amount of scaled lead exposure within cohorts due to distance from tracks. Treated third grade cohorts have at most 8 cumulative years of leaded-race exposure, and at least 1 year of leaded-race exposure.

We estimate the effect of lead emissions on test scores using versions of the following general specification:

$$Y_{sgty} = f(\text{cumulative lead exposure}_{sgy}; \beta_{\mathbf{lead}}) + \mathbf{X}_{syt}\beta_{\mathbf{X}} + \alpha_s + \gamma_y + \varepsilon_{sgty}. \tag{2}$$

Y_{sgty} is the school z-score, proficiency rate, or percent of students at a specific achievement level for school s, grade g, subject t, and year y. $f(\text{cumulative lead exposure}_{sgy}; \beta_{\mathbf{lead}})$ is a function f of cumulative lead emitted from NASCAR races near the school during a student's lifetime, scaled by distance from tracks. For example, for a 9-year old third grader, cumulative lead emissions are the sum of all scaled lead emissions within some distance d of the school in the 8 years before year y — our main results omit exposure in year y to avoid potential confounding from the role of contemporaneous particulate pollution from races on test outcomes (e.g., Ebenstein et al. (2016)). We compute lead emitted using the observed miles driven during each race, the known lead content of the fuel, and the average of two estimates of gasoline used per mile driven in the race.[21]

In our simplest specifications, f sums inverse distance-scaled cumulative lead emissions within 50 miles of each school s for each grade g in year y. A cubic spline regression estimate of the effect of unscaled exposure, which Figure 2 shows, provides support for our framework of 50 miles as the distance where statistically detectable test score effects disappear, and economic effects approach zero. This means the exposure in a given year for school s from lead-emitting racetrack r with a distance of distance_{sr}, the emissions treatment assigned to a

[20] The 2007 Daytona 500 was leaded; thus the 2008 and 2007 cohorts were exposed each year of life before the test year.

[21] Using the estimated quantity of lead emitted rather than miles driven also helps clarify when we mean quantities of exposure versus distances.

school is:

$$\text{cumulative lead exposure}_{sgy} = \sum_{r \in racetracks} 1\left(\text{distance}_{sr} \leq 50\right) \frac{\text{cumulative lead emitted}_{rgy}}{\text{distance}_{sr}} \tag{3}$$

where cumulative lead emitted$_{rgy}$ is the cumulative lead emitted at racetrack r during the lifetime of students in grade g in year y. We use an inverse distance-scaling procedure because we do not directly observe ambient lead levels at each school. The inverse distance-scaling recognizes that schools closer to racetracks have approximately exponentially greater exposure to lead emissions than those farther away. We explore both linear distance scaling, which assigns comparatively lower treatment for nearby lead emissions than inverse distance-scaling, and an unscaled specification, which treats all distances under 50 miles equally, in our appendix. We also test specifications where f is a flexible binned function of inverse distance-scaled emissions, or where f is a cubic spline in distance and we do not scale emissions by distance.

$\mathbf{X}_{syt}$ is our set of controls to address potential observable confounders. It includes county median income, county unemployment rate, and cumulative TRI facility lead emissions within 50 miles of each school over the same set of years described above. Controlling for income and unemployment addresses potential differential trends in socioeconomic status that influence school average test scores and spuriously correlate with declining NASCAR lead exposure for schools near racetracks. Controlling for cumulative TRI lead emissions addresses potential differential trends in industrial lead exposure for schools near racetracks versus those farther away.[22]

α_s is a set of school fixed effects that control for time-invariant school characteristics. γ_y is a set of year fixed effects addressing common annual shocks across all schools such as state-level education policy. ε_{sgty} is the error term. We cluster standard errors at the school district level, which in Florida aligns with county borders.

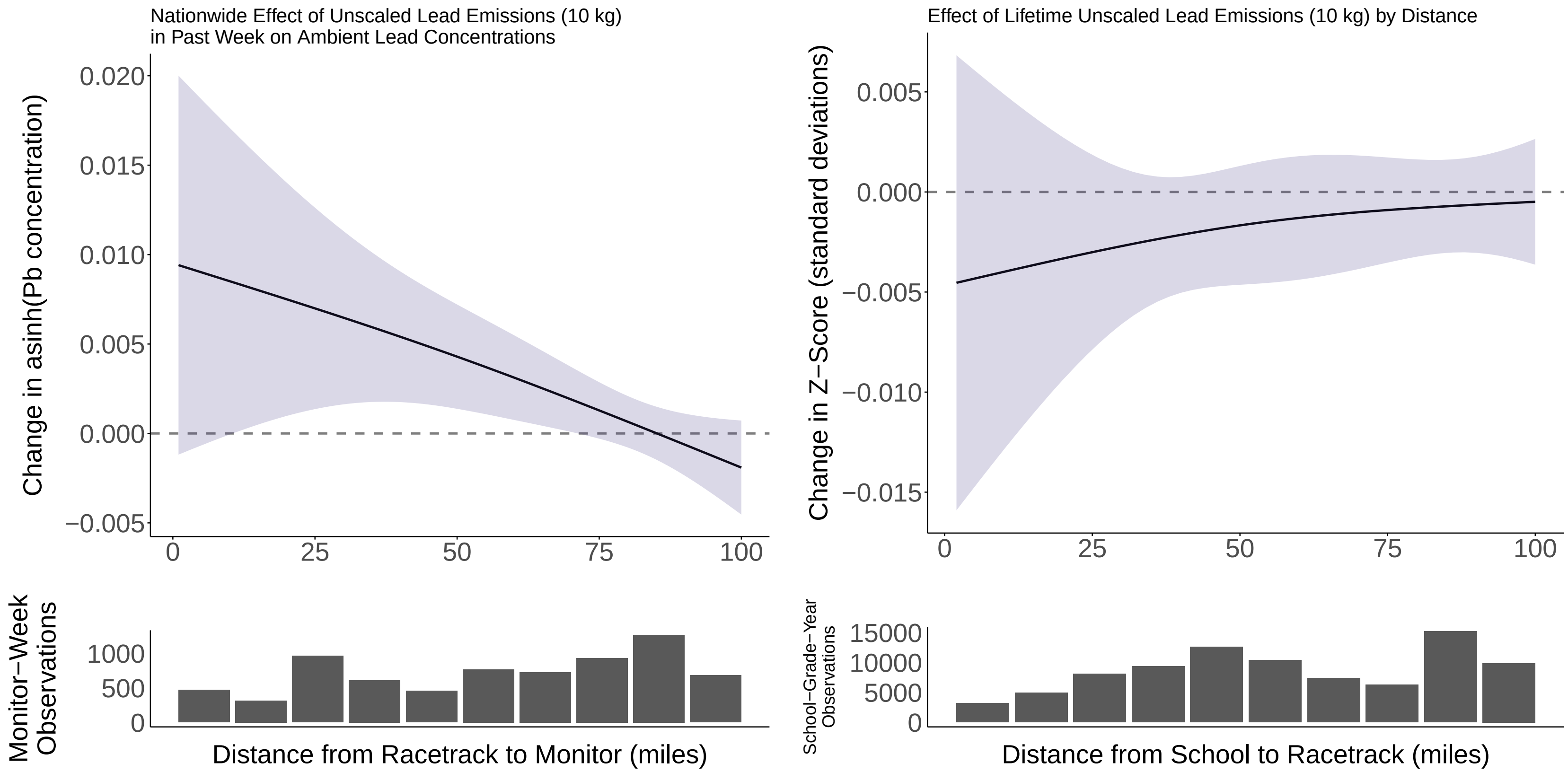
Our estimates of the effect of lead emissions on test scores are well-identified if—conditional on our controls—there remain no omitted variables varying within a school and over time that correlate with both test scores and cumulative lead emissions. The exogenous nature of the fuel switch circumvents many such concerns, since deleading did not affect other pollutants and is unrelated to changes in socioeconomic confounders for those who live nearby. In the appendix, we show our estimates are robust to a wide range of control, specification, and higher dimensional fixed effects. The stability regardless of our sets of controls and fixed

[22]TRI facilities represent another possible source of variation in lead exposure (Currie et al., 2015), but other economic and demographic factors that correlate with plant emissions complicate using such variation for identification.

effects further supports the exogeneity of our treatment measure.

We provide evidence for the validity of our distance-scaling approach in Appendix Figure A12. In the figure, we compare lead concentrations that decay following an inverse distance rule, to results from a widely-used atmospheric model, AERMOD. The American Meteorological Society and the US EPA developed AERMOD as a collaborative project. It models how emissions affect pollution concentrations across space accounting for decay differences depending on emissions height, velocity, source (point vs. area), weather, and other factors. The EPA recommends using AERMOD to model lead (United States Environmental Protection Agency, 2009), and one can calibrate AERMOD to model vehicle exhaust (Arizona Department of Environmental Quality, 2013; 2009 Regional/State/Local Modeling Workshop., 2011). Overall, the figure shows that AERMOD and our inverse distance-scaling approach generate remarkably similar predictions: changes in ambient lead concentrations from a lead source follow approximately an inverse distance-scaled relationship. The close alignment between the EPA-sanctioned model and our inverse distance-scaling approach gives us confidence that we are capturing a good approximation to the actual exposure to lead from NASCAR races.

Figure 2: Effect of lead emissions on ambient lead concentrations, and the effect of lifetime lead emissions on test scores by distance.



Left: The cubic spline is constructed with a knot at the 50th percentile of the data. The treatment variable is unscaled lead emissions. The outcome variable is the inverse hyperbolic sine of ambient lead concentrations. The blue shaded area is the 95% confidence interval computed from robust standard errors clustered at the county level. The estimates are conditioned on weather controls, monitor-by-year and week-by-year fixed effects. The panel includes all lead monitors in the US and all leaded races from major racing organizations in the US from 1996 to 2018.
Right: The cubic spline is constructed with a knot at the 50th percentile of the data. The treatment variable is now unscaled lead emissions. The blue shaded area is the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. The estimates are conditioned on the set of controls and fixed effects in equation (2).

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3 Results

Figure 2 shows how the estimated effect of NASCAR races on both lead and test scores by distance from the race. The left panel presents a cubic spline version of a specification from Hollingsworth and Rudik (2021), showing unscaled lead emissions from races have detectable effects quite a distance from the source. While the effect of race emissions on detectable ambient air lead concentrations decays with distance from the racetrack, there are nonzero effects over 50 miles away. This serves as evidence for the "first stage" for our source of variation. The decay in detectable lead across distance suggests overall exposure per lead emitted will be higher for those closer to racetracks, leading to larger effects on test scores for the same quantity of initial source emissions.

In the right panel, we plot results from equation (2), where function f is a cubic spline in distance from the racetrack to the school and the cumulative lead exposure variable is not distance-scaled. The spline shows the marginal effect of 10 *unscaled* kilograms of lead up to 100 miles away from the school. Standard errors are large for short distances because of the small number of schools directly next to racetracks, but the reduced form estimated effect of emissions on test scores clearly decays as distance from the source increases. Treatment effects fall to close to zero at 50 miles or further, supporting our cutoff distance for treated versus control schools, as detectable air lead effects appear to fall off around the same distance as detectable test score effects. The estimated effect is highest for schools closest to racetracks and declines with distance. Given the average treated exposure of about 460 unscaled kilograms, schools very close to racetracks experienced test score reductions of 0.23 standard deviations for the average cohort.

Jointly, these graphs show detectable lead and test score effects approach zero at approximately 50 miles, which serves three benefits. First, it allows us let the data determine a logical cutoff of a treatment and control zone. Second, it illustrates a strong link between emissions and outcomes at a basic level, as both lead levels and test scores shift with deleading within approximately the same distance range. Third, it suggests our empirical strategy should account for differential effects on test scores by distance, which drives our use of inverse distance-scaling in our main results.

Table 1 presents estimates using our preferred specification across outcomes and subsets of the data. Panel A shows the effect of inverse distance-scaled lead emissions on school z-scores; Panel B shows the effect on proficiency rates. Our main estimates in column 1 correspond to equation (2) and indicate that 10 additional inverse distance-scaled kilograms of lifetime lead emissions, a treatment equal to approximately 50% of the 21 scaled kilogram mean exposure for the 2005 cohort, decreases average test scores by 0.079 standard deviations, and the proficiency rate by 2.221 percentage points. This implies that, for the average 2005 treated cohort, test scores were 0.17 standard deviations lower than the counterfactual (as the bottom panel of our table shows). As a further point of reference, an additional 10 inverse distance-scaled kilograms is equivalent to locating a 42nd percentile lead-emitting facility 1 mile away from a school for the life of a 3rd-grader.

Columns 2–6 show estimates when our sample consists of only a specific grade or subject. All estimates are economically significant in size. Math effects appear larger than reading, and effects are larger for testing in grades 3 and 4 compared to grade 5, but none of the separate estimates are statistically different from each other.

Table 1: Effect of lead emissions from NASCAR on school z-score and proficiency rate.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Z-Score						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-0.079^{**}	-0.095^{**}	-0.064^{**}	-0.098^{**}	-0.099^{***}	-0.083^{**}
	(0.032)	(0.044)	(0.026)	(0.039)	(0.036)	(0.039)
Panel B: Outcome: Proficiency Rate						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-2.221^{***}	-3.355^{***}	-1.088^{**}	-1.616^{**}	-1.611^{**}	-1.308^{*}
	(0.616)	(0.885)	(0.452)	(0.655)	(0.626)	(0.703)
Average Z-Score Effect for 2005 Cohort	-0.172	-0.206	-0.138	-0.202	-0.217	-0.191
Grades Included	All	All	All	3	4	5
Subjects Included	All	Math	Reading	All	All	All
Observations	137,761	68,858	68,903	46,104	45,824	45,833

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by number of students. Panel A contains estimates where the outcome is the z-score of the school's average test score. Z-scores are calculated by standardizing within a grade-year-subject across all schools. Panel B contains estimates where the outcome is the proficiency rate and the proficiency rate spans from 0 to 100. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects.

In Table 2, we present results under a wider range of combinations of fixed effects, including a fully saturated model in which we control for school-by-subject-by-grade effects and subject-by-grade-by-year effects. For simplicity we expand solely on our main result in column 1 of Table 1. Z-score estimates are consistently statistically significant at 5% and span the tight range of 0.079–0.095: the choice of fixed effects has no real impact on the magnitude of our estimates, which supports the exogenous nature of the treatment.[23] Taken

[23]This also addresses possible bias issues from the Great Recession, which happened around approximately the same time as the deleading policy. Beyond our regional economic controls, adjustments for school district-by-year and school district-by-grade-by-year will capture economic year-over-year changes that vary by region. Given there is no reason to a priori expect that the recession would have differential impacts by distance from racetracks, it is not surprising that our estimates are unchanged with the inclusion of such effects.

together, Figure 2 and Tables 1 and 2 show evidence of a negative relationship between exposure to lead emissions and cohort performance.

Table 2: Effect of lead emissions from NASCAR on school z-score and proficiency rate with different fixed effects.

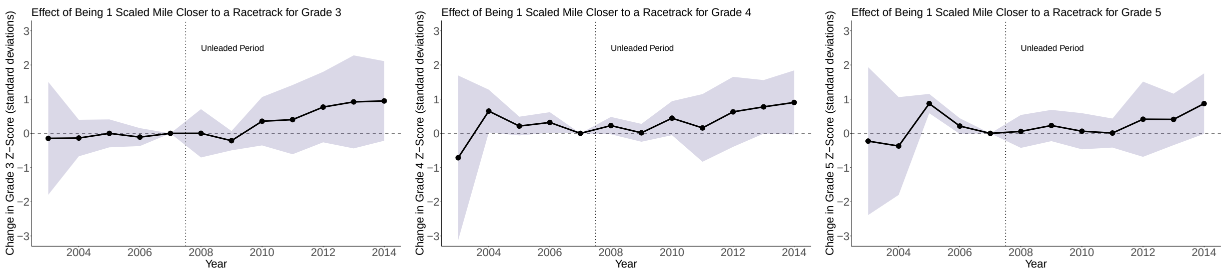
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Z-Score						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-0.079^{**}	-0.079^{**}	-0.084^{**}	-0.094^{**}	-0.095^{**}	-0.095^{**}
	(0.035)	(0.032)	(0.034)	(0.037)	(0.038)	(0.038)
Panel B: Outcome: Proficiency Rate						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-2.560^{***}	-2.221^{***}	-1.077^{*}	-1.249^{*}	-1.544^{**}	-1.544^{**}
	(0.633)	(0.616)	(0.580)	(0.634)	(0.645)	(0.646)
Controls	No	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	No	No	No
Year FE	Yes	Yes	Yes	Yes	No	No
School-Subject-Grade FE	No	No	No	Yes	Yes	Yes
Subject FE	No	No	Yes	No	No	No
Grade FE	No	No	Yes	No	No	No
Grade-Year FE	No	No	No	No	Yes	No
Subject-Grade-Year FE	No	No	No	No	No	Yes
Observations	137,761	137,761	137,761	137,761	137,761	137,761

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by the number of students. Outcome is the z-score of the school's average test score. Control variables include cumulative TRI facility emissions within 50 miles, the county unemployment rate, and county median income.

Figure 3 shows grade-specific event studies for z-scores. The point estimates show the effect of living 1 inverse distance-scaled mile closer to a racetrack. 2007 serves as our reference year. Effects for years prior to 2007 are largely statistically insignificant, economically small, and flat. For 2008 and beyond, we see a gradual relative increase z-scores across most grades and treatment variables, which coincides with subsequent treated cohorts having less and less exposure to lead.

In the appendix we provide further support for both the timing and distance factors in our empirical approach. We use the difference-in-difference inference approach proposed in Rambachan and Roth (2022). This approach constructs confidence intervals under assumptions on the structure of the parallel trend violation. In the top row of Appendix Figure A3, we plot the post-treatment confidence intervals that are robust to linear violations of parallel trends (i.e., constant trend differences). Confidence intervals are less noisy and, in the case of grades 4 and 5, shifted away from zero relative to Figure 3. Figure A2 repeats the event studies where we use an indicator variable for being within 50 miles as treatment, and using negative distance instead of inverse distance-scaling. Results are generally robust to these alternatives, though more suggestive of possible trends. The bottom two rows of Figure A3

Figure 3: Event studies of the effect of being one inverse distance-scaled mile closer to a racetrack.



The variable of interest is the negative inverse-distance to a racetrack so the coefficients can be interpreted as the effect of being 1 scaled mile closer to a racetrack. The left column is for grade 3, the middle column is for grade 4, and the right column is for grade 5. 2008 is the first year when all races in Florida were unleaded. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects. Standard errors are clustered at the school district level.

show the estimates robust to pre-trend violation. In the binary model, the results for grade 5 appear to violate pre-trend differences. However, in all other cases, results are similar to our main estimates, suggesting our estimates incorporating distance differences are not a result of pre-trends.

In Appendix Figure A4, we calculate the change in test scores at the school-grade-test level from the first to final years in our data. We then plot a scatterplot of effects grouped into 25 distance-based bins, with potential treatment distances from 0 to 100 miles. The figure shows two supportive pieces of evidence. First, schools within 50 miles of racetracks saw visibly positive changes in school-grade-test level scores from before to after deleading, while schools beyond 50 miles have largely zero change. Second, the effect on test improvement decreases with distance in a manner roughly similar to our more parametric spline estimates in Figure 2.

3.1 Flexible Quantity and Length of Lead Exposure Results

Figures 4 and 6 display flexible estimates distinguishing marginal effects in terms of quantity and length of exposure. Figure 4 plots the estimated dose-response function in black which allows the treatment effect to vary flexibly by quantity of exposure. The figure comes from estimating equation (2), where the function f is ten indicator variables corresponding to lifetime inverse distance-scaled lead exposure in 3 kilogram bins up to 30 kilograms, and an additional bin for greater than 30 kilograms. The estimated effects are relative to 0 kilograms of exposure. Increased lead emissions decrease z-scores approximately linearly.

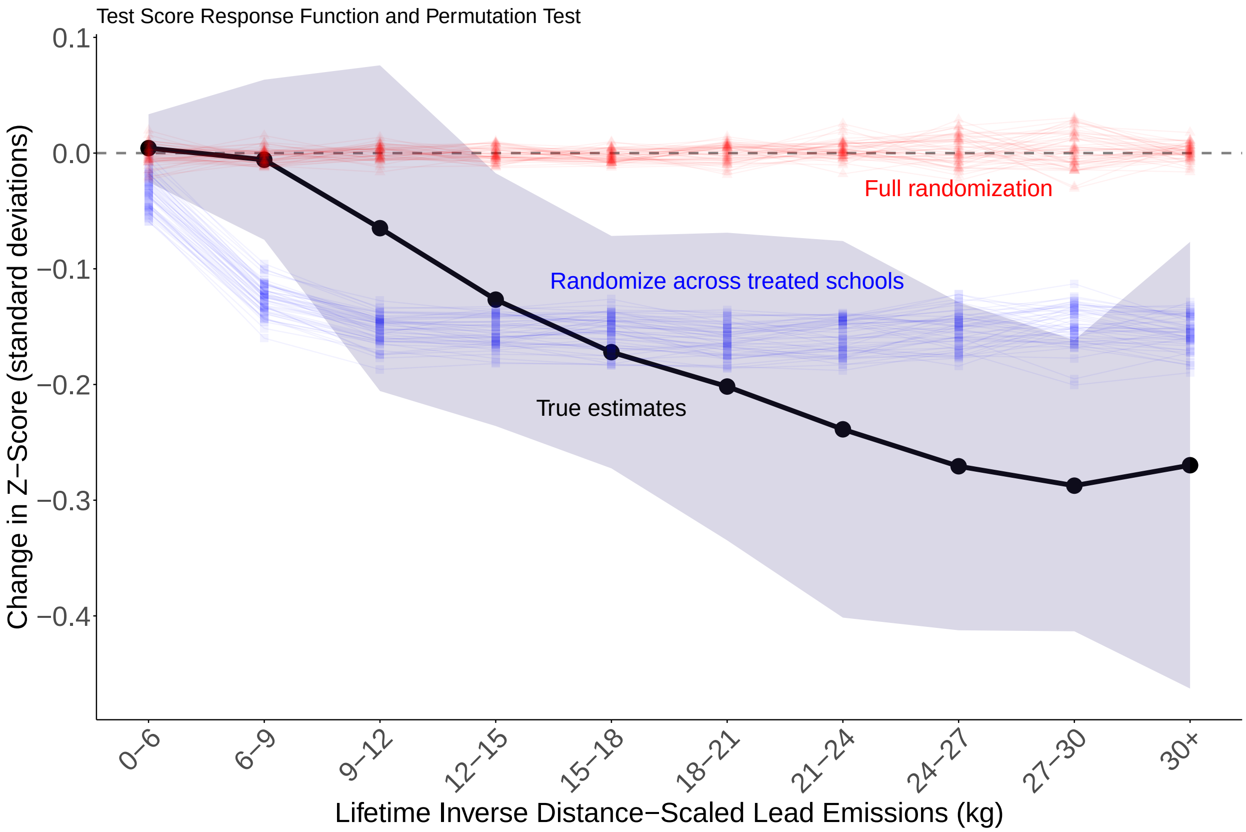
In red and blue, Figure 4 also plots the results from two permutation tests: one across all schools and one across treated schools within time. The first should yield results close to zero and the second should yield a flattened dose-response function if actual treatment decays with distance from the track. In red, we permute lifetime lead exposure across all schools and then re-estimate the dose-response function 50 times. The permuted dose-response functions are all flat and close to zero. In blue, we permute lead exposure only amongst schools within the 50 mile treatment distance in Figure 1, but within the same time period. This is equivalent to permuting the location of treated schools nearby the same racetrack, or randomly assigning treatment levels across treated schools, but still maintaining the true treatment versus control status of each school. Permuting treatment intensity only within the treatment group tests whether there is declining treatment intensity by distance within 50 miles. The permuted dose-response functions are essentially flat after the first bin and is close in magnitude to our simple difference-in-differences estimate in Appendix Table A3, indicating that schools closer to tracks even within 50 miles had greater test score effects.

Our main estimates are a function of both duration of exposure (number of cumulative years) and level of exposure (exposure in any given year). To better isolate how duration of exposure matters, we use a specification where treatment is a set of indicator variables corresponding to how many years of life a cohort experienced leaded race emissions, while conditioning on the total quantity of emissions. For example, take two 3rd grade cohorts in two districts, A and B. In district A, the cohort was exposed to 5 years of races, with 2 kg of lead in each year. In district B, the cohort was only exposed to 2 years of races, but each year had 5 kg of lead. This model tests whether these should be considered equivalent dangers. Figure 5 shows the distribution of quantity of lifetime exposure by duration of exposure and provides evidence we can separately identify effects of duration from effects of total lifetime quantity, given the substantial overlap in total exposure across years of exposure.

Figure 6 plots estimates of the effect of each additional year of exposure during a student's lifetime relative to zero years of exposure, *conditional on a given lifetime quantity of exposure.*[24] This plot teases out the difference between being exposed to a large amount of lead over a short period versus spread out over more consecutive years of a child's life. Since races happen annually in Daytona and Homestead, the two largest sources of exposure in our data, the estimate for X years of exposure maps almost perfectly into the cumulative effect of being exposed every year until a particular age. The figure depicts an escalating negative effect of additional exposure length, holding quantity of total exposure fixed. The effects of two, five, and eight years of exposure by grade 3 are -0.02, -0.09, and -0.22 standard

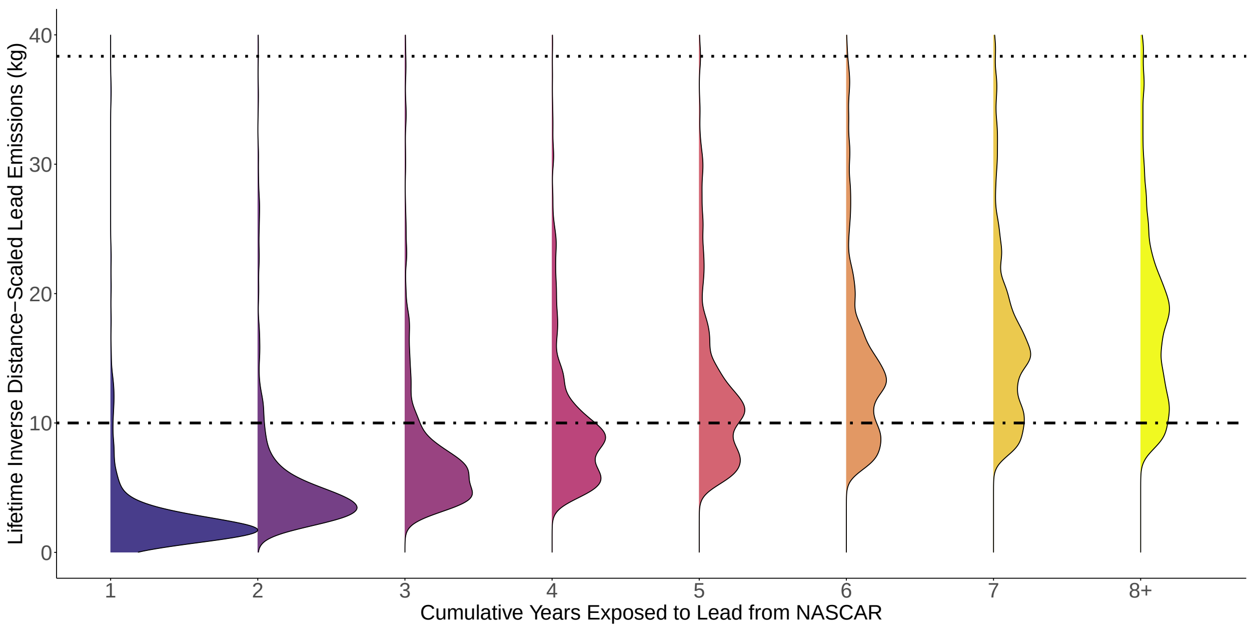
[24] These estimates are similar to equation (2), where treatment is a set of indicator variables for whether a particular student cohort was exposed to 2,,3 . . . , 8 years of leaded races, and now controlling for total exposure to separate the effects of duration and levels. The smallest value is 2 since all treated schools had at least 1 year of lead exposure.

Figure 4: Flexible effect of lifetime lead emissions on test scores by quantity and randomization tests.



The treatment variables are a set of indicators equal to 1 if a school's inverse distance-scaled lead exposure was in a particular range. Bins are open on the left and closed on the right so the first bin does not contain zero kilograms. We assign treatment only if the school was within 50 miles of the track where the leaded race occurred. The red points are the point estimates for 50 alternative dose response functions when lead exposure is permuted across all schools. The blue points are the point estimates for 50 alternative dose response functions when lead exposure is randomized only across schools within the treatment group. The black points or lines are the point estimates and the blue shaded area is the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. The estimates are conditioned on the set of controls and fixed effects in equation (2).

Figure 5: Lifetime inverse distance-scaled exposure quantity by years of exposure.



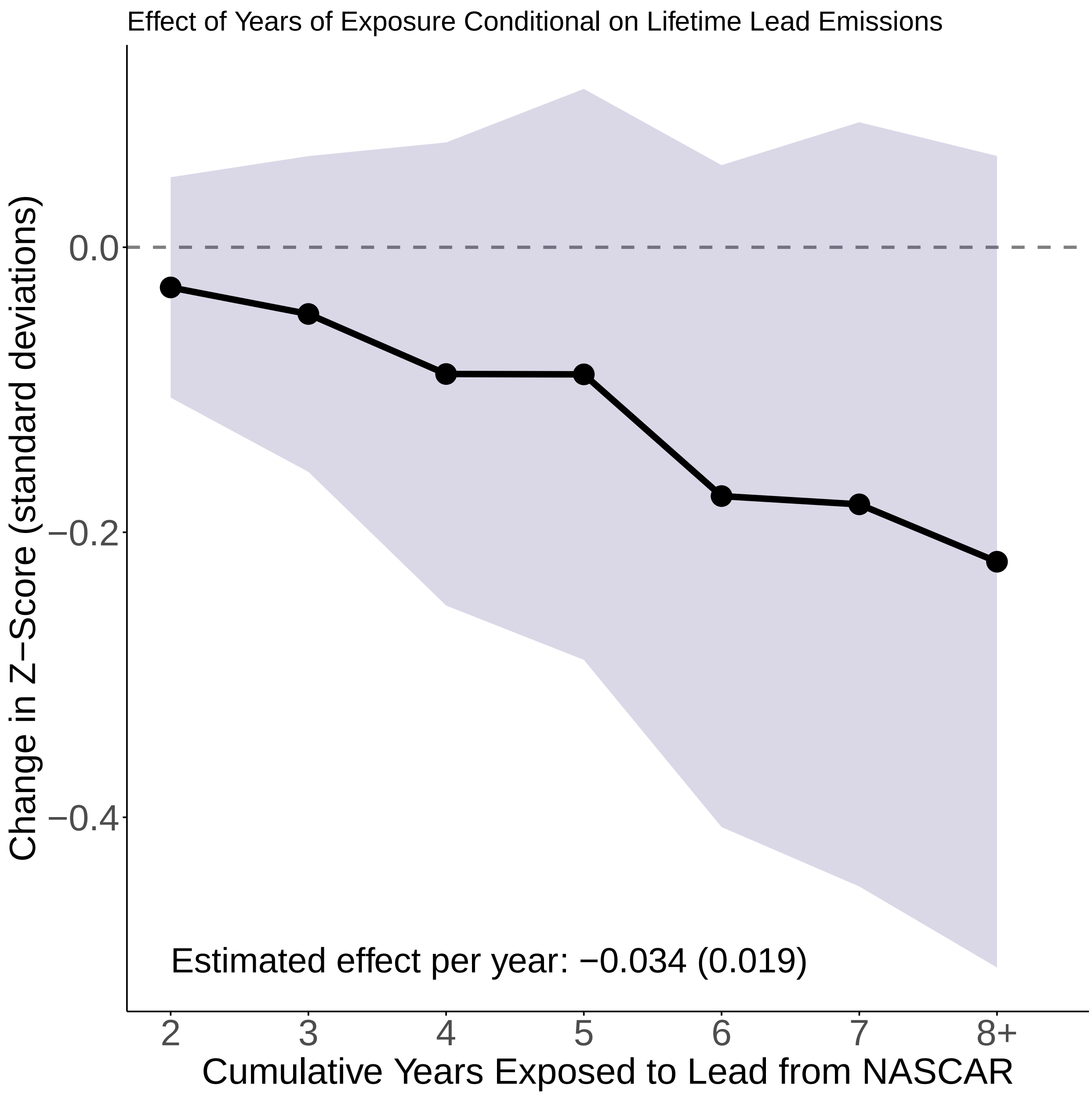
Note: Histograms are zoomed in to 0–40 inverse distance-scaled lifetime kilograms. Each density shows the distribution of inverse distance-scaled lifetime lead exposure (vertical axis) for a cohort with a given number of exposure years (horizontal axis). The dash-dotted line corresponds to 10kg of exposure, the amount reflected in our inverse distance-scaled estimates of marginal effects throughout the paper. The dotted line corresponds to the 90th percentile of exposure amongst treated schools. See Section 3 for discussion. Cumulative years of exposure align with estimates provided in Figure 6.

deviations, respectively. If we treat years of exposure as a continuous variable instead of binning — restricting the marginal effect of an additional year of exposure to be constant — we estimate that an additional year decreases test scores by 0.034 standard deviations and this is statistically significant at the 10% level.

There are several possible reasons that length of exposure might matter for a given intensity. Lead exposure may have differential long-term effects depending on the stage of an child's development, so lasting exposure could increase the probability of being affected at a given fundamental moment. For example, although early childhood is well-known to be important for aspects of cognitive development such as control of attention (Anderson, 2002), other key stages of development occur throughout childhood. Development of working memory — which is related to test performance and test anxiety (LeFevre et al., 2005; Ashcraft and Krause, 2007) — is largely linear up to around age 13 (Gathercole et al., 2004), while the abilities to process multiple sources of information and efficiently tackle defined tasks have a critical development stage closer to ages 7–9 (Anderson, 2002). It is also important to recall that for the majority of exposed students in our sample, differences in duration of exposure are driven by later years of life. For example, comparing two students with 4 vs. 8 years of exposure means one was exposed from ages 0-4, and the other from ages 0-8. Our results may also indicate that greater duration and frequency of exposure can lead to a higher chance that exposure can translate to health effects. For example, if children cycle through periods of poor nutrition, being exposed to smaller doses of lead over a longer time frame increases the chances of physical lead take-up. Mechanically, students near racetracks for fewer leaded years also have a lower probability of being in the actual area during the timing of a race; a student exposed to just one year of the leaded fuel period may have been out of town during pivotal races, which would increase the role of measurement error and attenuate effects. In the appendix, we investigate this by generating a hazard rate of incorrectly assigning lead exposure, and show that, while this does attenuate results, it does so in similar ways across the years of exposure, and does not generate the downward sloping effect we see in our results. Our results support that "death by a thousand cuts" may be more illustrative of how the lasting damages of lead accumulate.

Additional Robustness Checks: Here we briefly describe the suite of robustness and validation checks on our main results we perform in the appendix. Appendix Figure A5 shows further stability of our main estimates to various combinations of controls, fixed effects, and subsets of the data. Appendix Table A6 demonstrate the robustness of our estimates to alternative treatment variables, distance scaling, observation weights, and placebo tests, while A2 shows our estimates robustness to different winsorization thresholds. Appendix Table

Figure 6: Effect of lifetime lead emissions duration of exposure on test scores conditional on total quantity of lifetime exposure.



The treatment variables are a set of indicators equal to 1 if the school cohort was exposed to positive amounts of lead from NASCAR for the past 2, 3,...,8 years. We assign treatment only if the school was within 50 miles of the track where the leaded race occurred. The estimates are conditional on the lifetime quantity of lead exposure, the treatment variable in Table 1. Figure 5 shows common support of lifetime exposure totals by years of cumulative exposure. The black points or lines are the point estimates and the blue shaded area is the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. The estimates are conditioned on the set of controls and fixed effects in equation (2).

A4 shows the robustness of our results in Table 1 to not using an inverse distance-scaled treatment variable. Figure A6 shows that our results are robust to subsampling the control schools which outnumber the treatment schools by about 3:1 in the full sample.

3.2 Distributional Effects and Heterogeneity

3.2.1 Heterogeneity in Achievement

Figure 7 depicts how lead exposure affects the share of students in different parts of the achievement distribution in order to understand which types of students are being affected. It plots the marginal effect of 10 inverse distance-scaled kilograms of lead on the fraction of students that fall in each achievement level. We find lead exposure shifts the entire distribution of achievement, harming both high- and low-performing students. 10 kilograms of exposure reduces the school-level share of students in achievement levels 4 and 5 by just under 1 percentage points each. The shares of the lowest two achievement levels increase by about 1 percentage points each. The effect on the share of students in the middle achievement level is a decrease by about a quarter of a percentage point.

3.2.2 Heterogeneous and Distributional Impacts

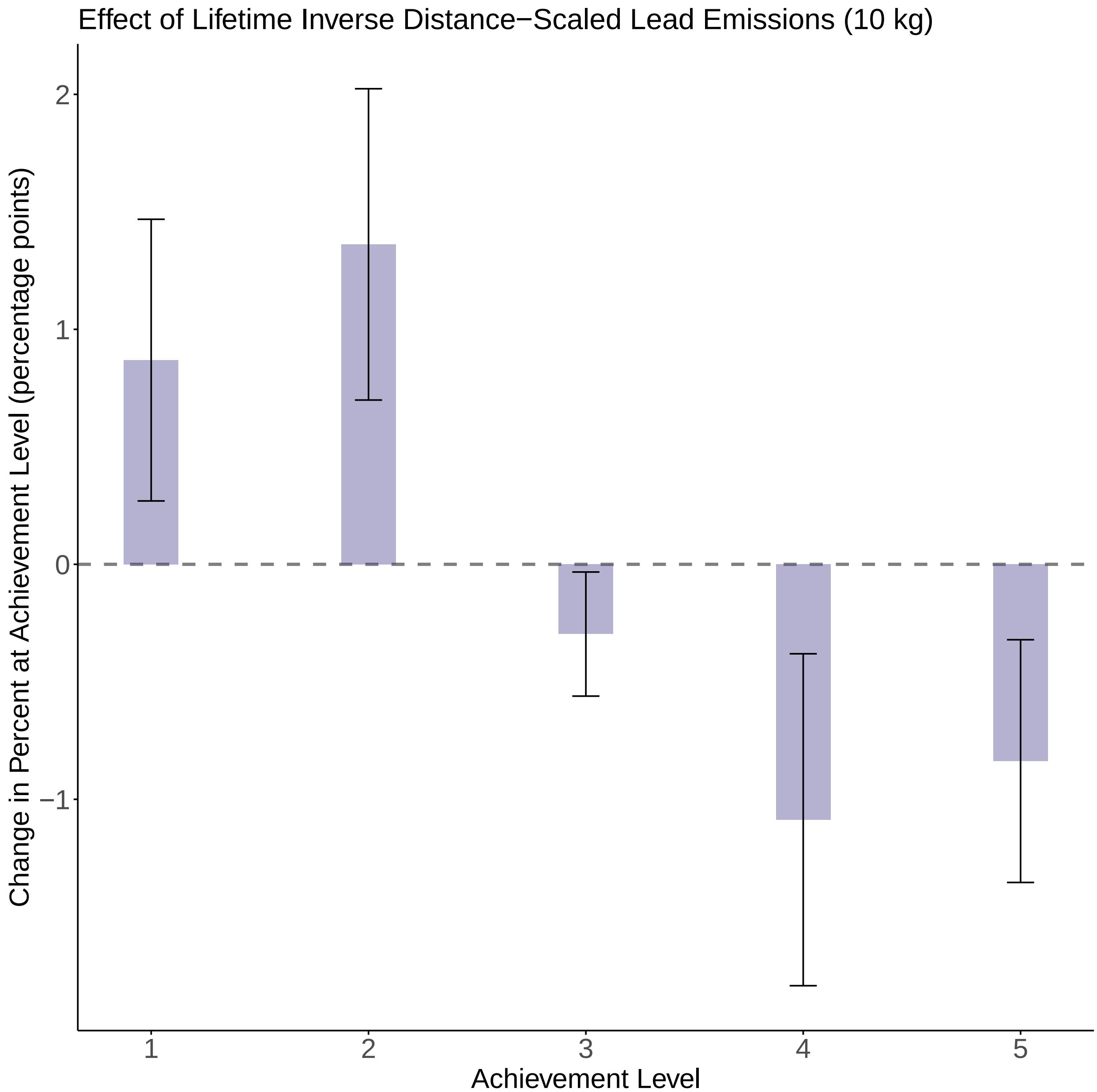
Figure 8 plots estimates of heterogeneous effects by proxies for race, age of home (related to lead exposure from leaded paint), socioeconomic status, and penetration of fast food establishments. The first estimate shows negative effects of lead exposure for counties with higher penetration of fast food establishments, which may indicate a greater prevalence of low-nutrition and unhealthy foods.

The second set of estimates show effects by district racial makeup. We find noisy zero effects for counties that are below median in terms of percent Black or Hispanic residents, and negative effects of around 0.1 standard deviations for counties above median. The estimates are statistically distinguishable for percent Black, but not Hispanic.

The third set of estimates shows the effect by percent of homes built before 1940. Homes built before 1940 likely have lead-based paint, potentially raising the baseline level of exposure for children in these homes, and putting them on a different part of the lead dose-response curve. We find little difference in the estimated effects between counties with above or below median fractions of homes built before 1940.

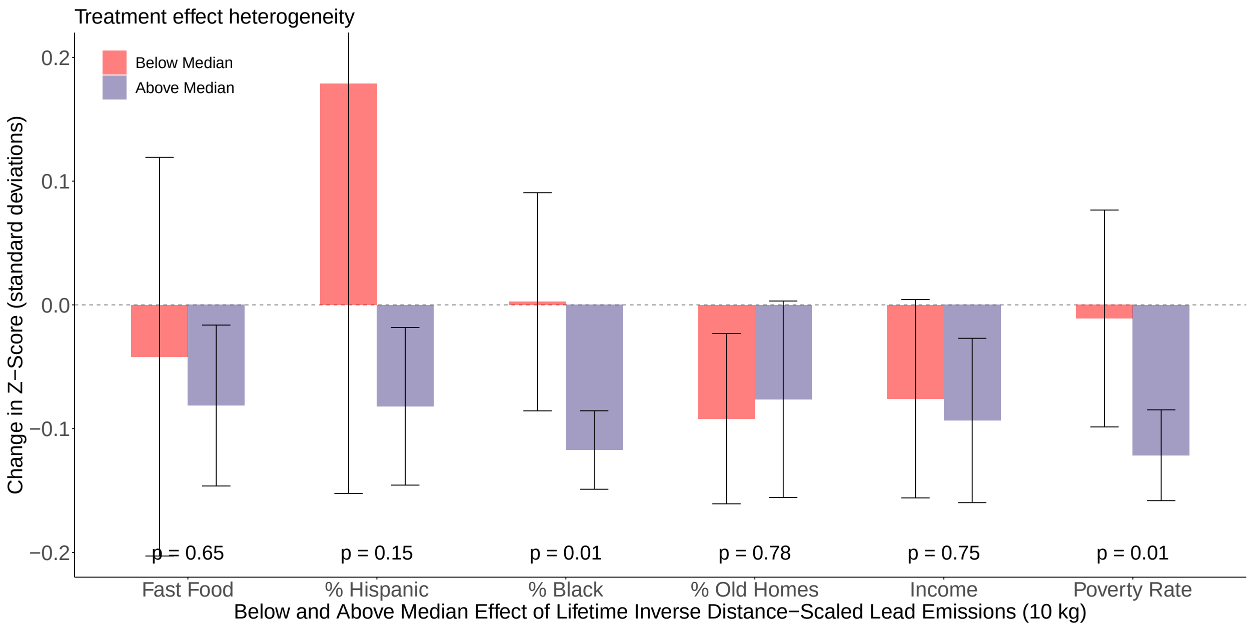
The last set of estimates show the effect by measures of socioeconomic status (SES). Lower SES families may have less ability to counteract the negative effects of lead exposure with remediation efforts or by adjusting other education inputs like tutoring. Here we find mixed results: there is little difference in terms of median income, but larger and statistically

Figure 7: Effect of 10 kilograms of inverse distance-scaled lead on percent of students in each achievement level.



The blue bars indicate the estimated effect of 10 inverse distance-scaled kilograms of lead emissions on the fraction of students in each achievement level. The black bars indicate the 95% confidence interval of the estimates computed from robust standard errors clustered at the school district level.
Note: The outcome variable is in terms of percentage points and ranges from 0 to 100. We assign treatment only if the school was within 50 miles of the track where the leaded race occurred. School-subject-grade-year observations are weighted by the number of students. The estimates are conditioned on the set of controls and fixed effects in equation (2).

Figure 8: Heterogeneous effects of 10 inverse distance-scaled kilograms of lead by above or below median in socioeconomic or variables.



The treatment variables are 10 inverse distance-scaled kilograms of lead interacted with dummy variables for whether a school is in a county that is above or below the median for the variable on the x-axis. We estimate the regressions separately for each variable on the x-axis. We assign treatment only if the school was within 50 miles of the track where the leaded race occurred. The bars are the point estimates, and the error bars denote the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. The estimates are conditioned on the set of controls and fixed effects in equation (2). The p-values at the bottom of the figure are for testing the null hypothesis that the two coefficient estimates are equal.

different effects in counties with higher poverty rates relative to those with lower poverty rates. The lower tail of the income distribution may matter the most in terms of ability to mitigate exposure.

An important consideration in interpreting these heterogeneous estimates causally is that levels of these variables are not randomly assigned and some variables may be correlated with one another (e.g., median income and % in poverty). Appendix Figure A11 shows pairwise correlations of the heterogeneous effects variables to indicate the extent to which any one variable is likely to be picking up the effect of another observable variable.

4 Valuation of Test Score Effects

Here we present several valuations of the effect of lead on academic achievement. First, we estimate the effect on future earnings by linking estimated reductions in test scores to estimated changes in future income. Second, we make non-monetary comparisons to test score improvements found from manipulating other in-school inputs to the education production function. Last we compare to outcomes based on changes in blood lead levels.

Test scores and future income: We use results from Chetty et al. (2014b) to translate changes in test scores into changes in lifetime earnings. Chetty et al. (2014b) report effects in terms of student-level standard deviations, while we report effects in terms of school-level standard deviations of the school mean score. To make a valid comparison we need to map our school-level standard deviations to student-level standard deviations following Ost et al. (2017).

For the average treated third grader in 2005, we find that lead exposure from racing results in a 0.92% reduction in earnings. Using a 3% real discount rate puts the present value of total lost future income per average student is \$5,200.[25] As a point of comparison, Isen et al. (2017) find that reduced fetal particulate exposure under the Clean Air Act raised lifetime earnings for relevant cohorts by approximately \$4,300 (discounted similarly).

Comparison to school-based inputs: To provide additional context for our test score effects, we compare them to the value of other school-based inputs. For exposed students, after making the adjustment for school versus individual level standard deviations, test scores are reduced by around 0.029 student-level standard deviations per 10 kilograms of inverse distance-scaled lead exposure. Here we consider effects on students at a school 1 mile away, which is the closest distance in the data. Removing that exposure would generate returns similar to: 15% of the magnitude of improving instructor value added by one standard deviation for one year—around a 0.2 standard deviation improvement in test scores (Chetty et al., 2014a; Hanushek and Rivkin, 2010; Bau and Das, 2020); reducing class size by 3 students—around a .03 standard deviation increase (Jepsen and Rivkin, 2009); increasing school spending per pupil by \$750—around a .03 standard deviation improvement (Jackson et al., 2021); or a quarter of the effect of avoiding an instructor with no previous teaching experience—around a .128 standard deviation reduction (Jepsen and Rivkin, 2009). Using estimates on the average increases in test scores grade over grade, our estimated effect of 10 kilograms of lead is equivalent to 21% of the expected annual increase for the third grade, which is roughly 8 weeks of lost learning (Hill et al., 2008).

Comparison to changes in blood lead levels: We also provide context by comparing our findings to those from prior work studying blood lead levels. Table A8 provides context

[25] On a per-kilogram basis for the average treated student who is located 31 miles from a racetrack, this equates to \$8 per student per kilogram of lead emitted. A more detailed exposition of this calculation can be found in Appendix section A.3.

for comparing our findings to prior work on lead exposure, blood lead levels, and educational outcomes. To do so, we use a national data set on lead poisoning rates by county, where lead poisoning indicates a blood lead level greater than or equal to 10 micrograms per deciliter (μg/dl). In column 1, we replicate the result of Hollingsworth and Rudik (2021) showing that counties with leaded races have around 16% higher lead poisoning rates. We then follow Section 5.1 of Hollingsworth and Rudik (2021) assuming that a negative blood lead test is just below the threshold for elevated at 9 μg/dl, and a positive test is just above the threshold at 10 μg/dl. Column 2 then estimates a lower bound on the average change in blood lead levels we should expect from children living in the race county for each 10kg of lead emitted. This indicates that for every 10kg of lead emitted, the average child's blood lead levels in the surrounding area increase by *at least* .03 μg/dl.

As estimates in Hollingsworth and Rudik (2021) do not scale exposure by distance, we make a comparison using our unscaled estimate in Table A4, which column 3 Table A8 provides. The 50 mile treatment radius here delivers an area approximately the same size as a county which is the treatment unit used in Hollingsworth and Rudik (2021), so the two estimates are relatively comparable. This tells us that an additional 10kg of (unscaled) exposure decreases test scores by 0.3% of a standard deviation. Combining the estimates from columns 2 and 3, a release of 10kg of lead raises average blood lead levels by at least .03 μg/dl and decreases school average test scores by .3% of a standard deviation.

Hollingsworth and Rudik (2021) estimate that NASCAR deleading reduced blood lead concentrations by at least 0.17 μg/dl, which generates lifetime benefits of at least $1,789 per child using estimates from the education literature to map changes in blood lead into income. Here we estimate the average treated child obtained benefits of $5,200 from deleading – about three times higher than the Hollingsworth and Rudik (2021) estimate. Since the Hollingsworth and Rudik (2021) estimate is a lower bound this suggests that the BLL reductions in Florida from deleading may be three times larger, around 0.5 μg/dl.

5 Discussion and Conclusion

Using a natural experiment in removal of intense leaded fuel use, we estimate the causal effects of exposure to lead emissions on student achievement. We demonstrate that exposure to lead emissions has economically significant effects for elementary students living near emission sources, and show that exposure to airborne lead correlates with reduced standardized test performance across the student achievement distribution. Our results bolster prior work suggesting that environmental quality is a key input in the education production function. We also find that duration of exposure matters, even conditional on total lifetime exposure.

While all treated students in our sample are exposed to leaded races for the first years of their lives, only some see exposure continue up until the year of the our observed exam. For a given quantity of nearby lead emissions, effects are larger if that quantity is spread across a greater span of years. Importantly, our findings are for students in Florida, a state with some of the lowest levels of lead contamination and measured blood lead in the United States. This suggests that there remain significant returns to further reductions in lead exposure even at lower modern levels. A basic model of interacting educational inputs suggests mitigating lead exposure may also increase the returns of other educational programs.

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A Appendix

A.1 Robustness Tests

Winsorizing Figure A1 shows the distribution of inverse distance-scaled lead emissions for the treatment group. The distribution is highly right skewed with some schools having treatment orders of magnitude higher than others. This is driven by a combination of schools exposed to more lead, and the inverse distance-scaling placing substantial weight on schools very close to racetracks. To address attenuation caused by this, we winsorize the top 5% of the treated data (approximately 1% of the full sample). This essentially only affects schools within a few miles of the racetracks. Table A2 shows results for different choices of the winsorization cutoff. Our results are robust to choices of 5% or more for inverse-distance scaling.

Specification chart Figure A5 presents a specification chart and shows the robustness of our main estimates of the effect of inverse distance-scaled lead emissions on test z-scores to different combinations of controls, fixed effects, and subsets of the data. The filled in circles in the bottom panel show which controls, fixed effects, grades, and tests were included. Larger effects are generally found for math and grades 3 and 4, while smaller effects are found for reading and grade 5 across the different combinations of fixed effects.

Simple Difference-in-Differences Table A3 estimates a simple difference-in-differences model where treatment is defined as being within 50 miles of a racetrack and the year is after 2007. This approach ignores timing of treatment throughout a child's life and differing treatment intensity by distance within 50 miles. The estimates show that after deleading in 200, schools nearby racetracks had increases in test scores relative to schools further away. This result is robust to a wide range of fixed effects and is consistent with our main results that exposure to lead reduces test scores.

Scaling, distance, placebos, building up FEs Table A4 repeats the same specifications as Table 1 but does not scale lead exposure by distance at all. We find qualitatively similar results. Table A5 repeats Table 1 but limits our observations to schools within 100 miles of a track to ensure that control schools far away are not driving our results. The estimated effects are slightly smaller but within one standard error of the estimates in Table 1. Table A6 demonstrates the robustness of our regressions to alternative choices of treatment variable, distance scaling, and observation weighting. It also shows estimates from several placebo tests. Column 1 is our base specification corresponding to Table 1 Panel A Column 1. Column 2 is the same as column 1 but does not weight the observations by number of students. Weighting by number of students has little effect on our estimates. Column 3 adds district-year effects while column 4 adds district-subject-grade-year effects. These effects attenuate the estimates but they are still significant. Column 5 scales lead emissions linearly by distance to reduce the influence of schools very close or far from a racetrack. Column 6 replaces lead emissions with just a count of the number of leaded races, indicating that each leaded race is associated with a 0.005 standard deviation reduction in test scores. Columns 7-9 perform three placebo tests where we assign all races after 1997, 1998, and 1999 to be unleaded. We estimate these specifications solely for the cohorts in our data that took tests during leaded years: 2003–2006. If our results were simply picking up on differential improvements in test scores for schools near racetracks that started even before deleading, then these placebos should show negative effects of lead exposure in the pre-period versus the placebo (unleaded) post-period. All three estimates have a positive sign and are statistically indistinguishable from zero, but the placebo tests reduce our sample size by about two thirds.

Wind direction One potentially important margin for exposure is wind direction. Figure A7 plots wind roses for each of the four tracks. The wind roses show the distribution of direction and speed of wind at each track using the nearest wind monitor, which for each track, is in the same city. The plots indicate the direction that the wind is blowing from, so for example, Miami-Homestead tends to have winds that blow from east to west while Daytona has a relatively uniform distribution of wind direction. This presents challenges for common upwind vs downwind empirical specifications. In Daytona, there is no general upwind versus downwind direction because of the uniformity of the distribution. In Miami, areas to the west of the racetrack are downwind while those to the left are upwind. However, to the east of Miami-Homestead Speedway is the Southern Glades Conservation Park and then the ocean, without any schools.

Does measurement error drive the cumulative exposure result? One assumption implicit in our approach of assigning years and quantity of exposure based purely on distance from the racetrack is that students are not moving during their lifetime. One may be concerned that the total amount of measurement error is systematically correlated with years of exposure and artificially driving our findings that additional years of lead exposure up to year 8 reduce test scores. If measurement error decreases over time, then attenuation bias may explain our results.

We test whether measurement error is driving our results with a simulation of third graders. We first generate a simulated data set, which we calibrate to have the same number of years, schools 'treated' by proximity to the Miami racetrack, schools 'treated' by proximity to the Daytona racetrack, and control schools as in the real data, assigning 100 students to each school/grade cell. We next calibrate the annual probability of moving (or incorrect treatment assignment) as a hazard function for a student, which we base on the idea of a student moving schools (and thus being assigned the wrong lifetime treatment). We use a recent report from the Florida Department of Education that reports the number of students who changed schools each year between 2003 and 2007 to calibrate a hazard rate of moving. Annual moving rates across years range from 5% to 0.5% with a harmonic mean of 1.09%. In the simulations we assume that, if a student moves, it is to a school that has a different treatment status than the current school since we do not observe whether a student moves to another school with the same treatment status or not. The simulation exercise thus likely overstates the amount of measurement error. Students can move multiple times and thus switch treatment status multiple times in our simulation.

We first test whether measurement error correlates with years of lead exposure. Figure A8 plots the relative error rate for each year of assigned exposure, where assigned exposure is using the rule in the main text where we assume children do not move. Specifically we plot the following:

$$\text{relative error rate} = \frac{\text{assigned years of exposure} - \text{true (simulated) years of exposure}}{\text{assigned years of exposure}}.$$

This gives us the average share of years we assigned incorrectly for each bin we use in the plot. The plot is monotonically decreasing, indicating that measurement error may be driving our findings. Note that there's a discontinuous drop at 8+ years of exposure because it captures all exposure over 8 years, and because that is the only bin where there are multiple cohorts. Correcting both of these so that all the bins are balanced in terms of exposure and the number of cohorts generates a smooth decline.

We next test whether our modeled error can generate the pattern in our findings simply due to the nature of building errors over time. We test several ways to assign moving rates, and several different ways we might think lead exposure actually determines test scores (i.e. the true data generating process for test scores). We consider five different hazard structures that capture heterogeneity in moving probabilities both over time and across treatment type:

1. Symmetric and constant: a 1.09% probability of a student moving from treatment to control and control to treatment in each year. This is the hazard structure used in Figure A8.

2. Control only and constant: a 1.09% probability of students in control schools moving

to treatment schools, 0% probability of the reverse.

3. Treatment only and constant: a 1.09% probability of students in treatment schools moving to control schools, 0% probability of the reverse.

4. Symmetric and increasing: an increasing probability of a student moving from treatment to control and control to treatment in each year, starting at 0% and increasing to $2 \times 1.09\%$.

5. Symmetric and decreasing: a decreasing probability of a student moving from treatment to control and control to treatment in each year, starting at $2 \times 1.09\%$ and decreasing to 0%.

First we test the effect of measurement error assuming that the "thousand cuts" idea is the true data generating process: if lead exposure has cumulative effects for each year exposed up to at least age 8, as Figure 6 suggests, does measurement error attenuate or overstate the overall trend? In the simulations we make the treatment effect match the per-year treatment effect of the estimates in the manuscript (-0.034 standard deviations per year exposed). We do not add noise to the test scores so the patterns induced by the measurement error of the treatment variable will be as clear as possible.

Figure A9 plots the results from estimating the model used for Figure 6 in the main text, but on our simulated data with the five different kinds of moving hazards. Assuming there are true cumulative effects, measurement error attenuates the estimates in all cases relative to the real effect plotted as the black stars. Error does change the true estimate, but in a manner that shifts the cumulative effect at all points.

We next test whether measurement error will induce this pattern if the "thousand cuts" story is incorrect and there is no accumulating impact from lead exposure throughout a child's life; can we artificially (and incorrectly) get this pattern under other plausible relationships between lead exposure and test scores?

We consider two alternative "true" data generating processes reflecting the commonly-held belief that early life exposure to lead is what drives negative outcomes. The first is that cumulative exposure reduces test scores, but only up to age four. Exposure after age four has no effect. The second is that test scores are reduced by a fixed amount for any exposure before age four (e.g. if you were exposed at age 1, or at all ages 1-4 your test scores decline by the same amount). These reflect alternative hypotheses that early life exposure is what really matters and there is not an extra effect from being exposed beyond early childhood.

Figure A10 plots the estimated effects for these cases. Here we omit the real effect estimates since we are interested in whether the estimates under the different moving hazard structures have a downward sloping trend when they should not. Neither of the proposed data generating processes generate the downward sloping trend we find with additional ages. Overall we find no evidence that plausible kinds of measurement error artificially generate our cumulative exposure findings: both sets of estimates closely replicate the actual data generating process. In fact, our simulation results suggest our findings may be attenuated and cumulative exposure is even more harmful.

A.2 Summary Statistics

Table A7 displays the summary statistics for the 2,330 schools in our dataset, split by whether schools are treated and within 50 miles of a racetrack or untreated and outside 50 miles. The data are almost evenly balanced across grades 3–5 and the means and standard deviations for nearly every variable are very similar across treatment and control. The average cohort of about 100 students has a proficiency rate of 63%, with students mostly falling in achievement levels 3 and 4. Nearly 40% of students have limited success on the FCAT and fall into achievement levels 1 or 2. School average proficiency rates span the full range from 0 to 100 percent, and z-scores span from over 6 standard deviations below average to almost 5 standard deviations above average. On average, treated cohorts are exposed to 464 unscaled kilograms, or 18.5 inverse distance-scaled kilograms of lead. The primary difference between treated and control schools is in the average lifetime exposure to TRI lead emissions which is around 300 for treated schools and 600 for control schools.

A.3 Detailed welfare calculations

Here we use associated estimates linking test scores to future earnings to construct an estimate of how lead exposure may affect future earnings. For this exercise we estimate lost earnings for the average 2005 treated third grader in Florida as a result of their cumulative lifetime exposure. Conditional on being exposed to at least one leaded race within the 50 mile treatment radius, the average third grader in 2005 was exposed to 20.5 inverse distance-scaled kilograms of lead. Column 4 of Table 1 indicates that this amount of lead exposure decreases school-level test scores at for third graders by 0.098 standard deviations.

We translate these effects on test scores into lost lifetime earnings using results from Chetty et al. (2014b), who report that a 1 standard deviation improvement in student-level standardized test scores is associated with 12% higher lifetime earnings.[26] Combining this with

[26] While this estimated relationship should not be interpreted to be causal, it represents the best estimate we can find between standardized test scores and future earnings. The estimate is conditional on teacher fixed effects as well as student and class-level controls. Chetty et al. (2014b) also report the unconditional relationship, which is 36%.

the 0.098 standard deviation reduction estimate, and that the ratio of school to student-level standard deviations is .371, the average 2005 treated third grader in our sample experienced a 0.91% decrease in lifetime earnings. Chetty et al. (2014b) also report that the present value of expected future earnings at age 12 is $618,705 in 2020 dollars using a 3% real discount rate (5% discount minus 2% wage growth). At grade 3 (age 9), the present value is $566,203. A 0.91% lifetime earnings loss is $5,196 in 2020 dollars. When using the unscaled leaded miles estimate in the appendix we obtain an average income loss of $5,320 for an average 2005 cohort treated exposure of 517 unscaled kilograms. This indicates that the choice of inverse distance-scaling has little effect at the average.

We next provide a back of the envelope approximation of the external cost of a gram of lead from gasoline and total cohort costs from NASCAR in Florida. We put the external cost in per student per kilogram terms so that our estimate is not a function of Florida's population distribution around racetracks. The external cost of a kilogram of lead per exposed student within 50 miles is the income loss per student divided by the average lifetime exposure:

$$\frac{\$5,196}{\text{student}} \Big/ 20.5 \text{ scaled kilograms} = \$254/\text{student}/\text{scaled kilogram}.$$

Being exposed to 1 kilogram of lead emitted 1 mile away by the third grade results in a present value income loss of $254. The average treated student is 31 miles from the nearest racetrack so that the marginal cost for the average is $8 per kilogram. Next we aggregate to the total loss to the entire Florida 2005 third grade cohort. There were 63,339 third graders in Florida in 2005 within 50 miles of a racetrack, which amounts to a total income loss of over $320 million from NASCAR lead exposure. Note that this is only for students in a single cohort in a single state. One limitation to our approach is that our test score outcome is a school average, not an individual student's. The average treatment effect at the school-grade-test level—even when deflated to approximate student-level data—may not be the same as the average treatment for the treated student.

A.4 Lead emissions and miles traveled

Our quantity estimates are based on two unique data elements and an estimate of average race fuel economy. First, we observe the actual distance driven by each racecar in each race.[27]

[27] Actual distance driven may vary due to crashes or weather, so inferring distance from the maximum potential distance driven, for example 500 miles per racer for the Daytona 500, would overstate the amount of lead emitted and bias our estimates toward zero.

Second, we observe the lead content of the race fuel.[28] The fuel for every race is provided by NASCAR and Sunoco, ruling out any potential cheating by using leaded fuel in the unleaded period. We combine miles driven and fuel lead content with an estimate of the average fuel economy of the racecars, derived from reported fuel usage over a full racing season. Fryer (2008) reports that the top series in NASCAR used 175,000 gallons of fuel in 2008. Our race data show that 566,130 in-race miles were run in the 2008 season, indicating that roughly 3.24 in-race miles awere traveled per gallon of race fuel used. This provides our estimate of the total quantity of lead emitted per race.

Note that we find a similar estimate when considering additional information from a single race. In-race miles per gallon have been estimated to be between four and five miles per gallon (Belson, 2011). This does not account for out-of-race miles traveled in qualifying and practice rounds and we want to account for fuel used for these purposes. Following Hollingsworth and Rudik (2021), we obtain estimates of the share of miles that come from these portions of the race using data from the 2019 Ticket Guardian 500. This race had 10,766 race miles and 3,053 practice miles.[29] Assuming that 330 miles were driven as a part of qualifying (see Hollingsworth and Rudik (2021) for more detail on this estimate), the 10,766 in-race miles are 76% of the total miles driven as a part of the whole event. Accounting for these additional non-race miles would mean adjusting in-race fuel economy estimates to be between 3 and 3.8. This is consistent with the 3.24 mpg estimate provided from the first approach.

A.5 Supplementary figures

Pairwise correlations for heterogeneous effects Figure A11 shows the pairwise correlations between the heterogeneous effect variables in Figure 8 to better understand whether one variable is simply proxying for another.

[28]NASCAR rules mandated the use of Sunoco Supreme, a 112 octane fuel with 5.2 grams of tetraethyl lead per gallon. The exact fuel can be found here: https://www.sunocoracefuels.com/fuels/fuel/supreme. It is still available to be purchased by the public as of 2020, and is continued to be used in a number of racing series such as TransAm Racing and the National Hot Rod Association.

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

Table A1: Comparison of grade 4 mean and standard deviation across student-level and school-level data for the same test.

Year	Student mean	School mean	Student S.D.	School S.D.	S.D. Ratio
Math					
2003	298	298	63.4	24.4	0.384
2004	312	312	58.7	21.7	0.370
2005	312	312	57.8	22.2	0.385
2006	318	318	60.8	24.1	0.396
2007	319	320	59.9	23.0	0.385
2008	324	325	60.8	23.0	0.378
Average	314	314	60.2	23.1	0.383
ELA					
2003	305	305	60.5	22.2	0.368
2004	318	318	51.4	18.1	0.352
2005	319	319	55.1	19.3	0.350
2006	314	314	53.5	19.2	0.359
2007	316	316	57.7	21.0	0.363
2008	319	319	56.2	20.0	0.357
Average	315	315	55.7	20.0	0.358

Notes: Student-level means and standard deviations come from Tables FL-5 and FL-6 from this document https://files.eric.ed.gov/fulltext/ED506142.pdf. School-level means and standard deviations are calculated using the data used in our analysis. Since we do not have access to the restricted student level data, we can only compare the means and standard deviations for the years, tests, and grades in this report.

Table A2: Effect of lead emissions from NASCAR on school z-score and proficiency rate with different winsorization thresholds.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-0.079^{**}							
	(0.035)							
Winsorized 99%: Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)		-0.023						
		(0.032)						
Winsorized 97.5%: Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)			-0.038					
			(0.036)					
Winsorized 90%: Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)				-0.107^{***}				
				(0.036)				
Winsorized 75%: Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)					-0.155^{***}			
					(0.043)			
UnWinsorized Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)						-0.012		
						(0.015)		
Lifetime Lead Emissions (10 kg)							-0.003^{*}	
							(0.001)	
UnWinsorized Lifetime Lead Emissions (10 kg)								-0.003^{*}
								(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	137,761	137,761	137,761	137,761	137,761	137,761	137,761	137,761

Note: Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by the number of students. Outcome is the z-score of the school's average test score. Control variables include cumulative TRI facility emissions within 50 miles, the county unemployment rate, and county median income.

Table A3: Simple difference-in-differences.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Z-Score						
Within 50 Miles of a Racetrack and After 2007	0.115*	0.111**	0.111**	0.112**	0.112**	0.112**
	(0.059)	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)
Panel B: Outcome: Proficiency Rate						
Within 50 Miles of a Racetrack and After 2007	1.637	1.588*	1.611*	1.628*	1.624*	1.624*
	(1.034)	(0.864)	(0.925)	(0.933)	(0.937)	(0.937)
Controls	No	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	No	No	No
Year FE	Yes	Yes	Yes	Yes	No	No
School-Subject-Grade FE	No	No	No	Yes	Yes	Yes
Subject FE	No	No	Yes	No	No	No
Grade FE	No	No	Yes	No	No	No
Grade-Year FE	No	No	No	No	Yes	No
Subject-Grade-Year FE	No	No	No	No	No	Yes
Observations	137,761	137,761	137,761	137,761	137,761	137,761

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by the number of students. Outcome is the z-score of the school's average test score. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects. The reported treatment variable is equal to 1 if a school is within 50 miles of one of the two racetracks and the year is after 2007.

Table A4: Effect of lead emissions from NASCAR on school z-score and proficiency rate without inverse distance-scaling lead.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Z-Score						
Lifetime Lead Emissions (10 kg)	-0.003^{*}	-0.003	-0.002^{**}	-0.004^{**}	-0.003^{*}	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Panel B: Outcome: Proficiency Rate						
Lifetime Lead Emissions (10 kg)	-0.090^{***}	-0.134^{**}	-0.047^{**}	-0.067^{**}	-0.049^{*}	-0.036
	(0.033)	(0.053)	(0.020)	(0.033)	(0.029)	(0.031)
Grades Included	All	All	All	3	4	5
Subjects Included	All	Math	Reading	All	All	All
Observations	137,761	68,858	68,903	46,104	45,824	45,833

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by number of students. Panel A contains estimates where the outcome is the z-score of the school's average test score. Panel B contains estimates where the outcome is the proficiency rate and the proficiency rate spans from 0 to 100. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects.

Table A5: Effect of lead emissions from NASCAR on school z-score and proficiency rate when limiting schools to be within 100 miles.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Z-Score						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-0.063^{**}	-0.079^{*}	-0.048^{*}	-0.082^{**}	-0.083^{**}	-0.066
	(0.031)	(0.045)	(0.024)	(0.037)	(0.037)	(0.040)
Panel B: Outcome: Proficiency Rate						
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-2.289^{***}	-3.629^{***}	-0.949^{**}	-1.356^{**}	-1.306^{**}	-1.073
	(0.619)	(0.956)	(0.457)	(0.607)	(0.630)	(0.707)
Average Z-Score Effect for 2005 Cohort	-0.137	-0.171	-0.104	-0.169	-0.181	-0.151
Grades Included	All	All	All	3	4	5
Subjects Included	All	Math	Reading	All	All	All
Observations	87,096	43,535	43,561	29,231	28,936	28,929

Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by number of students. Panel A contains estimates where the outcome is the z-score of the school's average test score. Z-scores are calculated by standardizing within a grade-year-subject across all schools. Panel B contains estimates where the outcome is the proficiency rate and the proficiency rate spans from 0 to 100. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects. The sample is limited to schools within 100 miles of a racetrack.

Table A6: Robustness checks for the effect of lead emissions on school z-score.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	-0.079^{**}	-0.082^{**}	-0.031^{*}	-0.050^{**}						
	(0.032)	(0.038)	(0.016)	(0.024)						
Lifetime Linear Distance-Scaled Lead Emissions (10 kg)					-0.009^{**}					
					(0.004)					
Lifetime Lead Emissions (10 kg)						-0.003^{*}				
						(0.001)				
Lifetime Leaded Races							-0.005^{*}			
							(0.003)			
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg): 1997 Placebo								0.007		
								(0.010)		
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg): 1998 Placebo									0.004	
									(0.009)	
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg): 1999 Placebo										0.003
										(0.009)
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Subject-Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subject-Grade-Year FE	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	No	No	Yes	No	No	No	No	No	No	No
District-Subject-Grade-Year FE	No	No	No	Yes	No	No	No	No	No	No
Observation Weights	# Students	None	# Students	# Students	# Students	# Students	# Students	# Students	# Students	# Students
Observations	137,761	137,761	137,761	137,761	137,761	137,761	137,761	42,624	42,624	42,624

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the school district level. School-subject-grade-year observations are weighted by the number of students. Outcome is the z-score of the school's average test score. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects.

Table A7: Summary statistics.

Panel A: Treated Schools

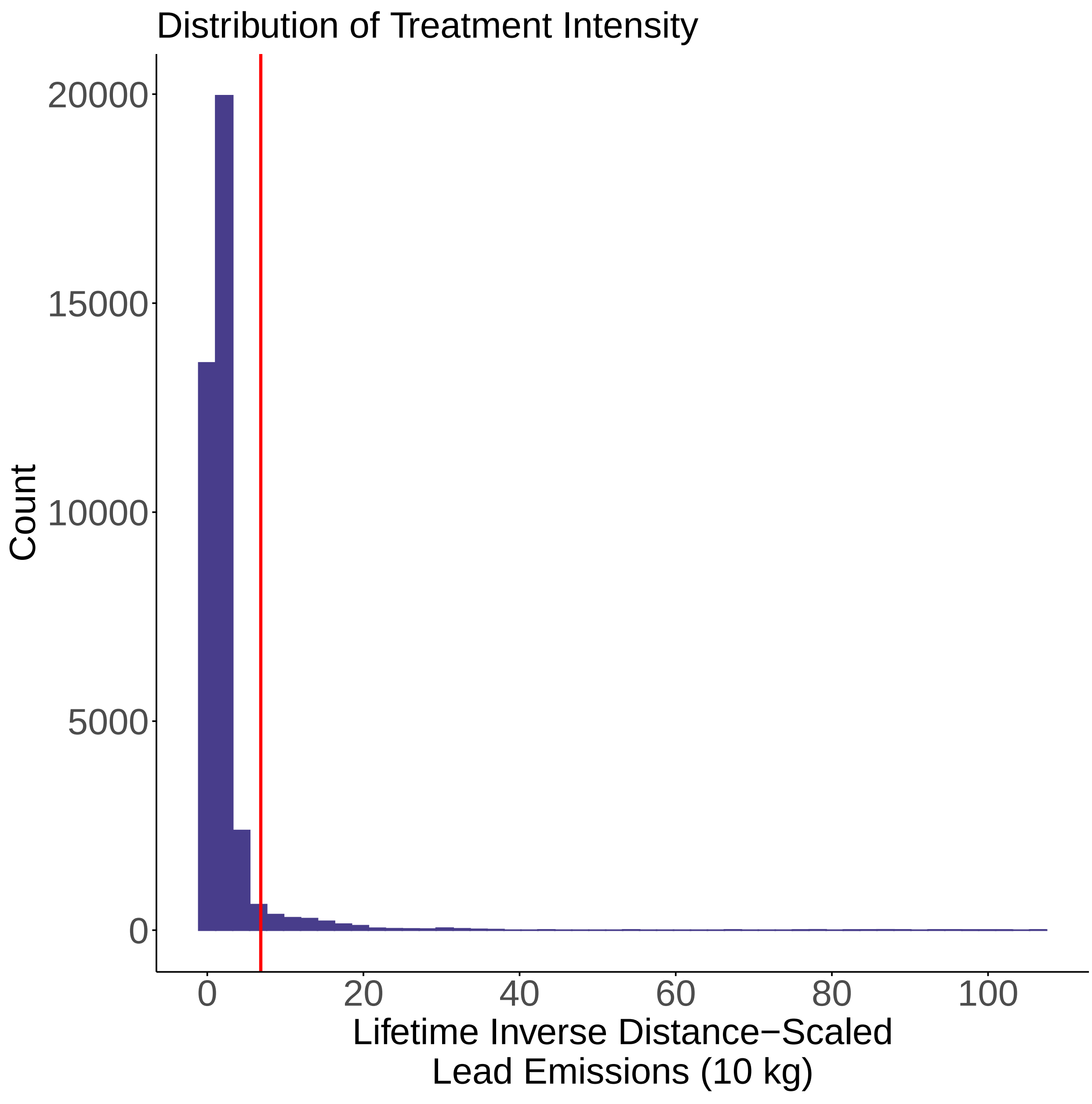
Statistic	Mean	St. Dev.	Min	Max	N
Z-Score	−0.02	1.02	−6.77	4.13	38,250
Proficiency Rate	63.11	17.92	0	100	38,250
Grade	4.00	0.82	3	5	38,250
% Achievement Level 1	17.86	12.64	0	100	38,250
% Achievement Level 2	19.05	8.91	0	80	38,250
% Achievement Level 3	30.85	8.17	0	90	38,250
% Achievement Level 4	23.80	10.79	0	83	38,250
% Achievement Level 5	8.46	7.67	0	87	38,250
Number of Students	110.07	50.26	10	425	38,250
Lifetime Unweighted Lead Emissions (10 kg)	46.38	24.25	5.50	92.02	38,250
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	1.85	1.55	0.11	6.85	38,250
Lifetime Leaded Years	6.29	2.12	1	8	38,250
Median Income ($)	43,942.28	5,789.72	28,664	67,238	38,250
Unemployment Rate	6.09	2.82	2	14	38,250
Lifetime Industrial Lead Emissions (metric tons)	312.89	265.21	0.00	1,274.98	38,250

Panel B: Untreated Schools

Statistic	Mean	St. Dev.	Min	Max	N
Z-Score	0.01	0.99	−6.56	4.88	99,511
Proficiency Rate	63.25	18.01	0	100	99,511
Grade	4.00	0.82	3	5	99,511
% Achievement Level 1	17.16	12.10	0	100	99,511
% Achievement Level 2	19.61	9.35	0	74	99,511
% Achievement Level 3	31.23	8.46	0	94	99,511
% Achievement Level 4	23.84	11.11	0	94	99,511
% Achievement Level 5	8.18	7.56	0	87	99,511
Number of Students	99.57	44.59	10	448	99,511
Lifetime Unweighted Lead Emissions (10 kg)	0.00	0.00	0	0	99,511
Lifetime Inverse Distance-Scaled Lead Emissions (10 kg)	0.00	0.00	0	0	99,511
Lifetime Leaded Years	0.00	0.00	0	0	99,511
Median Income ($)	45,007.59	5,895.39	25,201	67,238	99,511
Unemployment Rate	6.46	2.77	2	14	99,511
Lifetime Industrial Lead Emissions (metric tons)	631.39	613.47	0.00	2,927.36	99,511

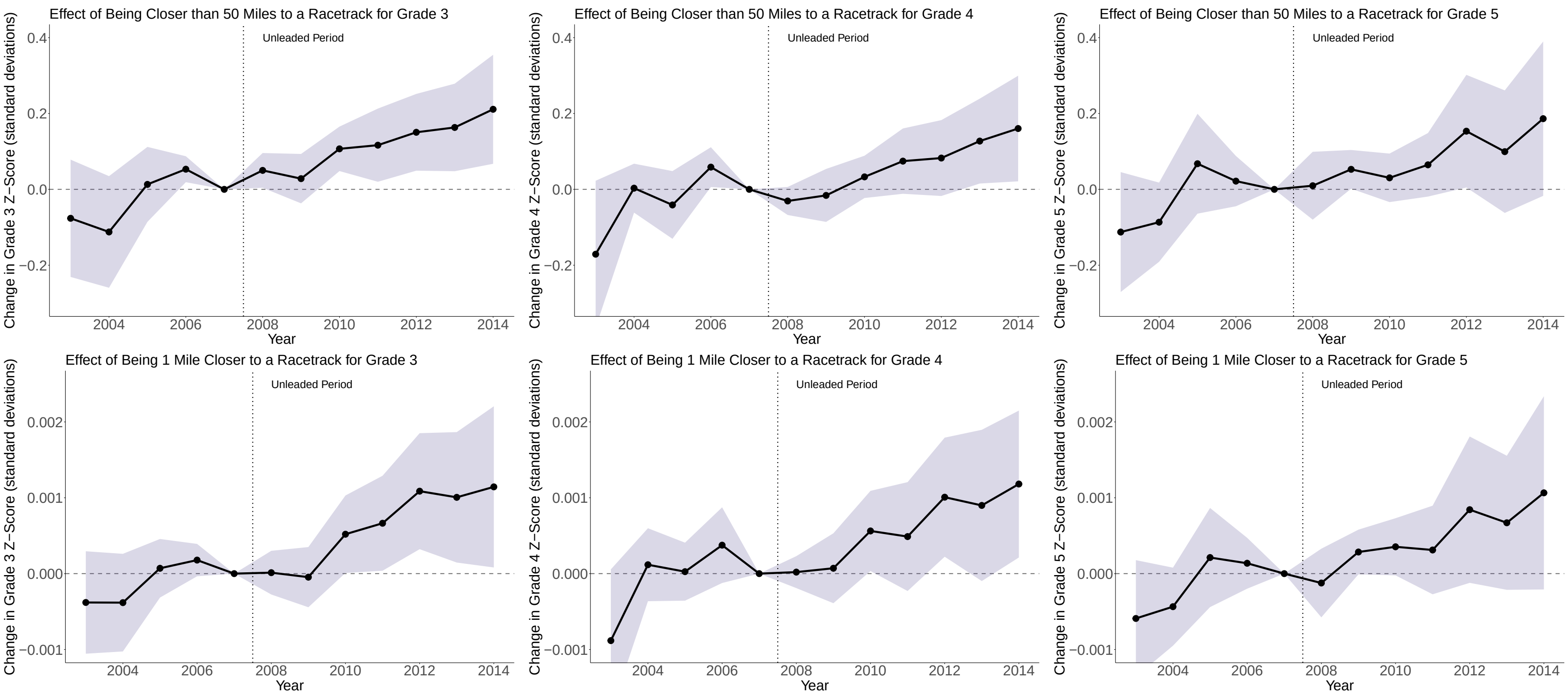
Note: An observation is a school-grade-subject-year. There are 645 treated schools and 1685 untreated schools.

Figure A1: Unwinsorized distribution of lead emissions in the treatment group.



Note: The histogram is distribution of treatment intensity conditional on positive amounts of lead emissions. The red line denotes the threshold we use in the main results for winsorizing the top 5% of the treated group.

Figure A2: Event studies of the effect of being within 50 miles of a racetrack or 1 mile closer to a racetrack.



The variable of interest in the top row is an indicator variable for being within 50 miles of a racetrack. The variable of interest in the bottom row is the negative distance to a racetrack so the coefficients can be interpreted as the effect of being 1 mile closer to a racetrack. The left column is for grade 3, the middle column is for grade 4, and the right column is for grade 5. 2008 is the first year when all races in Florida were unleaded. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects. Standard errors are clustered at the school district level.

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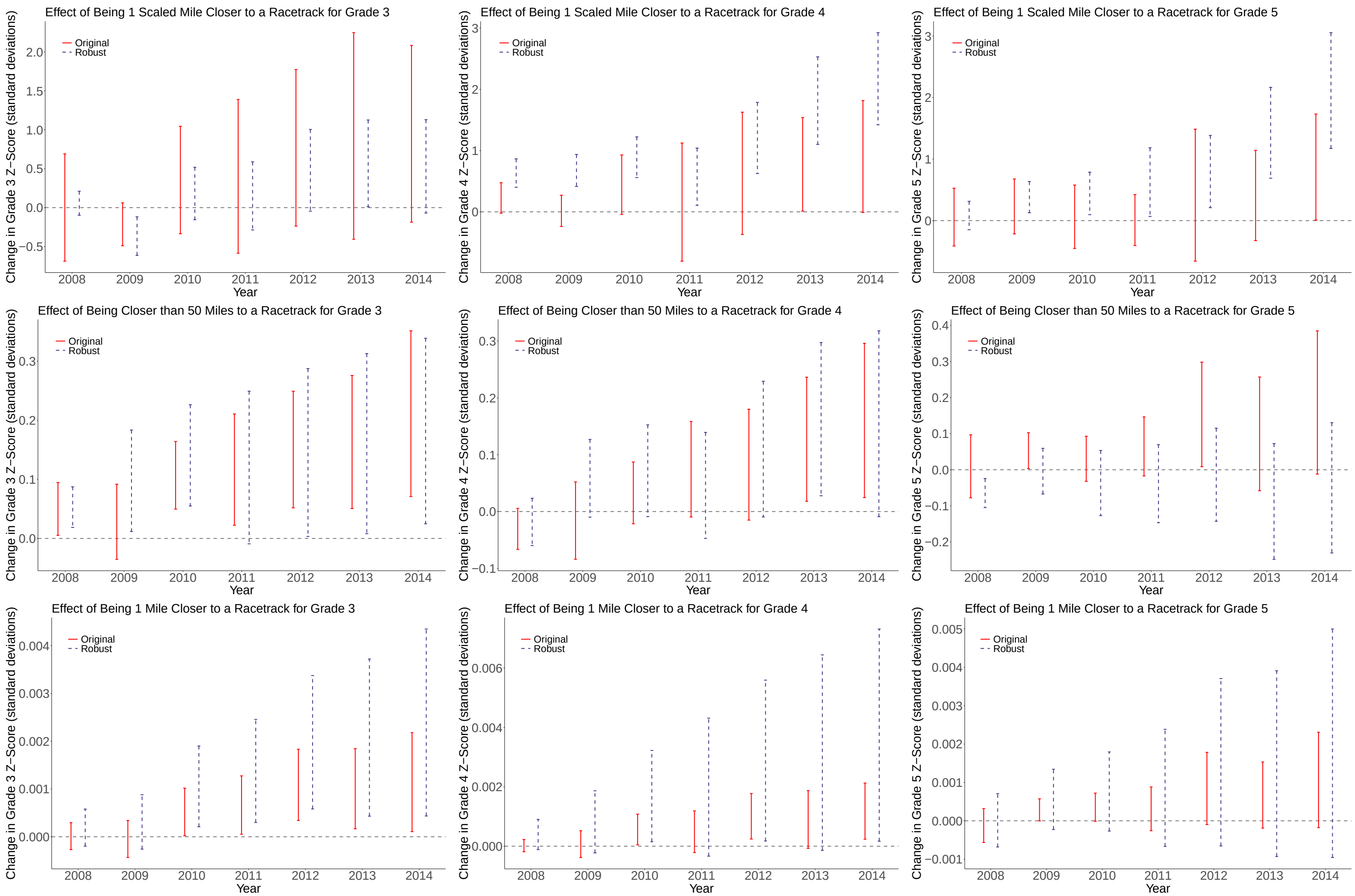
Figure A3: Event studies of the effect of being one mile closer to a racetrack allowing for linear trend violations.



The variable of interest in the top row is the negative inverse-distance to a racetrack so the coefficients can be interpreted as the effect of being 1 scaled mile closer to a racetrack. The variable of interest in the middle row is an indicator variable for being within 50 miles of a racetrack. The variable of interest in the bottom row is the negative distance to a racetrack so the coefficients can be interpreted as the effect of being 1 mile closer to a racetrack. The red line is our original confidence interval, the blue dashed line is our robust confidence interval. The left column is for grade 3, the middle column is for grade 4, and the right column is for grade 5. 2008 is the first year when all races in Florida were unleaded. The control variables included in all regressions are cumulative TRI facility emissions within 50 miles, county unemployment rate, and county median income. All regressions include school and year fixed effects. Standard errors are clustered at the school district level. Robust confidence intervals are computed using the method in Rambachan and Roth (2022) with $M = 0$ and $\Delta = \Delta^{SD}(M)$.

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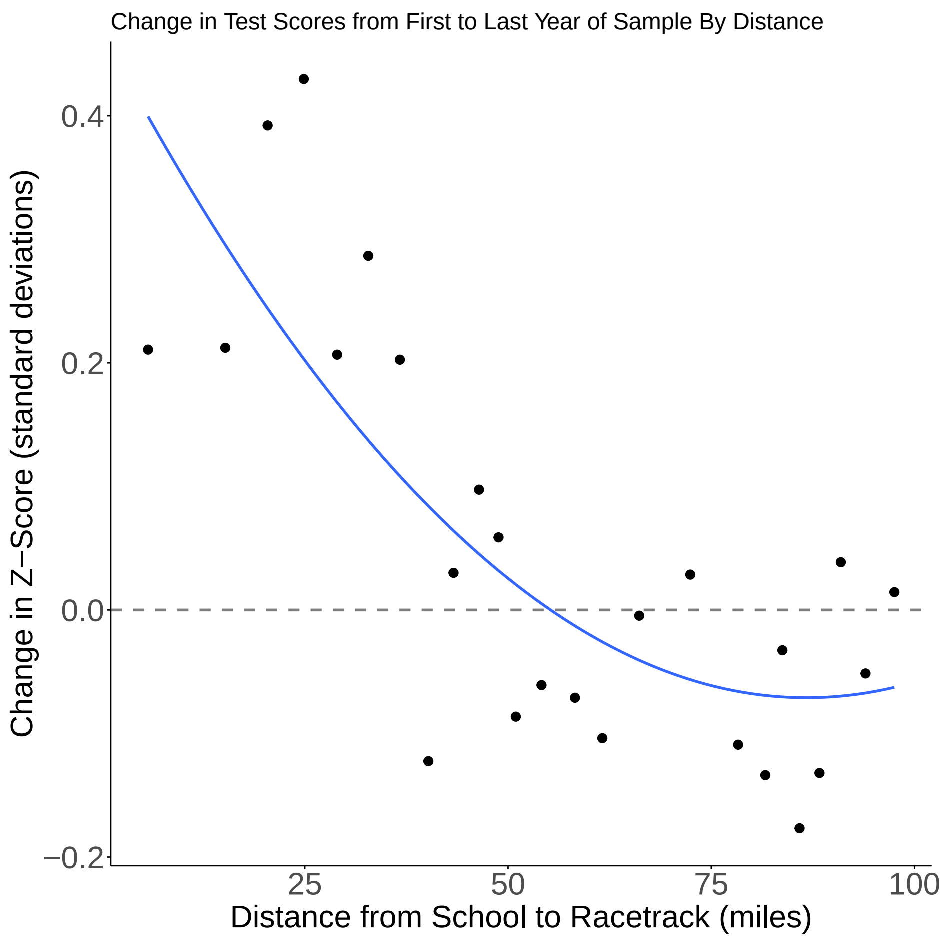
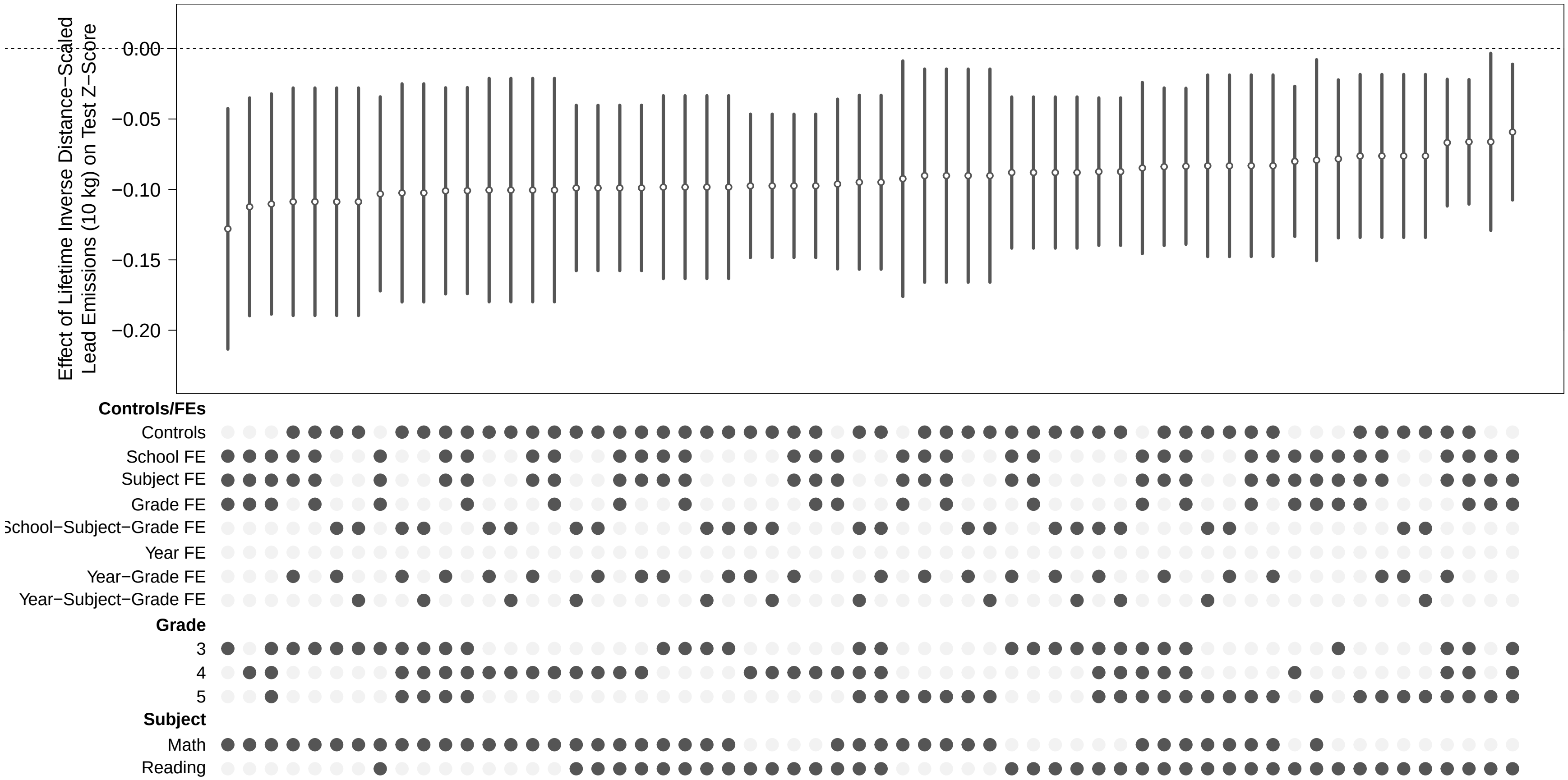
Figure A4: Change in test scores by distance.



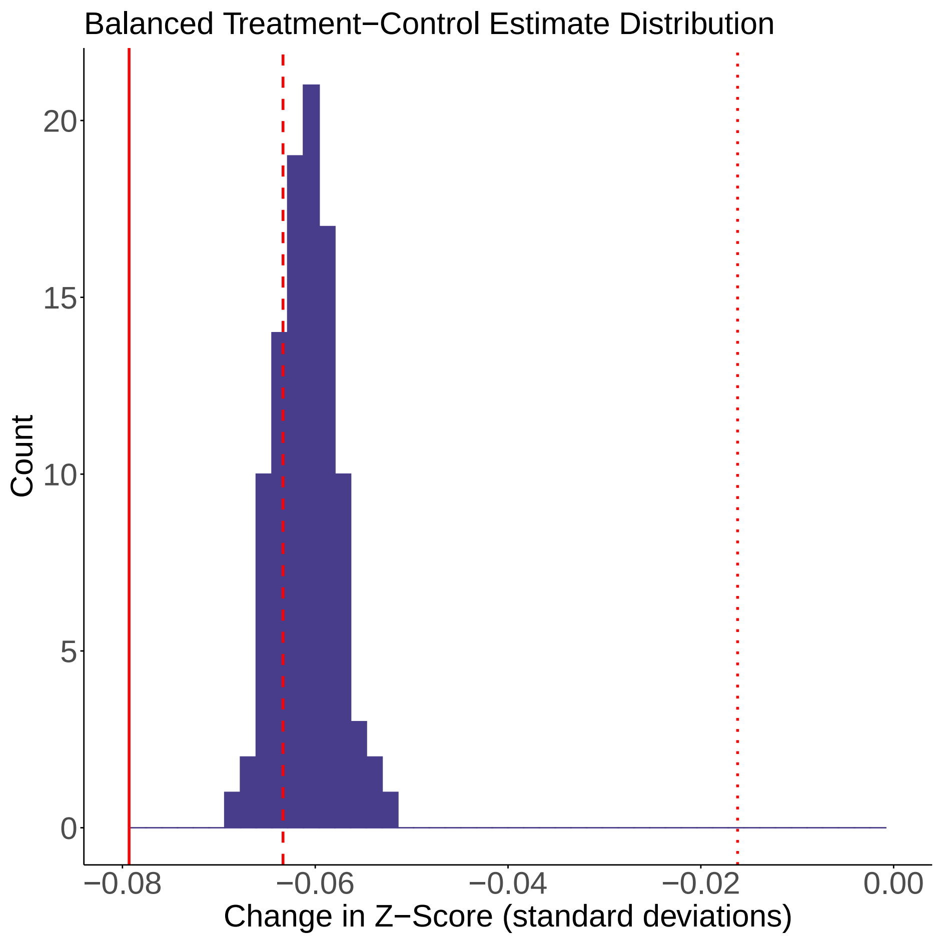
The points are generated from a 25 point binscatter scheme where each point averages scores and distance within each 4 percentile distance bin. The blue line plots the best fit quadratic line.

Figure A5: Sample and fixed effects subsets for the effect of 10 kg of lead emissions on school z-scores.



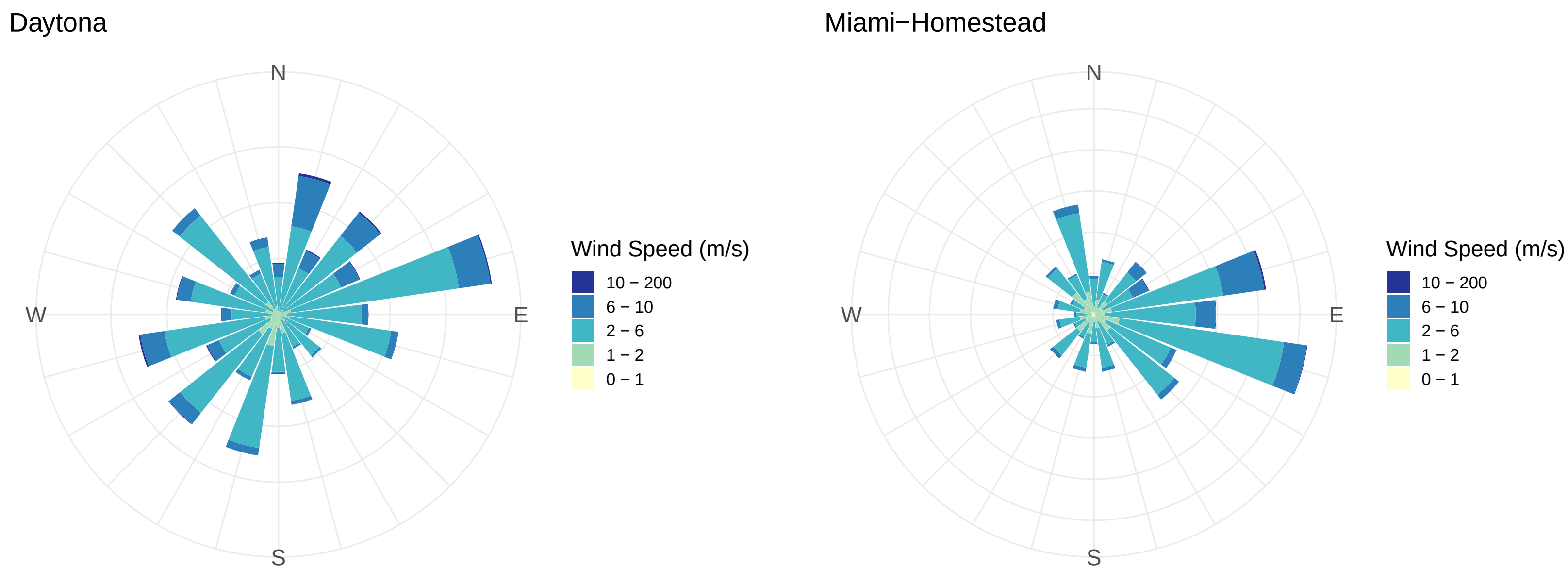
Note: The points are the point estimates from separate specifications. The bars are the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. Estimates are ordered by their magnitude.

Figure A6: Distribution of estimates with randomly sampled control schools with a balanced set of treated and control schools.



The solid line is our main result estimate. The dotted line is the upper bound of the 95% confidence interval for our main result estimate. The dashed line is the estimated effect on the subsample of schools within 100 miles of a racetrack we use for resampling. The blue bars are the distribution of the 100 resampled estimates when drawing 645 control schools 50–100 miles from a racetrack without replacement to match the 645 treatment schools. All estimates are from equation (2).

Figure A7: Distribution of wind direction and speed at each racetrack.



Note: The windroses show the distribution of daily average wind direction. The plots show where the wind is blowing from, not where the wind is blowing toward. Darker colors indicate higher speed winds.

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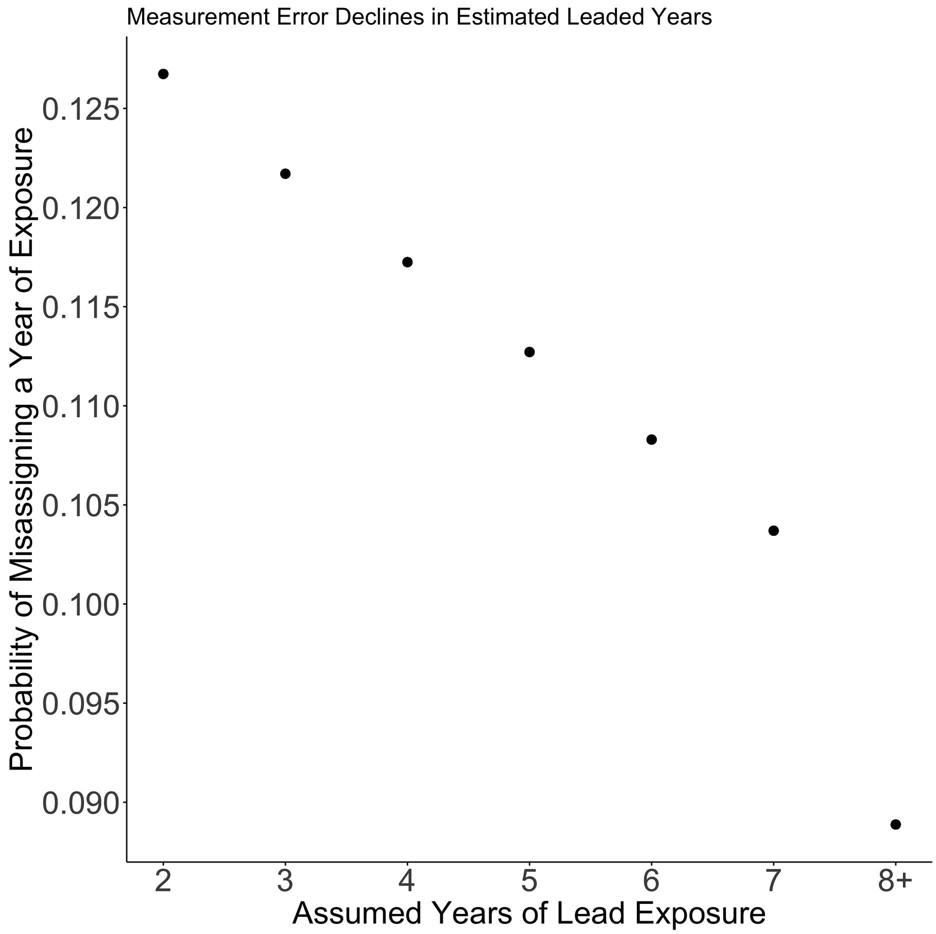
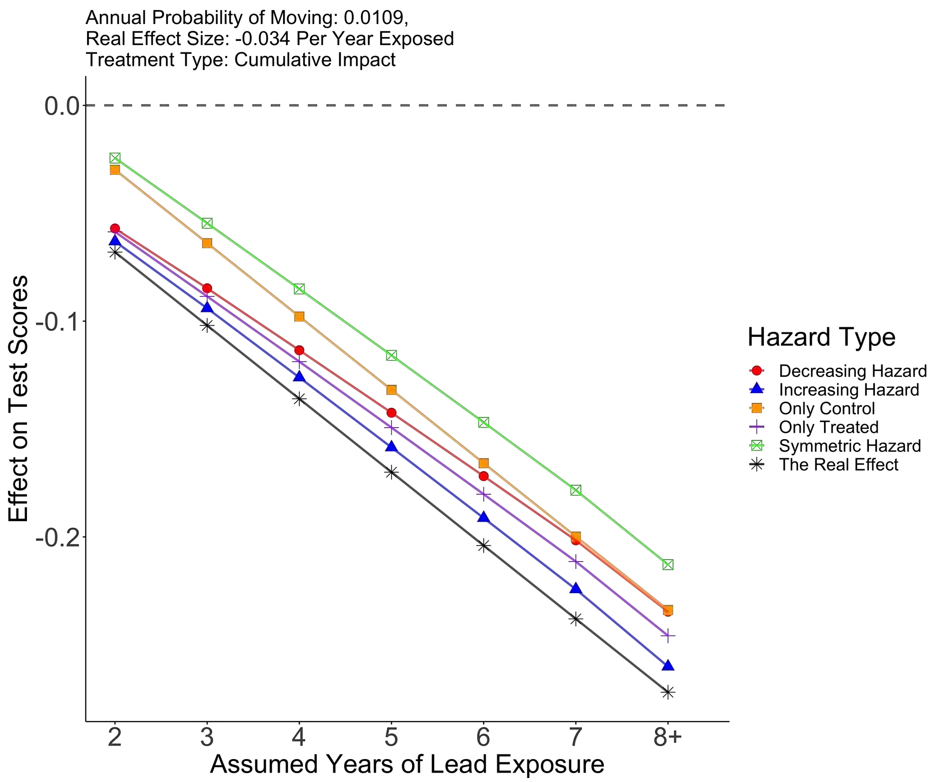
Figure A8: Measurement error decreases in years of exposure.



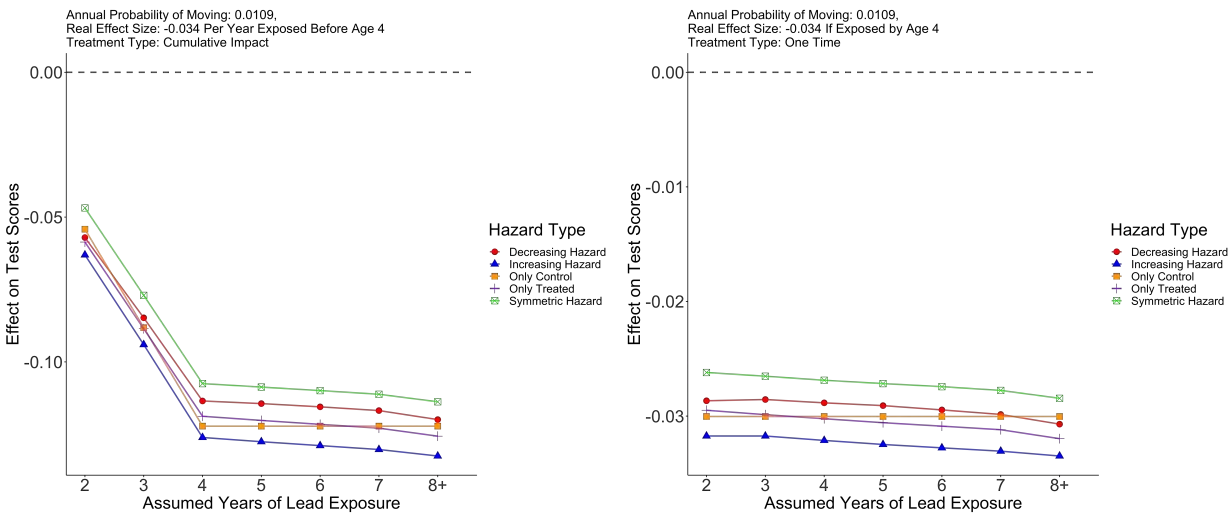
Note: Each point is the plots the average of the simulated measurement error using our rule that assumes students never move. This is the difference in the actual years of exposure and the assigned years of exposure divided by the assigned years of exposure. This gives the average share of years that we mismeasured.

Figure A9: Estimated effect of years exposed when the true test score data generating process has cumulative effects of years exposed.



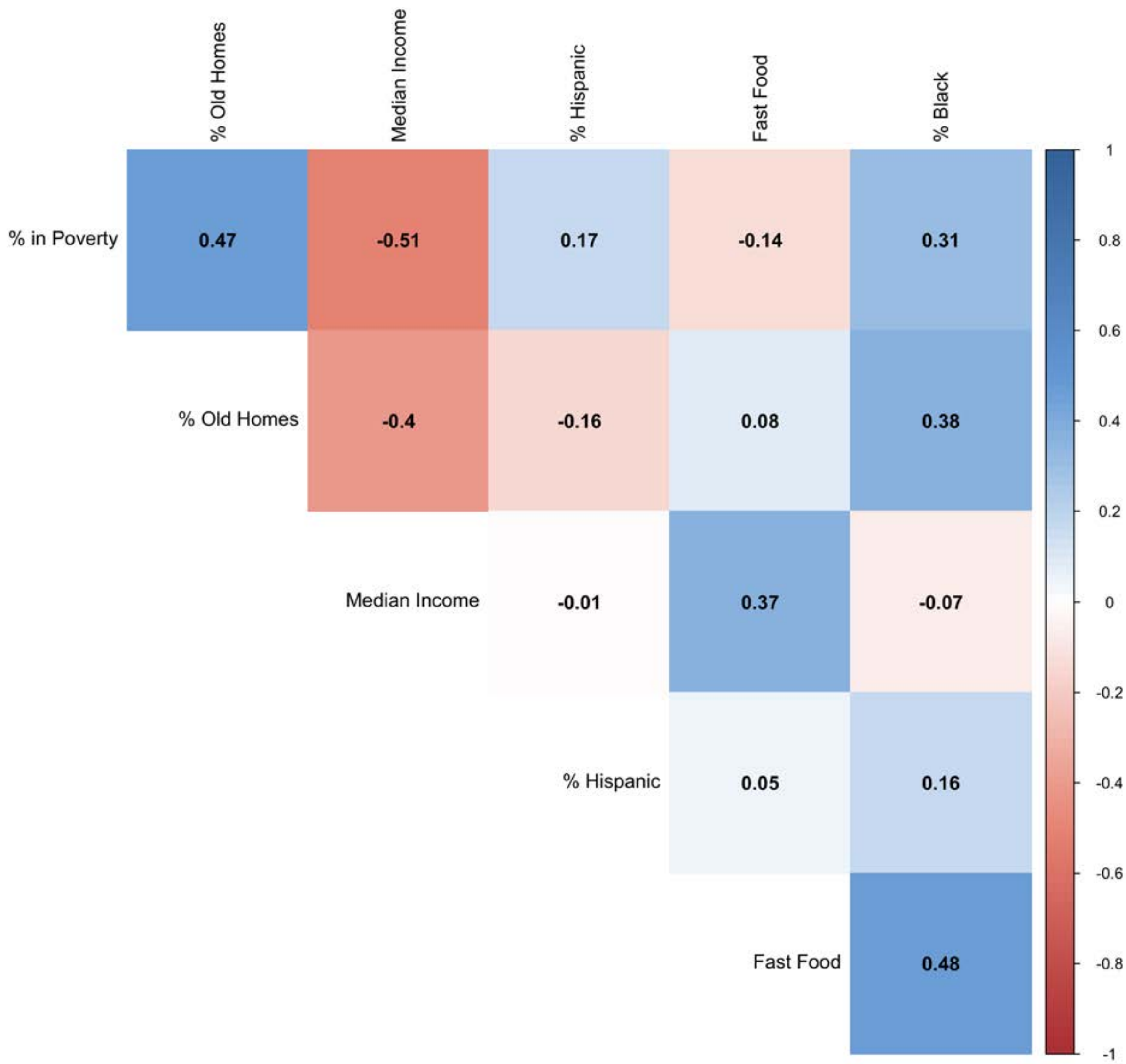
Note: Each set of connected lines is from estimating the same specification as in Figure 6, but where the simulated data have different kinds of actual moving probabilities for students across treated and control schools. The true effect is a 0.034 standard deviation decline in test scores for each year exposed. The estimates in the black stars correspond to when our assumption that students never move is correct. The remaining estimates are when we have misspecified the model.

Figure A10: Estimated effect of years exposed when the true test score DGP only depends on early life exposure.



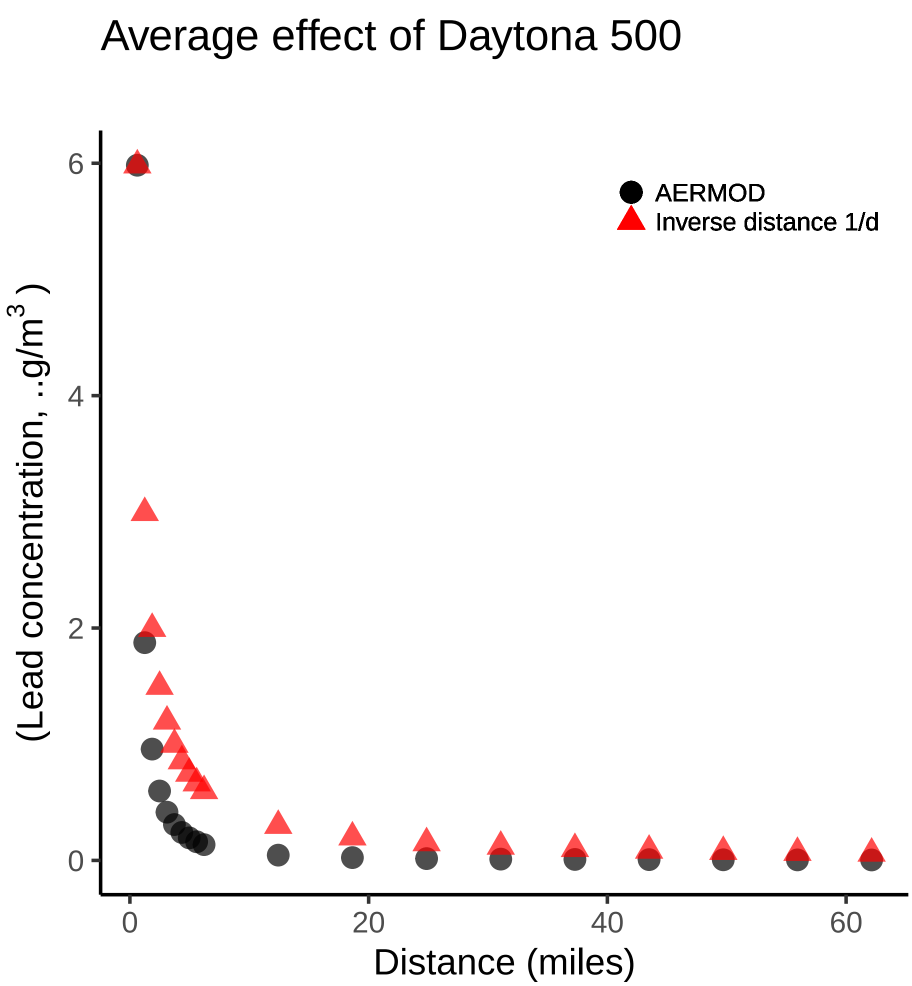
Note: Each set of connected lines is from estimating the same specification as in Figure 6, but where the simulated data have different kinds of actual moving probabilities for students across treated and control schools. In the left panel the true effect is a 0.034 standard deviation decline in test scores for each year exposed prior to age 4. In the right panel the true effect is a 0.034 standard deviation decline in test scores if the student was exposed in at least one year prior to age 4. This figure serves to test whether alternative data generating processes for test scores can generate our downward sloping curve in Figure 6 if there is measurement error in exposure driven by students moving schools.

Figure A11: Pairwise correlations between each pair of variables interacted with treatment in Figure 8.



Note: The numbers are the correlation coefficients. Data sources are outlined in Section 1.

Figure A12: Inverse distance-scaling closely tracks predictions using AERMOD.



Notes: AERMOD simulation is calibrated using an emissions rate of 2.83 grams of lead per second (average lead per second of the Daytona 500), a release height of 1.14 meters (average height of a NASCAR vehicle, 1.345 meters multiplied by 0.79), initial horizontal sigma of 4.22, and initial vertical sigma of 1.06. These factors were calculated following recommendations by 2009 Regional/State/Local Modeling Workshop. (2011) for using AERMOD for vehicle emissions. See Arizona Department of Environmental Quality (2013) for additional details. Location of initial release is the Daytona International Speedway. Meteorologic data in AERMET format is from the Florida Department of Environmental Protection.

Table A8: Comparing changes in elevated blood lead levels and schooling effects.

	% Elevated BLL, (μg/dl $\geq$ 10)	BLL (μg/dl)	Test Score (SD)
	(1)	(2)	(3)
1(Leaded race in county-year)	0.157**		
	(0.066)		
Same year lead emissions (10kg)		0.032**	
		(0.013)	
Lifetime lead emissions (10 kg)			-0.003^{*}
			(0.002)
Controls	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	No
Year FE	No	No	Yes
School FE	No	No	Yes
Observations	22,887	22,887	137,761

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are clustered at the county/school district level. School-subject-grade-year observations are weighted by number of students, BLL regressions are weighted by number of tested children. The first column replicates Hollingsworth and Rudik (2021) Table A8, column 4, but with a few more observations and only presenting the estimated coefficient of interest. This specification shows that having a leaded race in a given county is associated with a 15.7% increase in the prevalence of children with elevated levels of blood lead ($\geq 10\mu g/dl$). The second column estimates a lower bound on the average change in blood lead levels for children living in the race county. The analysis relies on the fact that in this sample the average county-year with a leaded race saw just under 50kg of lead emitted in each each and follows Section 5.1 from Hollingsworth and Rudik (2021) to convert this to BLL. The third column replicates Table A4 column 1, Panel A for comparison.