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:

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

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.

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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

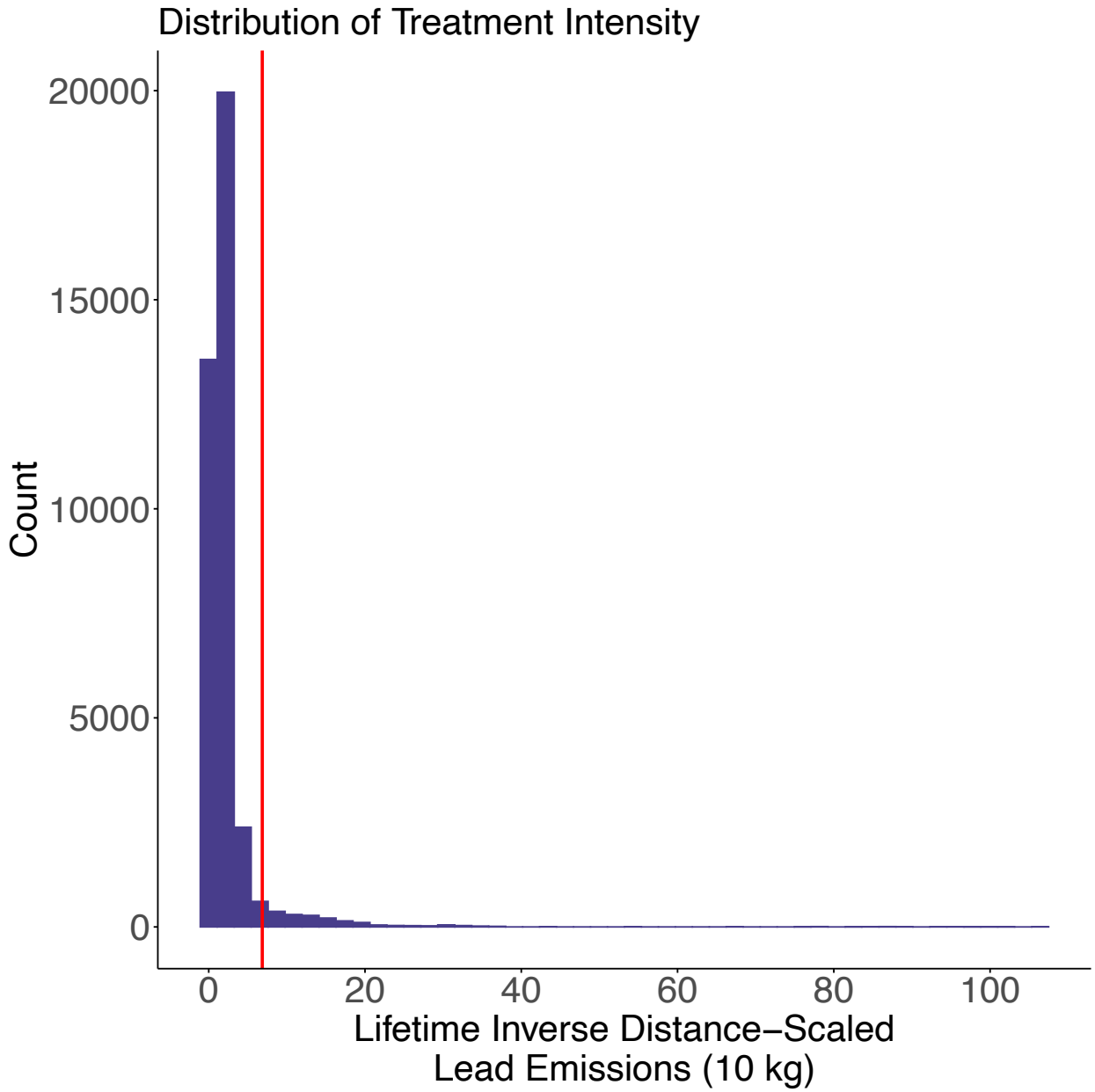
Note: An observation is a school-grade-subject-year. There are 645 treated schools and 1685 untreated schools.

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

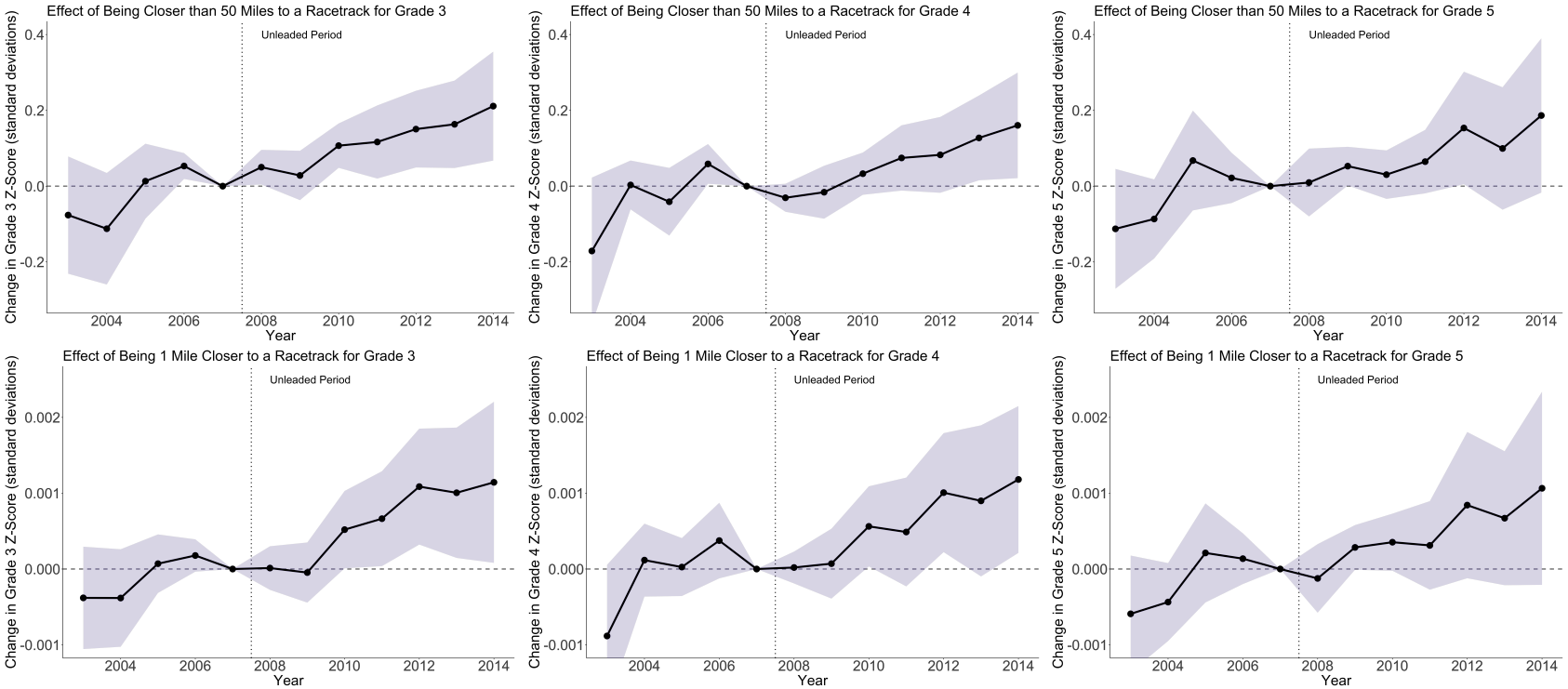
Note: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors are clustered at the county/school district level. School-subject-grade-year observations are weighted by number of students, BLL regressions are weighted by number of tested children. The first column replicates Hollingsworth and Rudik (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.

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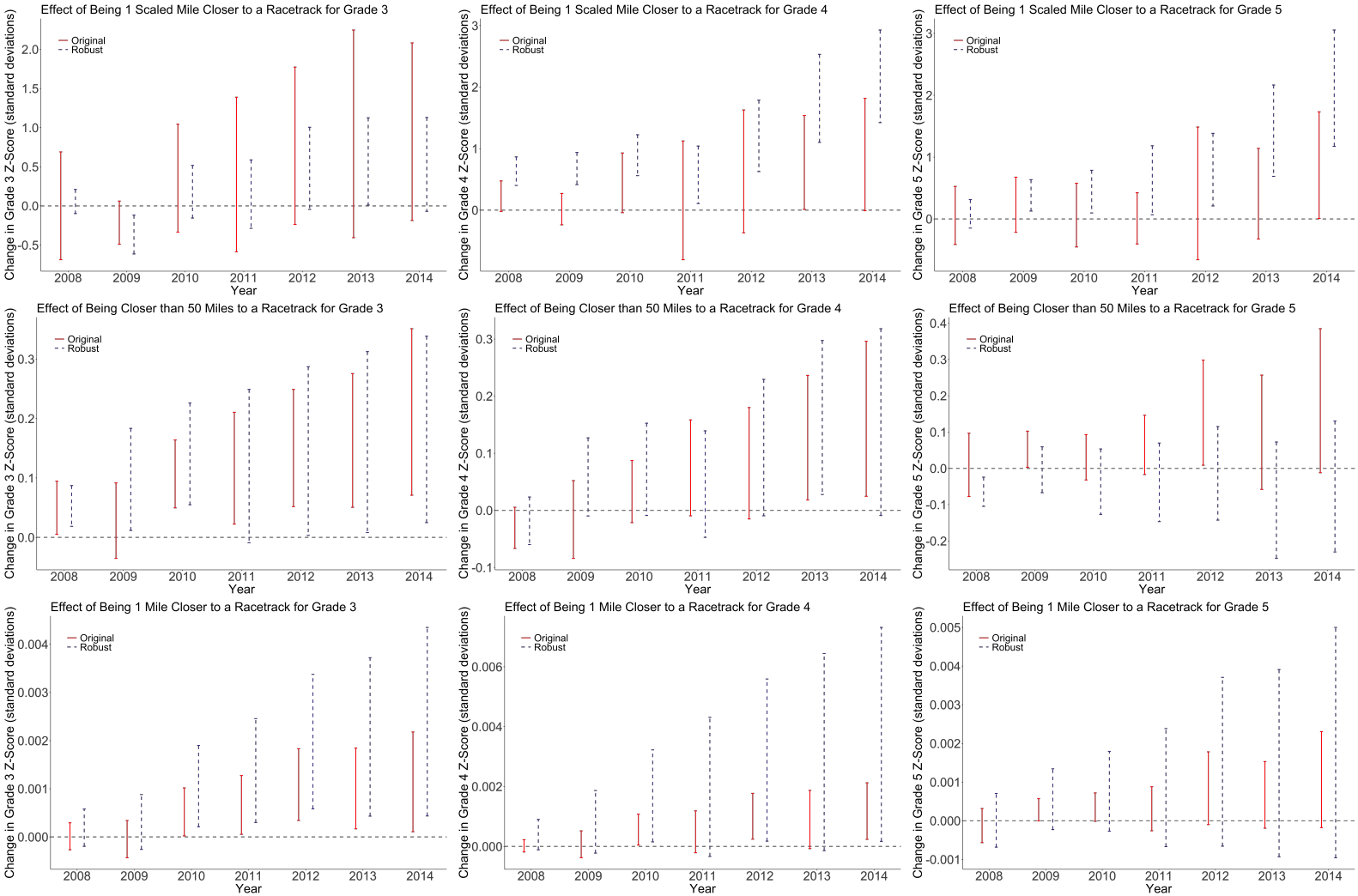
Figure A3: Event studies of the effect of being one mile closer to a racetrack allowing for linear trend violations.



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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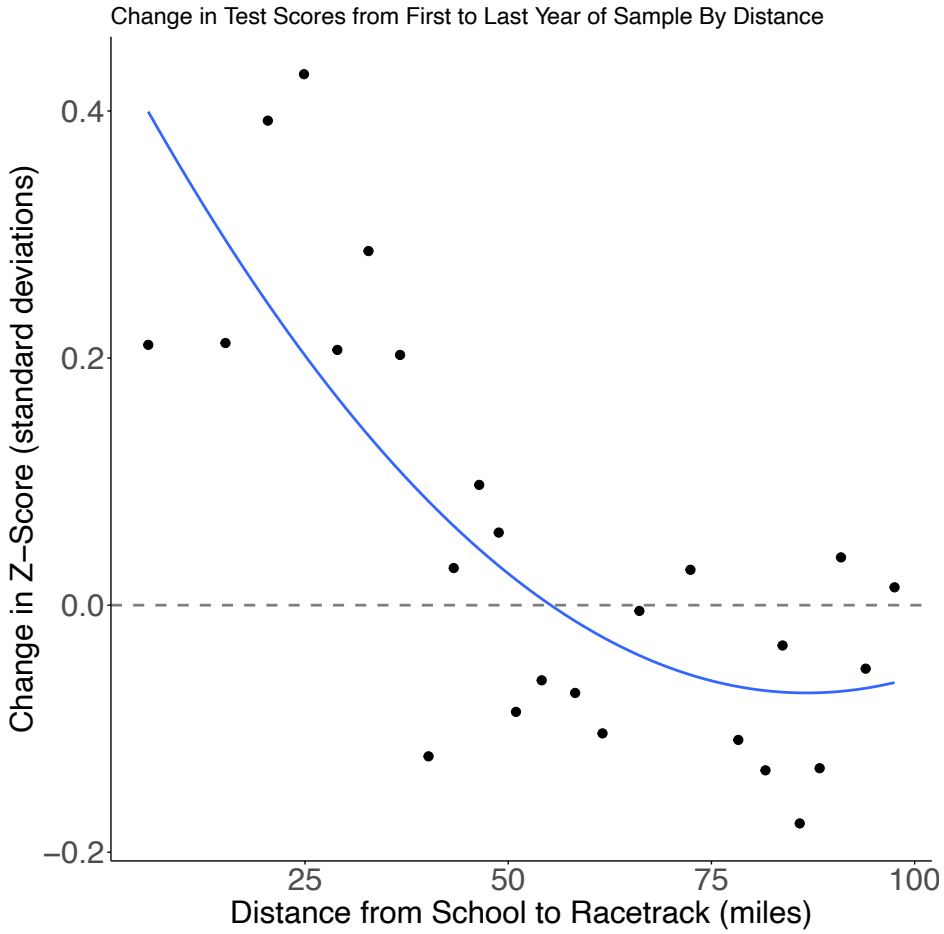
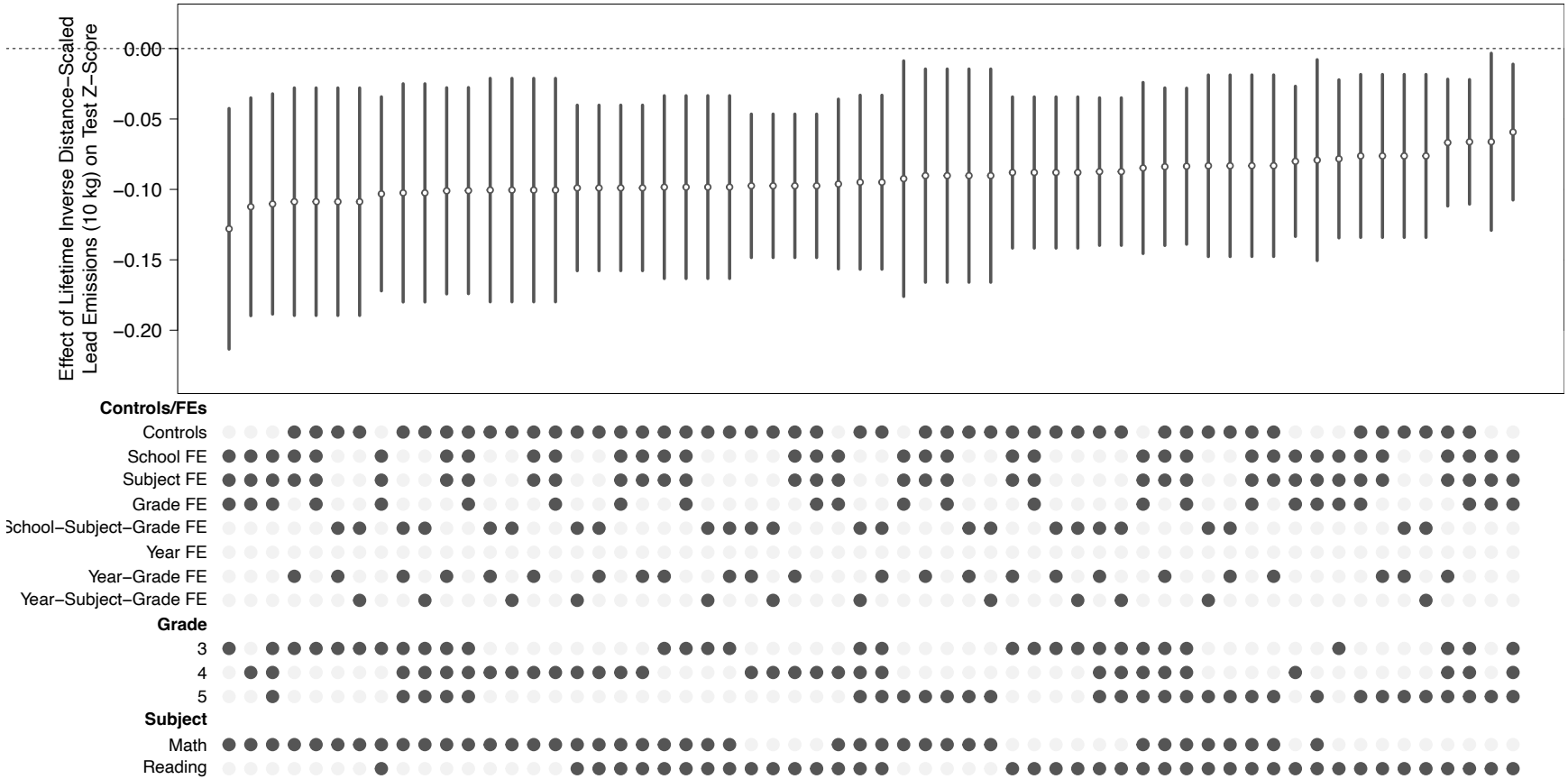
Figure A4: Change in test scores by distance.



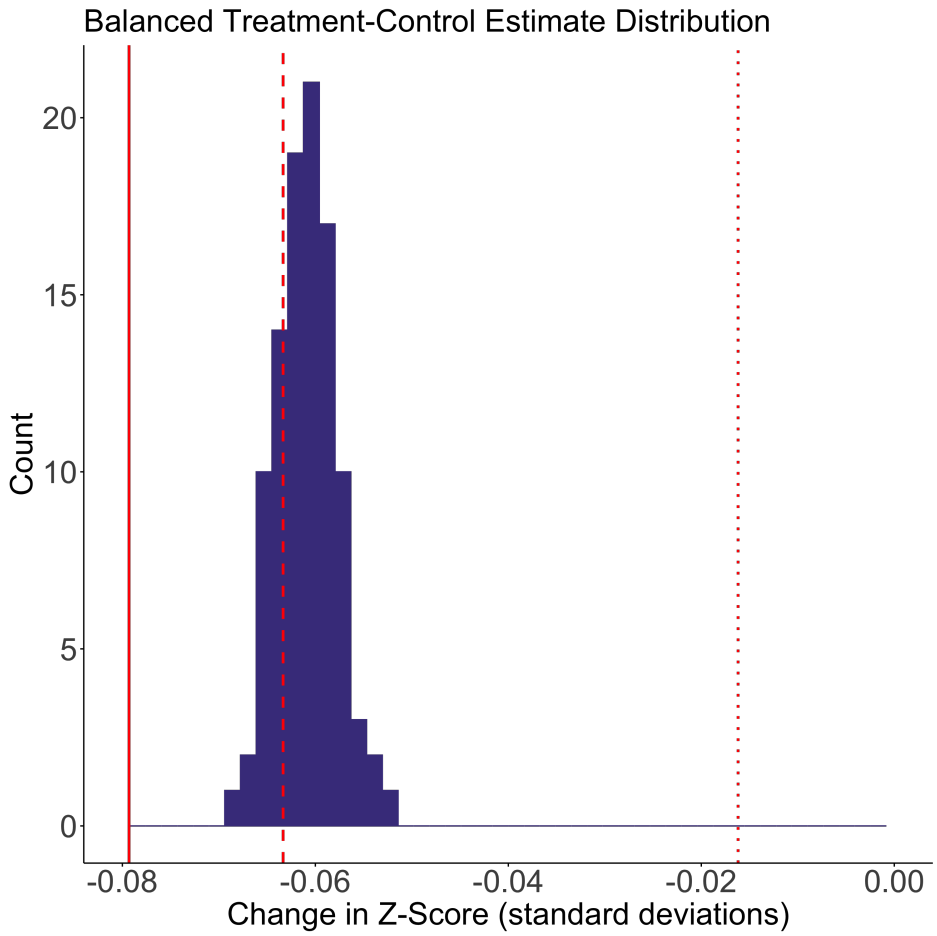
The points are generated from a 25 point binscatter scheme where each point averages scores and distance within each 4 percentile distance bin. The blue line plots the best fit quadratic line.

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



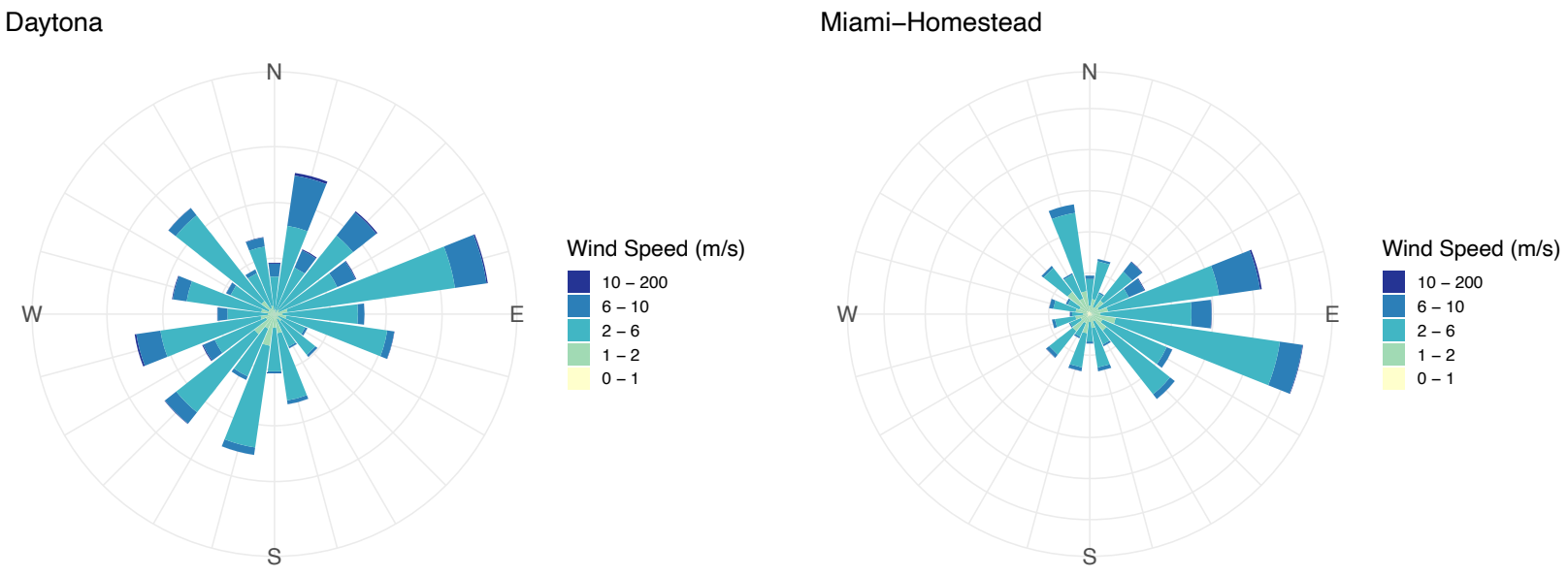
Note: The points are the point estimates from separate specifications. The bars are the 95% confidence interval computed from robust standard errors clustered at the school district level. School-subject-grade-year observations are weighted by number of students. Estimates are ordered by their magnitude.

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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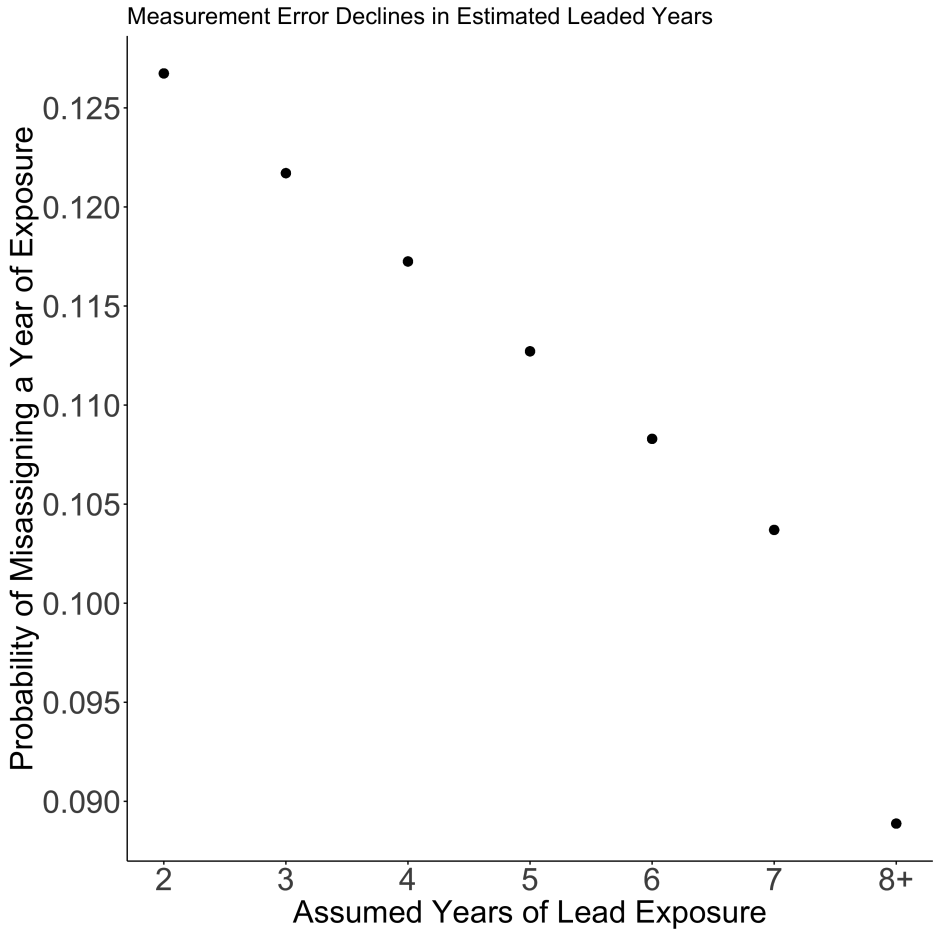
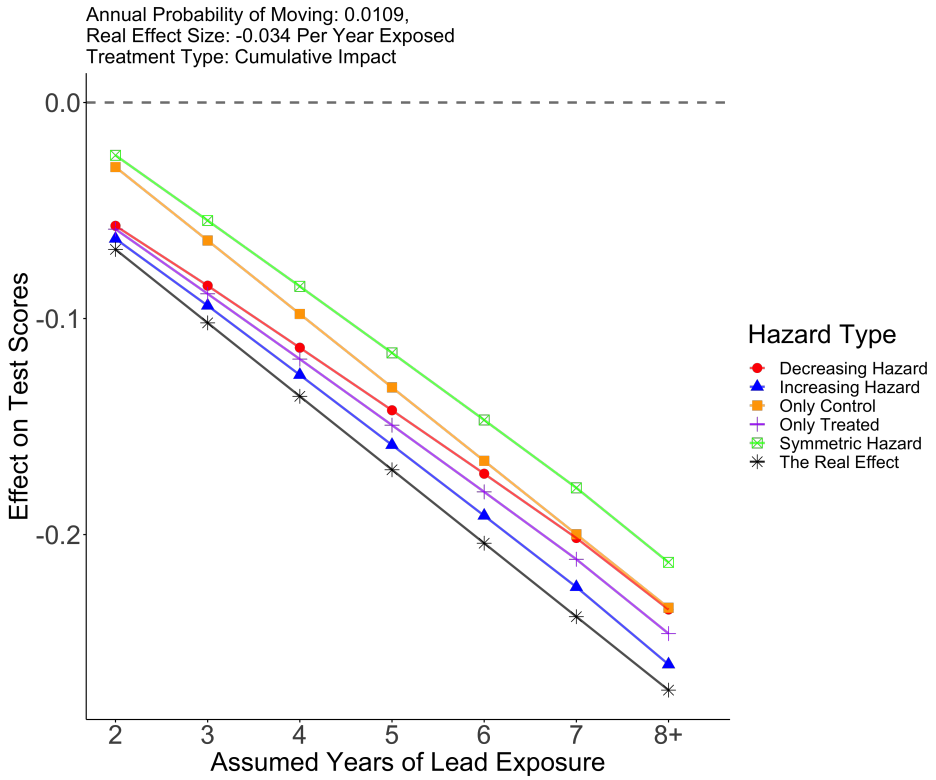
Figure A8: Measurement error decreases in years of exposure.



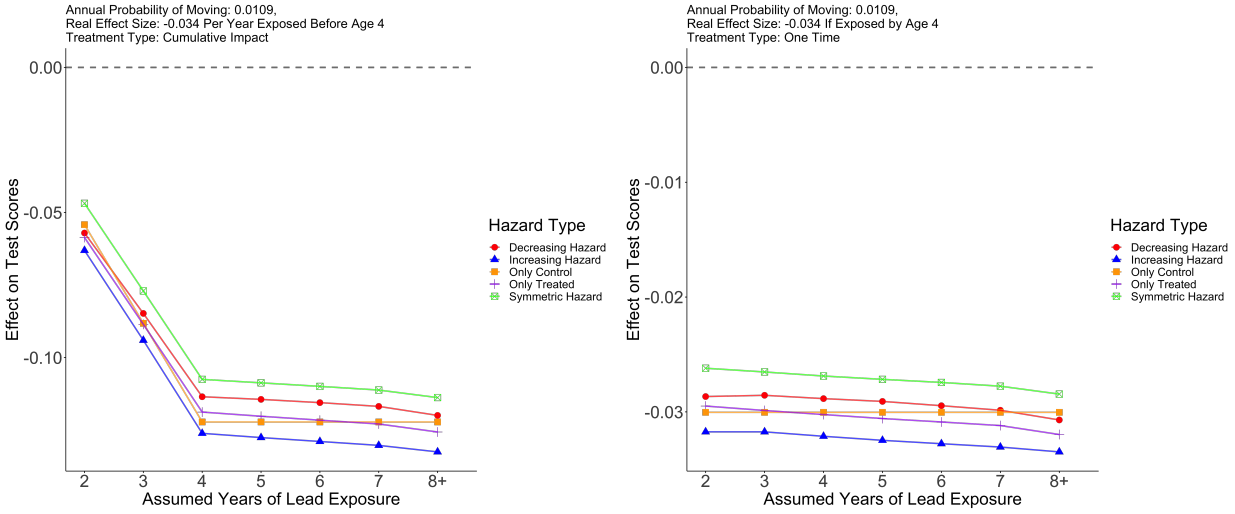
Note: Each point is the plots the average of the simulated measurement error using our rule that assumes students never move. This is the difference in the actual years of exposure and the assigned years of exposure divided by the assigned years of exposure. This gives the average share of years that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

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



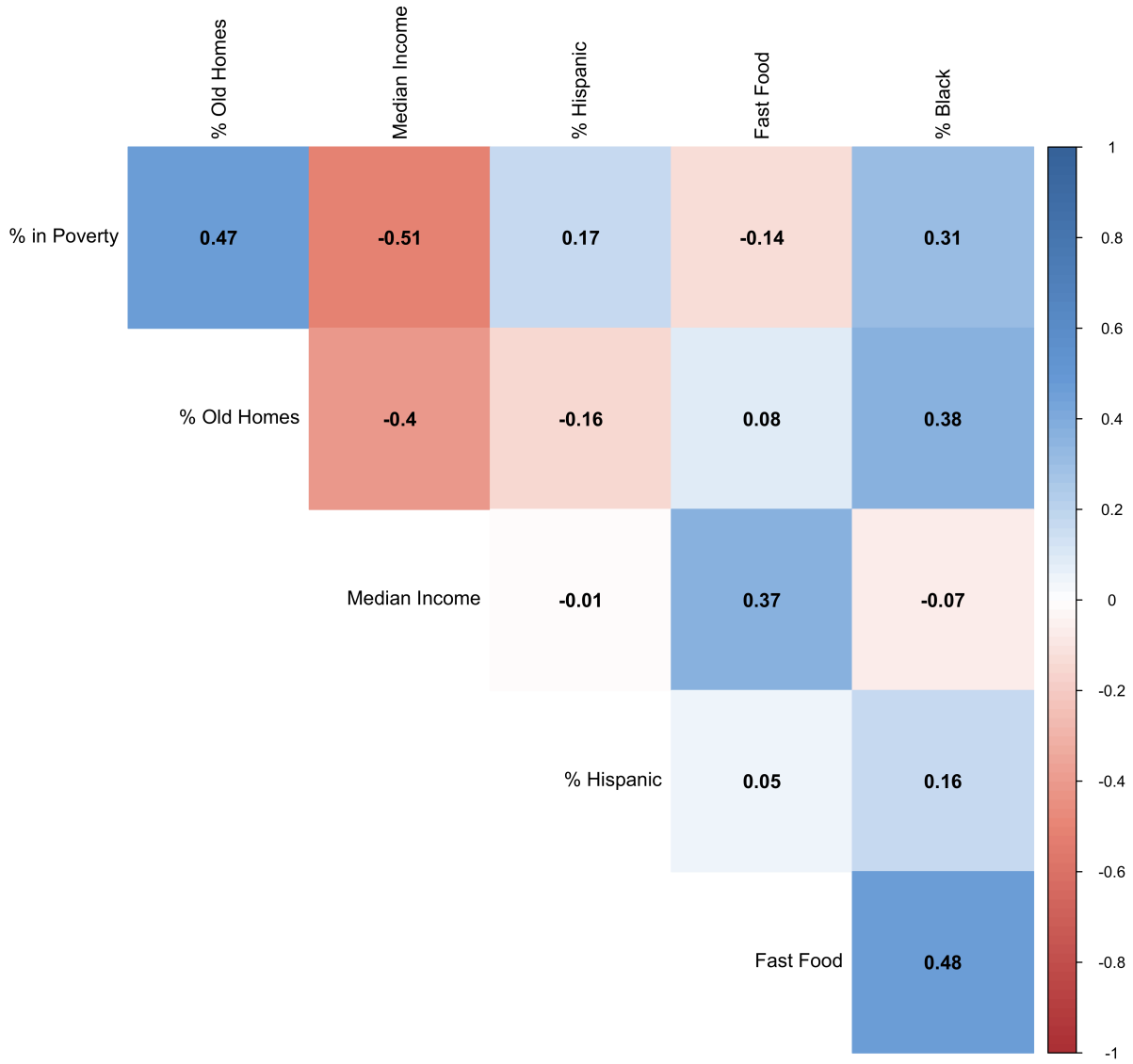
Note: Each set of connected lines is from estimating the same specification as in Figure 6, but where the simulated data have different kinds of actual moving probabilities for students across treated and control schools. The true effect is a 0.034 standard deviation decline in test scores for each year 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.



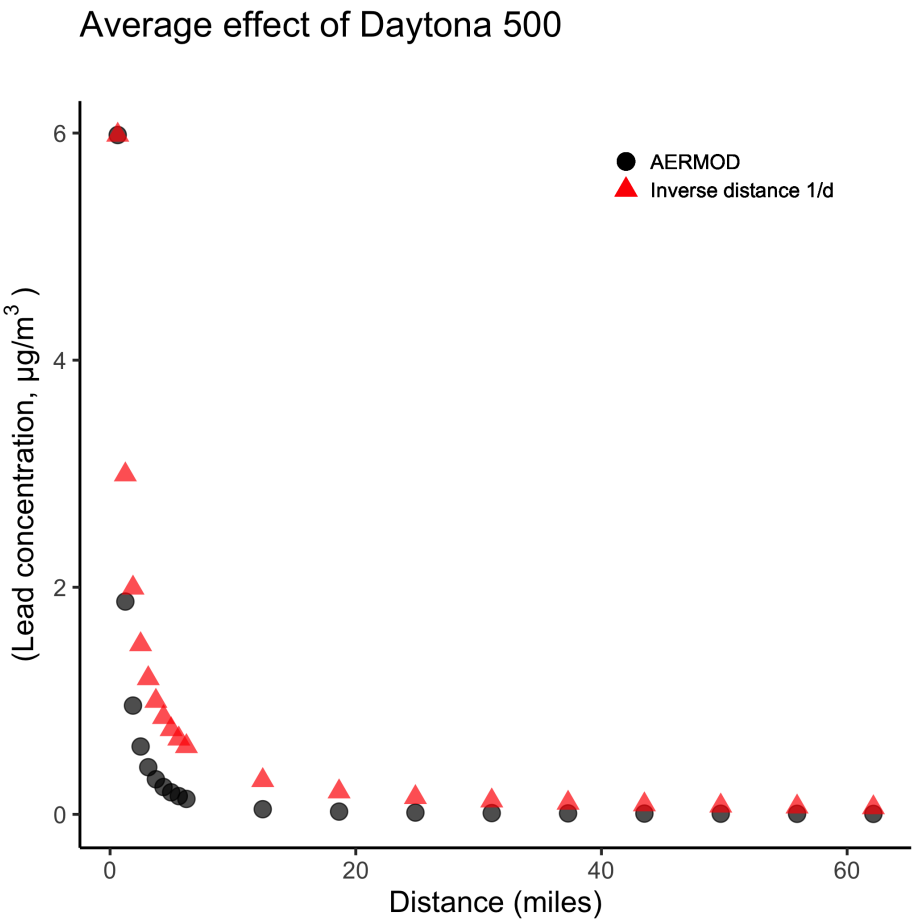
Note: Each set of connected lines is from estimating the same specification as in Figure 6, but where the simulated data have different kinds of actual moving probabilities for students across treated and control schools. In the left panel the true effect is a 0.034 standard deviation decline in test 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.