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Heissel, Jennifer A.; Persico, Claudia; Simon, David

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Does Pollution Drive Achievement? The Effect of Traffic Pollution on Academic Performance

Jennifer A. Heissel, Claudia Persico, and David Simon

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We examine the effect of traffic pollution on student outcomes by leveraging variation in wind patterns for schools the same distance from major highways. We compare within-student changes in achievement for students transitioning between schools near highways, where one school has greater levels of pollution because it is downwind of a highway. As students graduate from elementary/middle school to middle/high school, their test scores decrease, behavioral incidents increase, and absence rates increase when they attend a downwind school, relative to when they attend an upwind school in the same zip code. Even within zip codes, microclimates can contribute to inequality.

JEL Codes: I10, I21

Dr. Jennifer A. Heissel is an assistant professor of manpower and economics at the Naval Postgraduate School. Dr. Claudia Persico is an assistant professor in public administration and policy at American University and a research affiliate at the IZA Institute of Labor Economics; she can be reached at cpersico@american.edu. Dr. David Simon is an assistant professor of economics at the University of Connecticut and a faculty research fellow at the National Bureau of Economic Research. The authors are grateful to the state of Florida for providing the data used in this analysis. We thank David Figlio, Hilary Hoynes, Richard Dunn, Nick Sanders, Doug Miller, workshop participants at the University of Connecticut Agricultural Economics Department, and conference participants at the Association for Public Policy Analysis and Management, the UC Davis Alumni conference, and the Southern Economic Association for their thoughts and comments on this work. Any errors or conclusions are their own. The views expressed in this paper do not reflect the views of the U.S. Department of Defense, the U.S. Navy, or the Florida Department of Education. Sandra Spirovska, Colin Rohm, Joanna Venator, Michael DiNardi, and Colin Mortimer provided excellent research assistance.

The Florida school data is under contract and is not available for sharing with the public. The authors will share .do files and a readme file for replication purposes.

I. Introduction

Over 6.4 million children attend public school within 250 meters of a major roadway (Kingsley et al. 2014), and nearly one in five schools that opened in the 2014–2015 school year were built near a busy road (Hopkins 2017). Proximity to highways may make the land cheaper, but school districts and parents are often unaware of the health risks of highway pollution. Understanding the impact of traffic pollution in schools is critically relevant for social policy. Influences on academic achievement are largely absent from Environmental Protection Agency (EPA) estimates of the social costs of pollution, perhaps because relatively little research examines how pollution exposure over primary and secondary school influences human capital accumulation.

We build on earlier work by estimating the impact of attending a school with higher ambient pollution levels on the academic and behavioral outcomes of public school students. We use a novel identification strategy that leverages variation in pollution exposure caused by movement through the Florida school system as students transition from elementary to middle school or middle school to high school. We compare achievement in students moving between schools near highways, where one school has had greater levels of pollution because it is downwind of a highway, in models with zip code, grade, year, and student fixed effects.

Recent evidence demonstrates that even mild health shocks during gestation and early life can substantially affect long-term human capital outcomes (Almond, Edlund, and Palme 2009; Bharadwaj et al. 2017; Black et al. 2019; Currie, Greenstone, and Moretti 2011; Persico, Figlio, and Roth 2016; Sanders 2012), but we know much less about how pollution exposure between early life and adulthood affects human capital formation and child development. There are a few studies that document how acute, short-term exposure to air pollution on testing days affects test score performance. Marcotte (2017) used variation in air quality on different testing days and found that children who took tests on worse days for pollen and fine airborne particulate matter had worse test outcomes. Similarly, Roth (2016) found that pollution on testing days affected college students' performance in the United Kingdom, and Ebenstein, Lavy, and Roth (2016) found that pollution affected performance on high school exit exams in Israel. However, the present paper is the first to examine year-to-year exposure to pollution.

To implement our natural experiment, we use a unique administrative dataset on the universe of public school students born in Florida from 1992–2002. We follow students over time, observing rich information on their behavioral, demographic, and academic characteristics. We find that attending school where prevailing winds place it downwind of a nearby highway more than 60% of the time is associated with 3.98% of a standard deviation lower test scores, a 4.09 percentage point increase in behavioral incidents, and a 0.53 percentage point increase in the rate of absences over the school year, compared to attending a school upwind of a highway the same distance away. Given the size and diversity of the state of Florida, we can also examine these impacts by race, socioeconomic status, and gender.

Our research design contrasts with earlier papers that only examined either the influence of long-term in-utero exposure on test scores or the direct short-term effect of "day of test" exposure. Along with Persico and Venator (2019), we are one of the first papers to estimate the impact of medium-term, year-to-year variation in pollution exposure on child achievement throughout childhood, and the first to do so using policy-generated moves through a school system as an identification strategy. Finally, we are the first paper to look at the causal impact on achievement of school districts locating schools downwind of major highways. Such policies expose students to higher levels of pollution, and we therefore shed light on policy implications related to school location decisions.[1]

These contributions are relevant to broader discussions in the fields of the economic analysis of children, inequality, and health. As Almond, Currie, and Duque (2018) point out in their literature review, even mild health shocks in early life can lead to substantial long-term negative outcomes, but we know substantially less about the impacts of exposure to shocks in the intervening period between early life and adulthood. Likewise, most studies on pollution use larger geographic areas than the zip code level. Recent research suggests that there is significant within-commuting zone variation in intergenerational inequality (Rothstein 2019), and small geographic variations in childhood pollution exposure could be one factor behind this pattern.

II. Background

A growing literature has linked pollution to asthma attacks (Simeonova et al. 2019), bronchitis (Beatty and Shimshack 2011), and mortality (Chay and Greenstone 2003; Currie and Neidell 2005; Deryugina et al. 2016; Knittel, Miller, and Sanders 2015). Much of this literature focuses on either the very young or the elderly. Younger populations are potentially of interest because investments in child health could result in greater later-life productivity. While we know that early life health shocks can substantially affect long-term outcomes, we know very little about the impacts of exposure to health shocks in the years between early life and adulthood. A few exceptions include Aizer, Currie, Simon, and Vivier (2018), who find that early exposure to lead in preschool affects later test scores; Persico and Venator (2019), who find that being near

[1] This work likewise complements a descriptive literature in sociology and environmental health that finds a negative correlation between the distance of schools to sources of pollution and school performance. See London et al. (2016) for a comprehensive review.

an industrial plant harms tests scores and increases suspensions; and Simon (2016), who finds that early life exposure to cigarette smoke harms childhood health.

A few economists have focused on the effects of exposure to wind-based traffic pollution. Herrnstadt and Muehlegger (2015) argue that traffic pollution influences impulse control. They showed that short-term hourly variation in wind direction in Chicago lead to higher crime in areas downwind of highways than on the opposite upwind side. In a study similar to ours, Anderson (2015) documented that long-term exposure to being downwind of a highway was associated with higher mortality rates among the elderly, though housing characteristics were similar on either side of the road.

Pollution shocks during the school year could impede human capital formation through several channels. First, health effects may reduce attendance. Currie et al. (2009) found that high levels of carbon monoxide were associated with reduced school attendance under a difference-in-differences strategy accounting for persistent school and year effects. Ransom and Pope (1992) similarly found a relationship between pollution and school attendance, with more small particulate matter in the air associated with more absences. Second, exposure during periods of brain development could affect children in ways that persist even after the child is removed from a high-pollution environment. Finally, pollution could cause short-term cognitive and health disruptions in either children or teachers during the school day that affect performance and accumulate over the course of the school year. There is growing evidence in the economic literature that pollution has short-term impacts on cognition, productivity, and behavior. Chang et al. (2016; 2019) used hourly variation to show that increased exposure to fine particulate matter decreases productivity per hour of pear packers and call center workers, while Archsmith, Heyes, and Saberian (2018) showed that baseball umpires make more mistakes on days with higher pollution.

School districts and parents are often unaware of the risks of highway pollution, particularly because there are not many studies of the effects of traffic pollution on childhood health and achievement. In addition to examining the causal relationship between these factors, this paper presents a timely evaluation of the effects of locating schools near highways.

III. Identification Strategy

Naïve correlations between air pollution and academic outcomes cannot be interpreted as causal because pollution is not randomly assigned. In particular, students could sort into schools, such that pollution is associated with poor performance, but does not cause poor performance. However, there are many times where, within the same school district, students (or their parents) do not choose to move schools, but they instead must move into a new school due to district policy. A student who graduates from elementary school (in grade 5) to middle school (in grade 6) will switch school locations. Without necessarily moving to a new home, some of these students move from an upwind to a downwind school, some move from a downwind to an upwind school, and some move within the same category.

Our identification strategy follows children over time, comparing their outcomes as they transition from elementary/middle school to middle/high school when both schools are near a highway, but where some schools are upwind and others are downwind of the highway. Using zip code fixed effects, we identify the impact of small moves between geographically proximate schools near the same highway. By either applying first-differences or student fixed effects models, we likewise identify within-student changes over time. With this in mind, we think of our strategies as a type of difference-in-differences approach: we compare within-child differences in an outcome before versus after changing schools for children who attend a downwind relative to an upwind school in the same zip code.

To focus on moves to middle/high school we limit the sample to those students who attend elementary school in fifth grade before moving to a middle school the next year in sixth grade or those who attend a middle school in eighth grade and move to a high school the next year in ninth grade. To motivate our use of panel data, imagine the case where we did not follow students over time. In such circumstances, we could consider the following conceptual cross-sectional model:

$$(1)\quad Y_{isjgt} = \beta_0 + \beta_1 Downwind_{isjgt} + X_{it}\rho + S_{st}\omega + D_s\gamma + \theta_i + \tau_g + \delta_j + \sigma_t + \varepsilon_{isjgt}$$

where Y_{isjgt} is test scores for student *i*, attending school *s*, located in zip code *j*, in grade *g* and in year *t*. Because this is a case of repeated cross sections, θ_i is unobserved (and time invariant) student ability. To adjust for fixed differences in test scores between grades, we use grade fixed effects τ_g. Due to our sample restrictions, a student's grade is either 5, 6, 8 or 9. We adjust for changes over time using year fixed effects σ_t and for time-invariant characteristics of zip codes using fixed effect δ_j. The vector S_{st} controls for time-varying school characteristics.[2] The vector D_s controls for two time-invariant school characteristics related to location: a vector of 0.1-mile-bin distance dummies measured from the nearest highway (0–0.1 miles, 0.1–0.2 miles, 0.2–0.3 miles, and 0.3–0.4 miles) and a linear control for number of highways within a one-mile radius.[3]

[2] The time-varying controls we use include percent Black, percent Hispanic, average maternal education, percent of children from married families, percent of teachers with a master's degree by school; school size in hundreds of students; and the school's stability rate, where the stability rate is defined as the percentage of students in October who are still present in the February membership count.

[3] The count of roads provides an estimate of the density of roads in the area. Using the one-mile distance balances between not being collinear with number of nearest roads in our "downwind" model, while still capturing urban density.

The vector X_{it} controls for time-varying individual characteristics: free-and reduced-price lunch (FRL) status and an indicator for whether the student moved to a new school that year. Schools that are downwind more than 60% of the time are defined as "downwind" through a binary indicator. All other schools where $Downwind$ = 0 are referred to throughout as "upwind." Our key coefficient (β_1) therefore estimates the effect of attending a downwind school relative to an upwind school in the same zip code, independent of the effect of moving.

A problem with such a repeated cross-sectional model is that Estimates of $\beta_1$will suffer from omitted variable bias if unobserved student ability (θ_i) is correlated with within zip-code sorting into schools. Therefore, rather than employing a cross-sectional model, we take two approaches to estimate within-student changes. In both approaches, we interpret the coefficient β_1 as the differential change in test scores for a student who moves to a middle (high) school with a different prevailing wind direction relative to one who moves but whose school's wind direction does not change.

Our first approach to operationalize the thought experiment behind our identification strategy is to estimate a simple model of differences: regressing changes in wind status on changes in test scores. This is achieved through transforming Equation 1 by taking within-child differences between consecutive grades:

$$(2)\quad \Delta y_{isjgt} = \beta_1 \Delta Downwind_{isjgt} + \Delta X_{it}\rho + \Delta S_{st}\omega + \Delta D_s\gamma + \Delta\tau_g + \Delta\delta_j + \Delta\sigma_t + \Delta\varepsilon_{isjgt}$$

We estimate separate models for downwind and upwind moves. Specifically, in the case of a downwind move, we exclude students from the sample who moved from downwind to upwind in the move to either middle/high school but include those who never changed wind status as controls. Similarly, for upwind moves, we exclude those who moved from upwind to downwind.[4] We define $\Delta Downwind_{isjgt}$ as 0 when wind status does not change, 1 for a downwind move (in the downwind move regression), and -1 for an upwind move (in the upwind regressions). A negative value on β_1 indicates that the outcome is lower when the child is downwind.

Our second and preferred approach is to directly account for student ability using fixed effects models. Here we operationalize Equation 1 by including all observed fifth, sixth, eighth, and ninth graders (again dropping K–12 schools) but leverage the longitudinal aspect of our data to directly estimate θ_i as a student fixed effect. We identify the effect of being exposed to more highway pollution using both moves from upwind to downwind schools and moves from downwind to upwind schools. The primary difference between these two models is that the first differences model is identified off of consecutive changes in wind status, with the coefficient being the weighted average of two different potential moves (5^{th} to 6^{th} grade and 8^{th} to 9^{th} grade). Conversely, the fixed effects averages over ability and wind status for every year the student is observed in grades 5, 6, 8, and 9.[5] In both models, we multi-way cluster all standard errors at the zip code and student level.

A. Downwind Status

To operationalize assigning downwind status, we define a major highway as a U.S. interstate or U.S. highway and their immediate feeder routes.[6] Pollutants from car/truck exhaust

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[4] Once we make our sample restrictions, there are no cases that students who are observed in both sixth and eighth grade change their wind status between these grades.

[5] For example, in the first-differences model, a student who attended an upwind elementary, downwind middle, and downwind high school would contribute to the estimated treatment effect in the grade 5 to 6 move but would be counted as a control in their grade 8 to 9 move. In the fixed effect model, their upwind score in grade 5 would be compared to their average downwind scores across grades 6, 8, and 9 in the fixed effect model.

[6] We use Florida Department of Transportation (FDOT) shape files that have defined road segments along each highway. In a few cases, such road segments extend beyond the official

can be blown hundreds of meters by the wind from such highways, particularly nitrogen oxide (NO), nitrogen dioxide (NO_2), and ultrafine particles (UFP); the maximum distance we would expect pollutants to be blown is about 0.4 miles (Karner, Eisinger, and Niemeier 2010).[7] We thus limit our sample to only schools within 0.4 miles from the highway to ensure that our treated and control schools are similar in unobservable characteristics that might differ between schools near and far from a highway. This is the same as the cutoff in Anderson (2015), who found similar housing prices on either side of highways in Los Angeles, while the downwind side of the highway had higher pollution and higher mortality among the elderly. In a supplementary analysis, we use EPA data to directly document that pollution is elevated downwind of major highways in Florida.

We classify a school as downwind if the wind consistently blows across the highway and towards that school. Figure 1 illustrates our analytical strategy. In Panel A, the bold gray line represents a major highway in an anonymized part of the state. The dots represent schools. If the dominant wind pattern blows east to west in this part of the state, schools to the left of the vertical major highway will be exposed to additional pollution. Schools to the right of the highway, though still exposed to similar traffic, noise,[8] or other characteristics that come with being proximate to a major highway, will receive substantially less pollution exposure. Students

designation to include main feeder roads that we also include as a "highway" in our sample. We get similar results when excluding these roads. We provide more details on how we make use of the FDOT highway shape files in the Online Data Appendix available at http://jhr.uwpress.org/.

[7] NO/NO_2 blow up to 565 meters (0.35 miles) and UFP (defined as particulate matter 0.1 micrometers or less in diameter) blow up to 910 meters (0.57 miles) downwind, while larger particles do not travel as far (Karner et al., 2010). Other studies find that traffic pollution potentially travels further when pushed by wind. Currie and Walker (2011) examined exposure within 1.12 miles, and other work suggests that even two miles is possible in some cases (Hu et al. 2009). We limit our analysis to 0.4 miles to remain conservative and to not compare schools near a highway to those that are further away.

[8] Traffic noise does not vary significantly with wind direction (Allen et al. 2009).

who attend a school upwind in, say, elementary school move to a new school when they switch from fifth to sixth grade. If that middle school is downwind of a major highway, that student is now treated, and the analysis compares their outcomes before and after treatment within the same zip code relative to students who move between schools and do not change their up/downwind status. Panel B of Figure 1 displays the distribution of interstates and U.S. highways across Florida.

We proxy prevailing school-day winds using 2010 data from the U.S. Meteorological Assimilation Data Ingest System (MADIS), a part of the National Oceanic and Atmospheric Administration (NOAA).[9] For each hour of the school day, we define a school as downwind of a given highway if the wind direction blows within 45 degrees of a ray running from the nearest point on the highway to the school. Some schools are near multiple major highways. Taking the nearest five highway segments, we next measure whether a school is downwind of at least one of the nearest five major highway segments in a given hour. We then collapse the data over the year to obtain the percent of time a school is downwind from any nearby major highway during the

[9] See the Online Data Appendix for more details on the wind monitor, pollution monitor, and downwind status classification. Each MADIS station includes wind readings once per minute, and we take the first observation per hour. There were 1,029 stations in the state of Florida in 2010, and we connect each school to its nearest station and assign it that station's hourly wind data. We also match wind direction data to pollution monitor data. We use the 2010 wind data to proxy the other years because it is the most complete year available within the time frame of our education data. There were many fewer wind monitors in the early years, so we lack the power to reliably use annual variation in wind direction. Further, there were many more anomalous and missing wind direction readings for the wind monitors that we did have in the earlier years, making us concerned that using this data will add significant noise to our estimates. Finally, wind monitors were being non-randomly added in geographic areas over time in a way that could be correlated with other characteristics. We are therefore concerned that using an unbalanced panel of wind monitors could introduce bias into our estimates. With the wind monitor data we have, we verify that wind direction is consistent across years; for instance, the correlations between our annual wind direction measures in 2010 and 2012 is 0.81.

school day over the course of the year. We provide more explicit details in the Online Data Appendix.[10]

We create a binary variable to delineate treated from non-treated schools, using a cutoff of 60% of time downwind. We originally chose the cutoff of 60% to capture schools that are downwind a large amount of the time and therefore get the most consistent exposure to pollution over the school day. Further, by focusing on schools that are downwind a high proportion of the time, we make it more likely that students are regularly exposed throughout the day, as there is likely variation over the school day, depending on school and student schedules, in terms of when pollution matters the most for learning and cognition.[11] We test the robustness of this choice to other cutoffs empirically in the Results section. From here on we refer to "downwind" as a school that is downwind 60% or more of the time and any other school as "upwind," unless we explicitly say otherwise. Overall there are 59 downwind schools and 750 upwind schools in our sample.

B. Identifying Assumptions

We make two main identifying assumptions. First, we assume that, after implementing our difference-in-differences model, there are no factors other than differences in pollution levels that affect child outcomes when students transition to or from downwind schools. For example,

[10] Online Data Appendix can be found at http://jhr.uwpress.org/.

[11] Because 2010 is just one representative year, focusing on schools that are downwind the majority of the time in 2010 makes it more likely that we are capturing prevailing winds. As Figure A1 in the Online Appendix shows, there is fair amount of variation over time in wind direction and breaks from pollution exposure over the course of a day could provide respite. By focusing on schools that are downwind a high proportion of the time, we make it more likely that students are regularly exposed throughout the day. Another possibility is that there is a tipping point over which pollution exposure matters for academic performance. Compounding both of these issues is that measurement error in capturing the prevailing wind direction could downwardly bias estimates, and we are less likely to be capturing prevailing winds at low levels of percent of time downwind.

any identification strategy that relies on student moves might be biased if families select into moving. All of our models employ student fixed effects to account for any constant student ability or other constant family characteristics, but our results would still be biased if students who will, for unobserved reasons, have lower test scores in a given year systematically attend downwind schools. To help avoid such potentially choice-driven moves, we focus on changes between schools that occur as part of the "policy-induced" transition of graduating from elementary schools that feed into a middle school or from middle schools that directly feed into high schools. These students did not choose to move when their trajectories were changing; instead, their move was determined by district policy. In addition, we show the results of several balancing tests below.

Second, we assume that in the absence of switching to a downwind or upwind school, other school switchers (to schools of the same downwind status) can serve as a valid counterfactual over the same time period. Our estimates will be biased if students who transition to/from a downwind school are on a different trajectory relative to their peers who did not make such a transition. We directly test for differential trends between treated and control students using an event study design. Because estimating event studies requires defining a discrete move from an upwind to a downwind school (or the reverse) and looking only at students who have non-missing test scores over a balanced panel, the samples and specifications are slightly different from our main model. We discuss our methodology for the event study in detail in the Online Data Appendix.

The treatment that we measure is a student attending a school that is downwind. This makes it difficult to separate the direct impact of individual level student pollution exposure from the impact of attending a school that has had longer-term high-level exposure to pollution. It is therefore important to interpret our baseline reduced form results as capturing the casual effect of both student pollution exposure and of attending a school that has had extended pollution exposure.

Finally, the results may be biased towards zero if exposure effects are long-lasting. For example, even after a student moves from a downwind school to an unexposed upwind school, the student might continue to have lower academic performance because of the permanent damage pollution might have done (e.g., if the student gets asthma, they will have it for life). This scenario would bias our estimates towards zero, so we will explore the differences in to-upwind versus to-downwind moves in detail below.

IV. Student Data Description

Our sample contains the universe of students who were born in Florida in 1992–2002 and attended a Florida public school within 0.4 miles of a major U.S. highway in 1996–2012. The data came from the Florida Department of Education (FDOE). This administrative data provides rich demographic characteristics and student-level outcomes not typically available in the literature. We exclude virtual academies where the physical location of a school is unrelated to student pollution exposure, as well as adult education centers, schools for troubled youths and teen parents, schools for children with disabilities, and juvenile justice centers.

Our primary outcome of interest is individual-level scores on the annual Florida Comprehensive Assessment Test (FCAT) in math and reading. Students took the FCAT in math and reading in grades 3 to 10 in 2001–2012, and we take the average of the two test scores.[12] We

[12] Scores are standardized by year and grade at the state level for each test, with a mean of zero and a standard deviation of one, and we average the math and reading scores by year to create one summary measure of academic performance. We take the average of the two scores rather than estimating them separately to reduce noise and guard against Type 1 error from multiple

also examine two additional outcomes (only available in years 2002–2011): whether the student was written up for a behavioral incident during the year and the annual absence rate for students on a zero to one scale.[13] FDOE data includes individual-level characteristics such as race, ethnicity, gender, and free- or reduced-price lunch (FRL) eligibility. The Online Data Appendix contains additional information about the data sets and variable construction.

Column 1 of Table 1 shows the means for all children born in Florida. Since we observe a student multiple times (at most once per year), we average over these observations for every student in Florida. Column 2 shows the means for all children attending school within 0.4 miles of a highway, and Column 3 shows the mean characteristics of those students when they are in fifth, sixth, eighth, or ninth grades and attending a school within 0.4 miles of a highway (dropping K–12 schools). Column 3 is identical to our main analysis sample.[14]

V. Results

A. Main Results

Table 2 presents our main results. The sample is based on our preferred specification with policy-induced movers to middle school (grades 5–6) and high school (grades 8–9). Panels A and B present our first-differences estimation, while Panel C is our preferred fixed effect model. In all models, the coefficient indicates the difference in outcomes in downwind schools, relative to upwind schools.

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hypothesis testing. When looking at these outcomes separately, they both follow the same overall pattern.

[13] Some students are also missing an observation in one variable, but not the others, within the same year. This causes small differences in sample sizes across these three outcomes in addition to the differences caused by yearly differences in data availability.

[14] The sample size declines from Columns 2 to 3 both from dropping K–12 schools and because some students only attend a school near a highway in, for example, first through fourth grade and therefore aren't in our analysis sample.

The different specifications present broadly similar results: being downwind of a major highway is associated with lower average FCAT scores, a higher likelihood of having a behavioral incident, and a higher absence rate. When modeled separately, the move from upwind to downwind (Panel A) is larger in absolute terms than the opposite move from downwind to upwind (Panel B). In our preferred fixed effect model (Panel C), attending a school that is downwind of a major highway is associated with a 3.98 percent of a standard deviation decrease in scores, relative to attending a school that is not. There is a 4.09 percentage-point increase in the likelihood of having a behavioral incident and a 0.53 percentage-point increase in the absence rate. Given that the average rate of absences in Florida schools is 5.6 percent, this is a 9.5 percent increase in the rate of absences from school. Because moving to a school that is downwind is associated with a slightly larger effect on test scores than moving from a downwind school to an upwind school, our main specification can be interpreted as a lower bound of the effects on traffic pollution on test scores.

Appendix Table A1 displays a specification with all movers across all grades, rather than with the policy-induced movers.[15] Appendix Table A2 shows additional specifications for downwind status beginning only with a core set of fixed effects and controls, then gradually adding additional controls for school quality and demographic characteristics. If being downwind

[15] All Appendix tables and figures can be found at http://jhr.uwpress.org/. The drawback of our preferred approach in Table 2 is that we can only estimate effects for those who change their exposure status from fifth to sixth grade or eighth to ninth grade. We thus re-estimate Equation 1 for all students in all grades (3-12), regardless of whether they move, as long as they attend school within 0.4 miles of a highway. We show these results in the Appendix. Here, the coefficient β_1 on $Downwind_{isjgt}$ captures the change in test scores for a student who moves to any new school (for whatever reason) with a different prevailing wind direction. In this model both those who move and do not change wind direction as well as those who do not move at all are part of our counterfactual group, though we directly control for the effect of moving with an indicator variable.

in our model was correlated with school characteristics due to student sorting or other reasons, we would expect the coefficient on downwind to change as we add these controls. We find similar results regardless of the specification. For the remainder of our paper, we show results using our preferred fixed effects specification with policy-induced moves and the full set of controls.

We chose the 60% cutoff for downwind status because we believe schools that experience particularly consistent wind pattern exposure over the school day are the ones most likely to show detectable effects on student outcomes. Figure 2 directly tests this by presenting point estimates for average test scores using our main specification by the percent of time downwind, grouping schools into bins of downwind status: 0–20% of time downwind (the reference group), 20–30%, 30–40%, 40–50%, 50–60%, 60–70%, or more than 70% of the time. The effects are close to zero at low- to mid-levels of time spent downwind, while those schools downwind 60–70% or more than 70% of the year have larger negative effects on test scores.

We next test our parallel trends assumption in an event study on the move from elementary to middle school.[16] This will make it transparent if those students who moved into a school with a different prevailing wind direction in sixth grade were on a different trajectory in achievement. Furthermore, the associated graphs help us understand how outcomes change relative to the timing of pollution exposure. We include all students observed continuously between third and seventh grade (dropping K–12 schools). We estimate our equation jointly for upwind and downwind movers (our treated students), including students who never change wind status as a control group:

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[16] We focus on the move to middle school because it is a relatively transparent way of showing pre-trends for one of the major sources of policy-induced variation in the study.

$$(3)\ \ y_{isjgt} = \beta_0 + \sum_{g=3}^{7} \pi_g\, \mathbf{1}(\kappa_{ig} = g) + \sum_{g=3}^{7} \eta_g\, \mathbf{1}(\zeta_{ig} = g) + X_{it}\rho + S_{st}\omega + D_s\gamma + \theta_i + \tau_g + \delta_j + \sigma_t + \varepsilon_{isjgt}$$

where κ_{ig} is a set of dummies equal to 1 when a "downwind mover" is in grade g, and zero in all other cases. A downwind mover is defined as a student who will move from upwind to downwind in grade 6, and otherwise is equal to zero. Likewise, ζ_{ig} are equal to 1 when student *i* is both an "upwind mover" (one who moves from downwind to upwind in grade 6) and is in grade g. In both cases, the year before treatment ($g = 5$) is the excluded dummy normalized to zero. Notably, κ_{ig} and ζ_{ig} always equal zero for students who never change wind direction. Therefore, the coefficients π_g and η_g capture the differential effect of being in grade g for a student who changes wind status in sixth grade, relative to students who does not change wind status. We provide more details on how we construct this event study in the Online Data Appendix. We get similar estimates when we model upwind and downwind moves in separate equations.

Figure 3 shows these results. Several key patterns stand out. First, we see a relatively flat pre-trend in FCAT scores for both groups across grades 3–5. Second, scores drop sharply when students move to a downwind school in grade 6. Third, while there seems to be a slight increase in scores for students moving from a downwind to an upwind school, the impact is small and statistically insignificant. One explanation for this pattern is that the effects of pollution exposure may be persistent beyond the period of direct exposure. This would be the case if exposure to pollution in earlier grades has persistent effects on either cognition or skill acquisition that last even after pollution is alleviated. We also see a similar relatively flat pre-trend and sharp increase in behavioral incidents, with a more distinct effect of moving from downwind to upwind. Finally, the pre-period is mostly flat for percent of time absent, though there is a decline in absences in fifth grade followed by a sharp increase for those moving into a downwind school. We see a similar pattern if we instead use all student movers rather than our policy-induced movers.[17] We take these event studies as evidence that differential trends between students who move downwind (or upwind) and those who do not change treatment status are not driving our results.

B. Effects by Subgroup

We next examine several potential subgroups of interest. Different socioeconomic groups may have different access to resources to ameliorate the effects of pollution, such as academic help for more affluent students. Conversely, advantaged students are less likely to be exposed to pollutants at home, potentially leading to a larger marginal impact of attending a polluted school. Table 3 examines our preferred estimation for several subgroups: by race/ethnicity (White, non-Hispanic, Panel A; Black students, Panel B; Hispanic students, Panel C); by frequency that students identify as on free- or reduced-price lunch (always on FRL, Panel D; sometimes on FRL, Panel E; never on FRL, Panel F), and by gender (Panels G and H).

White, non-Hispanic students and Hispanic students have larger declines in FCAT when exposed to pollution, whereas Black students have larger behavioral incident and absence results. These differences in outcomes between White, Black and Hispanic students are all statistically significant. The largest test score effects are for the never-FRL students, with lower effects for the always-FRL students. The sometimes-FRL students fall between these groups. There is no statistical effect on behavioral incidents or absence for the never-FRL students. The behavioral effects are large and statistically about the same between the sometimes- and always-FRL

[17] Appendix Figure A2 presents results for using variation from all moves for students who move from an upwind school (year -1) to a downwind school (year 0) in any grade (i.e., not only those students transitioning from elementary to middle school).

students, though the sometimes-FRL estimate does not statistically differ from zero. The absence rate effect is driven by the always-FRL students. Always and sometimes FRL students have statistically significant differences in test scores from never-FRL students. There is no clear pattern of differences between boys and girls.

Overall, these patterns suggest that the test scores of more-advantaged students are the most harmed by pollution exposure. While it is difficult to say for certain, this may be because disadvantaged students are already exposed to other sources of pollution. On the other hand, the less-advantaged are more likely to have higher behavioral incidents and absence rates. The larger effects on behavioral incidents might reflect a differential reaction by administrators and teachers to writing up disadvantaged students.

C. Testing for Sorting into Schools

To test for sorting into schools, we first demonstrate that wind direction does not systematically predict school or student demographic characteristics. Figure 4 plots the relationship in raw means between the time a school spends downwind and several key school-level characteristics. These figures show that spending more time downwind is not consistently associated with being negatively selected on observable socioeconomic demographic characteristics. Table 4 shows a balancing test where we estimate Equation 1 with various demographic characteristics as the outcome variable of interest regressed on our measures of downwind status. The coefficients on downwind status are statistically insignificant. The coefficients are of mixed signs relative to being associated with lower socioeconomic status characteristics. We also see no effect of wind status on the accountability grade Florida assigns the school (Column 9).[18] While attending a downwind school lowers test scores, the effects are not large enough to drive changes in high-stakes accountability grades, one of the key signals of school quality that families use. Finally, the coefficients on many of the most important covariates related to socioeconomic status are small and relatively precisely estimated. For example, in the "maternal education" regression, the coefficient on being "downwind" is only 0.02 years out of an average of around 12 years of mother's education. The lower bound of the 95% confidence interval suggests that at most being downwind is associated with a 0.1 fewer years of mother's education.[19] To get a sense if school characteristics are jointly associated with being downwind, we do a balancing test on predicted test scores and find no relationship.[20]

The above test is consistent with sorting based on pollution being less of an issue with sorting into schools than residential sorting into neighborhoods. Selecting into schools based on traffic pollution is also less likely than some other forms of pollution because small pollutants such as CO, UFP, and nitrogen oxides are not perceptible by human senses and are only detectable with scientific equipment. Larger, more perceptible particles do not disperse as far from highways. Finally, due to the coastal wind patterns and the peninsular shape of the state, prevailing winds tend to shift during the school day relative to the evening. Thus, even if a

[18] Schools receive a grade of A through F in Florida's high stakes accountability system, which we transform into a linear variable such that A=5, B=4, etc. We estimate that a 0.04 standard deviation decline in test scores at the school level would only be associated with 0.068 of a high stakes accountability grade level decrease.

[19] Another key indicator of socioeconomic characteristics if the fraction of FRL students. Here the coefficient suggests that being downwind causes the percent of FRL students to decline by 1 percentage point (out of a mean of 67 percent). The standard errors are such that we can rule out that being downwind increases the percent of FRL students by more than 0.8 percentage points.

[20] Test scores are predicted in a supplementary model using the school characteristics from Equation 1 along with quadratics and first order interactions between each of the other control variables.

school is downwind over the course of a school day it does not necessarily mean the neighborhood it is located in is systematically downwind.

Zip code fixed effects help account for student sorting between zip codes. Our balancing test shows that within-zip code schools are similar based on observable demographic and quality characteristics, and the event study shows that treatment and control students are on comparable trajectories. One serious remaining concern is that our results could be driven by within-zip code sorting. We can formally test this by dropping all zip codes in which there are choices between elementary/middle/high schools in terms of being downwind 60% or more of the time. We begin by dropping the control-only zip codes without any downwind schools. Of the remaining zip codes, we next drop all those in which there are choices between elementary/middle/high schools that are both upwind and downwind. This limits the geographic coverage of our sample such that external validity is a concern, but an advantage of this approach is that now our results are exclusively identified off of cases where there is no room, within our observed sample, for within-zip code sorting. Table 5 shows these results. The negative effects of attending a downwind school are larger than our baseline results and statistically significant, with a -9.01 percent of a standard deviation impact on test scores. There are also larger effects on behavioral incidents and absences. However, the standard errors are roughly double, making it unclear if the estimates substantially differ from our core results. Importantly, if within-zip code sorting was driving our results, we would expect to see smaller effects in this sample with few attendance choices.

In a related test, we drop all zip codes without at least one downwind school to address concerns that the “downwind more than 60% of the time” variable is comparing a large number of control schools to a small number of treated schools. Once we drop these zip codes, roughly

25% of the schools in the sample are downwind. Results are in Appendix Table A3 and are largely similar to our core results.

D. Additional Robustness Checks

If our results are driven by traffic pollution, we would expect students to do worse if they are downwind of more heavily-trafficked roads. We operationalize this by examining the impact of being downwind of the road nearest to the school in order to categorize schools by traffic volume. We then estimate our main model by interacting the "downwind more than 60%" indicator with three different Annual Average Daily Traffic (AADT) bins (<50,000 average cars per day, 50,000–75,000 average cars per day, and >75,000 average cars per day). We include the un-interacted bins as controls. Figure 5 plots the coefficients on the interactions. The estimates indicate that being downwind of the lower-traffic-volume roads has a small positive effect on test scores. The coefficient for being downwind of middle-traffic road is close to zero. There is a large negative effect of being downwind of the highest-volume roads, and this effect statistically differs from the lowest-volume estimate (*p*-value of interaction=0.000). Overall, the main effects appear to be driven by the highest-trafficked roads.

We next perform placebo tests to provide additional evidence that our results are not spurious. These placebo tests also guard against a related concern that, in spite of employing difference-in-differences, our estimates are somehow capturing the negative effects of changing schools.[21] We run a placebo 500 times per outcome. For each estimate, schools were randomly assigned a percent of time downwind from the empirically observed distribution,[22] which was

[21] As another test against this concern, we also tried controlling for "moving school" dummies up to three years out. Our results were robust to this specification.
[22] We use a normal distribution, with the mean and variance of percent of time downwind estimated from the observed sample.

then translated into an indicator variable equal to one if the placebo percent downwind was greater than 60%. The sample was limited to schools within 0.4 miles of a highway that were not downwind. To ensure variation with zip and student fixed effects, each placebo set had to have at least 21 zip codes with variation and at least 46 placebo-treated schools. The results of these placebo tests are presented in Appendix Figure A3, and we find no consistent placebo effects. Only 20 of the 500 FCAT estimates (4%), two of the behavioral incident estimates (0.4%) and 31 of the absence rates estimates (6.2%) are larger in the predicted direction than estimates from our primary specification.

Table 6 runs a number of additional robustness tests across our models using the average FCAT outcome to address a variety of concerns. The first column is a replication of the preferred results from Table 2. Following Anderson (2015), the second column excludes schools that may be misclassified due to small errors in geolocation data. We do this by dropping schools within 0.03 miles of the highway, and the results do not substantially change after dropping these schools. Column 3 presents results from a placebo test where we replace our “downwind” measure with an indicator variable equal to one if the wind is blowing parallel along the road segment sixty percent or more of the time. This should capture whether there are any effects related to strong prevailing winds in a location that is not directly associated with increased pollution exposure; however, the coefficient on test scores is small and not statistically significant.

One worry with the analysis is that parents move their children in ways systematically related to achievement, even within the same school district. Though we do not know where students live, we know their school address, and in Column 4, we limit the sample to only students who stay in the same zip code in our observed data. Here, any differences cannot be driven by differential effects on cross-zip movers at any point through the end of high school. We have lower power due to a lower N, but the estimate for being downwind more than 60% of the time is larger in magnitude.

Another concern with our analysis is that the long-run equilibrium of downwind schools is worse than in upwind schools, such that the downwind effect is actually a combination of pollution effects and negative peer effects. Although this would not change the policy implication that downwind schools are bad, it would be an important caveat in interpreting our results. We test this, as best as we can, by controlling for peer test scores as a "bad control." Specifically, when we add the average FCAT results for each school-year as a control in Column 5, the results do not change from the main specification, suggesting that it is pollution and not peer test scores that drive our results. In addition, Appendix Table A4 also shows the results where we add controls for distance of the move from the prior year's school and use a number of alternative definitions of distance pollution traveled, as well as specifications using a measure of wind intensity exposure as an alternate measure of downwind status.[23] In Appendix Table A5, we also show that our results are robust to grade-by-year fixed effects in Panel A and district-by-grade fixed effects in Panel B.

As a final robustness check, we examine different ways of clustering our standard errors. Appendix Table A6 shows that our core results are robust to a range of different clustering schemes in addition to our baseline multi-way clustering on student and zip-code for average FCAT. This includes clustering on school, student, zip-code, and student-school. We have tried different types of clustering on the other outcomes, and they follow a similar pattern.

[23] Details on our measure of wind intensity exposure are discussed in the Online Data Appendix.

E. Pollution Estimates

So far, we have focused on the reduced form effects of being downwind of a major highway. We can also approximately examine the first stage of pollution. We cannot directly apply our model to estimate pollution exposure because we cannot track changes in student-level exposure over time. Instead, we use hourly EPA pollution monitor and hourly MADIS wind data to examine how much pollution increases when the monitor is downwind of a highway in 2010. Our preferred specification uses month and site fixed effects, such that the effect is interpreted as the level of pollution for hours when a monitor is downwind of a major highway, relative to hours when the wind blows such that the same monitor is upwind. We provide more details on this pollution data and on our exact estimating equation in the Online Data Appendix.

We do not have measures for UFP pollutants of less than 0.1 micrometers in diameter, which are known to affect both cognition and health. These pollutants travel farther distances on the wind than heavier particles. We do have data on several heavier particles (PM_{10}, CO, and NO_2) that we would expect to be blown by the wind but travel a smaller distance (Karner, Eisinger, and Niemeier 2010). We take logs of these three pollutants so we can interpret the coefficient as percent changes; results are similar using levels. We expect PM_{10} and CO to travel a maximum of about 0.12 miles, while NO_2 might travel up to 0.34 miles on the wind. We also examine a pollution index that normalizes each of these individual pollutants to have a mean of zero and a standard deviation of one over all pollution monitors, and then takes the simple average for each site-hour observation.

Table 7 presents the results. We include pollution monitors within 0.4 miles of a highway (Column 1) to match our main specification and pollution monitors within 0.1 miles (Column 2) to focus on the likely distance traveled of the particular pollutants we have available in the data.

While Column 1 has more observations, we expect Column 2 to more consistently estimate these pollutants. The rows of the various pollution types include the estimated effect of being downwind at a given site in a given hour.

Broadly, across most measures, the table confirms the general pattern that being downwind increases pollution exposure. For instance, in the preferred 0.1 mile range, when a site is downwind of a major highway, the pollution index increases by 0.187 standard deviations, relative to the hours of the day the site is not downwind. For the specific pollutants, PM_{10} increases by 11.2 percent when the monitor is downwind of a major highway, CO increases by 8.9 percent, and NO_2 increases by a statistically insignificant 7.8 percent. The results are in the same direction but smaller when we expand the radius to 0.4 miles.

Very roughly, we approximate that downwind schools experience a 25% higher level of ambient traffic pollution in a day, relative to upwind schools.[24] We can combine our reduced form student achievement estimates with the estimated impacts of being downwind on pollution to derive a "two stage least squares" type of parameter. Scaling our preferred estimate of a policy-induced move to a downwind school that has a 25 percent higher level of pollution implies that increasing the ambient traffic pollution exposure of a school by 10 percent per day over the course of a school year causes students who attend this school to experience a 1.6% of a

[24] Students attending schools that are downwind 60% or more of the time are downwind on average 76% of the day or 6.08 hours of the school day, whereas other students are only downwind 2.16 hours (27% of the time). Therefore, "treated" students are downwind an average of 3.92 hours more than control students. Taking the pollution index measure of a 0.187 standard deviation increase in pollution for exposure in an hour, we calculate 0.187*3.92 = 0.73 standard deviations, or 25% of the pollution distribution applying the properties of a standard normal distribution.

standard deviation decrease in test scores.[25] Similarly, a 10% increase in traffic pollution causes a 1.6 percentage point increase in behavioral incidents, corresponding to 4.6% of the mean.[26]

We can use the above estimates to compare our work to prior research on the impact of testing-day pollution exposure (e.g., Ebenstein, Lavy, and Roth 2016; Marcotte 2017; Roth 2016). Marcotte (2017) finds that doubling test day $PM_{2.5}$ exposure from an average day (25) to an unhealthy day (above 50) leads to a decrease in test scores of 2%, suggesting that a 10% increase leads to a 0.2% decline. Similarly, Roth (2016) finds that a 10% increase in PM_{10} on the day of the test results in a 1% decline in test scores.[27] Therefore, our impacts of prolonged exposure are roughly 1.5 times the size of test day exposure that Roth (2016) finds or 8 times the effect that Marcotte (2017)finds. That being said, it is possible that traffic pollution in aggregate is worse than either $PM_{2.5}$ or PM_{10} individually. It is also worth noting that both other studies use different samples, and differences in results could reflect different local average treatment effects.

As an alternative, we directly estimate short-term impacts through using our research design but with variation in wind direction based on the timing of the annual test. Similar to the above, we find effects that are two to four times smaller than our estimates of annual exposure; these results and the methodology associated with them are discussed in the Online Data

[25] Calculated by dividing a 0.0398 standard deviation decrease in test scores by a 25 percent increase in pollution and then multiplying by 0.10.

[26] We can also approximate the additional exposure to ultrafine particulate matter using the atmospheric sciences literature. Based on estimates from the Karner et al. meta-analysis, schools downwind within 0.207 miles of a highway (the average for our sample) would experience an increase in ultrafine particulate matters that is 2.5 times the background levels.

[27] We convert this to effects relative to the mean for comparability. From Table 2, Roth (2016) estimates a 0.3 percent decline in scores (0.055 points) based off of 1 ug/m3 increase (if 3.41 points is 20% of a standard deviation, then 0.055 points is 0.3% of a standard deviation). In Table 1, the average PM10 is 33.35 units, so a 1 unit increase is a 3% increase. Therefore, a 10% increase in PM10 implies a 1% decrease in test scores (0.003*0.1/.03=0.01).

Appendix and Table A7. A serious limitation of this exercise is that we assign weekly winds based only on the 2010 wind monitor data we have available. Therefore, we take these estimates as suggestive given that they are likely attenuated due to measurement error.

VI. Conclusion

This is the first study, to our knowledge, to show negative academic effects of pollution related to repeated daily exposure during the school year to traffic, rather than just exposure on the day of the test. We leverage the microclimates that exist within zip codes and the policy-induced changes in school attendance for middle and high school to study how localized pollution exposure can harm school children. Using within-child variation in exposure, we show that children who attend a school downwind of a major highway have lower test scores and a higher likelihood of behavioral incidents and missing school than when those same children attended schools with similar characteristics that were not downwind of a major highway. The effects are larger for more heavily-trafficked roads, and the effects appear to last even after the child moves away from a downwind school. This suggests that once damage from pollution is done, even during middle childhood, it might persist, potentially affecting outcomes far into the future.

In addition, the magnitudes of these effects are substantively important, especially when one considers that 6.4 million children (or about 12.6 percent of public and secondary school students) attend school within 250 meters of a major roadway. To put this in context, Chetty, Friedman and Rockoff (2014) find that a one standard deviation improvement in teacher quality increased test scores by 0.1 standard deviations. This suggests that removing exposure to local highway pollution would increase test scores as much as increasing teacher quality by 40 percent of a standard deviation. Our findings are about one-fifth of the magnitude of the Tennessee

STAR experiment (Krueger 1999), which found that reducing class sizes from 22 to 15 students increased test scores by about 0.2 standard deviations.

We can also use our estimates to evaluate the academic impact of environmental regulations that have decreased traffic emissions by applying our estimate that a 10% decrease in traffic pollution leads to a 0.016 standard deviation increase in scores. The EPA estimates increased auto and truck regulations as part of the Clean Air Act reduced traffic emissions by 70% between 1970 and 2015 (EPA 2015), which implies a large 0.11 standard deviation increase in test scores.[28] These estimates would suggest that the reductions in traffic pollution over the past three decades are on par with major education interventions in a way that has not been catalogued by EPA welfare estimates.

There is reason to believe that our estimates are a lower bound on the true effects of highway pollution. For instance, our study's findings are identified from children who attended school within 0.4 miles of a major road for at least two school years. While that includes about 36% of our study population in Florida, we do not include effects for children who move from schools very far from a major highway to being directly downwind of such a road. Conceivably, such children are exposed to less ambient pollution when they are farther away from roads, so the change in pollution exposure may be related to even larger effect sizes for them. However, selection issues prevent us from exploring this possibility further. We note, however, that our estimates show negative effects for all students. In addition, our results will be biased towards zero if the effects of pollution exposure are long-lasting. Then, even after a student moves from a

[28] See https://www.epa.gov/clean-air-act-overview, last accessed on 7/23/2018. While we believe this provides one application for understanding our estimates, there are some caveats to interpreting our results in this way. Our parameter is estimated off of our specific natural experiment, and more research would be needed to understand how to apply this more broadly.

downwind school to an unexposed upwind school, the student might continue to have lower academic performance.

Our results also imply several important policy lessons. First, districts may want to consider the benefits of placing schools away from major highways. Districts may be unable to move already-existing schools, but compensatory measures such as air filtration systems may reduce the amount of in-school pollution exposure for schools located near highways (South Coast Air Quality Management District 2013; Gilraine 2020). Schools downwind of a major highway are not the only ones exposed to pollution, so these measures may be beneficial for schools in polluted areas more broadly. Finally, recent work by Chetty et al. (2014) suggests that there is massive heterogeneity in intergenerational mobility across cities. However, there is reason to consider that even within zip codes, there are forces that create unequal outcomes due to, for example, the placement of schools. More broadly, pollution exposure is not evenly distributed across the socioeconomic spectrum. Given that low-income and minority students are more likely to be exposed to pollution and live near major roads, our demonstrated relationship between pollution and academic achievement may provide insight into why academic achievement gaps persist in the United States.

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Tables

Table 1: Characteristics of children attending school within 0.4 miles of a highway

	(1) All children born in Florida	(2) All children attending school within 0.4 miles of a highway	(3) Children in 5th, 6th, 8th or 9th grades within 0.4 miles of a highway, dropping K-12 schools.
School size (in 100s)	9.59	9.38	11.03
Percent of teachers with an MA degree	0.315	0.319	0.331
Percent of time free or reduced price lunch	0.517	0.578	0.553
Stability rate	0.940	0.938	0.938
Percent Black	0.224	0.319	0.326
Percent Hispanic	0.240	0.204	0.187
Average maternal education	12.37	12.26	12.29
Percent mothers who are married	0.643	0.563	0.576
N Students	1,682,489	481,706	107,463

Notes: Observations are at the student-year level. Column 1 shows the means for all children in Florida. Column 2 shows means for children attending school within 0.4 miles of a highway; and Column 3 shows means for children 0.4 miles from a highway only using observations in grades 5, 6, 8, and 9 and dropping K-12 schools. Column 3 is identical to our analysis sample.

Table 2: Impact of attending school downwind

	(1) Average FCAT	(2) Behavioral incident (0/1)	(3) Rate of absence
	Panel A: First differences, to downwind		
to Downwind more than 60%: Downwind=1	-0.0723** (0.0248)	0.0247 (0.0266)	0.0058** (0.0027)
Observations	74,316	62,476	61905
N students	65,088	55,332	54,842
Mean of outcome	0.0684	0.3319	0.0568
	Panel B: First differences, to upwind		
from Downwind more than 60%: Downwind= 1	-0.0465*** (0.0177)	-0.0061 (0.0119)	-0.0004 (0.0012)
Observations	74,358	62,506	61,917
N students	65,185	55,424	54,958
Mean of outcome	0.0684	0.3319	0.0569
	Panel C: Fixed effects specification		
Downwind more than 60%	-0.0398** (0.0192)	0.0409** (0.0197)	0.0053* (0.0029)
Observations	273,229	237,684	234,614
N students	107,463	94,324	93,249
Mean of outcome	0.0206	0.3558	0.0618

Notes: Each row and column shows results from a different regression. Panel A shows first differences for moves to a downwind school (excluding upwind movers). Panel B shows first differences for a move to an upwind school (excluding downwind movers). Panel C uses student fixed effects instead of first differences (includes upwind and downwind moves). All models include grade fixed effects, zip code fixed effects, year fixed effects, distance from nearest highway dummies, a grade-moved indicator, the number of highways within a mile, school demographic characteristics, other school-level characteristics (percent of teachers with a master's degree, size, and stability rate), and whether the student was on FRL that year. These controls are all differenced in the first-differences model and enter linearly into the FE model. Standard errors clustered on zip and student are in parentheses. Coefficients of interest are an indicator variable for whether a student's school was downwind 60% or more of the time. All models are estimated by dropping "K through 12" schools and limiting the sample only to students who change schools from fifth to sixth or eighth to ninth grade. $^{*}p < 0.1$ ** $p < 0.05$, *** $p < 0.01$

Table 3: Effects by demographic subgroups

	(1) Average FCAT	(2) Behavioral incident (0/1)	(3) Rate of absence
Panel A: White, non-Hispanic students			
Downwind more than 60%	-0.0829***	0.0432	0.0022
	(0.0307)	(0.0306)	(0.0033)
Panel B: Black non-Hispanic students			
Downwind more than 60%	-0.0034	0.0563***	0.0134***
	(0.0210)	(0.0176)	(0.0035)
Panel C: Hispanic students			
Downwind more than 60%	-0.049**	0.0247	-0.0006
	(0.0250)	(0.0275)	(0.0036)
Panel D: Always FRL students			
Downwind more than 60%	-0.0242	0.0418**	0.0065*
	(0.0187)	(0.0191)	(0.0035)
Panel E: Sometimes FRL students			
Downwind more than 60%	-0.0378	0.0482	0.0018
	(0.0254)	(0.0355)	(0.0042)
Panel F: Never FRL students			
Downwind more than 60%	-0.0781**	0.0162	-0.0010
	(0.0312)	(0.0142)	(0.0022)
Panel G: Girls			
Downwind more than 60%	-0.0357*	0.0491**	0.0048*
	(0.0189)	(0.0235)	(0.0029)
Panel H: Boys			
Downwind more than 60%	-0.0451**	0.0336*	0.0060*
	(0.0214)	(0.0183)	(0.0035)

Notes: The panel indicates the subgroup stratified on. All models are estimated using our fixed effects specification, dropping “K through 12” schools and limiting the sample only to students who change schools from fifth to sixth or eighth to ninth grade. All models include grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, distance from nearest highway dummies, a grade-moved indicator, the number of highways within a mile, school demographic characteristics, other school-level characteristics (percent of teachers with a master’s degree, size, and stability rate), and whether the student was on FRL that year. Standard errors clustered on zip and student are in parentheses. $^{*}p < 0.1$ ** $p < 0.05$, *** $p < 0.01$

Table 4: Balancing test

	(1) School size (in 100s)	(2) School stability	(3) Fraction teachers with master's degree	(4) Fraction FRL students	(5) Mean years of mothers' education by school	(6) Fraction Black by school	(7) Fraction mothers married at birth by school	(8) Fraction Hispanic by school	(9) School Accountability Grade	(10) Predicted Average FCAT
Downwind more than 60%	-0.1541 (0.7797)	-0.0012 (0.0043)	-0.0105 (0.0162)	-0.0088 (0.0104)	0.0246 (0.0829)	0.0076 (0.0092)	-0.0033 (0.0046)	0.0106 (0.0182)	0.0357 (0.1217)	0.007 (0.006)
Obs.	273,229	273,229	273,229	273,229	273,229	273,229	273,229	273,229	263,155	273,229
N students	107,463	107,463	107,463	107,463	107,463	107,463	107,463	107,463	103,933	107,463

Notes: Each column shows results from a different regression using our fixed effects model. Each column uses a different variable as an outcome to verify that treatment status is not related to observable characteristics. All models include grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, a grade-moved indicator, the number of highways within a mile, and distance from nearest highway dummies. The models also include the school-level characteristics, whether the student was on FRL that year, and school demographic characteristics, except when that variable is the outcome. Standard errors clustered on zip and student are in parentheses. Column 9 shows results limiting to schools where we observe their accountability grade: we code accountability as a linear variable (A=5, B=4, etc). Since we don't have grades for all schools the sample falls slightly in this specification. Column 10 shows results from a regression in which we predict FCAT using our baseline covariates and their quadratics/interactions. We then use the predicted FCAT values as an outcome variable in Column 10. $^{*}p < 0.1$ ** $p < 0.05$, *** $p < 0.01$

Table 5: Test for school sorting: including only zip codes without school choices near a highway

	(1) Average FCAT	(2) Behavioral incident (0/1)	(3) Rate of absence
Downwind more than 60%	-0.0901**	0.1064***	0.0076
	(0.0401)	(0.0410)	(0.0084)
Observations	8,304	7,470	7,385
N Students	5,893	5,415	5,358

Notes: Each column shows results from a different regression using our fixed effects model. All models include grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, a grade-moved indicator, the number of highways within a mile, distance from nearest highway dummies, school demographic characteristics, other school-level characteristics (percent of teachers with a master's degree, size, and stability rate), and whether the student was on FRL that year. Standard errors clustered on zip and id are in parentheses. Coefficient of interest is an indicator variable for whether a student's school was downwind 60% or more of the time. All models are estimated using only moves to middle/high school by dropping "K through 12" schools and limiting the sample only to students who change schools from fifth to sixth or eighth to ninth grade, only in zips where there is no "choice" on which type of school (upwind/downwind) a student attends in a given grade. $^{*}p < 0.1$ $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6: Additional robustness and validity tests for average FCAT

	(1) Baseline model	(2) Drop schools <0.03 miles from highways	(3) Parallel wind placebo	(4) Drop students changing school-zips, grades 5–10	(5) Controlling for average FCAT by school
Panel A: Downwind more than 60% of the time					
Downwind more than 60% of the time	-0.0398**	-0.0349*	NA	-0.0801***	-0.0462***
	(0.0192)	(0.0194)		(0.0251)	(0.0135)
Observations	273,229	230,340		53,603	273,229
N Students	107,463	92,817		20,689	107,463
Panel B: Winds blow parallel 60% of the time (placebo)					
Winds blow parallel 60% of the time	NA	NA	0.0070	NA	NA
			(0.0096)		
Observations			273,229		
N Students			107,463		
Baseline model	Yes	No	No	No	No
Drop schools <0.3 miles from highways	No	Yes	No	No	No
Parallel wind placebo	No	No	Yes	No	No
Drop students changing school-zips, grades 5–10	No	No	No	Yes	No
Controls for school-level FCATs	No	No	No	No	Yes

Notes: Each column and row show results from a different regression with average FCAT as the dependent variable using our fixed effects model. Column 1 replicates our results from Table 2. Column 2 drops schools within 0.03 miles (50 meters) of a road. Column 3 is a placebo test replacing our wind exposure measure with winds blowing parallel to the highway 60% of the time or more as the variable of interest. Column 4 drops any student whose school changes zip codes in grades 5 through 10. Column 5 adds controls for school-level FCATs. Otherwise, the regression includes all of the controls in our baseline models. Standard errors clustered on zip and student in parentheses. $^{*}p < 0.1$ $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: Pollution and wind direction

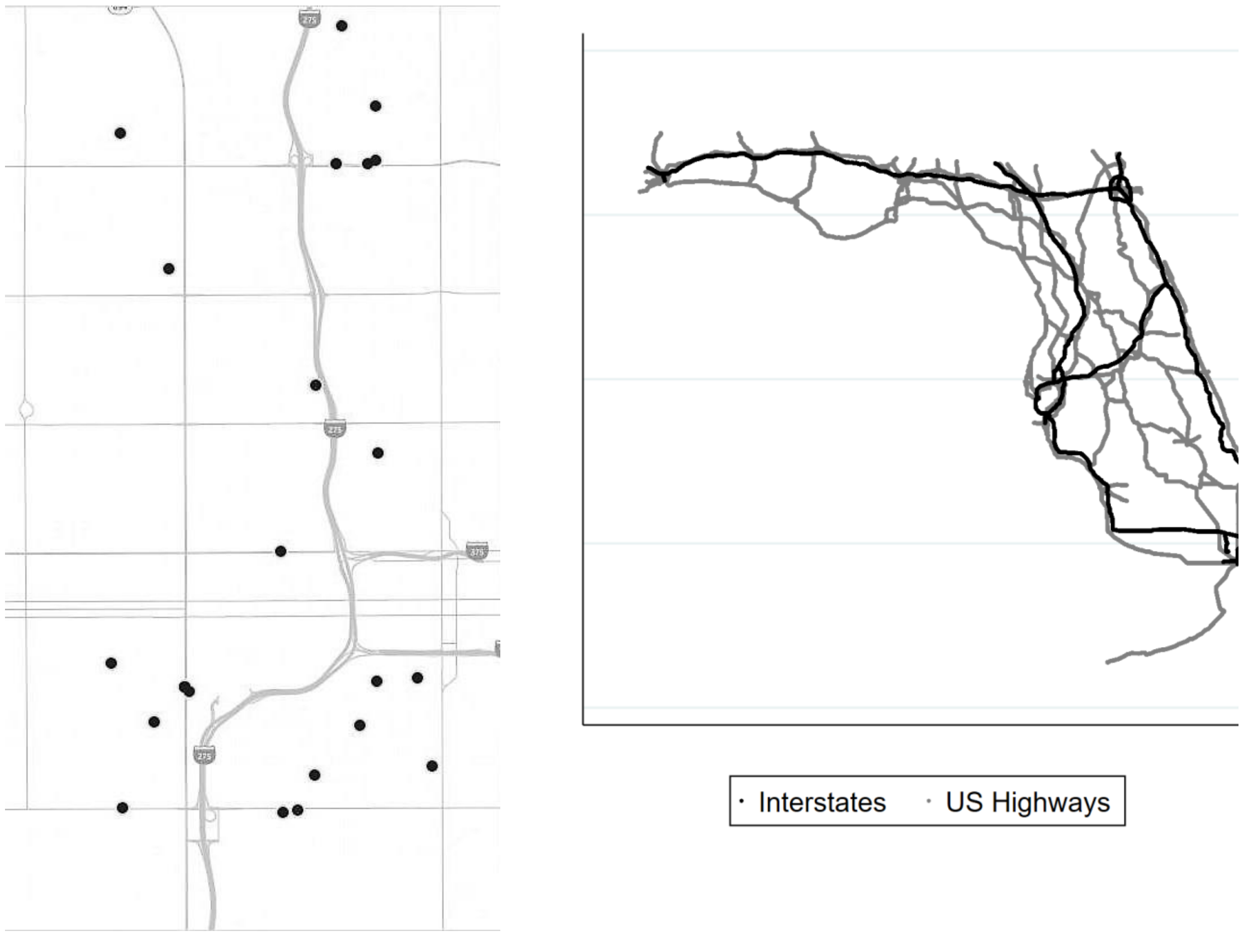
	(1) Monitors within 0.4 miles of road	(2) Monitors within 0.1 miles of road
	Outcome 1: Pollution index	
Downwind	0.043**	0.187***
	(0.022)	(0.039)
# of Observations	36,979	11,505
	Outcome 2: Log PM_{10}	
Downwind	0.023	0.112***
	(0.019)	(0.031)
# of Observations	13,244	5,520
	Outcome 3: Log CO	
Downwind	0.049**	0.089**
	(0.021)	(0.035)
# of Observations	13,966	2,777
	Outcome 4: Log NO_2	
Downwind	0.047	0.078
	(0.035)	(0.089)
# of Observations	5,805	590
Month FE	Yes	Yes
Site FE	Yes	Yes
Total # of monitors	15	5

Notes: The data used in this table is 2010 hourly MADIS wind monitor data merged with hourly pollution monitor data for all monitors within 0.4 miles of a highway. Each row and column shows results from a different regression. Downwind is an indicator for the pollution monitor being downwind in that hour. Within 0.4 miles, there are five PM10 monitors, three NO_2 monitors, and seven CO monitors. Within 0.1 miles, there are two PM_{10} monitors, two CO monitors, and one NO_2 monitors. Standard errors (in parentheses) are clustered at the site-date level.

Figures

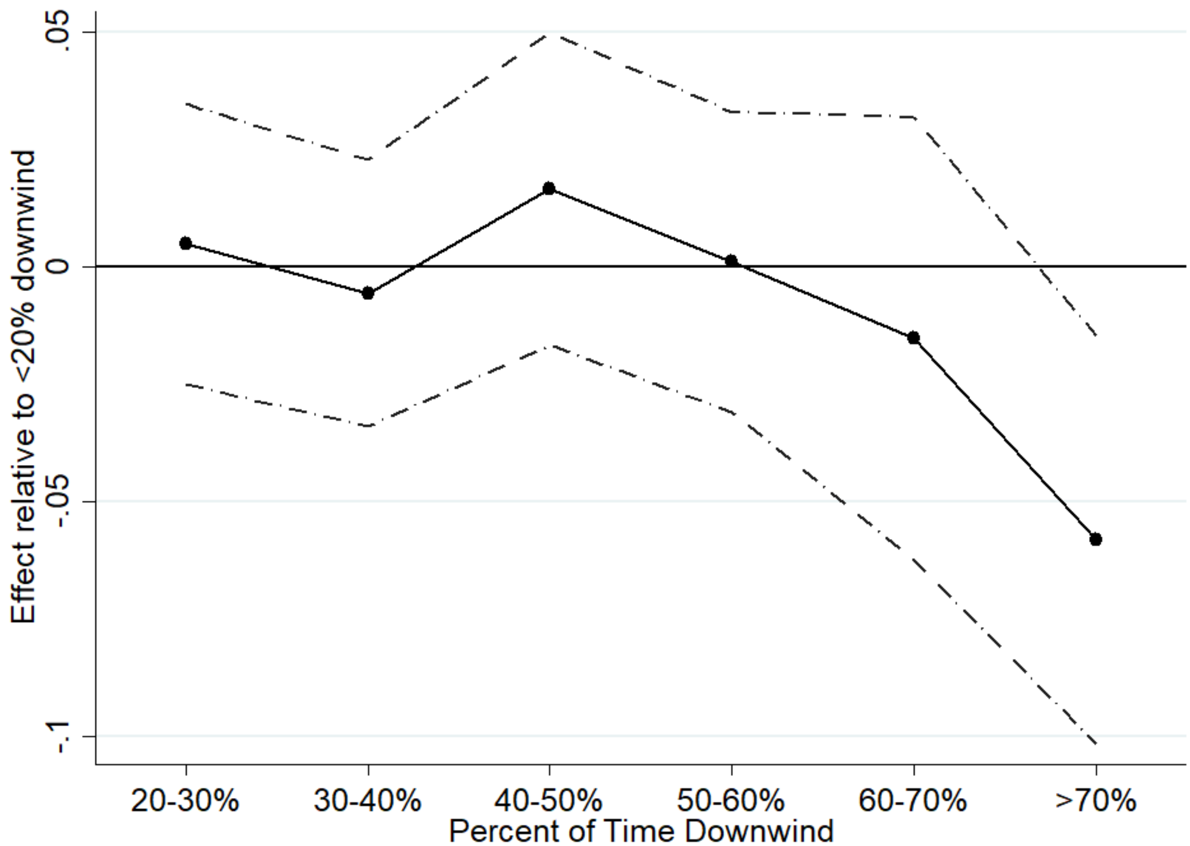
Figure 1: Identification of upwind and downwind schools

Panel A Panel B



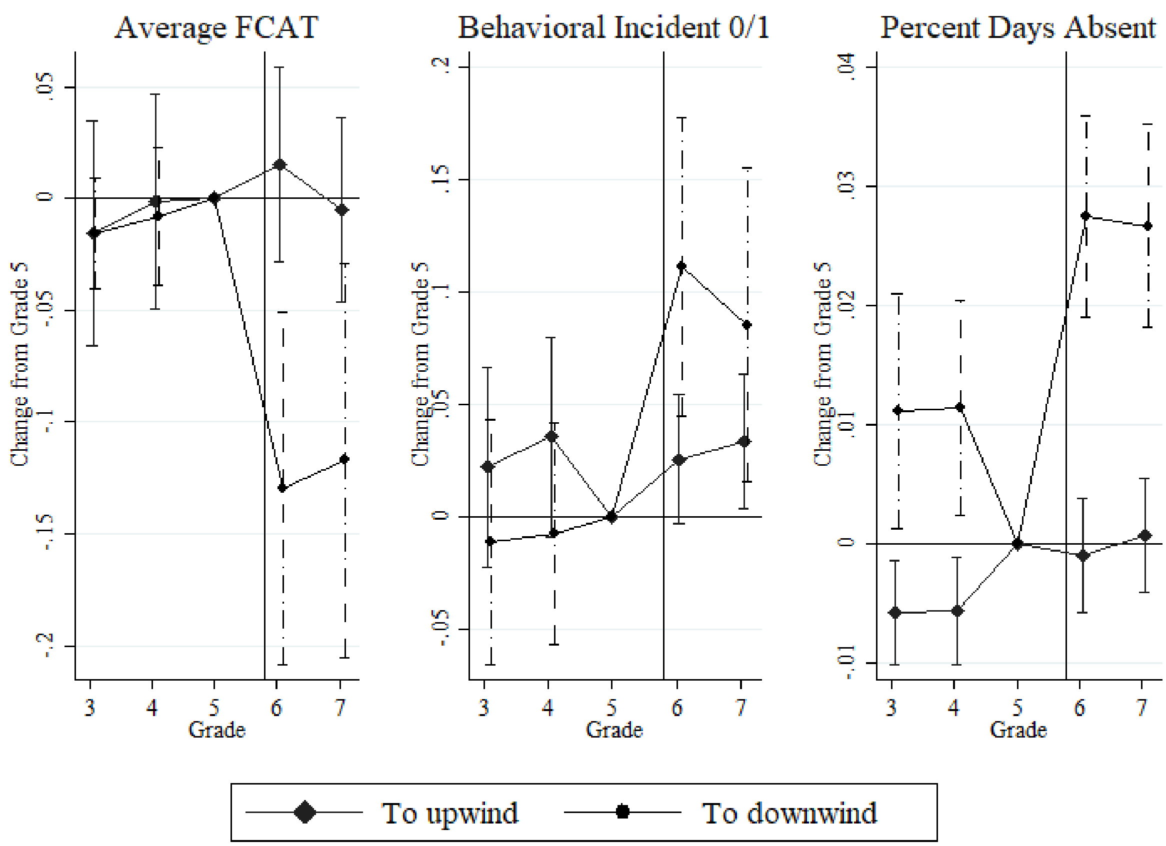
Notes: Panel A is an anonymized map of a portion of Florida that shows the relationship of some of the schools in our sample (the blue dots) relative to a major highway (the solid gray lines). Panel B is a map created from the Florida Department of Transportation shape files showing the interstates (in black) and U.S. highways (in gray) in Florida, which we define as “major highways” for the purpose of our research design.

Figure 2: Effects by percent of time downwind



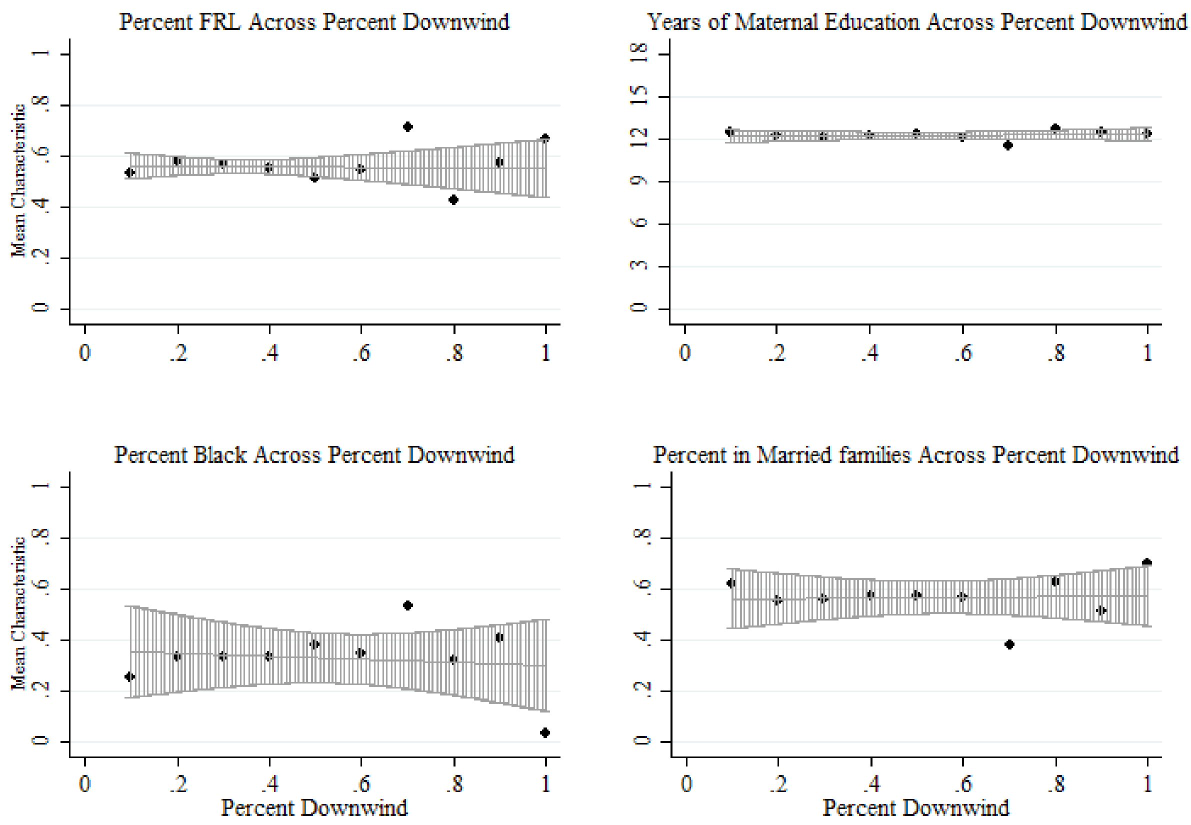
Notes: This figure shows the effect of percent of time downwind relative to a major highway on average FCAT for those moves generated by a "policy-induced" move to middle or high school. Each point plots the coefficient on a dummy for that bin of percent of time downwind. All models include grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, distance from nearest highway dummies, a grade-moved indicator, the number of highways within a mile, school demographic characteristics, other school-level characteristics (percent of teachers with a master's degree, size, and stability rate), and whether the student was on FRL that year. Includes 95% confidence intervals based on standard errors clustered on zip and student.

Figure 3: Sixth grade move event study by mover type



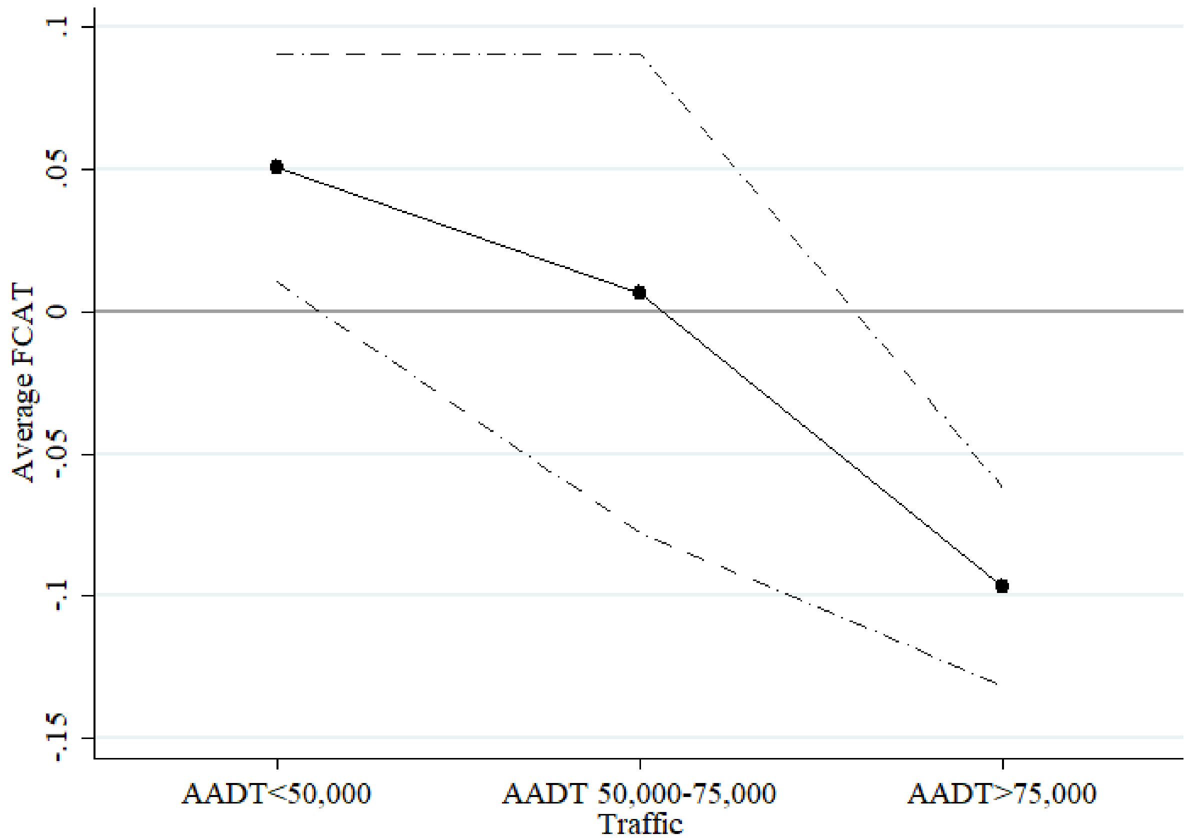
Note: This figure shows the effect of a move out of fifth grade of a student transitioning either to or from a school that is downwind more than 60% of the time. The Y-axis plots interactions between being in a given grade and the type of mover. Being a student who does not switch wind status is the excluded group to avoid collinearity with the student fixed effects. All models include grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, distance from nearest highway dummies, a year-mover indicator, the number of highways within a mile, school demographic characteristics, other school-level characteristics (percent of teachers with a master's degree, size, and stability rate), and whether the student was on FRL that year. Includes 95% confidence intervals based on standard errors clustered on zip and student. See the text for more details.

Figure 4: School demographics over percent of time downwind



Notes: This figure plots mean demographic characteristic for each school in our sample relative to the percent of time the school spends downwind of nearby highways. We fit a line to the data (the red lines), and plot its associated standard errors. Percent time spent downwind is based on 2010 wind monitor data matched with school location relative to the five nearest major highways.

Figure 5: Effects of being downwind 60% of the time by traffic



Notes: This figure shows how our estimates vary based on the effect of being downwind of the closest road segment with varying levels of average annual daily traffic (AADT) counts. AADT data comes from 2010 FDOT traffic monitor data. The Y-axis plots the coefficients on the interaction between being downwind of a road segment with the stated AADT bin. This model include AADT bin dummies, grade fixed effects, zip code fixed effects, student fixed effects, year fixed effects, distance from nearest highway dummies, a grade-moved indicator, the number of highways within a mile, school demographic characteristics, other school-level characteristics (percent of teachers with a master's degree, size, and stability rate), and whether the student was on FRL that year. Includes 95% confidence intervals based on standard errors clustered on zip and student. The mean AADT is 27,535 for the <50,000 bin, 59,160 for the AADT 50,000–75,000 bin, and 165,086 for the AADT >75,000 bin.