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School Buses, Diesel Emissions, and Respiratory Health

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ABSTRACT

School buses contribute disproportionately to ambient air quality, pollute near schools and residential areas, and their emissions collect within passenger cabins. This paper examines the impact of school bus emissions reductions programs on health outcomes. A key contribution relative to the broader literature is that we examine localized pollution reduction programs at a fine level of aggregation. We find that school bus retrofits induced reductions in bronchitis, asthma, and pneumonia incidence for at-risk populations. Back of the envelope calculations suggest conservative benefit-cost ratios between 7:1 and16:1.

1. Introduction

Pollution regulations are controversial, and economists and policy-makers debate their efficiency and cost effectiveness. Most economic evaluations of environmental quality examine the impact of ambient air pollution on health outcomes.[1] These studies are important for understanding national policy, but they are unlikely to shed light on programs targeting localized pollution exposure because widely dispersed ambient air quality monitors hide large local differences in pollution. Moreover, localized pollution policies may be especially effective at the margin; the basic insight is that abating pollution where people live, work, and study may return large benefits per dollar of cost.

This paper studies the health impacts and cost effectiveness of a new localized emissions reduction program that retrofits diesel school buses with aggressive pollution control technologies. We focus on school buses for several reasons. First, the particulate matter and air toxics common in diesel pollution may be responsible for as many as 15,000 premature deaths annually. In some regions, diesel mortality levels are similar to those of traffic accidents and second-hand smoke (CA Air Resources Board 2002). Second, school buses are ubiquitous. In 2005, buses carried nearly 25 million children between 5 and 6 billion miles in the United States. Third, school buses are disproportionately dirty. The national average bus age is over 9 years, and estimates suggest that the average school bus emits twice as many contaminants per mile as the average tractor-trailer truck (Monahan 2006). Fourth, school bus pollution has important local effects. In contrast to most diesel vehicles, buses primarily travel through residential areas and so individuals who are sensitive to pollution may be affected by bus emissions

[1] Notable studies in this vein include Chay et al. (2003), Chay and Greenstone (2003), Neidell (2004), Currie and Neidell (2005), Currie et al. (2009), and Janke et al. (2009).

where they live. Diesel air pollutants also collect inside of passenger cabins and in schoolyards, so school-aged children may be further impacted. Recent research finds that within-bus concentrations of particulate matter and air toxics were 4 to 12 times higher than ambient pollution levels.

Despite the potentially large health benefits of school bus retrofit programs, we know very little about their impacts. The dearth of empirical studies stems from at least two challenges. First, many of these programs are relatively new and data are scarce. Our study uses a comprehensive dataset on bus retrofits from the state of Washington, and detailed information includes retrofit type, retrofit date, and retrofit cost. We combine the novel program data with comprehensive morbidity and demographic data at the school district level. Second, statistical identification can be challenging. Health outcomes may drive program adoption or significant unobservables may influence both program adoption and health outcomes. We exploit a natural experiment and employ a differences-in-differences research design to help isolate causal impacts. Treatment school districts retrofitted eligible buses by the end of our sample period and control school districts retrofitted no buses by the end of our sample period. Identification exploits differences in adoption timing, rather than the adoption decision itself, as nearly all non-adopters began retrofits shortly after our sample period ends.

We find that school bus retrofits induced statistically significant and large reductions in bronchitis, asthma, and pneumonia incidence for both children and adults with chronic respiratory conditions. Empirical magnitudes are typically larger for children's health outcomes than for chronically-ill adult outcomes. Results, especially for asthma and bronchitis incidence, are robust to several falsification and sensitivity checks.

Most notably, while adopters and non-adopters experienced differential trends in health outcomes over the retrofit period, adopters and non-adopters experienced comparable trends in the pre-retrofit period. Adopters and non-adopters also experienced comparable trends over the retrofit period for illnesses plausibly unrelated to air quality.

To put our results in context, we combine our empirical results with the cost-of-treatment health valuation literature and perform a back of the envelope benefit-cost analysis. We conservatively estimate program benefits between 7 and 16 times program costs. This interpretation suggests that if the many states not aggressively pursuing school bus retrofits were to do so, social benefits are potentially large. Buses are an inexpensive and safe means of transport (in an accident sense), but our results suggest that they could be made safer (in the broadest sense) at modest cost.

We believe our analysis makes three contributions. First, our data and methods permit the first empirical economic assessment of the impact of *school bus retrofit programs* on morbidity outcomes. Second, we show that *local pollution policies* can significantly impact public health, and that these programs may produce a large "bang per buck." Third, our results may provide additional evidence on the broader effects of *air pollution on health*.[2] We cannot directly trace retrofit programs to lower ambient pollution levels, since our spatial unit of analysis is significantly smaller than the spatial distribution of pollution monitors. However, we do show that a program targeting air pollution exposure significantly reduces illnesses plausibly related to air quality (and only those illnesses).

[2] In this sense, our paper is in the spirit of recent work by Currie and Walker (2011), Schlenker and Walker (2010), and Moretti and Neidell (2011).

The paper proceeds as follows. Section 2 provides institutional detail on school buses, diesel emissions, retrofit programs, and respiratory health. Section 3 describes our unique retrofit, demographic, weather, and health data. Section 4 presents our empirical methods and Section 5 presents key results. Section 6 explores our identification and other empirical assumptions. Section 7 provides a conservative back of the envelope benefit-cost assessment and concludes.

2. Background

School buses and diesel emissions

Diesel emissions make up a substantial portion of ambient air pollution. Particulate matter from diesel engines accounts for 26 percent of total air pollution from fuel combustion and 66 percent of particulate air pollution from on-road sources (American Lung Association (2008)). On-road mobile sources emissions are often the largest single source of air pollution in a region.

School buses are common and polluting. In 2005, 25 million children traveled between 5 and 6 billion miles on school buses in the United States. Median routes for many of the buses in our sample were approximately 6 miles each way. The average child riding these buses spent nearly 45 minutes per day on the bus (Adar et al. (2008)). The average bus age in the United States is over 9 years, and the average is substantially higher in many states. Research indicates that, per mile, school buses are twice as polluting as semi trucks. The average bus emits nearly 15 pounds of particulate matter and approximately 400 pounds of smog-forming nitrogen oxides and hydrocarbons per year (Monahan 2006).

In addition to affecting background ambient air quality, school bus diesel pollution has important local effects. Since buses travel through residential areas, their

emissions may impact at-risk individuals at a neighborhood level. Research indicates that people living near roads are exposed to pollution levels that are significantly greater than broad ambient levels (Pearson et al. (2000) and Wilhelm and Ritz (2003)). Further, emissions from groups of buses idling outside schools can concentrate pollution within schoolyards and schools themselves.

Pollution exposure may be particularly high for children who ride buses. Air pollution concentrations inside mobile sources may be as much as 10 times background ambient levels (Shikiya et al. (1989), Chan et al. (1991) and Lawryk et al. (1996)). Diesel emissions collect through mechanisms such as direct flows from leaks or cracks in the crankcase or exhaust system. Such leaks or cracks may be more common in school buses than in other vehicles, as school bus engines are often less regularly maintained (Behrentz 2004). Adar et al. (2008) installed pollution monitors in a subset of the vehicles in our study. Their estimates suggest that within-bus concentrations of harmful particulates were more than twice roadway concentrations and 4 times ambient levels. Related studies found that within-school bus concentrations of particulate matter and air toxics were 4 to 12 times higher than ambient levels (Wargo et. al. (2002) and Sabin et. al. (2005)).

Diesel emissions and respiratory health

Diesel fumes contain high levels of particulate matter, other air toxics, nitrogen oxides, and hydrocarbons. Even at relatively low levels, these contaminants are known to exacerbate or cause asthma and other respiratory ailments. Daily changes in air pollution have been linked to daily changes in mortality, hospital admissions, and other public health indicators ((Spix et al. (1998), Brunekreef and Holgate (2002), Dockery (2009)).). Air toxics defined broadly are associated with asthma, lung inflammation, coughing,

wheezing, and reduced lung function (Peden (2002)). The fine particulate matter common in diesel emissions is linked to reduced lung function and increased incidences of pneumonia (Cohen and Nikula (1999) and Mcreanor et al. (2007)). Nitrogen oxides cause ground-level ozone, and high ozone concentrations are associated with aggravated respiratory illness and increased respiratory symptoms.

All children are potentially susceptible to the adverse effects of particulates and ozone (Committee on Environmental Health (2004)). Impacts on children are due to ongoing physiological respiratory development, smaller average lung size, and increased activity levels. The fine particulates contained in diesel exhaust have been shown to contribute significantly to children's morbidity and mortality, especially reduced lung function and lung growth (Gauderman et al. (2000) and Gauderman et al. (2004)). Diesel fumes can also increase the severity of children's asthma and can induce asthma in otherwise healthy children (McConnell et al. (2002) and Peters et al. (2004)). In contrast to children's morbidity, adult morbidity is pronounced only for individuals with pre-existing respiratory ailments.

Clean school bus initiatives

This study examines the impacts of the Washington State Clean School Bus Program. Washington was an aggressive early adopter of retrofits, but their program is otherwise similar to those under consideration or under way in other states. State senate bill ESSB6072 provided $5 million in annual funding for the five years spanning 2003-2008. The legislation's primary goal was to retrofit older school buses with modern pollution control equipment. Legislative priorities included targeting buses with model years prior to 1994, since retrofitting older buses yields greater emissions reductions than

retrofitting newer buses (Boyer and Lyons (2004)).

Under ESSB6072, the state offered school districts complete retrofit rebates. Further, a small number of districts were eligible for federal funding from the US Environmental Protection Agency's Clean School Bus USA Program. Washington's Department of Ecology (DOE) and the state's seven air quality control agencies administered the program. Washington emphasized retrofits, and approximately 88 percent of expenditures were devoted to equipment installations in the program's first operational year. About 7 percent of expenditures were devoted to administration and less than 5 percent went to low sulfur diesel fuel programs.

Adopting districts typically began retrofits by installing diesel oxidation catalysts (DOCs). DOCs are add-on ceramic substrate devices that catalyze chemical reactions in emissions, breaking down harmful pollutants into less harmful substances. On average, DOCs are expected to reduce particulates from these vehicles by approximately 20-30 percent and hydrocarbons by as much as 50-70 percent.

More recently, the program coupled DOC retrofits with additional crankcase ventilation filter (CCV) retrofits. The primary advantage of CCVs is that they reduce within-bus pollution. CCVs are add-on devices that filter unburned fuel and blow-by gases from the crankcase, the largest chamber of most diesel engines. When CCVs are installed, they are almost always coupled with diesel oxidation catalysts. CCVs are expected to reduce PM levels by 10-20 percent more than DOCs alone and may reduce within-bus concentrations by much greater margins.[3]

[3] After our sample period ends, the state moved towards greater use of low sulfur diesel fuel and greater use of diesel particulate filters. Both were extremely rare during our sample period, and remain relatively rare. However, increased penetration of these pollution control methods suggests that the marginal benefits of conventional retrofits will decrease over time.

Retrofit timing and scope

According to interviews with program staff members, the total number of districts retrofitting at a given time was largely determined by external budget considerations. While the state initially promised $5 million in regular installments, actual funding arrived at irregular and unpredictable intervals. Thus, the actual timing of program adoption across districts was exogenous to the districts themselves.

The order in which districts participated was driven by many factors. Program managers approached school district administrators early in our sample period. Most administrators offered permission to proceed with retrofits provided their bus fleet managers also agreed. A few administrators were slow in responding to agency requests. Conversations with school administrators suggested that unrelated administrative burdens may explain slow response during the sample period. Some districts' mechanics, once reached, immediately agreed to start retrofits. Many, however, wanted to see evidence that other districts had adopted the program without substantial disruptions to their buses or normal work schedules. These fleet managers eventually came on board, but often required sustained persuasion over time including repeated visits by DOE staff.

Once a district began the retrofit process, implementation scope was largely determined by available technologies and bus characteristics. Program managers and contractors maintained that buses with model years prior to 1982 were largely unsuited for retrofits. Diesel oxidation catalysts alone were most appropriate for buses with model years from 1982 to 1987. For most buses with model years later than 1987, DOCs coupled with CCVs were the appropriate technologies. Retrofitting newer buses with DOCs or CCVs was rarely cost effective. Since technology and bus fleet characteristics

usually determined the number of buses retrofitted and retrofit type, program scope was essentially exogenous to the district. As a rule, adopters retrofitted all suitable buses.

Linking retrofits and health

To summarize much of the preceding background section, school bus retrofits should substantially reduce emissions of diesel-related toxics, particulates, and other contaminants. These emissions reductions should in turn improve the respiratory health of at-risk individuals. First, bus tailpipe emissions impact background air quality as well as localized air quality in residential neighborhoods and near schools. The retrofits may therefore reduce adverse respiratory health outcomes for both adults with chronic respiratory illness and children. Second, since bus emissions may concentrate within passenger cabins and in schoolyards, retrofits should further reduce children's pollution exposure.

Two details bear noting. First, the relative health benefits of retrofits for children versus adults with chronic conditions are unknown a priori. While children may experience reduced exposure from retrofits through improvements in both inside-bus and outside-bus air quality, individuals with chronic conditions may be especially sensitive to changes in contaminant exposure. Second, health outcomes may respond quickly to changes in diesel emissions exposure. The medical literature indicates that hospital admissions and other public health outcomes respond to day-to-day variations in air pollution (Spix et al. (1998), Brunekreef and Holgate (2002), Dockery (2009)).

Our background section also raises an additional issue. While many elements that influenced adoption timing and scope were plausibly exogenous, some factors that influenced adoption status may have been nonrandom and correlated with other elements

influencing health. A plausible research design must therefore be robust to some non-random assignment. We adopt a natural experiment estimation strategy that is robust to endogeneity in levels and we explore the validity of our identifying assumptions in detail.

3. Data

Our data allow the first empirical assessment of the health benefits stemming from school bus retrofit programs. We use program data from the Washington State Department of Ecology and the Puget Sound Clean Air Agency. We use hospital discharge data from the Washington State Department of Health. We also augment retrofit and health data with demographic data from the National Center of Educational Statistics and temperature and precipitation data from the US Historical Climatology Network.

Our administrative retrofit database consists of approximately 4000 buses in 53 school districts of the Puget Sound area. We focus on the state of Washington because it is an innovator in school bus programs. We examine specifically the Puget Sound region due to extensive administrative data collection and the relative homogeneity of districts. For each bus in the area, we observe information related to equipment installations such as the retrofit type (DOC/CCV), the date of the retrofit, and the cost of the retrofit. Bus-specific data are aggregated to the district level to construct a fleet profile consisting of the cumulative share of buses having undergone each type of retrofit.

Data on health outcomes from 1996 to 2006 were extracted from the Washington State Comprehensive Hospital Abstract Reporting System (CHARS). CHARS data include a complete record of hospital inpatient discharges. Each observation consists of treatment date, patient age, patient sex, patient home zip code, and a detailed diagnosis

code. Illness-specific data is aggregated to the month level by summing the number of illnesses in each diagnosis code for each zip code over individual days. We obtain a profile of respiratory illness incidence for at-risk populations (which we define to be children and adults with chronic conditions) within each zip code each month.

Since we focus on an air pollution control program, our primary analysis emphasizes the major respiratory ailments: bronchitis, asthma, pleurisy, and pneumonia.[4] Bronchitis, asthma, and pneumonia outcomes for at-risk populations have been linked to air contaminants. To the best of our knowledge, pleurisy has not been linked to the pollutants common in diesel exhaust. However, CHARS diagnosis codes aggregate pneumonia with pleurisy.[5] Any potential measurement error from including pleurisy biases our program impacts towards zero and reduces statistical precision. No related measurement error is present for the analysis of asthma and bronchitis.

We match all data, including retrofit, health, demographic, and weather data, at the *school-district by month* level. Details of the matching procedure are provided in the data appendix. Bus fleets and retrofit programs are managed at the district level, so data analysis at spatial scales finer than school districts (e.g. zip codes or schools) would artificially inflate precision. Some health outcomes are rare events, so data analysis at temporal scales finer than months (e.g. days or weeks) would not ensure meaningful variation in outcome measures.

4. Methods

[4] The relevant diagnosis-related group (DRG) codes are 089, 091, 096, and 098. For each disease category, we consider the diagnosis group for children and for adults with chronic conditions.

[5] Pleurisy is an inflammation of the mucus membrane enveloping the lungs and rib cage. Individuals suffering from pleurisy typically report difficult and painful breathing.

Our goal is to assess the impact of retrofits on health outcomes for those districts that retrofitted eligible buses. Since we have a panel dataset, it is perhaps tempting to assess these impacts by simply comparing adopting districts' pre-retrofit health outcomes with post-retrofit health outcomes. However, we cannot attribute changes in health outcomes after the retrofits to the retrofits alone. The clearest way to isolate causal effects of the retrofits, accounting for confounding factors, would be to examine outcome differences between randomly assigned adopter districts and non-adopter districts over time. While this is not possible ex-post, our methods mimic this general structure.

Two-Period, Two-Group Difference-in-Differences

Our first empirical design is a standard two-period difference-in-differences approach. Here, we examine differential trends for adopter districts and non-adopter districts over time. If non-adopters provide information on the expected health outcome trends for adopters had adoption not occurred, the quasi-experiment afforded by the difference-in-differences in outcomes across adopting and non-adopting districts should remove the effect of confounding factors and isolate the effect of bus retrofits on health outcomes. Note that the analysis that follows exploits differences in adoption timing rather than differences in adoption decisions, as most non-adopters began retrofits after our sample period ended.

Some notation is helpful in presenting the estimator. Two groups $g \in [a,b]$ experience health outcomes y in two periods $t \in [1,2]$. Y^R is the health outcome in the presence of the retrofit treatment and Y^{NR} is the health outcome in the absence of the retrofit treatment. Group a is the non-adopter control group, group b is the adopter

treatment group, t=1 is the pre-treatment time period, and t=2 is the post-treatment time period. The treatment is observed only if g=b and t=2.

In our two period difference-in-differences context, time t=1 corresponds to 2002, the year before retrofits began in earnest, and time t=2 corresponds to 2006, the last sample year. Group g=b typically corresponds to the 34 districts that had completed significant retrofits by the beginning of the 2006/2007 school year. In practice, these treatment districts retrofitted more than 30 percent of their total bus fleet with diesel oxidation catalysts (DOCs) and/or crankcase ventilation filters (CCVs) by the beginning of the 2006/2007 school year. Alternatively, group g=b may correspond to the 8 district subset of the broader treatment group that had significantly augmented DOC retrofits with more aggressive CCV retrofits. In practice, these alternative treatment districts retrofitted more than 10 percent of their total bus fleet with CCVs by the beginning of the 2006/2007 school year. In all cases, group g=a corresponds to the 9 districts that retrofitted no buses by the start of the 2006/2007 school year. Districts that are neither treatment nor control are dropped from the main two period, two group analysis.

In the most basic model, the average treatment effect on the treated in the presence of retrofits can be written as:

$$(1)\ \tau^{DID} = E[Y_{b2}^{R}] - E[Y_{b2}^{NR}] = E[Y_{b2}] - E[Y_{b1}] - (E[Y_{a2}] - E[Y_{a1}]).$$

A regression analog to this model allows us to control for observable differences in the distribution of characteristics of the treatment and control groups. This regression model is parameterized following the difference-in-differences literature, and can be written as:

$$(2)\ y = \alpha + \delta t + \gamma g + \beta t \cdot g + \pi X + \varepsilon,$$

where y continues to represent health outcomes. In practice, we normalize respiratory health outcomes as the ratio of illness cases to affected population numbers.[6] X represents a vector of control variables including per capita income, poverty status, racial composition, and school staff per student ratios. ε represents the standard idiosyncratic disturbance term. The coefficient δ represents the effect of time on health outcomes for the non-treated group and γ represents the effect of the treatment on health outcomes in the pre-treatment period. β is the coefficient of interest, and it is our estimator for the difference-in-differences effect of the treatment on the treated (the regression analog of τ^{DID}).

The key difference-in-difference assumption is that non-adopters would experience the same trends in health outcomes as adopters absent the treatment, after conditioning on observables. Table 1 shows that demographic characteristics are generally similar across adoption classification groups. Adopters and non-adopters have statistically indistinguishable per capita income, poverty status, racial composition, and school staff per student ratios. Moreover, bus fleet average age is similar between adopter and non-adopter districts. Treatment districts are systematically larger, but we analyze health outcome variables scaled by population size. While demographics are generally similar across adoption classification groups, the final rows of Table 1 indicate that respiratory health treatments are significantly more common among early adopting districts. This suggests that it is possible that adoption timing across districts was not purely random. However, this is not necessarily a concern. Our difference-in-differences strategy is robustness to endogeneity in health levels; initial health may differ across

[6] For children, the ratio is respiratory illness cases among children per 100,000 children. For adults, the ratio is respiratory cases among adults with chronic conditions per 100,000 adults.

groups as long as expected trends (conditional on the covariates) are expected to be similar across the groups absent the treatment. We test this key assumption for pre-treatment periods, and our later sensitivity section devotes considerable attention to the plausibility of all identifying assumptions in the model.

In all two period difference-in-differences analyses, we omit July and August from the sample since schools were not in session during those months. The resulting general dataset consists of 860 observations: we observe 34 treatment districts and 9 control districts over 10 months in 2002 (before the retrofits) and over 10 months in 2006 (after the retrofits). A parallel dataset for examining the impact of retrofits that emphasize more aggressive crankcase ventilation filter (CCV) installations consists of 340 observations: we observe 8 treatment districts and 9 control districts over 10 months in 2002 (before the retrofits) and over 10 months in 2006 (after the retrofits).

In all two period analyses, we cluster standard errors to allow for arbitrary within-group correlations at the district level. We cluster over individual districts as they represent the unit of analysis. Clustered standard errors are then used to test difference-in-difference hypotheses against one-sided alternatives. We hypothesize that our key difference-in-differences results (the impacts of retrofits on illness outcomes) will be negative, so the appropriate alternative hypothesis is a non-negative coefficient.

Results from two-period, two-group difference-in-differences approaches are presented in Table 2. Clustered standard errors are reported in parentheses below the coefficient estimates. We defer interpretation of our key two-period, two-group difference-in-difference coefficients to the next section, but we note here the impact of controls on health outcomes. Districts with higher proportions of their population below

the poverty line experienced more respiratory ailments on average, especially for pleurisy and pneumonia. Districts with greater staff-student ratios and higher percentages of white students experienced fewer respiratory ailments on average. Interestingly, districts with older bus fleets experienced statistically more respiratory ailments on average, supporting the basic hypothesis that diesel school buses impact respiratory health for children and adults with chronic conditions. Warmer and wetter districts, on average, experienced fewer respiratory ailments on average.

Multiple Period Approaches

We believe the two-period, two-group difference-in-differences approach outlined above offers several advantages. First, it facilitates a transparent econometric analysis and generates readily interpretable empirical estimates (i.e. 'what happens if an average district retrofits its eligible buses?'). Second, the validity of the identifying assumptions can be more directly assessed. Third, the two-period, two-group approach imposes very little parametric structure on the problem. Imposing linearity, which implies the relationship between a district's share of buses retrofitted and health outcomes is exactly the same at every retrofit level and in every time period, is a strong assumption. Precise dose-response relationships in the real world are very difficult to understand a priori, so we prefer a specification that imposes little structure on relationships about which we have no functional form guidance.

Nevertheless, the intuition of the difference-in-differences identification strategy can be applied in more general settings with multiple time periods and multiple treatment classifications. This approach imposes significantly more structure on the problem, but exploits more information about the extent and exact timing of retrofit adoption. Our

multiple time period regression model, for district *i* in month *m* of year *t*, can be most generally written as:

$$(3)\ y_{itm} = \alpha_i + \lambda_t + \beta R_{itm} + \pi X_{itm} + \varepsilon_{itm},$$

where *y* continues to represent health outcomes scaled by affected population numbers. α_i are district-level fixed effects, which control for average group status and time invariant factors like income, poverty status, racial composition, school staff ratios, and relative bus fleet age. λ_t are year (time) dummies and *X* represents a vector of time varying controls including month dummies and weather variables. ε continues to represent an idiosyncratic disturbance term.

In (3), *R* is a retrofit policy variable that takes one of four possible forms. First, the policy variable may be an indicator variable for a district that had completed significant retrofits of any kind by month *m* of year *t*. This variable takes a value of 1 when a district retrofitted more than 30 percent of their total bus fleet with diesel oxidation catalysts (DOCs) and/or crankcase ventilation filters (CCVs). Second, the policy variable may be an indicator variable for a district that had completed significant CCV retrofits by month *m* of year *t*. This variable takes a value of 1 when the district retrofitted more than 10 percent of their total bus fleet with CCVs. Third, the policy variable may represent a district's continuous share of buses retrofitted with any technology prior to (inclusive of) month *m* of year *t*. Finally, the policy variable may represent a district's continuous share of buses retrofitted with CCVs prior to (inclusive of) month *m* of year *t*.

In all multiple period analyses, we omit July and August from the sample since schools were not in session during those months. The resulting dataset consists of 5830

observations: we observe all 53 districts over 10 months for each of the 11 years spanning 1996 (the earliest year for which we were able to obtain health data) to 2006 (the last year we were able to obtain health and program data).

We again cluster standard errors to allow for arbitrary within-group correlations at the district level. We cluster over individual districts as they represent the unit of analysis. Clustered standard errors are then used to test program impact hypotheses against one-sided alternatives. We continue to hypothesize that the impacts of retrofits on illness outcomes will be negative, so the appropriate alternative hypothesis is a non-negative coefficient.

Results from multiple period regression specifications are presented in Table 3. Clustered standard errors are reported in parentheses below the coefficient estimates. We defer interpretation of our key multiple group multiple time period difference-in-difference coefficients to the next section, but we note here the impact of several controls. There were no obvious monotonic trends in respiratory ailments across years. Within years, respiratory ailments tended to be lowest in the late fall. Finally, on average, warmer districts experienced fewer respiratory ailments.

5. Results

The two-period, two-group difference-in-differences coefficients presented in row 1 of Table 2 are all large in magnitude and for the most part statistically significant. Results in columns 1-3 provide the impact of significant retrofits for adopting districts on asthma and bronchitis outcomes when treatment classification is defined by the adoption of any type of retrofit technology. After controlling for changes to a quasi-control group, adopter districts experienced 2.9 fewer bronchitis and asthma cases per 100,000

individuals per month. More specifically, after controlling for confounders, adopter districts experienced 5.4 fewer asthma and bronchitis cases per 100,000 children per month and 1.8 fewer asthma and bronchitis cases for those with chronic conditions per 100,000 adults per month. These are substantial changes: 5.4 cases per 100,000 children per month is a 23 percent drop relative to pre-retrofit (2002) levels for the adopter districts.

Results in columns 7-9 of Table 2 provide the impact of significant retrofits on asthma and bronchitis outcomes for adopting districts, when treatment classification is defined only by the adoption of more aggressive crankcase ventilation filter (CCV) installations. Recall that CCV retrofits typically augment diesel oxidation catalysts with add-on devices that further reduce contaminants, especially within buses. After controlling for changes to a quasi-control group, CCV adopter districts experienced 4.3 fewer bronchitis and asthma cases per 100,000 individuals per month. More specifically, after controlling for confounders, CCV adopter districts experienced 10.2 fewer asthma and bronchitis cases per 100,000 children per month and 1.8 fewer asthma and bronchitis cases for those with chronic conditions per 100,000 adults per month. Again, these are substantial changes: 10.2 cases per 100,000 children per month is a 33 percent drop relative to pre-retrofit (2002) levels for the CCV adopter districts.

Results in columns 4-6 and 10-12 of Table 2 provide the impact of significant retrofits on pneumonia and pleurisy outcomes for adopting districts. Treatment classification in columns 4-6 is defined by the adoption of any type of retrofit technology and treatment classification in columns 10-12 is defined only by the adoption of more aggressive crankcase ventilation filter (CCV) installations. In general, the results suggest

that retrofits influence pneumonia outcomes, but perhaps only for children. After controlling for changes to a quasi-control group, adopter districts experienced 4.1 fewer pneumonia cases per 100,000 children per month. CCV adopter districts experienced 5.1 fewer pneumonia cases per 100,000 children per month. For context, note that 4.1 cases per 100,000 children is a 37 percent drop relative to pre-retrofit (2002) levels for the 'any technology adopter' districts and 5.1 cases per 100,000 children is a 40 percent drop relative to pre-retrofit (2002) levels for the 'CCV adopter' districts. In contrast to results for children, results presenting the impact of retrofits on pneumonia outcomes for adults with chronic conditions are statistically insignificant and often small relative to pre-retrofit levels.

Multiple period regression results in Table 3 suggest that asthma and bronchitis results are reasonably robust to specification. Results in column 1 indicate that a district that retrofits more than 30 percent of its bus fleet with any technology experiences 0.81 fewer asthma and bronchitis cases per 100,000 individuals per month. These results suggest that, on average, a retrofitting district experiences an approximately 10 percent reduction in asthma and bronchitis cases (across both children and adults with chronic conditions) relative to its pre-adoption (2002) baseline levels. Results in column 2 indicate that a 10 percent increase in the share of buses retrofitted with any technology is associated with 0.194 fewer bronchitis or asthma cases per month. These results suggest that, on average, a retrofitting district experiences an approximately 2.3 percent reduction in asthma and bronchitis cases (across both children and adults with chronic conditions) for every 10% of its bus fleet that is retrofitted. Bronchitis and asthma effects of CCV installations, presented in columns 3 and 4 of Table 3, are similar in magnitude to 'any

retrofit technology' results. These CCV results are not, however, statistically significant. This may reflect smaller statistical variation in technology adoption for CCVs or non-linearity in dose-responses relationships for CCV installations.[7]

Multiple period regression results in columns 5-8 of Table 3 indicate that pneumonia and pleurisy results are not robust to the multiple period specifications. While all of the difference-in-differences coefficients are negative, none are statistically significantly different from zero. This is not necessarily surprising, since pleurisy has not been linked to pollutants commonly found in diesel exhaust. Measurement error induced by Department of Health data aggregations can substantially reduce statistical precision. Note, however, that coefficient magnitudes for the CCV retrofit impacts are broadly similar to asthma and bronchitis results. For example, results in column 8 indicate that a 10 percent increase in the share of buses retrofitted with CCVs is associated with 0.312 fewer pneumonia cases per month. These results suggest that, on average, a retrofitting district experiences an approximately 1.7 percent reduction in pneumonia cases (across both children and adults with chronic conditions) for every 10% of its bus fleet that is retrofitted.

6. Robustness

Robustness: Identifying Assumptions

[7] In continuous specifications, we observe 53 districts over 10 months (July/August omitted) for each of the 11 years spanning 1996 to 2006. Within and between variations for key variables are as follows. Bronchitis and asthma cases among at-risk groups – 8.14, 2.41; Pleurisy and pneumonia cases among at-risk groups – 15.21, 4.03; discrete adoption for all retrofits – 0.29, 0.08; continuous adoption for all retrofits – 0.15, 0.04; discrete adoption for CCV retrofits – 0.14, 0.06; continuous adoption for CCV retrofits – 0.06, 0.02. Two issues bear noting. First, within variation is always greater than between variation, suggesting that fixed effects models generally have a source of variation. Second, variation for all retrofit variables is substantially greater than variation for CCV retrofit variables. This smaller variation may affect statistical precision in CCV multiple period regression results.

As is often the case in real world policy evaluations, our treatment status may not be randomly assigned. The difference-in-differences research design, however, still estimates the policy relevant average treatment effect on the treated *as long as health trends are uncorrelated with adoption*. This section explores the plausibility of our identifying assumptions. For transparency, we focus all robustness checks on the simpler two-period, two-group model. Note that these falsification test results are not sensitive to modeling choices.

What are the concerns? It is possible *a priori* that some group-specific trends are correlated with treatment status. Suppose, for example, that some unobserved factor was associated with both a decrease in health incidences and an increased likelihood of school bus retrofits. Active policy-makers might implement other air pollution or public health policies in conjunction with clean school bus programs. Or, suppose that school districts that experienced or expected falling health outcomes over time were more likely to adopt Clean School Bus programs. Finally, suppose that illness incidence was declining for both adopter and non-adopters due to unobservables but adopters' incidence was declining faster due to higher initial illness incidence and non-linear responses to unobservables. In all of these cases, difference-in-difference results may be biased. However, some simple sensitivity analyses alleviate these concerns:

- *Adopters and non-adopters experienced the same trends in health outcomes until retrofits were actually installed.* We replicated our analysis in periods before adoption occurred. Columns 1 through 6 of Table 4 show that adopter districts had similar changes in respiratory ailments to non-adopter districts between 1996 and 2001, before retrofits began in earnest. Other pre-retrofit year comparisons

yield similar results. As before, adopters sometimes had significantly higher initial levels of respiratory illness, but difference-in-difference coefficients were always insignificant. Standard errors were similar in magnitude or larger than the estimated coefficients themselves. In other words, improvements in health outcomes for adopters relative to non-adopters only appear over the sample period when retrofits were installed.

- *For illnesses plausibly unrelated to air quality, adopters and non-adopters experienced similar health trends over the sample period.* We replicated our difference-in-differences regressions for gastrointestinal diseases and kidney/urinary tract illnesses.[8] These are the most commonly treated non-respiratory ailments. Results in columns 7 through 12 of Table 4 show no significant differences in other illness changes between treated and quasi-control districts over our sample period, even though initial health outcome levels can differ. In other words, treatment and control districts experienced differential trends for diseases plausibly related to air quality, and only for diseases plausibly related to air quality.
- *For individuals plausibly unaffected by air quality, adopter and non-adopter districts experienced similar health trends over the sample period.* We replicated our difference-in-differences regressions for adults without chronic respiratory conditions. These individuals should not be sensitive to marginal changes in air quality. Results in columns 13 and 14 of Table 4 show no consistent difference in major respiratory ailment differences for healthy adults between treatment and control groups. In other words, treatment and control districts experienced

[8] The relevant diagnosis-related group (DRG) codes are 182, 184, 320, and 322.

differential respiratory health trends for individuals plausibly sensitive to air quality, and only for individuals plausibly sensitive to air quality.

- *Adopters and non-adopters experienced the same trends in health outcomes during summer months, when buses operate much less frequently.* We replicated our analysis, but only for months when school was not in session. In other words, we performed a falsification test leveraging the fact that buses run infrequently during the summer. Results indicated that adopters have statistical significantly higher initial levels of respiratory illness during summer months, but the key difference-in-difference coefficients were always statistically insignificant. In other words, significant improvements in health outcomes for adopters relative to non-adopters only appear in months where buses are active.[9]
- *Non-adopters do not systematically experience falling respiratory health outcomes.* Results on the post-treatment dummy in Tables 4 and 2 indicate that non-adopters experienced no statistically significant trends in respiratory health outcomes over the pre-retrofit period (1996-2001) or over the retrofit sample period (2002-2006). As a result, it is not possible that illness incidence was declining for all districts due to unobservables. Our results cannot be explained by non-linear responses to common factors causing illness declines in all districts.
- *Adopters and non-adopters do not systematically differ on most important characteristics.* Results in Table 1 show that demographic characteristics are similar across adoption classification. Adopters and non-adopters have statistically indistinguishable per capita income, poverty status, racial

[9] A relatively quick change in the response of health outcomes to changes in exposure is consistent with epidemiological evidence on the short-run effects of air pollution (Spix et al. (1998), Brunekreef and Holgate (2002), Dockery (2009)).

composition, and school staff per student ratios. Bus fleet average age is also similar between adopter and non-adopter districts. Treatment districts are systematically larger, but we analyze health outcome variables scaled by population size. Again note that most non-adopters adopt the program shortly after our sample period ends.

- *School bus retrofit decisions and implementation occur at the school district level*. In contrast, most air quality and public health programs are instituted at the county, state, or national level.

Robustness: Specification

In the two-period – two-group analysis, treatment status does not vary within the year. A possible concern is that district by month cells induce artificial statistical precision. To address this we use clustered standard errors, which allow for arbitrary within-district serial correlation. Nevertheless, as a sensitivity check, we repeated our two-period analysis using annual data. Results are in the first six columns of Appendix Table A1. Point estimates were very slightly smaller and standard error magnitudes were very slightly larger, but results were identical for all practical purposes.

In the two-period – two-group analysis, we omit districts with retrofits in progress from the treatment group. These districts are included in the multiple period analysis. Nevertheless, as a sensitivity check, we repeated the two-period analysis including omitted districts in the treatment group. Results are in the latter six columns of Appendix Table A1. As expected with a less cleanly identified control group (which now includes districts with retrofits in progress), difference-in-differences point estimates were smaller

in absolute value. Nevertheless, signs and statistical significance were similar to presented results.

In the two-period – two-group analysis, as well as the multiple period – two-group analysis, our 'adopter' thresholds are admittedly ad hoc. As a sensitivity check, we repeated all two-group analyses using both lower and higher adoption thresholds. Specifically, we increased the adoption threshold from thirty to forty percent and then subsequently decreased the adoption threshold from thirty to twenty percent. Results are in Appendix Table A2. Patterns of statistical significance were unchanged. As expected, lower adoption thresholds generally yielded somewhat smaller health impact point estimates and higher adoption thresholds yielded somewhat larger health impact point estimates.

In the two-period, two-group analysis, we scale our dependent variable by population. This control function approach is an especially flexible way to account for the influence of population. Further, our multiple period analysis contains fixed effects which inherently condition on factors like population. Nevertheless, as a sensitivity check, we repeated the two-period analysis with a specification that uses a dependent variable that is not scaled by population and includes an explanatory variable for population. Results are in Appendix Table A3. Note that coefficient magnitudes are not directly comparable to coefficient magnitudes in the main results. Sign patterns, however, were identical and results were more significant statistically.

7. Discussion and Interpretation

This paper analyzes the impact of school bus emissions reductions on human health. We find that school bus retrofits induced statistically significant and large

reductions in bronchitis, asthma, and pneumonia incidence for children and adults with chronic conditions. Broad findings are robust to a host of sensitivity checks, including falsification tests examining the pre-retrofit period, diseases not associated with pollution, and individuals not sensitive to marginal changes in air quality. Results for bronchitis and asthma are very robust to specification as well; results for pneumonia are less so.

Findings from our preferred specifications indicate that adopter districts experienced 23 percent fewer children's bronchitis and asthma cases per month, relative to a control group. These same districts also experienced 37 percent fewer children's pneumonia cases per month. We also typically find larger effects for the CCV retrofits, suggesting that the more modern crankcase ventilation filters may play a larger role in health improvements than diesel oxidation catalysts alone.

Asthma and bronchitis illness reductions occurred for both children and adults with chronic conditions. This suggests that clean school bus programs may impact background and localized air quality. However, when we detect significant pneumonia illness reductions, they occur for children but not for adults with chronic conditions. While there are several plausible explanations, these results are consistent with school bus programs further impacting public health through reductions in within-bus exposure.

Our analysis permits the first empirical economic assessment of school bus programs and illness outcomes. However, we note several limitations. First, we do not directly observe individual-level health and bus ridership data. We assume that children ride buses near where they live and that the share of bus ridership across treatment and control districts does not differentially change over time. Second, our analysis only considers acute, short-run responses to pollution exposure. Cumulative effects may also

matter; to the extent that these responses are important, our program impact results are understated. Third, we are unable to directly examine the impact of bus programs on air quality. Only seven air pollution monitors regularly measure air quality in the entire four-county Puget Sound region, and most have incomplete data, so we cannot separately match monitors to treatment and control areas. Fourth, people with vulnerable children may move into retrofitting districts. We do not see evidence for selective migration on a large-scale, as student populations at non-adopting districts grew by 6.3 percent over the retrofit period while student populations at adopting districts grew by only 2.2 percent. To the extent that migration as averting behavior is important, however, our program impact results are understated.

We also note caveats to external validity. First, Washington's respiratory illness rates are among the highest in the nation. Second, the Puget Sound region of Washington is whiter, wealthier, and less dense than most other urban areas in the United States. Extrapolating the numerical benefits of retrofits from our dataset to other contexts may misrepresent the case. Nevertheless, we would be surprised if overall policy implications differed substantially across the country since detected retrofit benefits were very large in our study area.

Several interesting directions for future research arise from this analysis. First, Currie et al. (2009) show that pollution influences school absences, so absences might provide some evidence on the human capital impacts of retrofits programs. Such an exploration is beyond the scope of this paper, however, as the Washington state schools in our sample were not required to track excused absences for our sample period. Second, we would ideally compare our results to a "cash for clunkers" school bus program or to a

low sulfur diesel school bus program. Unfortunately, we do not have the data to run the necessary empirical evaluations ourselves, and we are unaware of any other studies that provide the necessary empirical estimates for these comparisons.

Our empirical analysis investigates the impact of retrofits on incidences of bronchitis, asthma, and pneumonia severe enough to warrant hospital or clinical treatment. Retrofit impacts for less severe respiratory illnesses are unobserved. Further, the relationships between health outcomes and communicable disease transmission, pain and suffering considerations, and long-term welfare effects are complex. Addressing the full benefits and costs of bus retrofits are beyond the scope of this study. However, in order to provide an approximate guide to the economic significance of our results, we combine our empirical point estimates with cost-of-treatment health valuation estimates and observed retrofit costs to compute a conservative back of the envelope benefit-cost assessment of school bus retrofits.

The medical literature estimates health care costs per inpatient episode of bronchitis, asthma, and pneumonia at approximately $3000-$7000/visit. See, for example, Stanford et al. (1999) and Lave et al. (1999), for a more complete discussion. The average school district in our dataset serves approximately 10000 children. For the average district, our crankcase ventilation filter (CCV) coefficients translate approximately into 12.2 avoided hospital visits for children's asthma and bronchitis per year.[10] Similarly, CCV coefficients translate approximately into 6.1 avoided hospital visits for children's pneumonia and pleurisy per year. Estimates of a single district's annual benefits for children's health from observed CCV adoption range from

[10] The relevant DID coefficient in column 8 of Table 2 is -10.2 cases per 100000 students. (10.2/100000) × 10000 students × 12 months = 12.2.

approximately $54,900 to $128,100 (18.3 visits times $3000/visit and 18.3 times $7000/visit). Again, these benefit calculations exclude non-respiratory illnesses, long-term health effects, suffering considerations, and any impacts on adults with chronic respiratory conditions.

The average CCV adopting district retrofitted approximately 25 eligible buses of its 66 total buses with CCVs over the sample period. Each CCV retrofit cost approximately $1200 in total, including parts, labor, and testing. Most CCVs are coupled with pre-existing DOCs. Each DOC retrofit cost approximately $1300 in total, including parts, labor, and testing. Therefore, the average adopter school district spent approximately $62,500 (25 buses times $2500) on CCV retrofits. *Total CCV retrofit costs are likely less than annual benefits for children alone.* Assuming a 5 percent discount rate and a useful retrofit life of 10 years, the net present value children's health benefits are between 424,000 and 989,000 dollars per adopter school district.[11] Even *excluding benefits to adults with chronic conditions and omitting suffering considerations, the ratio of present value benefits to present value costs ranges between 7:1 and 16:1.* This interpretation suggests that if the many states not aggressively pursuing school bus retrofits were to do so, potential social benefits are likely to be large.

For perspective, best estimates suggest that the benefit-cost ratios of the 1990 Clean Air Act amendments are between 1:1 and 4:1 (U.S. EPA (1999), Portney (2000)). Like most major pollution control programs, the goals of the Clean Air Act are improving

[11] For a district, the range of the NPV of benefits is based on the upper and lower bounds of per incident health care costs (7000 and 3000) respectively. NPVs are calculated as $\left(\frac{18.3*3000}{.05}(1-\frac{1}{1.05^{10}})\right)$ and $\left(\frac{18.3*7000}{.05}(1-\frac{1}{1.05^{10}})\right)$.

ambient air quality. The difference between our back of the envelope calculations and these estimates suggests that, on the margin, policies targeting localized air pollution may be particularly cost effective relative to ambient air pollution policies.

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Table 1. Summary Statistics for the Baseline (Pre-Adoption) Year

Characteristic	The 53 Full Sample Districts	The 9 Non-Adopter Districts	The 34 Adopter Districts	Difference: Adopter vs. Non-Adopter	p-value for Difference
Student population	11239	2335	10597	-8262	0.01
Per capita income	24841	23609	24166	-557	0.79
Percent of pop. below poverty line	.074	.086	.075	.011	0.48
School staff members per student	.115	.120	.100	.020	0.15
Percent white	.799	.777	.737	.040	0.42
School Bus Fleet Age	8.92	8.94	9.59	-0.65	0.52
Children's Bronchitis and Asthma cases (per 100,000 children)	22.83	14.22	23.87	-9.64	0.01
Chronic Adult Bronchitis and Asthma cases (per 100,000 adults)	2.38	0.68	2.73	-2.04	0.01
Children's Pneumonia and Pleurisy cases (per 100,000 children)	9.28	3.29	11.11	-7.82	0.01
Chronic Adult Pneumonia and Pleurisy cases (per 100,000 adults)	19.27	14.32	20.38	-6.06	0.01

Table 2. Results: Two period, Two Group Difference-in-Differences

	All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases			CCV Retrofits Bronchitis & Asthma Cases			CCV Retrofits Pleurisy & Pneumonia Cases		
	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness
Diff-in-Diff	**-2.88****	**-5.35**	**-1.80****	**-3.94**	**-4.14****	**-3.20**	**-4.33*****	**-10.2****	**-1.83****	**-5.44***	**-5.06***	**-5.13**
	(1.47)	**(5.22)**	**(0.85)**	**(3.30)**	**(2.34)**	**(4.34)**	**(1.66)**	**(5.96)**	**(0.79)**	**(3.64)**	**(3.30)**	**(4.41)**
Treatment Group	2.55	3.13	1.81**	4.21	6.78***	3.16	3.26**	7.17	1.28*	0.76	6.46**	-1.44
	(1.64)	(5.60)	(0.87)	(2.57)	(2.27)	(3.00)	(1.35)	(4.34)	(0.69)	(3.40)	(3.00)	(4.50)
Post-Treatment	0.62	-1.47	1.29*	2.91	-0.37	3.76	0.78	-0.98	1.30	2.97	-0.41	3.92
	(1.31)	(5.02)	(0.73)	(3.15)	(2.07)	(4.01)	(1.37)	(5.24)	(0.79)	(3.62)	(2.20)	(4.21)
Per Capita Income	-0.00	0.00	0.00	0.00	0.00	0.00	0.00**	0.00**	0.00	0.00	0.00***	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
% Below Poverty	12.2	46.8	4.18	95.3**	20.9	120**	33.5	180	4.90	35.9	2.68	43.0
	(23.8)	(49.7)	(22.5)	(45.5)	(38.8)	(55.6)	(42.0)	(139)	(17.4)	(115)	(50.6)	(144)
Staff per Student	-23.9	-80.8*	-2.50	-69.9**	-26.8	-88.8**	-28.7	-123	-5.02	-39.1	-11.6	-50.0
	(15.4)	(42.2)	(12.2)	(29.1)	(19.3)	(34.5)	(22.5)	(73.7)	(9.89)	(52.9)	(28.4)	(66.3)
Percent White	-5.76*	-28.3***	-0.77	-1.98	1.95	4.17	-12.2*	-39.6*	-3.25	-9.46	-10.5	-7.92
	(2.92)	(9.32)	(1.66)	(6.44)	(5.00)	(7.97)	(6.15)	(19.0)	(2.79)	(15.4)	(6.24)	(20.2)
Average Bus Age	0.29**	0.90**	0.09**	1.11***	0.40	1.37**	0.56	1.21	0.18	2.29**	0.73	2.94*
	(0.11)	(0.40)	(0.04)	(0.41)	(0.25)	(0.61)	(0.36)	(1.00)	(0.18)	(1.06)	(0.46)	(1.50)
Temperature	-0.02***	-0.06***	-0.01***	-0.04***	-0.04***	-0.03**	-0.03***	-0.08***	-0.01**	-0.04**	-0.03***	-0.05
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)	(0.03)
Precipitation	-0.01*	-0.01	-0.00***	-0.00	-0.00	0.00	-0.00**	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Constant	21.8***	68.8***	6.05**	19.7**	23.7***	17.4	18.5**	53.3**	5.64***	21.3	12.9**	24.5
	(4.16)	(12.1)	(2.42)	(9.52)	(6.65)	(12.2)	(6.61)	(24.1)	(1.90)	(17.2)	(5.39)	(22.4)
Obs.	860	860	860	860	860	860	340	340	340	340	340	340
F-statistic	14.2	12.0	10.1	10.2	10.7	8.69	13.6	10.4	8.87	8.61	21.7	6.19
Prob > F	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.

Table 3. Results: Multiple-Period Regression Specifications

	All Retrofits Bronchitis & Asthma Cases Among all At-risk Groups		CCV Retrofits Bronchitis & Asthma Cases Among all At-risk Groups		All Retrofits Pleurisy & Pneumonia Cases Among all At-risk Groups		CCV Retrofits Pleurisy & Pneumonia Cases Among all At-risk Groups	
	Discrete Specification	Continuous Specification	Discrete Specification	Continuous Specification	Discrete Specification	Continuous Specification	Discrete Specification	Continuous Specification
Retrofits	**-0.81****	**-1.94****	**-0.79**	**-1.09**	**-0.10**	**-0.02**	**-0.71**	**-3.12**
	(0.50)	**(1.03)**	**(0.81)**	**(1.91)**	**(0.96)**	**(1.97)**	**(1.55)**	**(3.65)**
Constant	Constant Included		Constant Included		Constant Included		Constant Included	
District Fixed Effects	52 District Fixed Effects		52 District Fixed Effects		52 District Fixed Effects		52 District Fixed Effects	
Year Fixed Effects	10 Year Fixed Effects		10 Year Fixed Effects		10 Year Fixed Effects		10 Year Fixed Effects	
Month Fixed Effects	9 Month Fixed Effects		9 Month Fixed Effects		9 Month Fixed Effects		9 Month Fixed Effects	
Weather	Temp. and Precip. Controls		Temp. and Precip. Controls		Temp. and Precip. Controls		Temp. and Precip. Controls	
Obs.	5830	5830	5830	5830	5830	5830	5830	5830
F-statistic	31.4	31.4	31.3	31.3	17.3	17.3	17.3	17.4
Prob>F	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.

Table 4. Results: Falsification Tests

	Pre-Retrofit Period Bronchitis & Asthma Cases			Pre-Retrofit Period Pleurisy & Pneumonia Cases			Retrofit Period Gastrointestinal Cases			Retrofit Period Kidney & Urinary Tract Cases			Healthy Adults	
	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	Bronch. & Asthma	Pleurisy & Pneum.
Diff-in-Diff	**2.66**	**3.03**	**2.50**	**3.89**	**0.52**	**4.73**	**0.02**	**-0.05**	**0.28**	**0.28**	**7.98**	**-1.49**	**-0.19**	**0.23**
	(2.29)	**(3.88)**	**(2.57)**	**(3.84)**	**(1.78)**	**(4.82)**	**(4.19)**	**(1.73)**	**(5.21)**	**(3.11)**	**(7.99)**	**(2.91)**	**(2.54)**	**(0.68)**
Treatment	-0.58	1.33	-1.43	2.62	3.25*	2.91	2.49	-1.52	3.98	2.66	-1.38	3.59**	2.53	1.05
Group	(1.32)	(2.95)	(1.42)	(2.30)	(1.80)	(2.92)	(2.37)	(1.84)	(2.83)	(1.95)	(4.34)	(1.68)	(2.42)	(1.07)
Post	-1.63	-0.51	-2.10	-0.48	-0.30	-0.15	2.80	0.46	3.51	0.86	-8.32	3.31	-2.39	-1.08
Treatment	(2.17)	(3.61)	(2.46)	(3.70)	(1.19)	(4.70)	(4.25)	(1.59)	(5.29)	(3.10)	(7.82)	(2.89)	(2.05)	(0.65)
Constant	Constant Included			Constant Included			Constant Included			Constant Included			Included	
Controls	Full Demographics Included			Full Demographics Included			Full Demographics Included			Full Demographics Included			Included	
Weather	Temp. & Precip. Included			Temp. & Precip. Included			Temp. & Precip. Included			Temp. & Precip. Included			Included	
Obs.	860	860	860	860	860	860	860	860	860	860	860	860	860	860
F-statistics	22.3	23.4	2.62	10.3	14.1	11.6	5.04	14.0	4.21	3.48	2.89	7.86	7.23	3.72
Prob > F	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.

Data Appendix

This data appendix describes how data observed at a finer geographic level (zip code and latitude/longitude) is aggregated to match our unit analysis: school districts.

Health Data

Health outcome data consists of hospital discharge records from the Washington State Department of Health. Because of confidentiality concerns, records in the database only contain home zip code information, rather than a complete street address. As we do not observe a patient's exact street address, school district boundaries (i.e. GIS layers) cannot be used to assign an individual who appears in the health data to a specific school district. As a result, we need an algorithm to map health treatments observed at the five-digit zip code level to school districts.

Our algorithm uses schools within zip codes (as opposed to surface area, for example) as assignment weights. Specifically:

1. We compile a list of all schools in the Puget Sound region, their address zip codes, and their school district.
2. For each health treatment in the health data, we use the list constructed in (1) to identify all of the schools in that patient's zip code.
3. We assign health treatments to districts based on the following rules:
    a. If all schools in the patient's zip code belong to the same district, we assign this health record fully to that district.
    b. If the patient's zip code contains schools from multiple districts, we assign shares of the outcome to each district in proportion to the share of schools (per district) in the patient's zip code. For example, if a zip code contains 2 schools from district A and 3 schools from district B, we would assign 2/5ths of all health outcomes in that zip code to district A and 3/5ths to district B.
    c. If there are no schools physically located in a patient's zip code, we assign the health treatment for this patient to the nearest school district, as measured by distance from the centroid of the zip code to the edge of a school district.

Weather Data

All regression specifications contain mean monthly temperature and total monthly precipitation derived from the US Historical Climatology Network. To map from individual weather stations to school districts, we simply assign school districts to the closest weather station, where "closest" is determined by the Vincenty formulae for geodesic distances. In the final analysis, data from 10 distinct weather stations in the Puget Sound region are matched to the 53 school districts for each month.

Appendix Table A1. Specification Sensitivity Results

	ANNUAL DATA						CONTROL GROUP CONTAINS RETROFITS IN PROGRESS					
	All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases			All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases		
	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness
Diff-in-Diff	**-2.83****	**-5.18**	**-1.75****	**-3.88**	**-4.01****	**-3.15**	**-1.60***	**-2.29**	**-1.03***	**-1.75**	**-3.37****	**-0.65**
	(1.60)	**(5.63)**	**(0.93)**	**(3.59)**	**(2.49)**	**(4.74)**	**(1.00)**	**(2.99)**	**(0.64)**	**(1.85)**	**(1.52)**	**(2.61)**
Treatment Group	2.67	3.55	1.94**	4.66*	7.34***	3.54	0.68	-0.90	0.58	1.05	3.46**	0.04
	(1.63)	(5.51)	(0.92)	(2.66)	(2.31)	(3.24)	(1.10)	(3.34)	(059)	(1.82)	(1.61)	(2.35)
Post-Treatment	0.33	-2.74	1.02	2.91	-0.93	3.92	-0.68	-4.59*	0.51	0.81	-1.12	1.33
	(1.83)	(5.61)	(0.96)	(4.31)	(2.51)	(5.71)	(0.73)	(2.62)	(0.41)	(1.61)	(1.04)	(2.03)
Full Controls		INCLUDED			INCLUDED			INCLUDED			INCLUDED	
Constant		INCLUDED			INCLUDED			INCLUDED			INCLUDED	
Obs.	86	86	86	86	86	86	1060	1060	1060	1060	1060	1060

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.

Appendix Table A2. Specification Sensitivity Results

	HIGHER THRESHOLD FOR 'ADOPTER' STATUS						LOWER THRESHOLD FOR 'ADOPTER' STATUS					
	All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases			All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases		
	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness
Diff-in-Diff	**-2.91****	**-4.88**	**-1.93****	**-3.82**	**-5.01****	**-2.59**	**-2.81****	**-5.30**	**-1.76****	**-3.99**	**-3.67***	**-3.48**
	(1.58)	**(5.34)**	**(0.97)**	**(3.40)**	**(2.50)**	**(4.60)**	**(1.43)**	**(5.16)**	**(0.81)**	**(3.26)**	**(2.27)**	**(4.24)**
Treatment Group	2.49	2.87	1.80**	3.67	7.43***	2.07	2.50	2.84	1.91**	4.53*	6.45***	3.68
	(1.54)	(5.05)	(0.79)	(2.75)	(2.08)	(3.39)	(1.63)	(5.68)	(0.80)	(2.47)	(2.21)	(2.79)
Post-Treatment	0.67	-1.28	1.29*	2.98	-0.34	3.85	0.58	-1.59	1.27	2.90	-0.35	3.74
	(1.32)	(5.05)	(0.74)	(3.17)	(2.10)	(4.05)	(1.30)	(5.01)	(0.71)	(3.15)	(2.06)	(3.99)
Full Controls		INCLUDED			INCLUDED			INCLUDED			INCLUDED	
Constant		INCLUDED			INCLUDED			INCLUDED			INCLUDED	
Obs.	680	680	680	680	680	680	980	980	980	980	980	980

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.

Appendix Table A3. Specification Sensitivity Results

	DEPENDENT VARIABLES NOT SCALED BY POPULATION					
	All Retrofits Bronchitis & Asthma Cases			All Retrofits Pleurisy & Pneumonia Cases		
	All At-Risk Groups	Children	Adults with Chronic Illness	All At-Risk Groups	Children	Adults with Chronic Illness
Diff-in-Diff	**-1.02*** **	**-0.90*** **	**-0.12* **	**-0.67* **	**-0.43*** **	**-0.24**
	(0.23)	**(0.22)**	**(0.08)**	**(0.42)**	**(0.12)**	**(0.37)**
Treatment Group	0.41	0.32	0.12	1.17**	0.38***	0.77
	(0.30)	(0.28)	(0.10)	(0.50)	(0.13)	(0.40)
Post-Treatment	0.09	0.01	0.08	0.06	0.20	0.04
	(0.10)	(0.13)	(0.05)	(0.29)	(0.07)	(0.25)
Population	0.08***	-	-	0.15***	-	-
	(0.01)			(0.02)		
Children Population	-	0.21***	-	-	0.07***	-
		(0.02)			(0.01)	
Adult Population	-	-	0.02***	-	-	0.18***
			(0.01)			(0.02)
Full Controls		INCLUDED			INCLUDED	
Constant		INCLUDED			INCLUDED	
Obs.	860	860	860	860	860	860

Notes: clustered standard errors appear in parentheses. *,**, and *** indicate significance at the 10,5, and 1 percent levels. DID coefficients tested against one-sided alternatives.