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School district revenue shocks, resource allocations, and student achievement: Evidence from the universe of U.S. wind energy installations

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ABSTRACT

We examine the impact of wind energy installation on school district finances and student achievement using data on the timing, location, and capacity of the universe of U.S. installations from 1995 through 2016. Wind energy installation substantially increased district revenues, causing large increases in capital outlays, but only modest increases in current spending, and little to no change in class sizes or teacher salaries. We find zero impact on student test scores. Using administrative data from Texas, the country's top wind energy producer, we find zero impact of wind energy installation on high school completion and other longer-run student outcomes.

1. Introduction

There has been a resurgence in economic research over the last half decade examining whether more money in schools improves student outcomes. One group of studies examines the nationwide impact of statewide school finance reforms, answering the question of whether money matters in schools with strong external validity due to the national scope of these reforms (Jackson et al., 2016; Lafortune et al., 2018; Candelaria amd Shores, 2019; Johnson and Jackson, 2019; Biasi, 2019; Klopfer, 2017; Brunner et al., 2020). Another group of studies examines shocks to school funding in a particular state either due to a school finance reform (Hyman, 2017), a kink or quirk in the state aid formula (Kreisman and Steinberg, 2019; Gigliotti and Sorensen, 2018), local tax elections (Baron, Forthcoming), or local capital campaigns (Martorell et al., 2016; Lafortune and Schönholzer, Forthcoming). One very recent study exploits local tax elections in several states (Abott et al., 2020). These state-specific studies provide important contributions, but have weaker generalizability due to their more localized focus. School finance reform is the only studied policy to increase school funding on a national scale, and while it is an important reform, its effects on student outcomes may not generalize to other types of school revenue shocks or policies affecting school funding.[1]

In this paper, we provide evidence on the impacts of increased school funding from a novel source of variation affecting most states since the 1990s: wind energy installation. Wind energy production has grown substantially in the U.S., with less than 2 GW of capacity in 1995, and over 100 GW in 2019 (U.S. Energy Information Administration, 1995; AWEA, 2020). Wind projects represented 39 percent of new commercial energy installations in 2019, and generated $1.6 billion in revenues to states and local jurisdictions (AWEA, 2020). The growth in wind energy production over time, coupled with the significant variation both across and within states in the geographic location of wind energy production, provides an ideal setting to examine how wind energy installation has impacted school district finances and student outcomes.

We use data on the timing, location, and capacity of the universe of wind energy installations in the U.S. from 1995 through 2016 to examine the impacts of wind energy installation on school district revenues, expenditures, resource allocations, and student achievement. We geocode wind energy installations to school districts, and combine data on the timing and capacity of wind

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[1] Jackson et al. (2021) examine the closely related question of whether *decreases* in school funding matter by exploiting negative shocks to school spending due to the Great Recession. Their paper is national scale, however, examining the impacts of decreases in spending is substantively different from examining the impacts of spending increases.

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installations with National Center for Education Statistics (NCES) and Schools and Staffing Survey (SASS) school district data on revenues, expenditures, staffing, enrollments, and teacher salaries, and with student achievement data from the National Assessment of Educational Progress (NAEP) and Stanford Education Data Archive (SEDA). We use event-study and difference-in-differences methodologies that exploit the plausibly exogenous timing and location of wind energy installations.

We find that wind energy installation led to large, exogenous increases in total per-pupil revenues for districts at the mean level of installed wind capacity per pupil due to increases in local revenues, with only minimal offsetting reductions in state aid. State aid formulas often penalize locally financed increases in operating expenditures and, as such, districts spent the new revenues primarily on capital outlays, causing dramatic increases in capital expenditures, but only modest increases in current expenditures, with little to no reductions in class sizes or increases in teacher salaries. We find important heterogeneity by installed capacity per pupil: the majority of districts, with relatively larger enrollments and smaller wind installations saw only minor impacts on revenues and expenditures, while districts in approximately the top third of the per-pupil wind energy capacity distribution, with smaller enrollments and larger wind farms, experienced large effects.

Turning to student achievement, we find fairly precisely estimated zero impacts of wind energy installation on school district average test scores overall, and find no evidence of positive test score effects for those districts in the top portion of the distribution of wind energy production where revenues increased the most. To examine whether wind energy installation affected student outcomes other than test scores, we focus on Texas, which is the nation's top wind energy producer, and has administrative data on longer-run student outcomes in addition to test scores for our entire sample period. We find the same pattern of effects in Texas as we do nationwide on district revenues, expenditures, and student test scores. We also find a precisely estimated zero impact of wind energy installation on high school graduation rates, and no evidence of improvements in other outcomes, such as Advanced Placement or college entrance exam-taking.

To reduce concerns about other possible channels through which wind energy installation could affect student achievement, we show that wind energy installation had zero or only small impacts on district enrollment, student demographic composition, child poverty, and unemployment. However, wind energy installation does appear to affect other outcomes like county per-capita income and wages, and local spending on public goods aside from education, for example, hospitals and roads (Brunner et al., 2021; De Silva et al., 2016; Mauritzen, 2020). While we cannot firmly rule out the possibility that these other impacts affected student achievement, we argue that any achievement effects of such investments should be minor relative to investments in schools, and should, if anything, improve student outcomes, biasing our results upward and thus not explaining the null effects that we find on student achievement.

Finally, we explore an additional way in which school districts may benefit from wind energy installation: property tax relief. The large increases in local revenues from wind energy installation suggest that districts are not taking all of these windfalls as tax relief, but are they taking any? We use historic school district property tax rate data in Texas and Illinois to examine the impact of wind energy installation on school district property tax rates. We find that, in Illinois, districts respond to the increased revenues from wind installation by reducing their property tax rates. In Texas, where state laws incentivize districts with wind energy installations to pass new bonds to promote capital spending and to pay for these bonds by increasing property tax rates, we subsequently see tax rates slightly increase after wind energy installation.

Our study makes several contributions to the literature. First, it contributes to the environmental economics and local public finance literature examining the impacts of energy installation on local finances and welfare. Wind energy has grown significantly over the past two decades, and is now the nation's leading source of new commercial energy installation (AWEA, 2020). Given the country's transition to renewable energy, it is important to understand the effects of wind energy installation on local school districts revenues, resource allocations, and student outcomes, and to compare these impacts to those from fossil fuel development, which is in decline. An empirical advantage of studying the school finance implications of wind energy installation relative to fossil fuel development is that fossil fuel booms and busts often come with large labor market effects (e.g., Marchand and Weber, 2020), while such effects from wind turbine installation are negligible (Brown et al., 2012).[2] Prior work has examined impacts of wind energy installation on school finances in a single state, such as Texas and Oklahoma (De Silva et al., 20126; Reategui and Hendrickson, 2011; Ferrel and Conaway, 2015; Kahn, 2013; Castleberry and Greene, 2017; Loomis and Aldeman, 2011). Our study adds to this literature by estimating effects nationwide and on student achievement. The effects we find on district revenues grow over time. This stands in contrast to recent research studying the impacts of shale development for natural gas on Texas school districts, which find that revenues initially increase, but then quickly decline as drilling slows (Marchand and Weber, 2020; Weber et al., 2016). These findings suggest that the shift toward renewable power may provide a more stable revenue source for local jurisdictions than oil and gas development.[3]

Second, our paper contributes to the public economics literature on "flypaper" effects that examines whether intergovernmental grants and exogenous increases in local tax revenue "stick where they hit" rather than being crowed out by local responses, such as property tax relief. Some studies in this literature find substantial or even complete flypaper (Feiveson, 2015; Dahlberg et al., 2008), while others find little or no flypaper (Knight, 2001; Gordon, 2004; Lutz, 2010; Cascio et al., 2013). While some states, such as Illinois, reduce their local property tax rates in response to wind energy installation, the large increases in local revenue that we find imply strong flypaper effects. Further, as in other recent work (Brunner et al., 2020), we find that local context affects the extent to which revenue shocks are taken as property tax relief instead of increasing school budgets.

Finally, and perhaps most importantly, our study provides nationwide evidence on the effects of increased school spending on student achievement from an exogenous source of variation in spending other than school finance reform. The key issue with generalizability of studies examining the impacts of school spending is that studies vary along several dimensions in ways that potentially modify test scores impacts: 1) spending type (e.g., current versus capital) and amount, 2) baseline expenditures and local context (e.g., income, infrastructure quality), and 3) miscellaneous state context (e.g., school funding laws and other education policy, preferences for education, other child and family or social service policy). The generalizability of state-specific studies suffers along

[2] Supporting this point, we find a precisely estimated zero impact of wind energy installation on local unemployment.

[3] Other studies examine the impacts of fossil fuel energy production on local and school district finances, and in some cases, student achievement, in the following contexts: U.S. shale development (Newell and Raimi, 2015), hydroelectrical power in Norway (Hægeland et al. 2012), oil development in Brazil (Caselli and Michaels, 2013), and fracking in the U.S. (Cascio and Narayan, 2022; Bartik et al., 2019). Another recent study examines the impacts of large power plant openings in the U.S. on school district finances and housing values (Fraenkel and Krumholz, 2019).

this third dimension. National studies examining school finance reforms, as with any examination of a particular policy, have limited generalizability along the first and second dimensions, for example, examining the impact of (primarily) current as opposed to capital expenditures operating through increased state aid to districts. Our study also has limitations in generalizability along these first and second dimensions, namely that we examine the impacts of (primarily) capital spending in primarily rural areas. Our study improves our understanding of the impacts of school spending by providing an additional national case study using variation in spending generated from a policy other than school finance reform.

Our finding that most of the increases in school spending are devoted to capital expenditures, and that these have no discernible impacts on student outcomes, contributes to the growing literature on the impacts of capital expenditures on student achievement. There are nine relevant prior studies (to our knowledge): two focus on new school construction in impoverished urban districts with dilapidated school facilities finding strong positive impacts on student achievement from exposure to newly built schools (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, Forthcoming); three find suggestive evidence of small positive achievement impacts either after early negative effects or for specific subgroups (Hong and Zimmer, 2016; Conlin and Thompson, 2017; Rauscher, 2020); and four, studying some of the largest states in the nation (e.g., California, Texas, Ohio, and Wisconsin) find zero evidence of any impacts on student outcomes (Cellini et al., 2010; Martorell, et al., 2016; Goncalves, 2015; Baron, Forthcoming).

All of the aforementioned studies focus on a single state or school district. Our study is the first to provide nationwide evidence on the impacts of capital spending, finding that capital investments do little to improve students' academic achievement in our context. This result is consistent with the majority of the related literature, and stands in contrast only to the two studies examining impacts of expensive, newly built schools in low-income, urban areas with low baseline infrastructure quality (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, Forthcoming). The most likely explanation for why we, and other studies, find no overall impacts on achievement, while Neilson and Zimmerman (2014) and Lafortune and Schönholzer (2021) do relates back to the first and second dimensions outlined above regarding generalizability: spending amount and baseline local context. These two studies evaluate especially large spending amounts (e.g., newly constructed schools costing tens of millions of dollars) in baseline poor areas with low infrastructure quality, whereas the districts in our context typically see smaller capital investments in areas with more typical income and infrastructure quality levels. While increases in operating expenditures appear to improve student outcomes in most contexts (Jackson, 2020), our study provides additional, nationally-based support of prior work showing that the contexts in which increases in capital spending lead to improve student achievement appear to be quite limited.

2 Wind energy and tax revenue

As noted previously, wind energy production in the United States has increased substantially over the last several decades, growing from less than 2 GW of total capacity in 1995 to over 100 GW in 2019. Furthermore, there is wide variation in the geographic location of wind energy installations both within and across states. For example, wind energy currently comprises 36%, 34%, and 32% of generated electricity in Kansas, Iowa, and Oklahoma, respectively, and 3%, 0.7%, and less than 0.01% in New York, Massachusetts, and Connecticut. Commercial wind installations in the United States typically consist of many individual turbines, usually ranging in capacity from 1 to 3 megawatts (MW) each. By 2019, there were over 1,500 commercial wind installations in the United States comprised of over 61,000 individual turbines. The mean and median number of turbines in a commercial wind installation as of 2019 was 42 and 21 respectively, while the mean and median capacity of commercial wind installations was 76 and 44 MW, respectively.[4]

Fig. 1a – 1d document the geographic location and growth of wind energy production in the continental United States between 1995 and 2016. The figures illustrate installed wind turbine capacity per pupil (in kilowatts) by county and year. In 1995, wind energy production was extremely rare and was concentrated almost entirely in California and to a lesser degree in Texas. There were only 16 school districts in the U.S. with wind energy installed within their boundaries at that time. By 2002, wind energy production had begun to spread across the mid- and north-west while also expanding throughout Texas counties, affecting 99 school districts. By 2009, there were 419 affected districts, and as illustrated in Fig. 1d, by 2016, wind energy production had spread across 38 states, affecting 900 school districts, in the continental US, the main exception being the southeastern US.[5]

There is substantial variation across states in the property tax treatment of commercial wind energy installations. Specifically, as noted by the American Wind Energy Association (AWEA, 2017), property tax treatment typically falls into five broad categories: 1) states that offer no special property tax treatment, implying wind installations are taxed just like other real property; 2) states that adopted specific formulas for taxing wind energy installations; 3) states where local jurisdictions or the state have the authority to offer special property tax treatment; 4) states that utilize an income generation or production tax method for wind energy installations; and 5) states that offer full or partial property tax exemptions.[6] Furthermore, many states allow local jurisdictions to offer commercial wind installations special tax treatment through mechanisms such as payments in lieu of taxes (PILOTS), property tax abatements, and tax increment financing (See Appendix A for details on state-specific wind energy policies).

Because most school districts in the United States are independent jurisdictions with their own taxing authority, when a wind energy installation begins operation within the boundaries of a school district, the district will typically benefit financially from the expansion of its property tax base. However, the degree to which a school district benefits from a wind energy installation will depend on both the state and local laws and ordinances governing wind energy property taxation discussed above and the interaction of those laws with state school finance formulas. For example, during our sample timeframe, Kansas granted a full lifetime exemption from property tax payments on wind installations and although some wind installations made PILOT payments to hosting counties, individual school districts typically received little to no revenue from the installations. Similarly, Wyoming has a centralized system of school finance and thus any revenue that is generated from wind energy installations is captured entirely by the state and redistributed through the state's school foundation program.

Texas provides an example where state laws governing the taxation of wind energy installations and state school finance formulas result in a complicated system of local taxation of wind energy.

[4] Authors calculations based on data from the United States Wind Turbine Database (USWTDB) (Hoen et al., 2020).

[5] The primary reason that there are no wind energy installations in the Southeast is because the winds there are not strong enough. See Appendix Figures Ia – Id for analogous maps of county-level total installed wind turbine capacity, not per-pupil (in MW), which look very similar to main Figures Ia – Id.

[6] For more details on the property tax treatment of wind energy, see "Property Tax Treatment of Commercial Wind Projects", American Wind Energy Association and Polsinelli PC, 2017.

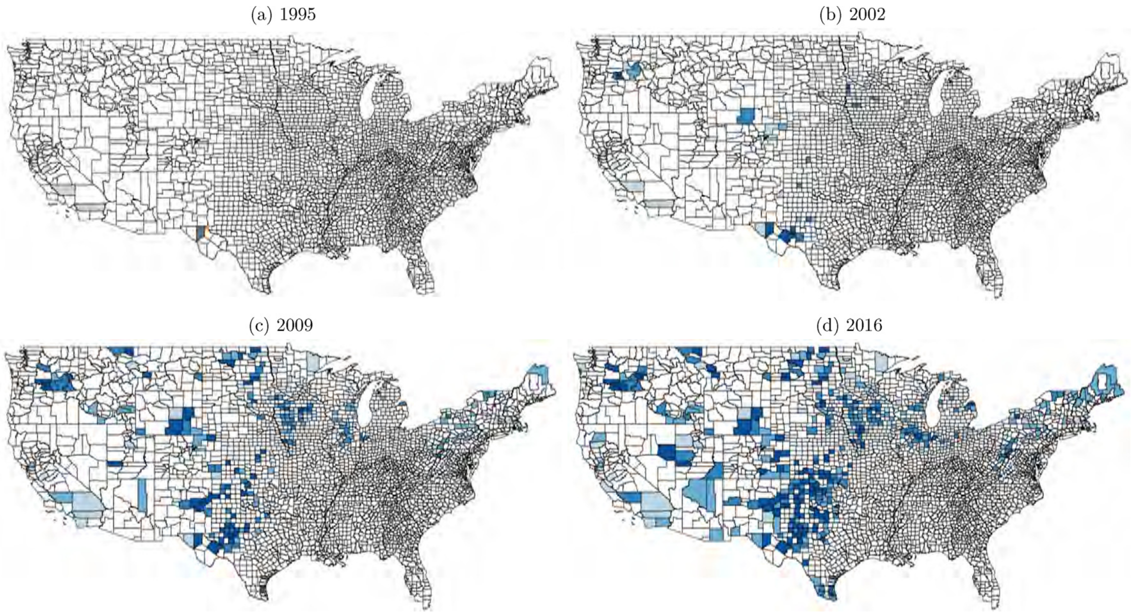

Fig. 1. United States County Map by Installed Wind Turbine Capacity Per-Pupil. Notes: Map shows installed wind turbine capacity per-pupil in kilowatts (KW) by county and year. Unshaded counties have no installed capacity. The four shades ranging from lightest to darkest represent quartiles of 2016 installed capacity per-pupil (at the county-level): <1.8 KW/pupil, 1.8–16.0 KW/pupil, 16.0–87.8 KW/pupil, and >87.8 KW/pupil, respectively.

School districts in Texas may approve a tax abatement agreement which allows a temporary, 10-year limit on the taxable value of a new wind project. These agreements, formally known as Chapter 313 agreements, apply only to school district taxes levied for maintenance and operations (M&O). Taxes for debt service, known as interest and sinking (I&S) fund payments are not subject to the limitation. Once a Chapter 313 agreement ends, most of the property tax revenue generated from a wind project goes back to the state due to the Chapter 41 Recapture law in Texas (commonly referred to as Robin Hood). Because revenue designated for I&S (debt service) is not subject to recapture and furthermore because the full increase in assessed value due to a wind project immediately goes on a school district's tax rolls for I&S, there is a strong incentive for school districts in Texas to pass a bond for school capital projects and use the wind project revenues to "subsidize" the capital improvement projects.

Appendix A provides more information on state and local laws and ordinances governing wind energy property taxation and how those laws interact with state school finance formulas. We present this information for the 21 states with the largest installed capacity as of 2018. These states account for approximately 95% of the total installed wind capacity in the nation.

2. Data

We construct an original panel dataset that combines information on: 1) the universe of wind energy installations in the continental United States; 2) school district revenues, expenditures, pupil-teacher ratios, and teacher salaries; 3) student achievement, as measured by standardized test scores; and 4) census data on the socio-economic characteristics of school districts.

National data on installed wind capacity comes from the United States Wind Turbine Database (USWTDB). The USWTDB contains information on the date each wind turbine became operational, the installed capacity of each turbine measured in kilowatts, and the longitude and latitude of each turbine. We use this information to geocode every turbine to a single school district using 1995 school district boundary files maintained by the National Center for Education Statistics (NCES). [7]We then create a panel dataset containing annual data on total installed wind capacity in each school district by aggregating information on the capacity of every turbine in operation in a school district in a given year up to the school district level.

We combine the annual data on school district installed wind capacity with annual data on district revenue and expenditures from the Local Education Agency Finance Survey (F-33) maintained by the NCES. The F-33 surveys contain detailed annual revenue and expenditure data for all school districts in the United States for our sample period of 1994–95 to 2015–16. In the empirical work that follows we utilize seven revenue and expenditure outcomes: 1) local revenue, which is primarily composed of property tax revenue; 2) state revenue, which primarily consists of state aid (grants) to local school districts; and 3) total revenue, which is the sum of local, state, and federal revenues.[8] The expenditure outcomes are: 1) current expenditures, which consists of expenditures for daily operations such as teacher salaries and supplies; 2) capital outlays, which consist of expenditures for new school construction and modernization as well as the purchase of equipment and land; 3) other expenditures, which consists of community and adult education, interest on debt, and payments to other governments (such as the state) and school systems (such as charter and private schools); and 4) total expenditures, which is the sum of current, capital, and other expenditures. We divide all of these variables by enrollment to obtain per-pupil measures and adjust them to real 2017 dollars using the Consumer Price Index (CPI).

We merge our combined dataset with several other data sources. First, for our entire sample period, we merge in data from the annual Common Core of Data (CCD) school district universe surveys that provide staff counts and teacher salary spending for

[7] The matched USWTDB and school district boundary data include 1,916 "behind the meter" turbines. Because these turbines are intended for on-site use rather than being part of a larger wind energy project designed for commercial electrical generation, we drop these turbines from the analysis. We note, however, that all of our results are robust to including them.

[8] We do not present results separately for federal revenues, because they are very small and have little to no response to wind energy installation.

every school district. We then construct district-level estimates of: 1) the pupil-teacher ratio by dividing total full-time equivalent (FTE) teachers by total district enrollment, and 2) average teacher salary by dividing total teacher salary by total FTE teachers.[9] Second, we combine our dataset with data from the Special School District Tabulations of the 1990 Census on median household income, fraction of the population at or below the poverty line, fraction white, fraction rural, fraction age 65 or older, and fraction of adults 25 and older with a Bachelor's degree.[10]

Third, we combine our dataset with additional information on teacher compensation. Teacher salaries are typically a lock-step schedule based on years of experience and whether or not a teacher has a Master's degree. While we examine impacts on district average teacher salaries provided in the CCD, average salaries conflate changes to the teacher salary schedule with changes in hiring of new teachers that are usually paid less than the average teacher in the district. Because information on district teacher salary schedules are not available in the CCD data, we use salary schedule information from the U.S. Department of Education Schools and Staffing Survey (SASS), which surveys a random cross-section of school districts every few years about staffing, salaries, and other school, district, teacher, and administrator information. We focus on district base teacher salary, which is available in every wave and particularly informative about average teacher salaries given the high rate of teacher attrition and relatively large degree of compression in teacher wages. Unfortunately, given the limited number of years and overlap of districts across waves, we lose about 94 percent of our sample size.

Finally, we use restricted-access microdata from the National Assessment of Educational Progress (NAEP) to examine student achievement. The NAEP provides math and reading test scores in grades four and eight from over 100,000 students in representative samples of school districts nationwide every other year since 1990.[11] We restrict the data to the NAEP reporting sample and to public schools. Following Lafortune et al. (2018), we then standardize students' scores by subject and grade to have a mean of zero and standard deviation of one in the first year each subject and grade was tested.[12] Standardizing the scores in this way allows our effects to be interpreted in standard deviations, and allows scores to change naturally over time reflecting learning gains or losses. We then aggregate these individual-level scores to the district-subject-grade-year level, weighting the individual scores by the individual NAEP weight.[13,14]

While the NAEP provides nationally representative test score data back to the 1990s, it suffers from small sample sizes relative to our baseline data because it is only every other year and a sample of districts.[15] We attempt to partially remedy this drawback by merging the NAEP with a newer source of test score data: the Stanford Education Data Archive (SEDA). For every state and for grades three through eight, researchers at Stanford collected district test scores since 2009 and standardized those test scores to the NAEP scaling (Reardon et al., 2018).[16] We start with the NAEP grade 4 and 8 data from 1996-2007, and then append the 2009–2016 SEDA grade 4 and 8 scores for all districts. The result is a dataset containing test scores for a sample of districts every other year from 1996-2007, and for the universe of districts every year since 2009. We standardized all scores to mean zero and standard deviation one within year, grade, and subject.

We restrict our main sample in several ways. First, we limit the sample to traditional school districts, namely elementary, secondary and unified school systems, and thus drop charter schools, college-grade systems, vocational or special education systems, non-operating school systems and educational service agencies.[17] Second, we drop states (and thus all districts within a state) without any wind energy installations over our sample time period of 1995–2016.[18] Third, because the NCES finance data tends to be noisy, we restrict the sample to school districts with enrollment of 50 students or more in every year of our sample. Fourth, we drop Kansas from the analysis since the state provides a full lifetime exemption from property tax payments, and thus school districts do not benefit from wind energy installations. We similarly drop Wyoming from the analysis because its school finance system prevents revenue generated from wind energy installations from flowing to local school districts (see Section II). We show in Table 3 that the results are only slightly smaller when we include Kansas and Wyoming, and in Appendix Fig. 2a, that there are no effects of wind energy installation on local revenues in those two states.

Our final sample consists of 11,124 school districts located in 35 states over the period 1995–2016.[19] Among the 11,124 districts in our sample, 724 had a wind energy installation at some point between 1995 and 2016. Table 1 presents summary statistics for the outcome measures used in our analysis. The table presents

[9] Because staff counts tend to be noisy, we follow Lafortune et al. (2018) and set values of the pupil teacher ratio that were in the top or bottom 2% of the within state-year distribution to missing.

[10] 1990 district demographic data is missing for a small number of school districts. Rather than excluding these districts, we matched school districts to counties and then replaced the missing district-level values of each variable with their county-level equivalent.

[11] The NAEP also tests in grade twelve and in other subjects such as writing, science, and economics, but we focus on math and reading in grades four and eight because they were tested most consistently across years.

[12] Rather than providing a single score for each student, NAEP provides random draws from each student's estimated posterior ability distribution based on their test performance and background characteristics. We use the mean of these five draws for each student, essentially creating an Empirical Bayes "shrunken" estimate of the student's latent ability.

[13] We stack scores by subject rather than averaging math and reading scores for a district-grade-year, because the tested subject alternated between math and reading in the early NAEP years. Results are almost identical if we use the mean of math and reading scores.

[14] We merge the data to our primary dataset using the NCES unique district ID that is available in the Common Core of Data (CCD) and in the NAEP data from 2000 onward. Prior to 2000, the NAEP data did not include this unique district ID. NCES provided us with a crosswalk that they developed in collaboration with Westat to link the NAEP district ID and the NCES district ID for those earlier years.

[15] Of particular concern is the number of treated ("wind") districts in this reduced sample. See Appendix Table 1 for detailed information about the number of districts and district-year observations in the NAEP and NAEP+SEDA samples. The number of wind districts observed in at least one year in our NAEP data (589) is only somewhat smaller than the number of wind districts we observe in our overall sample (724). However, the more substantial loss is in the number of observations per district, with only 6.5 yearly observations per district on average, and only 2.4 of these occurring after wind energy installation.

[16] State exams and scoring practices vary across states in many ways. The basic approach of the SEDA data is to create score distributions by state-year-grade-subject and then equate the distributions across states using NAEP scores by state-year-grade-subject as a benchmark, given that NAEP uses the same test and scoring practice nationally so can speak to cross-state differences in performance. For more details on the process of creating the SEDA data, please see: https://edopportunity.org/methods/.

[17] We also drop a small number of observations associated with the following types of educational agencies: 1) Regional education services agencies, or county superintendents serving the same purpose; 2) State-operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; 3) Federally operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; and 4) other education agencies that are not a local school district.

[18] Those states are: Alabama, Arkansas, Connecticut, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia.

[19] The states are: Arizona, California, Colorado, Delaware, Idaho, Illinois, Indiana, Iowa, Maine, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Utah, Vermont, Washington, West Virginia, and Wisconsin.

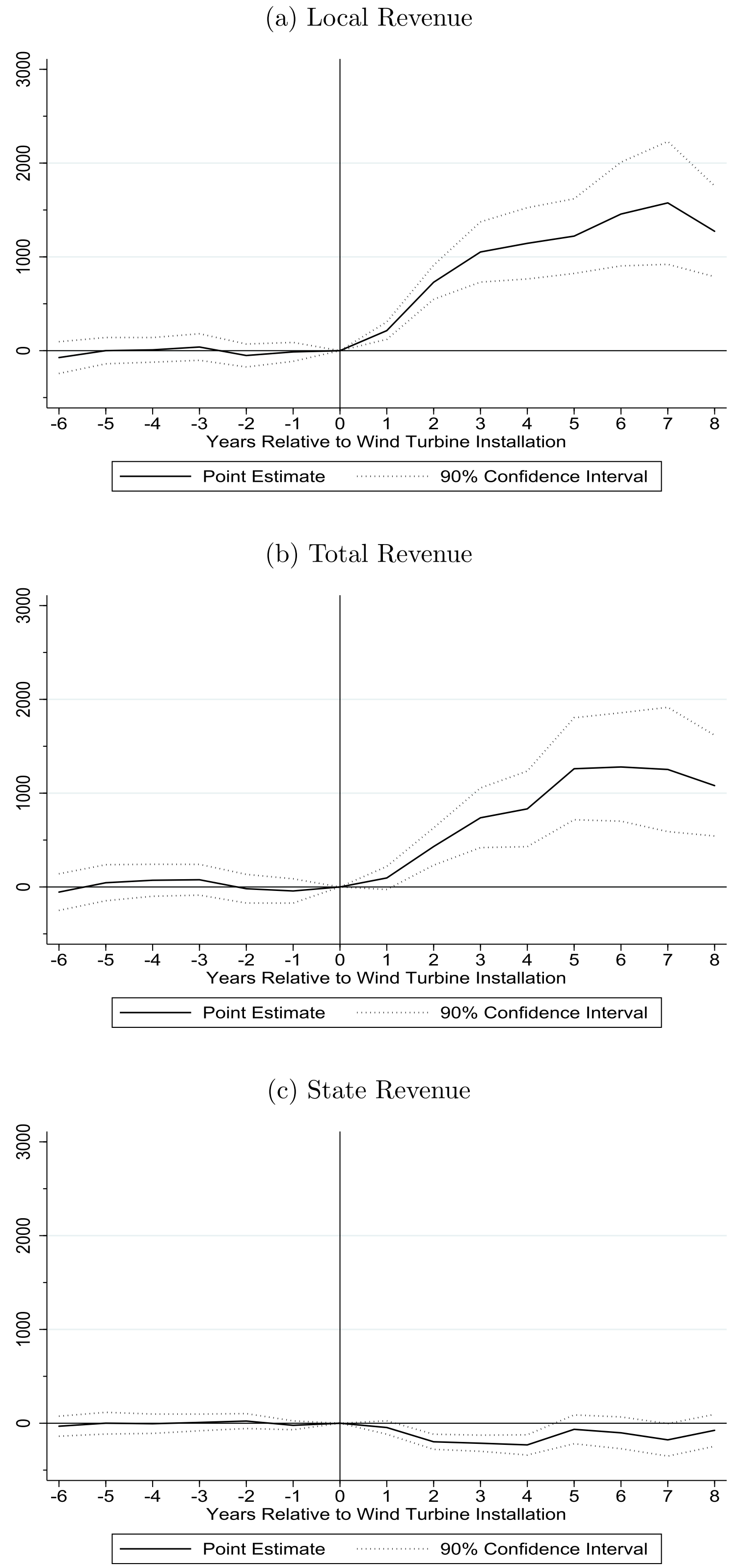

Fig. 2. Effects of Wind Turbine Installation on School District Revenues. Notes: Figures show event study estimates of the effects of wind turbine installation on per-pupil school district revenues. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

means and standard deviations for the full sample and separately for districts with and without wind energy installations. Districts with wind energy installations have slightly lower per-pupil local and total revenue and also slightly lower per-pupil total and current expenditures. Districts with wind energy installations also have lower pupil-teacher ratios and base teacher salaries.

To provide additional context about how districts with and without wind energy installations differ, Table 1 also presents

Table 1
Summary Statistics.

	Full Sample		Wind Turbine District		District Without Wind Turbine	
	Mean (1)	St. Dev. (2)	Mean (3)	St. Dev. (4)	Mean (5)	St. Dev. (6)
Per-Pupil Outcomes						
Total Revenue	13,464	5,622	13,266	5,721	13,478	5,615
Local Revenue	6,422	5,413	6,005	5,575	6,451	5,400
State Revenue	5,971	3,222	6,167	2,904	5,958	3,243
Total Expenditures	13,496	5,970	13,271	6,470	13,512	5,933
Current Expenditures	11,233	4,135	10,920	3,230	11,255	4,189
Capital Expenditures	1,256	2,586	1,346	3,211	1,249	2,537
Other Expenditures	1,008	2,178	1,005	3,116	1,008	2,097
Other Outcomes						
Pupil-Teacher Ratio	15.0	3.6	13.9	3.5	15.1	3.6
Mean Teacher Salary	67,976	18,596	62,036	14,957	68,390	18,754
Base Teacher Salary	37,506	6,242	35,442	5,414	37,666	6,273
Per-Pupil Outcomes in 1995						
Total Revenue	10,721	4,021	10,401	3,624	10,744	4,047
Local Revenue	5,471	4,325	4,857	3,770	5,515	4,359
State Revenue	4,618	2,164	4,876	1,985	4,600	2,175
Total Expenditures	10,610	3,877	10,128	2,785	10,644	3,941
Current Expenditures	9,043	2,922	8,722	2,098	9,065	2,970
Capital Expenditures	878	1,713	900	1,557	877	1,724
Other Expenditures	689	1,205	505	658	702	1,233
Other Outcomes in 1995						
Pupil-Teacher Ratio	16.3	3.6	15.3	3.4	16.4	3.6
Mean Teacher Salary	64,012	16,297	58,262	12,008	64,417	16,482
Base Teacher Salary	36,185	5,387	34,495	4,708	36,316	5,415
Control Variables						
Baseline Enrolment	2,985	14,081	2,450	7,194	3,022	14,438
Median Income in 1990	29,327	11,653	24,791	6,429	29,642	11,868
Fraction BA or Higher in 1990	0.152	0.099	0.122	0.052	0.154	0.101
Fraction Rural in 1990	0.627	0.484	0.745	0.436	0.619	0.486
Fraction White in 1990	0.883	0.176	0.890	0.181	0.882	0.176
Fraction Poor in 1990	0.134	0.099	0.151	0.089	0.133	0.100
Fraction Age 65+ in 1990	0.187	0.061	0.204	0.059	0.186	0.061
Number of Districts	11,124		724		10,400	
Number of Observations	239,518		15,596		223,922	

Notes: The sample is all school districts in the 35 continental United States that had wind energy installed between 1995 and 2016. We exclude Kansas because it provides a permanent 100% exemption on property taxation of all wind energy installation. We exclude Wyoming because its centralized school finance system causes all wind energy installation revenue to captured by the state.

summary statistics for our outcomes and control variables at baseline. For the outcome measures and enrollment, baseline corresponds to the 1994–95 year. For all the control variables other than enrollment, baseline corresponds to 1989–90. Similar to the first panels of Table 1, districts with wind energy installations have lower per-pupil local and total revenues and lower per-pupil total and current expenditures, although the differences are larger than in the first panels of Table 1. Not surprisingly, districts with wind installations tend to be smaller and significantly more rural. They also tend to contain households with lower income and lower educational attainment.

3. Methodology

To examine the effect of wind energy installation on school district revenues, expenditures and resource allocations, we employ a difference-in-differences identification strategy. We begin with a non-parametric event-study specification of the following form:

$$y_{ist} = \sum_{j=-6}^{8} \gamma_j T_{j,ist} + X_{is}\theta_t \kappa + \delta_i + \lambda_{st} + \eta_{ist} \tag{1}$$

where y_{ist} denotes an outcome of interest for district i in state s in year t; $T_{j,ist}$ represents a series of lead and lag indicator variables for when a wind energy installation became operational in district i, X_{is} is a vector of school district characteristics at baseline interacted with a linear time trend, θ_t; δ_i is a vector of school district fixed effects; λ_{st} is a vector of state-by-year fixed effects, and η_{ist} is a random disturbance term. We re-center the year a wind energy installation became operational so that $T_{0,ist}$ always equals one in the year the installation became operational in district i. We include indicator variables for 1 to 6 or more years prior to an installation becoming operational ($T_{-6,ist} - T_{-1,ist}$) and 1 to 8 or more years after the beginning of operation ($T_{1,ist} - T_{8,ist}$). Note that T_{-6ist} equals one in all years that are 6 or more years prior to the wind installation becoming operational, and $T_{8,ist}$ equals one in all years that are 8 or more years after the beginning of operation.[20] The omitted category is the year the installation became operational, T_{0ist}. 37.5% of wind districts experience multiple installations over time. In all of our analyses we consider as the year of treatment the year of the first installation, and we show in Appendix Fig. 2b that our effects are similar when dropping those 37.5% of treated districts with multiple events.

The coefficients of primary interest in equation (1) are the $\gamma_j's$, which represent the difference-in-differences estimates of the impact of wind energy installation on our outcomes of interest in each year from t_{-6} to t_8. The estimated coefficients on the lead treatment indicators ($\gamma_{-6}, ..., \gamma_{-1}$) provide evidence on whether our outcomes were trending prior to the time a wind energy installation became operational in district i. If wind energy induces exogenous increases in district revenues, expenditures etc., these lead treatment indicators should generally be small in magnitude and statistically insignificant. The lagged treatment indicators ($\gamma_{+1}, \cdots, \gamma_{+8}$) allow the effect of wind energy installations on our outcomes of interest to evolve slowly over time and in a nonparametric way. Given that treatment (wind energy installation) occurs at the district level, in all specifications we cluster the standard errors at the school district level.

The inclusion of state-by-year fixed effects in equation (1) implies that our estimates are identified off of within state variation in school district exposure to wind energy installations. Thus, our specifications control nonparametrically for differential trends in our outcomes of interest that are common to all districts within a state and across time. In our most parsimonious specification, X_{is} includes 1995 district enrollment, 1990 district median income, and the fraction of adults 25 and older who have a Bachelor's degree. We then add 1990 district fraction of the population at or below the poverty line, fraction white, fraction 65 or older, and fraction rural. We exclude time-varying characteristics because they could be affected by the installation of a wind energy project within a school district (i.e., endogenous controls). Therefore, we include each characteristic interacted with a linear time trend to allow for differential trending by districts with different baseline values of these characteristics.

Given the substantial effect of statewide school finance reforms (SFRs) on district finances and 'student achievement during our sample period, we additionally control in all models for the impacts of SFRs. Specifically, we created an indicator variable that equals unity after the implementation of a SFR and allow the effects of SFRs to vary by district income by interacting the SFR indicator with indicators for terciles of the within-state 1990 median income distribution.[21]

We complement the event-study specification with a standard difference-in-differences model to increase our precision by pooling estimates within both the pre- and post-wind energy installation periods:

$$y_{ist} = \alpha_0 + \alpha_1 Treat_{ist} + X_{is}\theta_t + \delta_i + \lambda_{st} + \varepsilon_{ist} \tag{2}$$

where $Treat_{ist}$ is an indicator that takes the value of one in all years after a wind installation becomes operational in district i, ε_{ist} is a random disturbance term, and all other terms are as defined in equation (1). The coefficient of primary interest in equation (2) is α_1 which represents the difference-in-differences estimate of the effect of treatment (wind energy installation) on our outcomes of interest.

Finally, to account for the fact that the capacity of wind energy installations varies across districts and increases over time for districts with multiple installations, in our preferred specifications we allow for continuous treatment by replacing $Treat_{ist}$ with the installed per-pupil wind installation capacity in a district:

$$y_{ist} = \beta_0 + \beta_1 KWPP_{ist} + X_{is}\theta_t + \delta_i + \lambda_{st} + \upsilon_{ist} \tag{3}$$

where $KWPP_{ist}$ is installed per-pupil wind installation capacity in district i in state s in year t measured in kilowatts per-pupil, υ_{ist} is a random disturbance term, and all other terms are as defined in equation (1). $KWPP_{ist}$ is equal to zero for district-years with no installed wind energy. The coefficient of primary interest in (3) is β_1 which represents the effect of a one-kilowatt per-pupil increase in wind energy capacity on our outcomes of interest.

All of the regression models described above are what has been referred to in a growing literature as two-way fixed effects models, which can be biased when some of the difference-in-differences variation is driven by comparisons in which previously treated units are used as controls for later treated units (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020). Following Bacon (2021), we document that almost 97% of the difference-in-difference variation driving our estimates is due to "good" comparisons between never treated districts and wind districts (see Appendix Table 2). This is not surprising given that the overwhelming majority of our sample is districts without wind energy installations, and suggests very little scope (less than 3% of the weights) for bias in our estimates due to so called "negative weights."[22]

Nonetheless, to be sure that we are not biased by such comparisons, we implement all of our main analyses using methods designed to avoid bias in two-way fixed effects models. Specifically, we implement all of our event-study models (Equation (1)) using the interaction-weighted estimator by Sun and Abraham

[20] We show that the event-study results are identical when we include a full set of event time dummies, and only present years −6 through 8, rather than "capping" the end points at −6 and 8 (see Appendix Figure III).

[21] We follow the SFR codings from Brunner, Hyman, and Ju (2020). Note that we do not include the SFR indicator separately given that it would be perfectly collinear with the state-by-year fixed effects. We show in Table 3 that the results are not sensitive to the inclusion of the SFR control.

[22] For comparison purposes, 37% is the analogous number to our 3% that Bacon (2021) finds in his replication of Stevenson and Wolfers (2006), suggesting much greater possibility of bias.

Table 2
Effects of Turbine Installation on District Revenues, Expenditures, and Resource Allocations.

		Treatment: Wind Turbine Installed (0/1)		Treatment: Installed Turbine Capacity Per-Pupil (KW)		Effect by Capacity Per-Pupil (KW)			
						Mean	Median	75th Pctl	90th Pctl
Dependent Variable		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
School District Revenues									
	Local	928.70***	873.06***	3.94***	3.79***	798.75***	140.06***	692.75***	2078.25***
		(189.27)	(188.55)	(0.71)	(0.71)	(149.00)	(26.13)	(129.23)	(387.68)
	Total	742.09***	719.97***	3.67***	3.59***	757.76***	132.88***	657.21***	1971.62***
		(195.83)	(195.57)	(0.82)	(0.83)	(174.13)	(30.54)	(151.03)	(453.08)
	State	−98.76*	−75.08	−0.31	−0.25	−52.89	−9.27	−45.87	−137.600
		(57.54)	(57.10)	(0.26)	(0.26)	(55.54)	(9.74)	(48.17)	(144.52)
School District Expenditures									
	Total	935.83***	919.09***	4.87***	4.81***	1015.18***	178.02***	880.47***	2641.4***
		(227.05)	(226.72)	(0.97)	(0.98)	(206.05)	(36.13)	(178.70)	(536.11)
	Current	144.64**	123.52**	0.96***	0.88***	186.5***	32.7***	161.75***	485.26***
		(59.51)	(58.95)	(0.20)	(0.20)	(42.48)	(7.45)	(36.84)	(110.53)
	Capital	368.89***	371.03***	2.11***	2.12***	447.23***	78.42***	387.88***	1163.65***
		(90.49)	(90.75)	(0.44)	(0.44)	(92.21)	(16.17)	(79.97)	(239.91)
	Other	422.29***	424.54***	1.80***	1.81***	381.45***	66.89***	330.83***	992.49***
		(150.53)	(150.69)	(0.59)	(0.59)	(124.52)	(21.84)	(108.00)	(323.99)
Education Production Inputs									
	Pupil-Teacher Ratio	−0.09	−0.07	−0.20**	−0.16*	−0.03*	−0.01*	−0.03*	−0.09*
		(0.05)	(0.05)	(0.09)	(0.09)	(0.02)	(0.00)	(0.02)	(0.05)
	Mean Teacher Salary	−241.76	−224.18	0.33	0.34	71.78	12.59	62.25	186.76
		(200.94)	(200.27)	(0.28)	(0.29)	(60.17)	(10.55)	(52.18)	(156.55)
	Base Teacher Salary	−469.75	−469.75	0.18	0.18	37.17	6.52	32.24	96.71
		(301.72)	(301.72)	(0.31)	(0.31)	(65.37)	(11.46)	(56.69)	(170.08)
Expanded Controls		No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: The sample is as in Table 1. Columns 5, 6, 7, and 8 multiply the coefficient and standard error from column 4 by the respective level of installed capacity/pupil (i.e., mean 211, median 37, 75th 183, 90th 549). The pupil-teacher ratio is multipled by 1000 for columns 3 and 4, because the impact of a 1 KW/pupil increase in installed capacity would be tiny. Subsequently, columns 5–8 then divides by 1000, so the interpretation is in pupils per teacher.
*** = significant at 99% confidence level; ** = 95%, * = 90%.

(2021). That method is specific to event-studies, and is not applicable to our difference-in-difference models, so for all of these models (Equations (2) and (3)), we use the "stacked difference-in-difference" approach recommended by Goodman-Bacon (2021) and used in recent studies, such as Cengiz et al. (2019), Deshpande and Li (2019), and Fadlon and Nielsen (2015). For Equations (2) and (3) we create a stacked sample where we define a "cohort" for each wind district by the year in which it first installs wind energy. We create a panel for each cohort where the panel contains all yearly observations for that cohort of wind districts and all yearly observations for all untreated districts. We then stack the panels, and interact all of our fixed effects with cohort, so that the fixed effects are cohort-by-district and cohort-by-state-by-year. The cohort fixed effect interactions ensure that all comparisons are between the given treated cohort and the untreated districts, with no comparisons made across different cohorts of treated districts, and thus no scope for "negative weights." We show in Appendix Fig. 4 and Appendix Table 3 that the results are nearly identical when not using the Sun and Abraham (2021) or "stacked difference-in-difference" methods, which is unsurprising given the extremely small scope in our context for bias resulting from negative weights.

4. Results

We begin our analysis by examining the impact of wind energy installation on school district revenues and expenditures using the event-study model described above. We estimate equation (1) for our baseline sample of school districts from 1995 to 2016, and plot estimated $\gamma_j's$ and associated 90% confidence intervals from these regressions. Fig. 2a shows that within two to three years of when a district first installs wind energy, local revenues increase by approximately $1,000 per pupil. This increase in revenue grows to approximately $1,500 per pupil several years after installation. This effect represents a large increase given the mean local revenue in districts with installed wind energy of $6,005. Importantly, we see no evidence of a pre-trend in local revenue prior to installation.

Fig. 2b shows similar, though slightly attenuated impacts of wind energy installation on school district total revenue of approximately $1,300 several years out. Again, the point estimates are near zero and statistically insignificant prior to wind energy installation. Finally, given that the other large revenue source for districts aside from local revenue is state aid, Fig. 2c examines impacts on district revenue from the State. We find small, marginally statistically significant declines in state aid after wind energy installation of between $100 and $250 per pupil. These decreases are consistent with the fact that many state aid formulas provide less aid to districts when local revenues are higher. Again we see no evidence of pre-trends.

We next examine whether these increases in revenues translate into increased expenditures, and toward what types of expenditures districts allocate the revenue increases due to wind energy installation. Fig. 3a shows that total expenditures per pupil increase in a similar pattern over time as total revenues after wind energy installation, though with slightly higher magnitudes. Total expenditures increase by between $1,200 and $1,700 per pupil several years after installation. Current expenditures increase only slightly, by between $100 and $200 per-pupil relative to a mean of just under $11,000 (Fig. 3b). Districts spend a significant share of the revenues toward increased capital spending, which increases by up to $1,000 five years after wind energy installation, off a mean of $1,346 per pupil (Fig. 3c). Finally, in Fig. 3d we find that other expenditures, which is simply non-capital and non-current

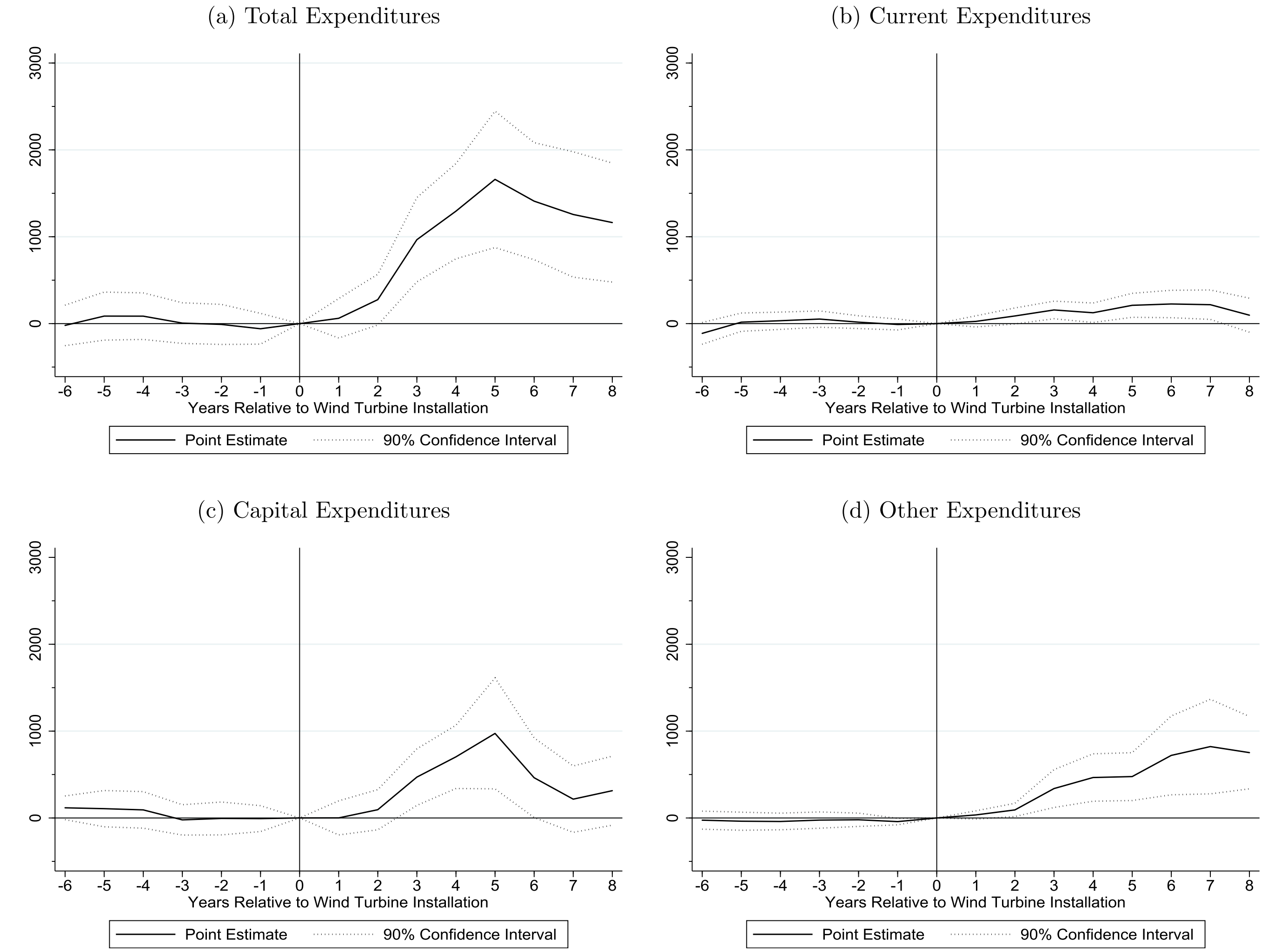

Fig. 3. Effects of Wind Turbine Installation on School District Expenditures. Notes: Figures show event study estimates of the effects of wind turbine installation on per-pupil school district expenditures. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

expenditures, increases substantially, by up to approximately $800 several years after wind energy installation.[23] None of the figures examining district expenditures show any evidence of differential pre-trends.

We next examine whether any of these expenditure increases lead to impacts on commonly studied inputs to education production, for example, class size and teacher compensation. Fig. 4a shows a small, and not quite statistically significant decline in the pupil-teacher ratio, which is our measure of class size, on the order of 0.1 pupils per teacher, relative to a mean of 13.9. This is less than a 1% decline in class size, consistent with the small (1–2%) increases in current spending. As shown in Fig. 4b there is no apparent impact on either mean or base teacher salaries. However, given the far smaller sample using the SASS data, the base salary results are too imprecise to gain much inference.

One noticeable pattern in the revenue and expenditure results is that the effects of wind energy installation grow over time during the first several years post-installation. It is not immediately clear why this would be the case, as another possible scenario could have been that the installation occurs and districts immediately and permanently reap the tax benefits, leading to a sudden increase in the level of revenues, but no change in the trend. We examine and rule out several possible explanations for this pattern. First, the effects on revenues and expenditures are per-pupil, so if installations cause enrollments to decline, then this would cause the pattern that we observe. We estimate the event-study model where the dependent variable is district enrollment and find no impact (see Appendix Fig. 8a).[24]

A second possible explanation for the growing effects over time is that we are examining the impact relative to the year of the *first* wind energy installation in the district. However, 37.5% of districts in our sample with installed wind energy install additional wind turbines over time. To examine whether the growing effects are due to these districts with "multiple events," we drop those districts that install additional wind turbines in years following the initial installation. As shown in Appendix Fig. 2b, even after

[23] We explore this result further in Section V(c), finding that it is driven primarily by Texas, and represents payments from districts to the state. Thus, it is not a true increase in district spending, but rather a transfer of a portion of the local revenue increases due to wind installation back to the state due the recapture design of Texas school finance laws.

[24] We also present in Appendix Figure V results from four additional event-study models where the dependent variables are district fraction white, district fraction free lunch, the district child poverty rate, and county unemployment rate. We find no effect on fraction white, no effect on child poverty, and a small negative effect on fraction free lunch beginning around six years after wind energy installation. We find a precisely estimated zero impact on unemployment: we can rule out a decrease of more than approximately 0.1 percentage points off a mean of 6.1 percent.

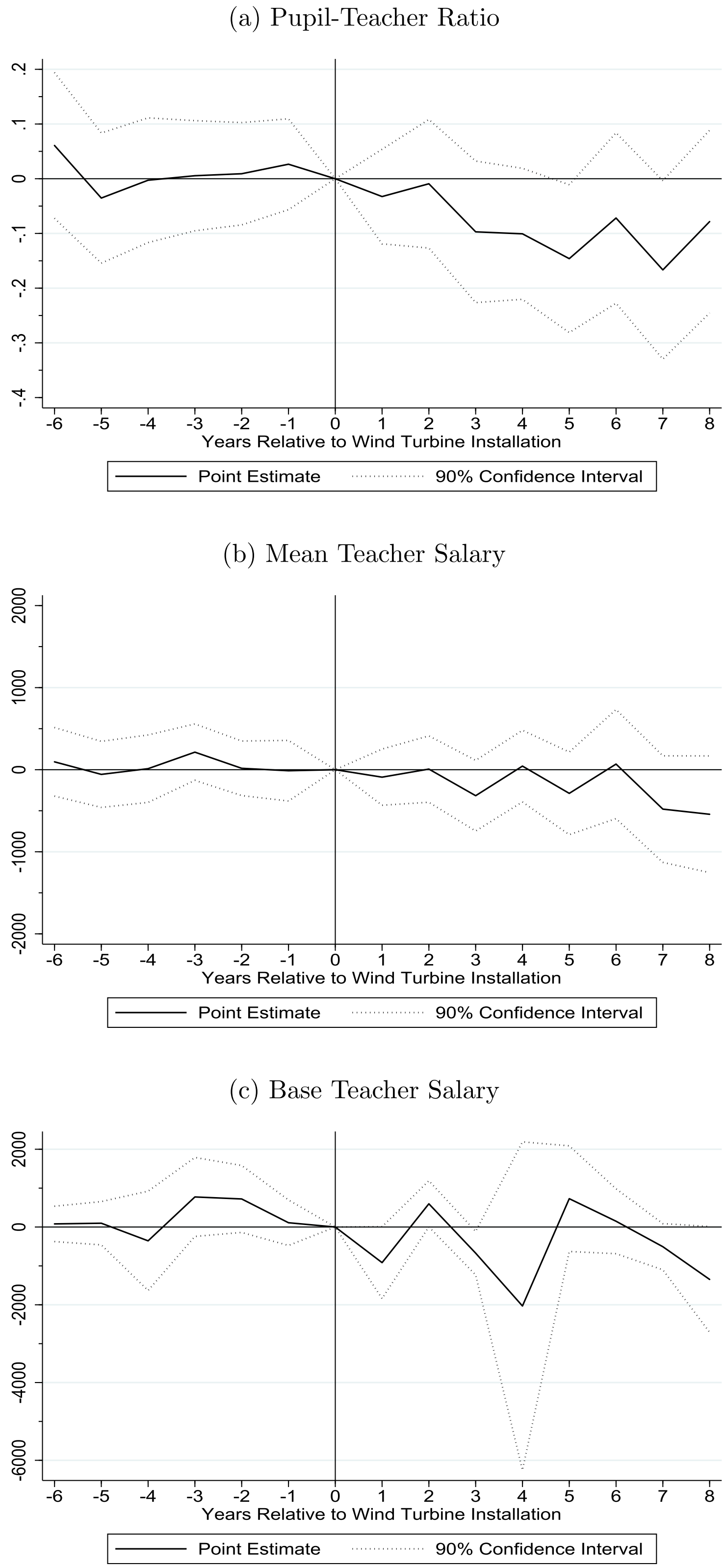

Fig. 4. Effects of Wind Turbine Installation on Education Production Inputs. Notes: Figures show event study estimates of the effects of wind turbine installation on inputs to education production. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

dropping districts with multiple installations we still observe a pattern of rising local revenue over time.

The final explanation is a combination of sun-setting tax abatements and other tax rules that delay the generation of tax revenue from wind energy installations. Many states and local jurisdictions enter into some type of agreement in order to encourage wind development that allows wind developers a grace period in which they do not pay (or pay significantly lower) property taxes. For example, under Iowa's wind energy conversion tax ordinance, a wind project is taxed at 0% during the first year of operation and then in the second through sixth assessment years, a wind project is taxed at an additional 5% of net acquisition costs for each year (5% in year 2, 10% in year 3, etc.) until the seventh year when taxes are capped at 30% of net acquisition cost. While we cannot confirm empirically, laws and agreements such as those in Iowa, appear to be the most likely reason for the growing effect over time.[25]

4.1. Difference-in-difference estimates

We present difference-in-differences (DD) estimates of the impact of wind energy installation in Table 2. Results based on equation (2) with binary treatment are presented in columns 1 and 2, while columns 3–5 present results based on equation (3) with continuous treatment. Row 1, column 1 shows that installation causes a $929 per pupil increase in local revenue. Column 1 includes the basic set of controls: baseline enrollment, 1990 median income, and 1990 fraction earning a BA or higher, all interacted with a linear trend; and a dummy for school finance reforms interacted with terciles of the 1990 within state median income distribution. The effect is very similar, $873 per pupil, or 15%, relative to the mean of $6,005, after including the expanded set of controls that adds 1990 percent poor, 1990 percent white, 1990 percent age greater than 65, and 1990 rural status, all interacted with a trend. The effect on total revenues (column 2) is $720 (5%), and the (insignificant) effect on state revenues is -$75 (-1%).

Focusing on our preferred specification in column 2, total expenditures increase by $919 (7%), almost $200 per pupil more than total revenues increase. The reason that total expenditures can increase more than total revenues is that revenues in our data do not include proceeds from bond sales, while expenditures include the spending resulting from bond sales. For example, when a district passes a bond to finance a capital project, the proceeds do not count toward revenue, but the capital spending on the project is included in capital, and therefore total, expenditures.

Current expenditures increase by $124 per pupil, an increase of only 1% relative to the mean current spending in wind energy districts of $10,920. On the other hand, capital expenditures increase by $371 per pupil, or 28%, relative to the mean of $1,346. The larger increases in capital than operating expenditures are perhaps unsurprising given that the school finance laws in many states require a reduction in state aid when local revenue placed in the general fund is used to finance operating expenditures, but do not require a reduction in state aid when local funds are placed in the capital fund and used to finance capital projects.[26] Appendix Table 4 shows that the effects on capital expenditures are driven nearly exclusively by spending on construction of new buildings, and modernization or major renovations to existing buildings, as opposed to purchases of land or equipment. Finally, other expenditures increase by $425 per pupil, or 42%.

Given the small effect on current expenditures, it is unlikely there would be large impacts on either teacher hiring (i.e., class size) or on increasing teacher compensation. Accordingly, we find statistically insignificant decreases in class size of less than 0.1 pupils per teacher, representing a 0.5% decrease. Similarly, we find no evidence of impacts on mean or base teacher salary, with insignificant point estimates of -$224 and -$470 (representing a 0.3% and 1.4% decrease), respectively. Another explanation for the null impact on class size and teacher salaries is that a large share of the effect on current spending is driven by spending on district administration, the central business office, and operations and maintenance (see Appendix Table 5).

While the estimates from the basic DD model with binary treatment are useful, there are two aspects of the model that are suboptimal. First, as in the event-study analysis, the binary treatment variable turns on when the first installation in a district occurs, and so it does not further capture the increased capacity of subsequent installations for the 37.5% of districts with multiple installations over time. Second, the binary treatment variable misses the important variation stemming from different wind energy installations having very different installed capacity, while local property tax generation from wind energy installation almost always reflects installed capacity. For example, the 10th percentile of installed capacity per pupil in our sample among districts and years with installed wind energy is 0.4 KW/pupil, while the 90th percentile is 549 KW/pupil.[27] These installations clearly have very different tax implications, but the binary installation variable treats them identically.

Given the limitations of the binary treatment results, in columns 3–5 we present results based on equation (3) where we use a continuous measure of treatment, namely installed kilowatts per pupil. In district-years without installed wind energy, this variable equals zero. Once again, the results in column 3 (basic controls) and column 4 (expanded controls) are very similar, so we focus on column 4. Row 1 shows that one additional KW/pupil of installed capacity leads to $3.79 per pupil of additional local revenue. Column 5 multiplies the point estimate by 211, which is the mean level of installed capacity per pupil among districts and years with installed wind energy. For example, a district with the mean level of installed capacity per pupil experiences an increase of $799 (=3.79 $\times$ 211) per pupil in local revenue. Total revenues increase by $3.59 with a 1 KW/pupil increase in capacity, for a $758 increase at mean capacity. We again find small, statistically insignificant decreases in state revenue: a $0.25 decrease per KW/pupil, corresponding to a (insignificant) $53 decrease at the mean.

In terms of expenditures, we find that total, current, capital, and other expenditures increase by $4.81, $0.88, $2.12, and $1.81 per one KW/pupil increase, respectively, which corresponds to a $1,015 (total), $187 (current), $447 (capital), and $381 (other) increase at the mean level of installed capacity. The effects on current and capital expenditures represent increases of 1.7% and 33%, respectively. Turning next to pupil-teacher ratio, to aid in interpretation for the continuous DD we multiply the point estimates in columns 3 and 4 by 1,000. Thus, a 1 KW/pupil increase in capacity causes a marginally significant decrease of 0.00016 pupils per teacher (presented as -0.16 in column 4 of Table 2), which is equivalent to 0.03 pupils per teacher at the mean. While this is marginally significant, it is essentially zero. Note that the increase in current expenditures is almost 2% while the pupil teacher ratio decreases by substantially less than 2%. Thus, one interpretation of these findings is that districts are not spending the increases in current expenditures on hiring new teachers, although we do not have enough statistical precision to be confident in this claim. We conservatively interpret these effects as consistent with the prior results that there are small impacts on current spending, and near

[25] Another possible explanation for which we find some support is that districts with earlier installed wind installations for which we can observe effects several years out are of greater installed capacity per-pupil. We find that the 36% of wind districts with installations from 2008 and earlier, for which we can observe effects 8 or more years out, have a mean installed capacity of 243 kW/pupil as compared to 162 kW/pupil for the 64% of wind districts with installations since 2009.

[26] Discussions between the authors and school district superintendents in several districts with wind energy installations anecdotally confirm that these state laws are the primary reason districts tend to spend the money on capital expenditures. We also show this to be the case empirically in Section V(c).

[27] See Appendix Table 3 for a detailed examination of the distribution of installed wind energy capacity per pupil, and a breakdown of how the distribution varies by installed capacity versus district enrollment, as well as by rural status.

Table 3
Effects of Installed Turbine Capacity: Sensitivity Checks.

	School District Revenues			School District Expenditures				Education Production Inputs			Sample Size
	Local	Total	State	Total	Current	Capital	Other	Pupil-Teacher Ratio	Teacher Salary		
									Mean	Base	
Specification/Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Baseline	3.79***	3.59***	−0.25	4.81***	0.88***	2.12***	1.81***	−0.16*	0.34	0.18	239,518
	(0.71)	(0.83)	(0.26)	(0.98)	(0.20)	(0.44)	(0.59)	(0.09)	(0.29)	(0.31)	
Include District-Specific Linear Time Trends	3.67***	3.29***	−0.54**	4.36***	0.57***	2.48**	1.31*	−0.20**	0.68*	0.49***	239,518
	(0.87)	(1.01)	(0.25)	(1.57)	(0.14)	(1.02)	(0.75)	(0.10)	(0.36)	(0.12)	
Drop School Finance Reform Controls	3.79***	3.60***	−0.24	4.82***	0.89***	2.12***	1.81***	−0.16*	0.30	0.22	239,518
	(0.71)	(0.82)	(0.26)	(0.98)	(0.20)	(0.44)	(0.59)	(0.09)	(0.28)	(0.29)	
Include States with No Wind Turbines	3.80***	3.60***	−0.25	4.82***	0.89***	2.12***	1.81***	−0.16*	0.32	0.18	273,139
	(0.71)	(0.83)	(0.26)	(0.98)	(0.20)	(0.44)	(0.59)	(0.09)	(0.28)	(0.31)	
Include KS and WY	3.71***	3.53***	−0.24	4.75***	0.87***	2.10***	1.77***	−0.17*	0.31	0.09	245,868
	(0.69)	(0.81)	(0.26)	(0.96)	(0.20)	(0.43)	(0.58)	(0.09)	(0.28)	(0.36)	
Restrict to Counties with Wind Turbines	3.55***	3.39***	−0.23	4.63***	0.86***	2.14***	1.63***	−0.21*	0.17	0.20	58,714
	(0.70)	(0.81)	(0.25)	(0.92)	(0.22)	(0.42)	(0.58)	(0.11)	(0.34)	(0.35)	
Drop High Enrollment Non-Wind Districts	3.71***	3.49***	−0.26	4.69***	0.84***	2.08***	1.77***	−0.16*	0.37	0.29	200,584
	(0.70)	(0.82)	(0.26)	(0.98)	(0.20)	(0.44)	(0.59)	(0.09)	(0.29)	(0.22)	
Drop Non-Rural Non-Wind Districts	3.74***	3.52***	−0.27	4.71***	0.84***	2.10***	1.77***	−0.16*	0.29	0.20	154,219
	(0.70)	(0.83)	(0.26)	(0.98)	(0.20)	(0.44)	(0.59)	(0.09)	(0.30)	(0.29)	
Propensity Score Weighting	4.12***	3.91***	−0.24	5.28***	0.92***	2.19***	2.18***	−0.17**	0.38	0.16	239,518
	(0.91)	(0.99)	(0.25)	(1.12)	(0.20)	(0.43)	(0.77)	(0.09)	(0.25)	(0.27)	
Wind Speed IV	4.15***	3.41***	−0.39	4.38***	0.61**	1.82***	1.95***	−0.23	−0.77	−4.70	237,961
	(0.82)	(0.85)	(0.26)	(0.95)	(0.25)	(0.37)	(0.69)	(0.24)	(0.88)	(3.50)	
Expanded Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	

Notes: Each coefficient is from a separate regression of the outcome (listed in the column header) on installed wind turbine capacity (in KW) per-pupil. High enrollment non-wind districts are districts with no installed wind capacity that have enrollment larger than the 90th percentile of enrollment among districts with installed wind capacity. The propensity score weighting weights higher those non-wind districts that are observationally similar to wind-districts. The wind speed IV instruments for installed wind capacity with the interaction of average wind speed in the school district and being in a year with installed wind energy (first stage F-statistic = 123).]
*** = significant at 99% confidence level; ** = 95%, * = 90%.

Table 4
Effects of Installed Turbine Capacity on District Finances, State Heterogeneity.

				No Texas		
	Sample Mean				Current Exp. Restrictions	
Dependent Variable	($/pupil) (1)	Baseline (2)	Texas Only (3)	Baseline (4)	Strongest (5)	Less Strong (6)
Local Revenue	6,422	3.79***	7.78***	2.34***	2.04***	2.45***
		(0.71)	(1.57)	(0.28)	(0.10)	(0.38)
Total Revenue	13,464	3.59***	8.02***	1.99***	2.44***	1.86***
		(0.83)	(1.61)	(0.37)	(0.26)	(0.44)
State Revenue	5,971	−0.25	0.39	−0.48*	0.20	−0.71***
		(0.26)	(0.36)	(0.28)	(0.16)	(0.27)
Total Expenditures	13,496	4.81***	10.04***	2.91***	5.58***	1.99***
		(0.98)	(1.86)	(0.92)	(0.90)	(0.51)
Current Expenditures	11,233	0.88***	0.87***	0.90***	0.23**	1.16***
		(0.20)	(0.32)	(0.25)	(0.11)	(0.31)
Capital Expenditures	1,256	2.12***	4.35***	1.31**	2.88***	0.76**
		(0.44)	(0.88)	(0.53)	(0.51)	(0.31)
Other Expenditures	1,008	1.81***	4.82***	0.70	2.47***	0.07
		(0.59)	(1.42)	(0.49)	(0.40)	(0.06)
Community Services & Adult Edu.	86	0.31	−0.00	0.42	1.55***	0.02
		(0.25)	(0.01)	(0.32)	(0.30)	(0.01)
Interest on Debt	286	0.30**	0.51***	0.23	0.83***	0.01
		(0.13)	(0.10)	(0.18)	(0.18)	(0.03)
Payments to State Government	90	1.14**	4.23***	0.00	−0.00*	0.00
		(0.55)	(1.37)	(0.00)	(0.00)	(0.00)
Payments to Local Governments	26	−0.01	0.00	−0.01	−0.01	−0.01
		(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Payments to Other School Systems	475	0.07**	0.07	0.06*	0.11	0.05
		(0.03)	(0.05)	(0.03)	(0.10)	(0.03)
Payments to Private Schools	70	0.00	−0.00	0.00	−0.00***	0.00
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Payments to Charter Schools	24	−0.00	0.00	−0.01	0.01*	−0.01
		(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
Observations		239,518	22,143	217,375	50,445	166,926
Expanded Controls		Yes	Yes	Yes	Yes	Yes

Notes: Each coefficient is from a separate regression of the outcome (listed in the row header) on installed wind turbine capacity (in KW) per-pupil for the sample listed in the column header. Column 5 includes the states with the strongest restrictions against using revenues from wind energy installation toward current expenditures. Column 6 excludes those states.
*** = significant at 99% confidence level; ** = 95%, * = 90%.

Table 5
Effects of Turbine Installation on Student Achievement.

					Restrict Wind Districts by Capacity/Pupil		
	Baseline		No Texas		Below Med.	Above Med.	>75th Pctl
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: NAEP Data							
Post	−0.011	−0.011	0.000	0.000	−0.025	0.018	−0.009
	(0.017)	(0.017)	(0.017)	(0.017)	(0.020)	(0.030)	(0.038)
Post*Trend	0.000	0.000	0.000	0.000	0.000	−0.002	−0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)
Trend	0.000	0.000	−0.000	−0.000	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Effect 5 Years Post		−0.008		0.001	−0.022	0.012	−0.030
		(0.017)		(0.017)	(0.019)	(0.027)	(0.035)
Observations	85,256		82,157		84,340	83,724	83,227
Panel B: NAEP and SEDA Data							
Post	−0.019	−0.019	−0.012	−0.012	−0.006	−0.021	−0.014
	(0.022)	(0.022)	(0.023)	(0.023)	(0.025)	(0.033)	(0.063)
Post*Trend	0.001	0.001	0.003	0.003	−0.001	0.003	−0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.011)
Trend	−0.005	−0.005	−0.007**	−0.007**	−0.005	−0.005	−0.006*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Effect 5 Years Post		−0.036		−0.033	−0.036	−0.032	−0.071
		(0.023)		(0.024)	(0.027)	(0.034)	(0.060)
Observations	282,928		258,818		277,668	276,539	273,254
Expanded Controls	No	Yes	No	Yes	Yes	Yes	Yes

Notes: The level of observation is the district-year-grade-subject. The dependent variable is standardized student test scores. In Panel A, we use NAEP data, which are available for a sample of districts in every other year from 1996 to 2015 for grades 4 and 8. In Panel B, we supplement the grade 4 and 8 NAEP scores from 1996 to 2007 with annual scores from the Stanford Education Data Archive for all school districts during 2009 to 2016.
*** = significant at 99% confidence level; ** = 95%, * = 90%.

zero impacts on class size reduction. As in the previous models, there is no impact on either mean or base teacher salary. However, using the continuous DD model the zero effect on base salary at the mean capacity is quite precisely estimated with a point estimate of $37 and standard error of $65, allowing us to rule out a positive effect on base salaries greater than approximately $164.

The above described effects at the mean level of installed capacity per pupil correspond closely to the binary difference-in-difference results. However, as previously noted there is a very wide distribution of capacity per pupil, and the mean of 211 kW/pupil is skewed upward by very large capacities per pupil in the smallest enrollment districts with the largest wind installations. Appendix Table 6 breaks the capacity per pupil distribution into terciles, showing that districts in the bottom tercile, for example, have mean capacity per pupil of 0.1 kW, mean enrollments of 12,010, see effects of wind installation on local revenue per pupil of only $0.41, bringing in a total of around $360,000 in local revenue for all districts in the tercile. On the other hand, districts in the top tercile have mean capacity per pupil of 1,320.5 kw/pupil, mean enrollments of 244, see an effect of wind installation on local revenue per pupil of $5,005, bringing in over $88 million of local revenue in total across these districts.

To illustrate all of our effects not only at the mean, but also at different points in the installed capacity per pupil distribution, we show in columns 6, 7, and 8 of Table 2, effects at the median, 75th percentile, and 90th percentile of capacity per pupil, which are 37, 183, and 549 kW/pupil, respectively. The effect on local revenue at the median is $140, at the 75th percentile is $693, and at the 90th percentile is $2,078. The effects on capital expenditures at the median, 75th and 90th percentiles are $78, $388, and $1,164, respectively. We show in Fig. 5 the effects on local revenue, total expenditures, and capital expenditures by ventiles of installed capacity, revealing the same patterns. Given the skewed capacity per pupil distribution, the effects of wind installation for districts in the bottom half of the distribution are quite small, and they only grow to be substantial toward the top (approximately) third of the distribution.

In summary, at the mean level of installed capacity per pupil, districts that install wind energy see large increases in local revenues that are only minimally offset by reductions in state aid, leading to large increases in total revenue. The districts spend these increases primarily on capital outlays, and on other, non-current and non-capital expenses, which we examine in further detail below. However, these effects are driven by the large minority of treated districts with the greatest installed capacity per pupil, with most districts experiencing minimal per pupil impacts from wind installation in their district.

4.2. Sensitivity analysis

In this section, we conduct nine sensitivity checks to examine the robustness of our results to decisions about the way we construct our sample and implement the difference-in-differences analysis. We proceed with our preferred specification, which is the continuous DD model with the expanded set of controls (Table 2, column 4). The first row of Table 3 replicates our baseline preferred model for comparison purposes.

In our first check, we replace the baseline controls interacted with time trends with district-specific linear time trends. The results are robust; the only noticeable changes are that the negative effect on state revenues, and positive effects on mean and base teacher salaries, are larger and statistically significant, though they are all still very small. In our second check, we omit the school finance reform dummy and within-state income tercile interactions. In our third check, we include the eleven states, primarily in the South census region, with no installed wind energy during our sample period. In the fourth check, we include the two states, Kansas, and Wyoming, which we removed because their laws prevent wind energy tax revenue from being directed toward local school districts. In these second, third, and fourth checks, the

Table 6
Effects of Turbine Installation on Student Outcomes in Texas.

	Avg. Test Scores		High School Grad. (%)		Long-Run Index		Restricting Wind Districts by Installed Capacity/Pupil: Below Median Scores	Below Median HS Grad.	Below Median LR Index	Above Median Scores	Above Median HS Grad.	Above Median LR Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	−0.052	−0.043	0.164	0.144	0.029	0.041	0.128	0.462	0.051	−0.110	−0.104	0.009
	(0.065)	(0.065)	(0.549)	(0.548)	(0.061)	(0.060)	(0.095)	(0.663)	(0.099)	(0.075)	(0.671)	(0.068)
Post*Trend	0.004	0.004	−0.099	−0.099	−0.010	−0.011	−0.019	0.022	0.001	0.013	−0.125	−0.013
	(0.013)	(0.013)	(0.083)	(0.082)	(0.011)	(0.010)	(0.022)	(0.102)	(0.021)	(0.014)	(0.099)	(0.010)
Trend	−0.005	−0.007	0.027	0.034	−0.001	−0.003	−0.009*	0.040	−0.002	−0.007	0.038	−0.002
	(0.005)	(0.005)	(0.050)	(0.049)	(0.005)	(0.005)	(0.005)	(0.049)	(0.005)	(0.005)	(0.049)	(0.005)
Effect 5 Years Post		−0.058		−0.179		−0.028	−0.014	0.769	0.046	−0.081	−0.541	−0.063
		(0.056)		(0.452)		(0.056)	(0.100)	(0.746)	(0.107)	(0.061)	(0.473)	(0.060)
Observations	22,824		20,817		22,882		22,143	20,118	22,166	22,587	20,557	22,622
Expanded Controls	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table uses a separate adminstrative dataset from the Texas Department of Education. The level of observation is the district-year. The sample includes all districts in Texas from 1995-2018. Test scores are for grades 4 and 8, standardized to mean 0, SD 1. High school graduation is a percent with a mean of 90.9. The long-run index, is mean zero, SD 1, and includes the following outcomes: 1) % take AP exam, 2) % take ACT/SAT, 3) % take ACT/SAT and score above national median, 4) % take an advanced / honors course, 5) % complete state recommended high school curriculum, and 6) % graduate high school.
*** = significant at 99% confidence level; ** = 95%, * = 90%.

results are nearly identical to our baseline estimates. The results including Kansas and Wyoming are uniformly smaller, but only slightly, due to the small number (46) of districts with wind energy in those two states.

In our fifth check, we restrict the sample to counties with installed wind energy. In our baseline sample, we include counties with no installed wind energy if they are in a state with installed wind energy, even though these counties may be quite different from counties in that state with installed wind energy. This check is meant to create a control group of school districts without installed wind energy that looks more like the treated school districts, by drawing within state comparisons (due to the state-by-year fixed effects) between school districts with wind energy and those without, but that are in counties with wind energy. In spite of the sample size dropping from 239,518 district-years to 58,714, the point estimates are very similar.

Given that treated districts, especially those with the greatest installed capacity per pupil, tend to be smaller and more likely to be rural than untreated districts, in our sixth and seventh checks we drop large untreated districts and non-rural untreated districts. Specifically, for the sixth check, we drop districts with no installed wind energy that have enrollment greater than the 90th percentile of enrollment among treated districts. In the seventh check, we drop districts with no installed wind energy that are a city, suburb, or town, leaving only rural untreated districts. In both cases, the estimates are nearly identical to those using our preferred specification. In our next specification check we use propensity score weighting to weight higher those non-wind districts that are observationally similar to districts with wind energy.[28] Once again, the estimates using the propensity score weighting are very similar to our baseline results.

Finally, to further account for any differences between districts with high versus low (or zero) installed wind energy capacity, we use average wind speed as an instrument for installed capacity. This instrumental variables strategy can account for strategic location of wind energy, for example, if wind developers choose to develop in places with higher or lower local tax rates. We use the average wind speed at each school district's centroid at a 100-meter height during the period 2007–2013.[29] We instrument for installed capacity with the interaction of our time-invariant wind speed measure with a dummy for having installed wind energy in that year. This instrumental variables strategy produces results that are very similar to those from our main analysis.

4.3. State heterogeneity and other expenditures

As described in Section 2, there is substantial heterogeneity not only in state laws regarding taxation of wind energy installation, but also in school finance laws. The interaction of these two quite heterogeneous sets of laws could create very different impacts of wind energy installation in different states. While the average national effect of wind energy installation we have presented is of primary interest, it is also important to understand whether our results are driven in part by any particular state, or by sets of states with particular types of laws. An obvious first state to consider in our case is Texas, which is by far the largest producer of wind energy in the country, comprising 28% of installed capacity in our national sample.[30] In this section, we first explore whether and to what extent our national results are driven by Texas. We then restrict the analysis to the handful of states other than Texas (i.e., California, Iowa, Michigan, Indiana, and Washington), which due to their school finance formula place the most restrictions on using local revenues to increase current expenditures. We then compare the effects in those states to the remaining states, where such current expenditure restrictions are either non-existent or smaller in scope.

Table 4 presents effects of wind energy installation on revenues and expenditures using our preferred specification (continuous DD with expanded controls) for our national sample (baseline – column 2), Texas only (column 3), and our national sample without Texas (columns 4–6). Column 4 includes all states other than

[28] Specifically, we run a logit regression of a dummy for a district having wind energy on 1990 rural status, median household income, fraction BA or higher, fraction age 65 or older, fraction white, fraction poor, and baseline enrollment. We then create a propensity score from that regression, which is simply the predicted probability that a district has wind energy. Finally, we create inverse propensity score weights, equal to wind / pscore + (1-wind)/(1-pscore).

[29] These data come from the Wind Integration National Dataset (WIND) Toolkit (Draxl, et al., 2015). The 100-meter height reflects typical wind turbine height, and the period 2007–2013 is the period of available data. The first stage F-statistic for our IV regression is 127. Appendix Table 7 shows all of our main results (i.e., binary and continuous difference-in-differences, with and without the expanded controls) using the wind speed instrument, again showing very similar effects.

[30] The next largest, California, produces only 9% of installed wind capacity in our national sample.

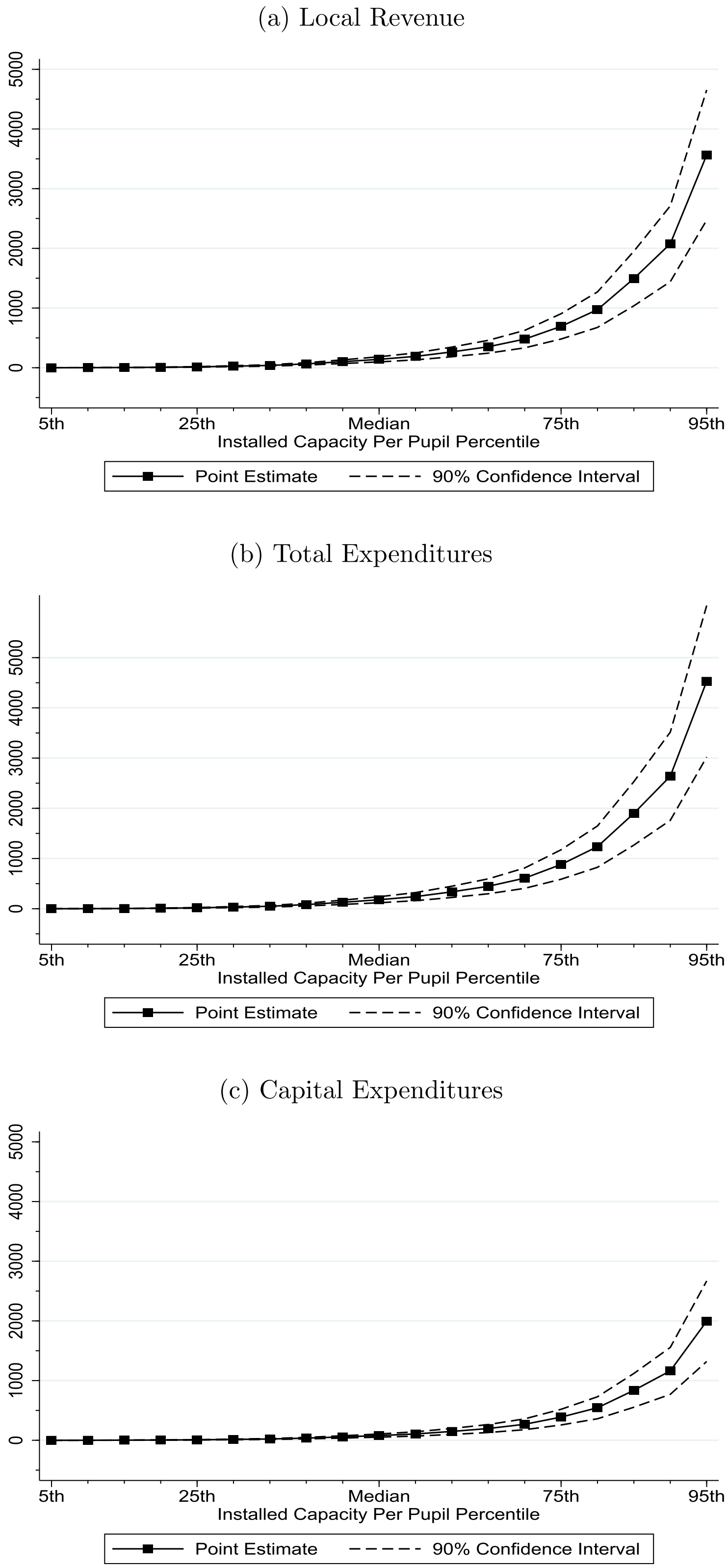

Fig. 5. Effects of Wind Turbine Installation by Installed Capacity per Pupil Percentile. Notes: Figures show event study estimates of the effects of wind turbine installation on revenues and expenditures at various percentiles of installed capacity per pupil. Each point is the coefficient from Table 2, column 4 multiplied by the appropriate capacity per pupil. Dashed lines are 90% confidence intervals.

Texas, and columns 5 and 6 break that sample into those states with the strongest current expenditure restrictions and those states with weaker restrictions, respectively. We find much larger impacts in Texas than in the national sample on local revenue and total revenue of $7.78 and $8.02 per pupil from a 1 KW/pupil increase in capacity. In column 4, where we drop Texas, the point estimates for local and total revenue are substantially smaller at \$2.34 and \$1.99 respectively. Also, in column 3, the reduction in state revenue increases slightly in magnitude and becomes marginally significant, though it is still quite small (\$-0.48). Total, current, capital, and other expenditures in Texas increase by \$10.04, \$0.87, \$4.35, and \$4.82, respectively. The effect on current is identical to the baseline estimates, but the other three outcomes have much larger point estimates (i.e., an even smaller share of the expenditure increase is devoted toward current spending). Importantly, Texas completely drives the large increases we observed in other expenditures: without Texas, the coefficient on other expenditures drops from \$1.81 to \$0.70 and becomes statistically insignificant.[31]

The large impacts in Texas on other expenditures begs the question of what specific type of expenditure is driving that effect. In the bottom rows of Table 4, we show effects on the expenditure sub-categories that comprise other expenditures. The effect on other expenditures in Texas comes almost completely from payments to the state government, with a small increase as well in interest payments on debt. The large increase in payments to the state government is a function of the Texas school finance laws, whereby property tax revenue from districts with high property tax bases is recaptured by the state and redistributed to districts with low property tax bases, a policy commonly referred to as Robin Hood. The large impact on other expenditures, therefore does not actually reflect school district spending on any productive education input, but rather a different form of state aid reduction. This implies that while the effects on total revenue and expenditure appear to be double the baseline effect we estimated, once you subtract off the payments to the state, the effects are only somewhat larger than our national baseline estimates. Furthermore, as discussed in Section 2, the laws in Texas incentivize school districts to spend wind energy revenues on capital and not current spending, which is why the effects are concentrated so highly in capital expenditures relative to current.

In spite of the sizable impacts on capital expenditure in Texas, the results for the national sample sans Texas, though attenuated, are still precise and present a similar pattern as before: large increases in revenues and expenditures, with larger effects on capital spending (1.32) than current (0.90), especially considering mean current spending is about ten times larger than mean capital spending.[32]

Moving to columns 5 and 6 presenting the effects for the states with the strongest current expenditure restrictions, we find that the large effects on capital spending and small effects on current spending are driven by those states where the laws predict such a pattern of effects. Specifically, for the states that restrict current spending, we find a \$0.23 effect of a 1 kW/pupil increase in installed capacity on current spending. This is 4% of the \$5.58 effect on total expenditures. The effect on capital spending in these states is \$2.88. On the other hand, current expenditures increase by more than five times that amount, or \$1.16 in the remaining states, compared to a capital effect of \$0.76, out of a total expenditure effect of \$1.99.[33] This pattern of heterogeneity by strength of state current expenditure restrictions lends credence to the explanation for our large effects on capital that it is school district responses to these types of rules that drives their spending the local revenue from wind energy installation for purposes other than current expenditures.

It is also worth noting that the states restricting spending on current expenditures, not only spend more on capital, but also spend more on other expenditures (\$2.47 relative to \$0.07 in the other states). Some of this is increases in interest on debt (\$0.83), which is a result of the capital increases and accompanying bond payments. But most of it (\$1.55) is spending on community services and adult education, which is another expenditure type typically not included as current spending in education state aid formulas, and so districts can spend on without losing state aid.

4.4. Student achievement

In this section, we examine to what extent, if any, the increases in revenues and expenditures from wind energy installation translate into improvements in student achievement. Unlike effects on revenues and expenditures, there is no reason to expect that the effects of wind energy installation would immediately impact achievement, even if expenditures were affected immediately, given that achievement would be affected slowly over time as students are exposed to additional years of increased school funding. Consequently, we first present the event-study figures, where the outcome variable is district test scores. We then present the DD model, but we modify it to allow the impact to evolve linearly during the post period instead of including a single post indicator as we do in equation (2):

$$NAEP_{ijgst} = \phi_0 + \phi_1 Treat_{ist} + \phi_2 YearsPost_{ist} + \phi_3(YearsPost_{ist} * Treat_{ist}) + X_{is}\theta_t + \delta_i + \lambda_{st} + \pi_{jg} + \omega_{ijgst} \tag{4}$$

where $NAEP_{ijgst}$ is the average score in district i, in tested subject j and grade g, in state s, and year t, $Treat_{ist}$ is the dummy for whether a district has installed wind energy, $YearsPost_{ist}$ is a relative-year trend variable that captures the number of years since wind energy was installed (this is negative prior to installation, positive after installation, and zero during the installation year and for districts without wind energy), and $YearsPost_{ist} * Treat_{ist}$ is the interaction of the two, which gives the number of years since installation during the post period. The coefficient on $Treat_{ist}$ gives the jump in the level of test scores, while the coefficient on $YearsPost_{ist}$ shows whether there is any pre-trend, and the coefficient on the interaction term gives the additional increase in scores for every 1 year after installation. We include subject-by-grade fixed effects, π_{jg}, given that the unit of observations is now district-year-subject-grade.

Fig. 6a shows the event-study analysis, where the outcome is standardized district NAEP scores. Relative years are grouped into pairs to help with precision given the smaller sample size. There is no evidence of a pre-trend, and scores remain flat after installation. There is no evidence of any positive effect on scores, and if anything, there appears to be a very small, and statistically insignificant decrease. We can rule out increases of about 3–4% of a standard deviation. Given the starkly different results in Texas, and somewhat different results in our baseline sample after dropping Texas, we also show the effects on achievement dropping Texas. The picture looks nearly identical, with no detectable pre-trend or effect post-installation. We obtain a similar, though noisy, null result when we restrict our sample to Texas. In section V(e), we use Texas administrative data to explore this result with greater precision.

We present the results from equation (4) in Table 5, Panel A. Effects are nearly identical with and without expanded controls: in our baseline sample and in the sample dropping Texas, as in

[31] The effect on pupil-teacher ratio without Texas is a statistically significant 0.28 reduction, which is larger than our baseline estimate, but still very small. The effects on mean and base teacher salary are still small and statistically insignificant.

[32] We present all of our main event-study figures dropping Texas in Appendix Figures VI, VII, and VIII. As in Table 4, column 3, the results are somewhat attenuated, but still precise and show the same pattern.

[33] There is a point estimate of 0.01 (SE=0.13) for pupil-teacher ratio for states with the strongest current expenditure restrictions, and of −0.38 (SE=0.12) for all other states.

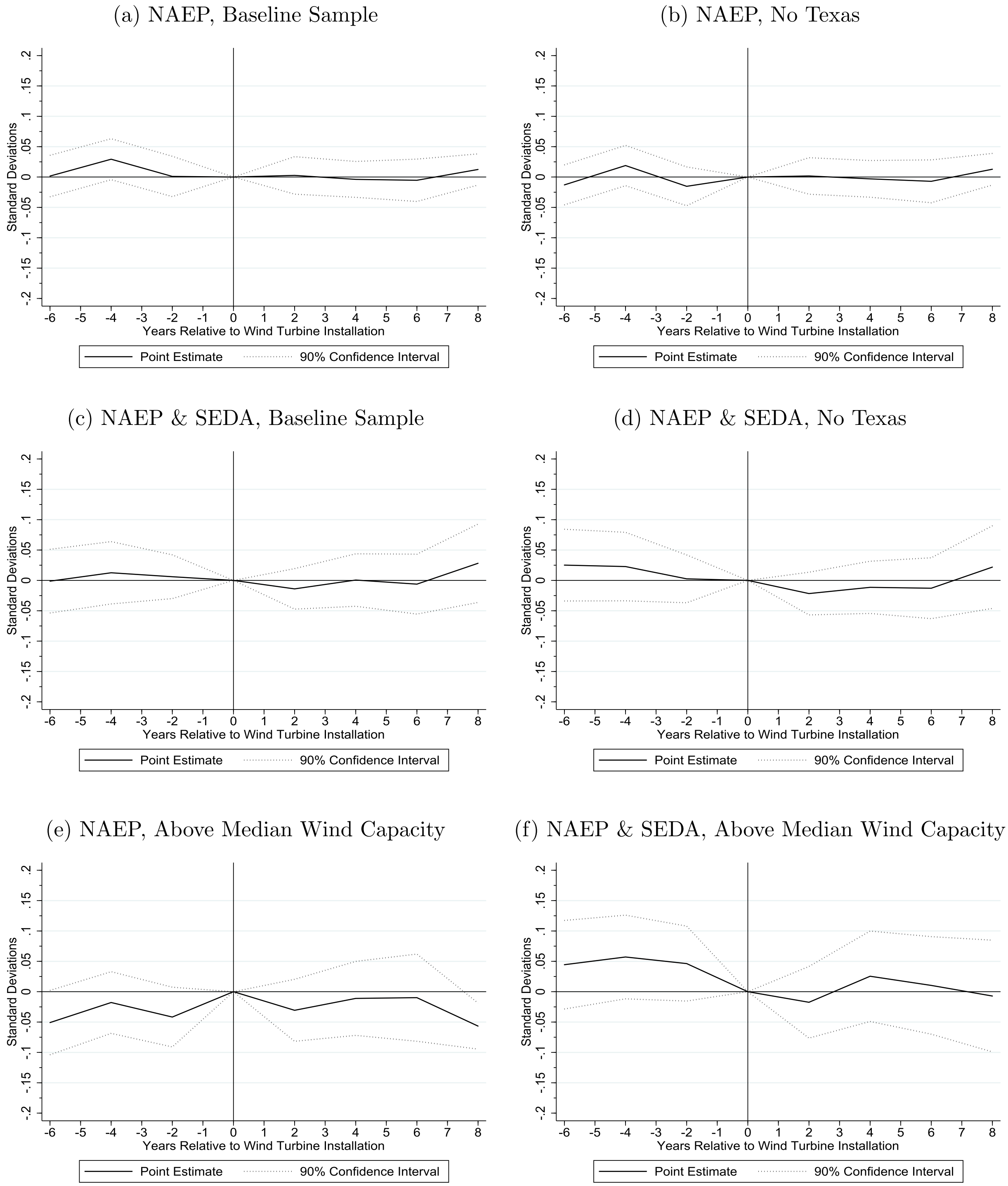

Fig. 6. Effects of Wind Turbine Installation on Student Achievement. Notes: Figures show event study estimates of the effects of wind turbine installation on standardized district mean test scores. Subfigures (a) and (b) use NAEP scores. In subfigures (c) and (d), we supplement the grade 4 and 8 NAEP scores from 1996 to 2007 with annual scores from the Stanford Education Data Archive for all school districts during 2009 to 2016. Subfigures (e) and (f) drop district-years with installed wind energy that is below the median of installed capacity/pupil. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

Fig. 6, there is no statistically significant coefficient on the *YearsPost*, suggesting no pre-trend, or on *Treat* or *YearsPost*Treat*, suggesting no impact. The calculated impact 5 years out is a statistically insignificant negative 0.8 percent of a SD for the baseline sample, and a statistically insignificant positive 0.1 percent of a SD increase for the sample without Texas. Five years post, we can rule out positive impacts of 2.5 percent of a SD with 95% confidence.

Fig. 6c and 6d show the impacts on test scores using the combined NAEP and SEDA achievement data, with and without Texas, respectively. Recall, that while the NAEP data are available only every other year and for a sample of districts, the SEDA data are available annually from 2009 – 2015 for the universe of school districts. Again, we see no evidence of any positive impact of wind energy installation on achievement, and can rule out increases of about 0.05 SDs for most years. We show results from the DD model in Panel B of Table 5. As in Panel A (NAEP only), we again see no statistically significant coefficients on any of the three parameters of interest, nor on the effect 5 years post installation. The effects 5 years post are −0.036 and −0.033 with and without Texas, respectively, but again neither is statistically significant.[34] Given the standard errors of 0.023 and 0.024, we can rule out with 95% confidence positive impacts of 1.5 percent of a SD. While these estimates do suggest a negative effect, we hesitate to interpret them as such given the imprecision of the estimates, and prefer to conservatively infer a lack of positive impacts. It is worth noting, however, that a negative impact on achievement is not entirely implausible. To the extent that the large increases in capital spending are for building new schools, in the short-run switching schools has been shown to be detrimental to student achievement (Brummet, 2014; Conlin and Thompson, 2017).[35]

Given the heterogeneity we found in the effects on revenues and expenditures by installed capacity per pupil, we next examine whether there is any evidence of positive achievement effects for those treated districts with greater capacity per pupil. In Fig. 6e and f, we show the event study for the NAEP sample and NAEP/SEDA sample, respectively, dropping treated district-years in the bottom half of the capacity per pupil distribution. While we lose some precision, neither picture provides evidence of a positive effect. In columns 5–7 of Table 5, we show effects including only treated district-years below median (column 5), above median (column 6), and above the 75th percentile (column 7). In no cases for either the NAEP or NAEP/SEDA sample do we see any evidence of a pattern of positive achievement effects at higher installed capacities per pupil.

We show in Appendix Table 10 the same sensitivity checks for our achievement analysis that we conducted for our revenues and expenditures results in Table 3, and all of the results are robust. However, an additional threat to the validity of our achievement analysis is the possibility of noise pollution from the wind turbines negatively affecting achievement, and therefore biasing our results downward. There is a debate in the academic literature and among policy-makers about the existence, extent, and effects of noise pollution from wind turbines (see, for example, Zou (2020), Guski et al. (2017), and Michaud et al. (2016)), with no clear consensus. Any evidence suggests an effect that diminishes with distance from the wind turbine. This motivates our attempt to test whether noise pollution biases our results by estimating impacts of wind installation on achievement by the distance from the installed wind turbines to the district centroid. We also, using school-level achievement data in Texas, examine effects by the distance from installed wind turbines to the school. As shown in Appendix Table 11, we find no evidence that achievement effects are smaller for districts or schools closer to wind turbines. This finding suggests that noise pollution from wind turbines, while perhaps important in some contexts, does not explain our null finding of wind energy installation on student achievement.

While we find no evidence of achievement effects from the increases in capital spending in our context, it is worth formally comparing the magnitude of our effects on a per-dollar basis to those we see in other recent studies examining the impacts of school capital. Jackson and Mackevicius (2021) provide a meta analysis of prior work, finding overall that a $1,000/pupil increase in capital spending increases test scores by 0.015 SDs six years after treatment.[36] We see a mean increase in capital spending of $447 (Table 2, column 5). Using the estimate from Jackson and Mackevicius (2021), such an increase should cause a 0.007 increase test scores after six years. Our effects are fairly precise, as we can rule out approximately a 1 percent of a SD increase in test scores, but not precise enough to rule out 0.007 SDs. Thus, we cannot reject positive achievement effects on the order of those found in the recent literature, as summarized by Jackson and Mackevicius (2021).

What about any achievement effects of the positive impacts on current spending? Our estimated effect on current spending of $187 per pupil (Table 2, column 5) is 31.0% (=$187/$604) of that found in Lafortune et al. (2018) for low-income school districts (see their Table 4, column 3). They find that school finance reforms increased test scores by 0.007 SDs a year for those districts, or 0.035 after five years. Scaling that 0.035 by 31.0% to account for our smaller impact on current spending yields an effect of 0.011 SDs, which we cannot quite rule out given our estimated effect five years out using the combined NAEP and SEDA data. Note also that Lafortune et al. (2018) find significant increases in expenditures on teacher salaries and reductions in class sizes, while we do not, which could help explain why we find zero impacts on achievement, even given the small increases in current spending.[37]

4.5. Effects of wind energy installation in Texas

One weakness of our achievement analysis relative to the revenues and expenditures analysis, is that we do not have annual, district-level, national achievement data for the bulk of our sample period. A second weakness is that we have no longer-run student outcomes, even though it is possible that capital spending could increase a student's educational attainment, for example, without necessarily improving test scores, by improving students' experience in, and attitudes toward, school. To address these weaknesses, we turn to a case study focusing on a single state: Texas. Texas is the second most populous state (after California), is by far the top wind energy producer in the nation, and, importantly, has publicly available district-level administrative data on average test scores going back to the beginning of our sample period (1994–95), as well as longer-run student outcomes, such as high school graduation rates.[38]

We begin our case-study with event-study pictures showing the effects of wind energy installation on per-pupil district revenues and expenditures using the Texas administrative data (Fig. 7). Local and total and revenues quickly increase by roughly $2,000 per pupil just a couple years post-installation. Total expenditures increase by more than twice that amount, which, as in the national data, can be explained by bond proceeds from capital campaigns being counted only in expenditures and not in revenues. Unsurprisingly, the impacts on total expenditures are driven by large, nearly immediate increases in capital spending approaching $4,000 per pupil, and also by slowly emerging increases in payments servicing debt, presumably to pay off capital outlay bonds. In fact, we see very large increases in outstanding debt on the order

[34] Appendix Table 8 shows zero impacts on test scores overall or for districts with greater capacity per pupil using only the SEDA data.

[35] We unfortunately do not have enough statistical power to examine heterogeneity in the effect of wind installation on achievement by district characteristics. Appendix Table 9 shows effects by 1990 district median household income, and suggests that the negative achievement effects may be driven by wealthier districts, but the noisiness of the results precludes firm conclusions.

[36] In prior capital spending papers, the capital spending increases are temporary, often due to the passage of bonds for specific capital projects. Jackson and Mackevicius (2021) therefore distribute the cost of these temporary increases across the life of the new capital structure, drastically reducing the per-pupil capital exposure of the immediately affected cohorts. In our context, this approach makes less sense, because we see permanent, growing impacts on capital spending due to the permanent nature of the wind installations. Therefore, in our comparison, we do not attempt to smooth the costs of the capital spending increases.

[37] For example, they find a reduction in the pupil teacher ratio of 0.65 pupils, while we find a reduction of 0.03 pupils. We can rule out a reduction greater than 0.08 pupils.

[38] The data come from Academic Excellence Indicator System (AEIS) reports from 1994-95 through 2011–12, and from the Texas Academic Performance Report (TAPR) from 2012-13 through 2017–18.

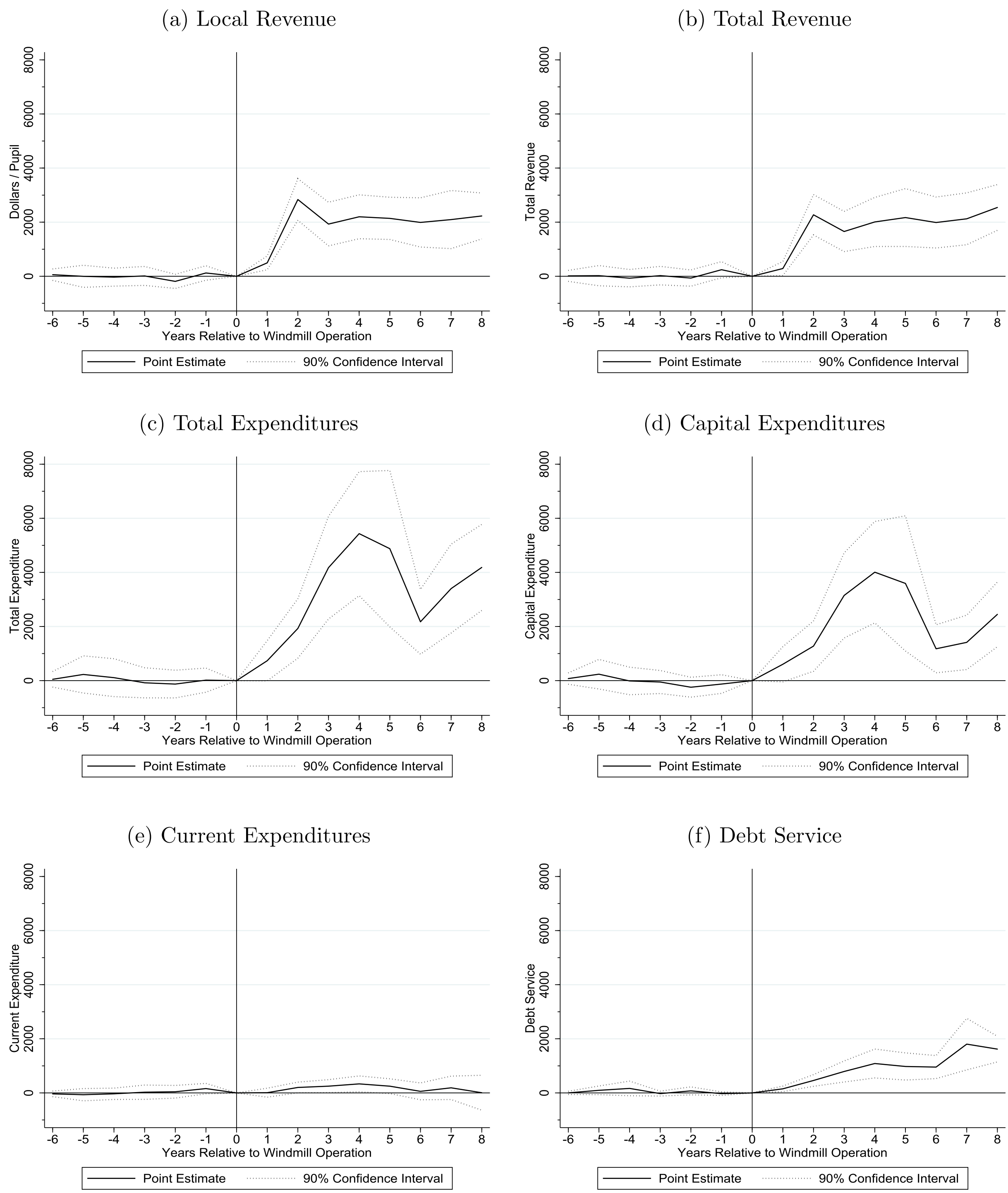

Fig. 7. Effects of Wind Turbine Installation on District Finances in Texas. Notes: Figures show event study estimates of the effects of wind turbine installation on district revenues and expenditures using administrative data from Texas from 1995-2018. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

of $10,000 per pupil (see Appendix Fig. 9), suggesting that, indeed, the large increases in capital spending are due to the passage of new bonds. Compared to these large increases in capital spending and debt, there are only tiny positive impacts on current spending.

We next examine the impacts of these increased capital expenditures in Texas on student outcomes. Focusing first on student test scores, Fig. 8a shows a somewhat noisy, null effect post-installation.[39] For most years, we can reject anything greater than a ten percent of a standard deviation score increase. Turning to the estimates from equation (4), neither the *Treat* or *PostYears*Treat* coefficients, nor the effect five years post-installation are statistically significant (Table 6, columns 1 and 2). For the latter, given the −0.058 point estimate and 0.056 standard error, we can reject with 95% confidence an increase in test scores of more than 5 percent of a standard deviation, though we note that the confidence interval includes fairly large negative effects.

Given that capital spending could impact important longer-run students outcomes, such as educational attainment, in spite of its zero impact on scores, we turn to examining high school

[39] To be consistent with the national analysis, we use the average of math and reading scores for grades 4 and 8.

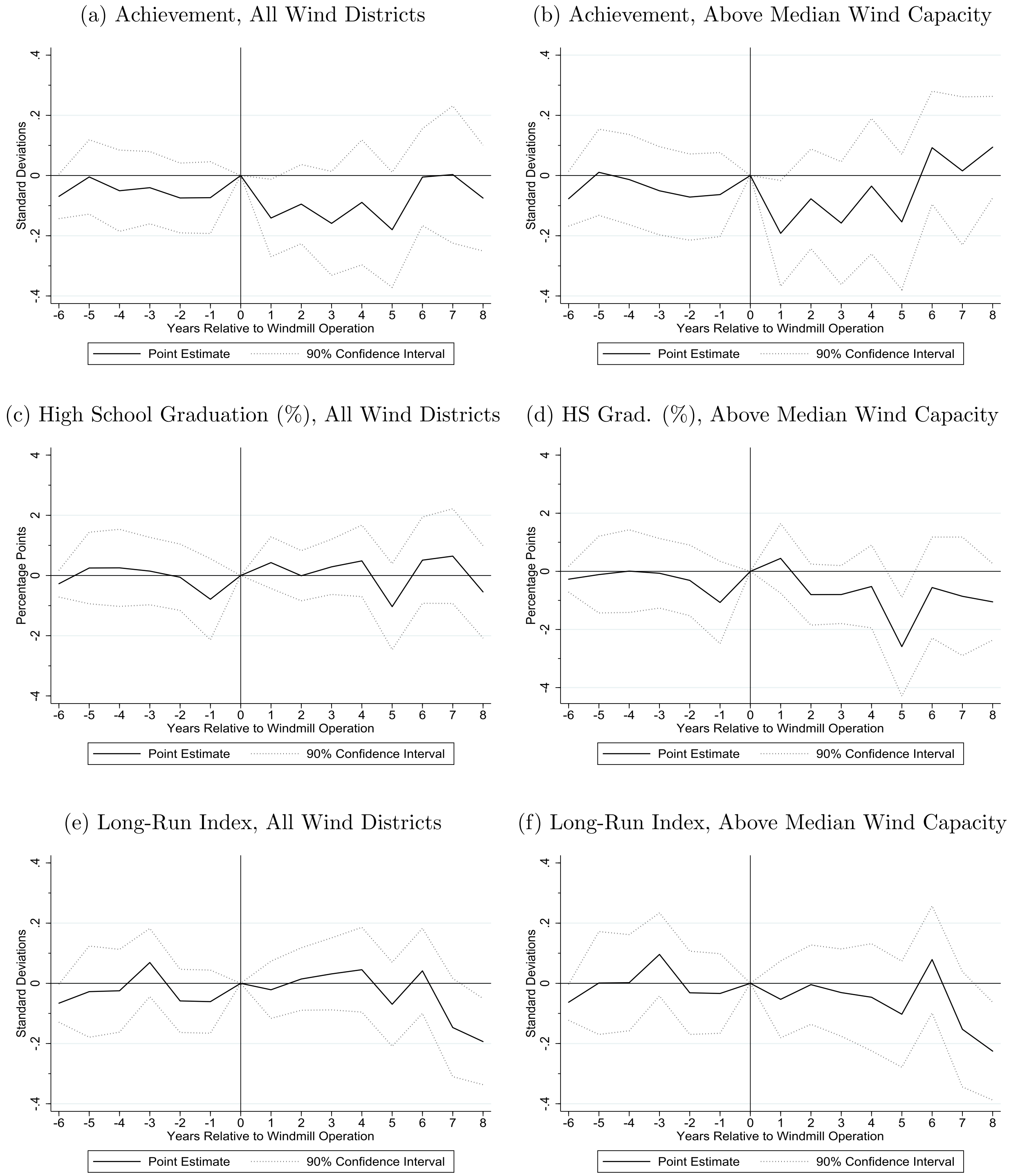

Fig. 8. Effects of Wind Turbine Installation on Student Outcomes in Texas. Notes: Figures show event study estimates of the effects of wind turbine installation on district average test scores, high school graduation rates, and an index of long-run student outcomes using administrative data from Texas from 1995-2018. Test scores and the long-run index are standardized to mean 0, SD 1. Graduation is a percent (0–100%). Solid lines are point estimates, and dashed lines are 90% confidence intervals.

graduation. Available beginning in 1996–97, a district's graduation rate is defined as the number of graduates in year *t* divided by the number of 9th graders in year *t-4*, subtracting transfers out of the district and adding transfers into the district. We find a precisely estimated null result of wind energy installation on high school graduation. The event study (Fig. 8c) coefficients hover between −1 and 1 percentage point, and given the point estimate and standard error from the calculated effect 5 years post-installation, we can reject an effect greater than 0.7 percentage points (off a mean of 90.9 percent).

To examine whether capital spending affected other longer-run measures of student performance aside from graduation, we create a standardized index of longer-run student outcomes combining the high school graduation rate with five additional measures

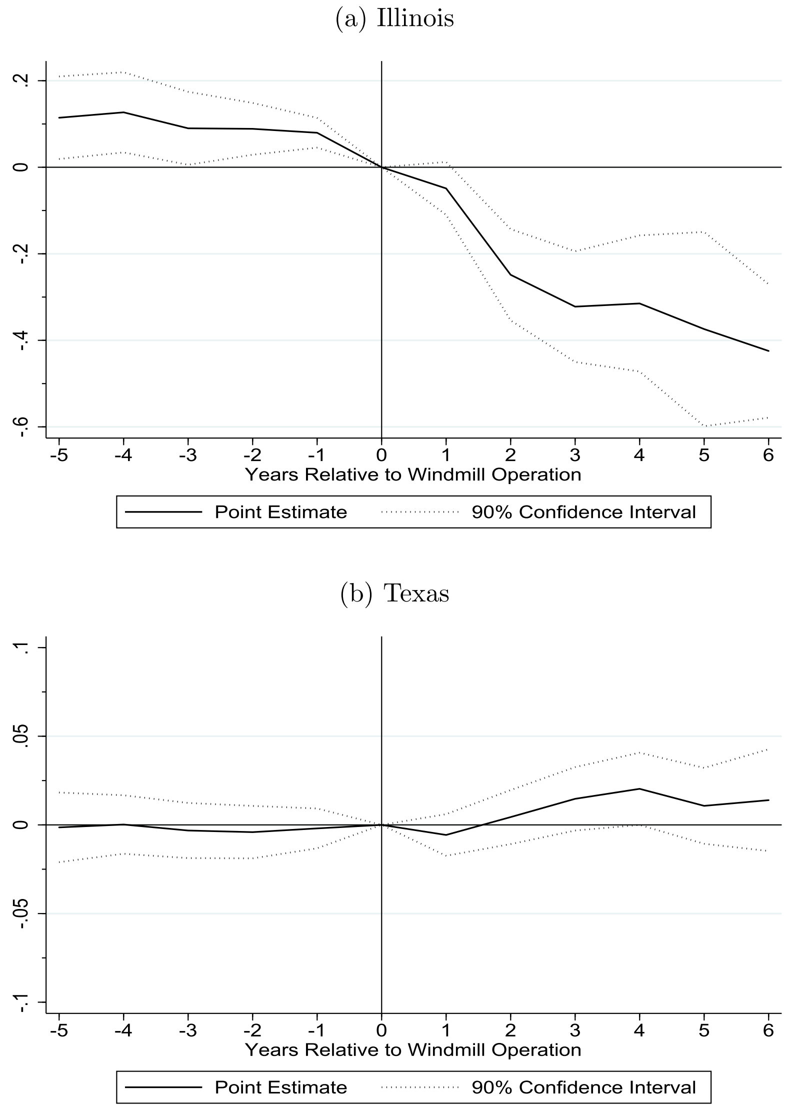

Fig. 9. Effects of Wind Turbine Installation on District Property Tax Rates. Notes: Figures show event study estimates of the effects of wind turbine installation on local school district property tax rates. Solid lines are point estimates, and dashed lines are 90% confidence intervals.

reflecting advanced course-taking, Advanced Placement (AP) exam-taking, and college entrance exam-taking and performance.[40] Following Kling et al. (2007), we create the index by normalizing each outcome to have a mean of zero and standard deviation of one, and then take the simple average of all of the outcomes. Again, we find no evidence of any positive impact of wind energy installation on this longer-run student outcome index (Fig. 8e), although the point estimates are not particularly precise: five years post-installation we can reject an effect larger than 8 percent of a standard deviation.[41] Finally, we find no evidence of any positive impacts on test scores, high school graduation, or the longer-run index when focusing on treated districts with installed capacity per pupil above the median (see Fig. 8a, c, and d).

One potential explanation for the null effects we find of capital spending on student outcomes is that districts are inefficiently using the new revenue on capital instead of current spending, due to the incentives previously discussed, but that there is little need for additional capital spending in the district. This would be a concern as it would raise questions about the generalizability of our results to situations where districts voluntarily increase capital spending. We test this hypothesis by examining heterogeneity of our effects by baseline building age and infrastructure quality in the district, under the assumption that districts with greater baseline average building age and lower infrastructure quality are more in need of capital spending.[42] We present the results in Appendix Table 13. We find no evidence of larger effects for the districts with

[40] The five additional measures are: 1) Percent of 11 and 12th graders taking an AP exam, 2) percent of graduates who took the SAT or ACT, 3) the percent of graduates who took the SAT or ACT and scored above a state-determined college-readiness threshold slightly above the national median, 4) percent of 9th-12th graders who took any state-determined advanced coursework or dual-enrolled in a college course, and 5) percent of graduates who completed the state-determined recommended high school curriculum. Effects for each outcome individually are presented in Appendix Table 12.

[41] Given that these longer-run outcomes may take several years to be affected, in Appendix Figure X, we show event study pictures for our student outcomes results nationally and for Texas adding relative year dummies for 9, 10, 11, and 12 (or more) years post wind installation rather than combining them into the 8 or more relative years dummy. The patterns of results look the same.

[42] Data on baseline district average building age and infrastructure quality come from Martorell, Stange, and McFarlin (2016), and we thank these authors for sharing their data. The data are from a 1991 detailed statewide Texas survey that collected information about (nearly) every school building in the state. Our measure of infrastructure quality is a standardized index that includes quality ratings for all available categories: floors, ceilings, rooms, structure, foundation, exterior, windows, roof, heating, cooling, lighting, plumbing, outdoor area, as well as the number of computers.

older buildings or lower infrastructure quality at baseline on student test scores, high school graduation, or the long-run outcome index. These results, while statistically imprecise, suggest that our null effect of capital spending on student outcomes is not driven by the typical wind district having high baseline infrastructure quality, or otherwise inefficiently spending the revenue on capital expenditures.[43]

4.6. Flypaper and local tax rates

Given that there appear to be no benefits to school districts of wind energy installation in the form of higher student achievement, how else might districts benefit? One possible way school districts may benefit is through taking a share of the revenue increase as property tax relief. A large literature in public economics examines the extent to which local jurisdictions reduce local tax effort in the face of a windfall of revenues that are designated for a particular purpose (e.g., education), versus the extent to which the money "sticks where it hits," a phenomenon often dubbed the flypaper effect.[44] Some studies find substantial or even complete flypaper (Feiveson, 2015; Dahlberg et al., 2008), while others find little or no flypaper (Knight, 2001; Gordon, 2004; Lutz, 2010; Cascio et al., 2013). In our context, we clearly find at least some flypaper, given the large increases in local revenue. But isolating the precise magnitude of flypaper is challenging, given the heterogeneity in state and local laws governing wind energy taxation.

In theory, one could use information on wind energy taxation laws and pre-installation local tax rates to predict the amount of revenue that should flow to local school districts from a 1 KW/pupil increase in installed capacity, and then compare this predicted amount to our estimated effect on local revenue – any difference between the two would be the estimated amount of property tax relief. Unfortunately, as described in Section II, laws governing wind energy taxation are extremely opaque, usually interact in a complicated manner with school finance formulas, and are often determined at the county or local level, preventing us from undertaking this calculation for our main sample. To our knowledge, Illinois is the only state that during our sample period had relatively clear and straightforward state-level laws determining the flow of revenues from installed wind energy capacity to school districts. In Appendix B, we use information on Illinois state laws and districts' pre-installation tax rates to conduct a back-of-the-envelope calculation comparing our estimated effect on local revenues in Illinois to the effect of a one MW increase in installed wind energy on local district revenues as predicted from the state laws. We find that the predicted local revenue increase using tax laws and pre-installation tax rates, and therefore assuming no crowd-out, is $3,698, compared to our estimated effect restricting our sample to Illinois of $3,020. Thus, we estimate that property tax relief accounts for 18% (= ($3,698 - $3,020) / $3,698) of the total predicted revenue increase, while 82% of the revenue flows to schools. This is a non-trivial amount of local crowd-out, but is on the high end of estimated flypaper effects.

We provide additional evidence on the flypaper effect by directly estimating the impact of wind energy installation on local school district property tax rates for two states, Illinois and Texas, where we could obtain historic school district property tax rate data.[45] These data are available for a somewhat shorter, more recent period than our baseline data: 2001–2017 for Illinois, and 1998–2018 for Texas.

Fig. 9a shows that in Illinois, wind energy installation leads to a statistically significant reduction in the tax rate of about $0.40 by six years out, which is a 11% decrease relative to the mean tax rate in Illinois of $3.75 (for every $100 of assessed value). This result implies that in addition to the local revenue increases after wind energy installation in Illinois, local school district residents are benefiting from property tax relief. In Texas, we see a different story (Fig. 9b). Here we see near zero, slightly positive impacts on tax rates. While seemingly counterintuitive, this result is consistent with the Texas school finance laws described above. Specifically, the laws incentivize wind energy districts to pass bonds to raise capital expenditures, and these bonds require increasing tax rates to pay the bonds. Thus, because of the particular formula and recapture aspect of the Texas laws (which focus on current expenditures), districts are incentivized to actually increase their tax rates, a form of crowding-in, after installing wind energy. In neither state do we see any evidence of pre-trends, suggesting that wind developers were not strategically locating based on trends in local tax rates. While we could only obtain historic tax rate data from two states, these two states provide examples of: 1) how school districts in some cases are taking some of the benefit of wind energy as property tax relief, and 2) the significant degree to which local context matters for whether and to what extent local tax effort is crowded out (or crowded in) in the face of a windfall of tax revenue.

5. Discussion

How does our null effect on achievement compare to the prior literature, and what are some possible reasons that other studies find positive impacts of achievement on school spending while we do not? The primary difference between our study and the majority of those examining the impacts of school spending is that most focus exclusively or primarily on operating expenditures, almost universally finding positive impacts on student achievement (see Jackson, 2020). As previously noted, given the small impacts on operating expenditures in our sample, we cannot rule out the small achievement effects that would be predicted from those increases given recent estimates in the literature (e.g., Lafortune et al., 2018).

Where we believe our null effect is more informative and useful to compare to prior work is in comparison to the relatively small number of studies examining the impacts of school capital spending. Table 7 provides information about the nine studies (to our knowledge) that examine the impacts of capital spending on student outcomes. Two of the studies, Neilson and Zimmerman (2014) and Lafortune and Schönholzer (Forthcoming) find large achievement effects. They focus on very large capital projects in impoverished urban districts (New Haven, CT and Los Angeles, CA, respectively) with dilapidated school facilities, where the treatment is exposure to newly built schools, as opposed to more common and less expensive uses of capital expenditures such as renovations and modernization of school buildings or equipment purchases. Three of the studies Hong and Zimmer (2016), Conlin and Thompson (2017), and Rauscher (2020) find some suggestive evidence of small positive achievement effects, but these either occur only after negative impacts or for certain years, subgroups,

[43] A related potential explanation for the null effect is that districts are using the capital expenditures for construction or purchase of buildings, land, or equipment that are not academic in nature and unlikely to affect student achievement, for example, building a football stadium. We collected information on the use of capital spending from all school district bond elections in Texas over our sample period from the Texas Bond Review Board. The data are somewhat vague, but suggest that most of the capital outlays are being used toward new school construction and modernization and not building football stadiums or related uses such as gymnasiums.

[44] See Hines and Thaler (1995) and Inman (2008) for detailed discussions of and evidence on the flypaper effect.

[45] We observe actual property tax rates, not rates estimated by dividing tax receipts by assessed valuations.

Table 7
Prior Literature Examining the Impact of School Capital Spending.

	Study (1)	State or City (2)	Years (3)	Context / Methods (4)	Treatment (5)	Student Outcomes (6)	Effect Size and Heterogeneity (7)
1)	Baron (Forthcoming)	Wisconsin	1997–2015	RD comparing winning vs losing district referenda to exceed revenue limits.	$4,000/pupil (200%) in year 1; $2,000/pupil year2; zero by year 3	Test scores, high school dropout, college enrollment	No impact on any outcome.
2)	Cellini, Ferreira, Rothstein (2010)	California	1987–2006	RD comparing winning vs losing bond elections.	$1,000/pupil per year over 3 years.	Test scores	No impact
3)	Conlin, Thompson (2017)	Ohio	1997–2011	IV for capital spending using district eligibility for state subsidy program	$1,000/pupil per year over four years	Test scores	Negative in first 1–2 years; Positive 0.02 SDs in years 3–4
4)	Goncalves (2015)	Ohio	1998–2014	Event study using timing of receipt of state subsidy	Exposure to new funding ($/pupil N.A.)	Test scores	Negative initially, then no impact.
5)	Hong, Zimmer (2016)	Michigan	1996–2009	RD comparing winning vs losing bond elections.	$1,000/pupil per year over three years	Test scores	2–6% increase in proficiency 5–7 years after
6)	Lafortune and Schönholzer (Forthcoming)	Los Angeles, CA	2003–2013	Event study using timing of student attending newly constructed school	Exposurure to newly constructed school ($/pupil N. A.; median cost $56.5 million)	Test scores, attendance, teacher-reported effort	0.05–0.1 SD increase in scores; attendance and effort also improve
7)	Martorell et al. (2016)	Texas	1997–2010	RD comparing winning vs losing bond elections.	Cumulative $5,000/pupil over 6 years	Test scores	No impact
8)	Neilson, Zimmerman (2014)	New Haven, CA	2004–2010	Event study using timing of student attending newly constructed school	Exposurure to newly constructed school ($/pupil N. A.; median cost $56.5 million)	Test scores	Immediate, growing positive impacts on reading of 0.4–1.2 SDs; No effects on math
9)	Rauscher (2020)	California	1999–2013	RD comparing winning vs losing bond elections.	$2,000/pupil per year over 3 years	Test scores	No impact years 1–5; positive 5–7% effect for baseline low-scoring districts in year 6

Notes: This table describes all papers (to our knowledge) that estimate the causal impact of school capital spending on student outcomes. See main text for more discussion of these papers and how their context, treatment, and estimated effects compare to those in our paper.

or subjects. Finally, four studies, studying some of the largest states in the nation (e.g., California, Texas, Ohio, and Wisconsin) find zero evidence of any impacts on student outcomes (Cellini et al., 2010; Martorell, et al., 2016; Goncalves, 2015; Baron, Forthcoming).

Our assessment of this literature is that the only two studies finding large achievement effects evaluate very large spending amounts (e.g., newly constructed schools costing tens of millions of dollars) in baseline poor areas with the lowest infrastructure quality, whereas the districts in the other prior literature and in our context typically see smaller capital investments in areas with more typical income and infrastructure quality levels. Ideally we would restrict our sample to urban, low-income districts exposed to large capital investments and test whether student achievement increased. Unfortunately, while we do show some suggestive evidence of relatively larger achievement gains in low-income relative to higher-income districts in our sample (see Appendix Table 9), we just do not have sufficient samples of urban districts to explore these issues empirically, especially because such districts in our sample have the smallest installed wind capacity per pupil, and thus see the smallest impacts on revenues and expenditures. In spite of this limitation, our study provides further evidence that the positive achievement gains due to school facility investments appear to be limited to capital projects and settings such as those in Neilson and Zimmerman (2014) and Lafortune and Schönholzer (Forthcoming), as compared to more typically-sized capital investments in more common statewide or, in our case, national settings.

6. Conclusions

The only well-identified, national-scale evidence of whether increased school resources improves student outcomes comes from a single policy reform: school finance reform. In this paper, we provide evidence on the impacts of increased school funding due to wind energy installation, a novel source of funding variation affecting most states since the 1990s. We use data on the timing, location, and capacity of the universe of wind energy installations in the U.S. from 1995 through 2016 to examine the impacts of wind energy installation on school district revenues, expenditures, resource allocations, and student achievement. We geocode wind energy installations to school districts, and combine data on the timing and capacity of wind installations with National Center for Education Statistics (NCES) and Schools and Staffing Survey (SASS) school district data on revenues, expenditures, staffing, enrollments, and teacher salaries, and with student achievement data from the National Assessment of Educational Progress (NAEP) and Stanford Education Data Archive (SEDA). We use event-study and difference-in-differences methodologies exploiting the plausibly exogenous timing and location of wind energy installations.

We find that at mean levels of wind energy installation, districts saw large, exogenous increases in total per-pupil revenues due to increases in local revenues, with only minimal offsetting reductions in state-aid. Per-pupil expenditures increased accordingly, with the majority of the revenues spent on capital outlays, causing dramatic increases in capital expenditures, and only modest increases in current expenditures, with little to no reduction in class sizes or increase in teacher salaries. These effects were driven by districts with smaller enrollments and larger wind installations, and thus greater installed capacity per pupil. We find zero impacts of wind energy installation on school district average test scores overall and for districts with larger installed capacity per pupil. We replicate our main analyses using administrative data in Texas, the largest wind producing state, and further show that wind energy installation had no impact on high school completion or other longer-run achievement measures.

Finally, we examine the impacts of wind energy installation on local school district property tax rates in two states, Illinois and Texas. In Illinois, districts respond to the increased revenues from wind installation by reducing their property tax rates and taking part of the windfall as property tax relief. In Texas, where state laws incentivize districts with wind energy installations to pass new bonds to promote capital spending and to pay for these bonds by increasing property tax rates, we subsequently see tax rates slightly increase after wind energy installation.

Our study provides several contributions to the literature. First, we extend the literature examining the impact of wind energy installation on school districts by examining effects nationwide and on student achievement, compared to prior studies that focused on a single state and only examined impacts on school finances (De Silva et al., 2016; Reategui and Hendrickson, 2011; Ferrel and Conaway, 2015; Kahn, 2013; Castleberry and Greene, 2017; Loomis and Aldeman, 2011). Second, we contribute to the public economics literature on flypaper effects, finding strong evidence of flypaper, but also, as in other recent work (Brunner et al., 2020), finding that local context affects the extent to which revenue shocks are taken as property tax relief instead of increasing school budgets.

Finally, our study provides nationwide evidence on the effects of increased school spending on student achievement from an exogenous source of variation in spending other than school finance reform. Our finding that most of the increases in school spending are devoted to increased capital expenditures, and that these increases have no discernible impacts on student achievement, contributes to the growing literature on the impacts of capital expenditures on student achievement. We provide the first national evidence on the impacts of capital spending, supporting the findings in Cellini et al. (2010), Martorell et al. (2016), Goncalves (2015), and Baron (Forthcoming) that these capital investments do little to improve students' academic achievement or attainment.

This is not to say that money doesn't matter in schools. There are specific contexts, such as low-income urban areas with decrepit facilities where large capital investments such as new school construction have strong positive impacts on student achievement (Neilson and Zimmerman, 2014; Lafortune and Schönholzer, Forthcoming). Furthermore, most recent work using school finance reforms and other natural experiments to examine the impacts of increased operating expenditures find positive impacts (see Jackson, 2020). Our study highlights that money may matter, but it matters how you spend the money; and capital investments, at least in this setting, appear to be an inefficient use of funds if the goal is increasing student achievement. This may not be the goal – school buildings, especially in rural communities, can be a source of community pride and used for community events. Having higher quality and more modernized school facilities due to wind energy revenues may improve resident well-being in ways other than improved student achievement.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2021.104586.

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