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# Property Taxation as Compensation for Local Externalities: Evidence from Large Plants *

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**Abstract**

Large capital infrastructure projects often create important regional benefits, but impose substantial negative harms on nearby residents. When local jurisdictions have control over land use, spatial mismatch between the benefits and costs of these projects can prevent socially beneficial projects from moving forward or allow socially harmful projects to be built. In theory, Coasean bargaining can solve this problem, but in practice high transaction and coordination costs make such a solution infeasible. In this paper, we explore instead how one practical compensation mechanism, local property taxation, affects this problem. Using evidence from large plant openings and school districts, we first demonstrate that property tax payments from large plant openings are economically large and valued by local residents. We show that following a power plant opening, the average host district sees a 8-10% increase in tax base per student, leading to both reduced property taxes and increased educational spending. We next show that these changes are valued by local residents—home values on the plant district's side of a boundary increase by 4%-6% following an opening relative to similar homes on the opposite side of the border. We finally demonstrate that limiting jurisdictions' access to local property tax revenues decreases local exposure to large plants. We use state school finance equalization reforms as a plausibly exogenous shock to districts' marginal value of tax base with respect to school spending. Using a contiguous border county design, we show that after the implementation of these reforms, manufacturing employment and the number of large manufacturing establishments fall in affected counties by 10-15%, both in absolute and relative terms. These results suggest decreased industrial development is an important unintended consequences of higher-level federal policies that affect local jurisdictions' ability to raise and retain property tax revenues.

**Keywords**: power plants; property tax; fiscal decentralization; school finance equalization; housing values; Coase Theorem

**JEL Classification**: H20; Q50; R30

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# 1 Introduction

Large capital projects often produce benefits and costs on substantially different spatial scales (Greenstone, Hornbeck, and Moretti, 2010; Luechinger, 2014; Currie et al., 2015; Persico and Venator, 2018). If local jurisdictions have control over land use, approval for these types of projects will depend upon whether a project's benefits outweigh the costs for the host jurisdiction regardless of any net benefits or costs to society. As a result, spatial mismatches between costs and benefits have the potential to create large inefficiencies. For instance, projects that create a net benefit for society may not produce net benefits for any given local jurisdiction, while other types of projects may create net benefits for a given host jurisdiction but not in the aggregate. Because local control of land use is ubiquitous in the United States, these types of inefficiencies likely have a significant effect on economic growth.

In theory, this problem could be solved through Coasean barganing between affected households and/or jurisdictions. However, in practice high transaction costs, coordination costs and the hold-up problem make such compensation difficult (Coase, 1960). In this paper, we study how one major existing low-cost compensation mechanism, local property taxation, affects this problem. Property tax payments represent a localized benefit stream that is restricted to the project's home jurisdiction. If these payments are valued, they should be an important driver in the creation of local projects, exacerbating inefficiencies where projects are oversupplied and dampening inefficiencies where projects are undersupplied. However, it is unclear ex-ante if these payments are valued by local homeowners; they may be too small in magnitude, captured by local bureaucrats or spent in a manner that does not increase local well-being. Because many state-level policies affect local jurisdictions' ability to collect and retain local property tax revenue, understanding both how the stream of benefits from property taxation are valued by local residents and whether these benefits affects decisions about local land use is extremely policy relevant.

We study this question empirically in two steps. We first establish that property tax payments from large capital projects are large in magnitude and valued by local homeowners as proxied through home prices. We then show that restricting these payments has large effects on where large capital projects locate. Our specific empirical context is large power and industrial plants and school districts. These types of large plants exemplify projects that create large and spatially divergent costs and benefits. We adopt school districts as our measure of local government because they are the majority recipient of property tax dollars and a major determinant of home prices and locational choice across the United States.

We begin by examining the fiscal effects of power plant openings on host jurisdictions. Using a triple-difference design centered around power plant openings with different expected

local fiscal impacts, we show that on average an opening increases a host district's tax base per student by 8-10% per student. This tax base increase leads to both a small decrease in property tax rates and an increase in educational spending concentrated on capital expenditures. We next show using a border difference-in-differences design and Zillow home transaction data that these changes are valued by local homeowners. Specifically, we find that home prices increase by 4%-6% following a plant opening in the host district relative to similar homes directly across the district boundary.

We then provide evidence that these property tax payments are not only valued by homeowners, but are also important determinants of local exposure to externality-producing projects. To examine this question empirically, we use school finance reform legislation/litigation as a plausibly exogenous shock to the marginal value of an additional dollar of tax base with respect to local school revenues.[1] Using a contiguous-border county difference-in-differences design, we first replicate past work showing that these reforms led to large increases in state revenue, decreases in property tax revenue, and decreases in revenue disparities between high and low poverty districts. We next show that these reforms led to meaningful (10-15%) declines in large manufacturing establishments and manufacturing employment in the years following enactment both in absolute terms and as a share of total county establishments and employment. Both sets of results are robust to a variety of specifications, covariates and weighting schemes and show no evidence of any pre-trends.

This paper makes two major contributions to the existing literature. First, we demonstrate local property tax payments are an important localized benefit stream produced by large projects, which has large distributional implications for who benefits and who is harmed from a given opening. This finding builds on a large literature on the specific costs and benefits of plants and other large capital projects (Greenstone, Hornbeck, and Moretti, 2010; Bartik et al., 2018; Luechinger, 2014; Currie et al., 2015; Persico and Venator, 2018; Barrows, Garg, and Jha, 2018), the distributional consequences of these projects (Banzhaf, Ma, and Timmins, 2019), as well as a growing public finance literature examining the local fiscal effects of natural resource windfalls (Marchand and Weber, 2015; Martinez, 2016; Sances and You, 2017).

This finding also contributes to a large public finance literate studying how various local amenities and tax rates are capitalized into home values (Oates, 1969; Nguyen-Hoang and Yinger, 2011; Black, 1999; Bayer, Ferreira, and McMillan, 2007; Anderson, 2006). While previous research has focused largely on the effect of outputs (school quality, changes in tax rates), we examine the effects of a shock to inputs (tax base). Our finding that changes

[1] Because power plant openings are relatively rare, we instead use manufacturing establishments and employment as a proxy for exposure to polluting plants in this section.

in the (non-residential) tax base itself leads to home price increases provides new evidence that local politicians are putting this increased revenue to work for the benefit of the local homeowners rather than engaging in bureaucratic capture (Martinez, 2016).

Second, we provide new evidence that shifts in local governments' ability to raise and retain property tax revenue has important effects on local land-use decisions and industrial mix. This finding is particularly important in a federal system, as many state and federal policies aimed at other economic goals can affect this incentive, potentially creating large unintended consequences. This result builds upon a large literature focused largely outside the United States on the development incentives embedded in fiscal decentralization. Theoretical work has established the importance of local governmental incentives in encouraging economic growth (Weingast, 2009), while empirical work focused primarily in the developing world has found support for the idea that local government's share of local taxation matter for local public good provision (Han and Kung, 2015; Careaga and Weingast, 2003; Burnes, Neumark, and White, 2011; Zhuravskaya, 2000). However, because reforms are often nationwide and come with large income and political consequences, well-identified studies of these effects are scarce (Gadenne and Singhal, 2014).

These results also contribute to a large literature within the United States on the effects of a series of school finance reform that have transformed the American education system over the past fifty years. One strain of existing research has shown that these reforms have greatly increased low-income students long-run educational and earnings prospects (Biasi, 2019; Jackson, Johnson, and Persico, 2015; Hoxby, 2001; Card and Payne, 2002). A second strain of research shows that these reforms have decreased housing values by diluting the value of local tax dollars (Hoxby and Kuziemko, 2004), limiting mobility by restricting increases on assessed value of homes (Ihlanfeldt, 2011; Wasi and White, 2005) or decreasing local property tax burdens (Lutz, 2015; Ross, 2013). Our paper is the first to show that by divorcing the size of the local tax base from available revenue from schools, these reforms also had large effects on local land use decisions and in particular, on the development of local industry— an important unintended consequence.

The remainder of the paper is organized as follows. Section 2 provides background on the plant siting and property assessment process. Section 3 examines the effects of plant openings on local fiscal outcomes and estimates to which these changes are valued by local households. Section 4 investigates how changing local jurisdiction's ability to raise and retain property taxes affects local industry. Section 5 concludes.

# 2 Background

In this paper, we investigate the extent to which local property taxation can act as a compensation mechanism for exposure to local externalities as well as how this compensation mechanisms facilitates the creation of large projects within a jurisdiction. To answer these questions, we undertake a number of separate analyses that rely upon institutional details in plant siting, school district budgeting and state school finance systems. In this section, we provide some necessary background information to allow the reader to better understand the validity of the assumptions behind our identification strategies and the plausibility of our observed effects.

## 2.1 Plant Siting

The first portion of our paper examines the effects of power plant openings. Power plant siting is a complex process governed by a large web of state and local regulations.[2] Utilities take into account a large number of factors including access to transportation and energy infrastructure, local regulations, construction costs and environmental concerns (Cirillo et al., 1977). Further, as Davis (2011) points out, utilities face a significant trade-off between the low-cost and low-environmental impact of locating in rural areas and increased electricity transmission costs.

However, utilities must compare the benefits of a location with constraints imposed by local, state and federal governments. In general, new plants must be permitted by state and local governments. In 22 states, approval of a specific site does not require approval from the state (although general permits for plant construction are still necessary). In these states, local bodies (typically municipal and/or county governments) have the final say over whether or not a plant can locate in their jurisdiction. Conversely, twenty-eight states have power plant siting boards whose approval is necessary for a plant to locate at a specific site. These regulations appear to have changed little since the 1970s (Cirillo et al., 1977; Ferrey, 2016). In sixteen of these twenty-eight states, the siting board is able to preempt local land use rules and grant approval to a site over local opposition. In the remaining twelve states, local land use approval is a prerequisite for siting board approval (although there are some avenues for exceptions.) However, even in the sixteen states with preemption powers, local governments are allowed to be active participants in the permitting decision, and it is unclear in practice how often the wishes of these local governments are overruled. For non-power plants, there are no state siting boards and so local bodies have an even larger say in siting decisions.

[2] The discussion in this section owes a large debt to Ferrey (2016).

An additional important characteristic of siting is that local land use is typically governed by the local city council (in incorporated areas) or county commissioners (in unincorporated areas). School districts, the focus of our empirical study, have no control over local land use.[3] However, in many localities school districts are nearly coterminous with municipalities. For instance, Fischel (2010) finds that two-thirds of medium-to-large cities in the United States have substantial overlap with a single school district suggesting that municipal or county leaders will internalize any fiscal benefits to the school district. This overlap is likely even larger in rural areas. Further, even if a district is not coterminous with a local zoning jurisdiction, if the harms of a prospective plant are concentrated among individuals within the same school district we would again expect the relevant municipal leaders to internalize their preferences.

## 2.2 Local Taxation of Plants

In almost all states, power plants are required to pay local property taxes. In the majority of states, power plants are assessed by a state body tasked with valuing public utility property, but pay property taxes locally.[4] In a smaller number of states, utility property is both assessed and taxed locally. With few exceptions (i.e. wind power in Kansas), all privately-owned utilities pay local property tax. Taxation of publicly-owned utilities is more complex. Most major publicly owned utilities including the Tennessee Valley Authority and plants owned by Nebraska's public power districts make payments in-leiu of taxes (PILOT) to local areas. The amount of these PILOTs are typically set by statute and apportioned based on the fraction of a utility's property in a given jurisdiction. Non-power plants are almost always assessed and taxed locally.

Anecdotally, large industrial plants and other projects are recognized to be major contributors to local budgets. In communities nearby plants, local newspapers frequently remark on the magnitude of local power plant tax payments and discuss possible downward reassessments as being disastrous for local communities (Samilton, 2018; Williams, 2018). Similarly, a large threatened downward reassessment of pipelines in Northern Minnesota was reported as being potentially disastrous for local municipalities and schools. Public schools receive the majority of property tax revenue and about half of state and local education funding comes from property taxes. Additionally, local property taxes are often the only source of funding school districts have direct control over (Oates and Fischel, 2016). In the top panel

[3]The exception is in New England and in some states in the Mid-Atlantic where schools are run directly by municipalities/the county.

[4]This can happen either directly or indirectly with the state paying each jurisdiction its share of the total payment based on the proportion of utility property located in its jurisdiction.

of Figure A1 we show the importance of utility property to the tax bases of districts exposed to power plants. The figure shows the share of total valuation made up by utility property by district generation capacity (100 MW bins) in eight states with local utility valuation data.[5] Among districts with no generating capacity, utility property typically makes up 5% of the total tax base. However, this proportion rises quickly as generation capacity increases; in districts with 1000 MW of generating capacity, utilities make up 15% of the local tax base and in districts with 2500 MW capacity they make up over 30% of the tax base. Since these increases are typically due to the entrance of one or two plants, it suggests that utilities have the potential to radically change the nature of a locality's tax base.

We can also perform a similar analysis for industrial plants using data from Ohio (the only state that provided consistent assessment data for the industrial class). We see that the more TRI-reporting plants in a school district (a proxy for exposure to industry), the larger the share of industry as a proportion of total taxable value value (bottom panel of Figure A1).

Property taxes are typically charged as a proportion of the assessed value of local properties. The value of a property upon which taxes may be levied is commonly known as the taxable value and is often some state-set proportion of market-value.[6]. In some states, utility and industrial property have a different assessment ratio than other types of property leading to a higher or lower taxable rate. In most states it would be difficult to increase rates on these types of property without equivalently increasing rates on local homeowners.The process of setting local school property tax rates also differs significantly by state. In some states, tax rates are set annually by local elected officials, while in other states, rates are set by local referendum. Additionally, because many states have created strict limit on tax and revenue growth, meaningful increases in tax rates often must be approved directly by voters even if small changes need not be. This is also true for school bonds, which are repaid through increases in the local property tax rates. We discuss this process more in the next subsection.

## 2.3 School Finance Equalization

In response to both court orders and the threat of litigation, many states have undertaken dramatic reforms to their school finance systems over the past forty years (Jackson, Johnson, and Persico, 2015), moving from primarily local-based systems to systems with greater levels of state support. These reforms have typically centered around ensuring some combination of

[5]Connecticut, Georgia, Iowa, Minnesota, Ohio, Oklahoma, Oregon and Washington.

[6]In most states, assessment ratios are created at the state level. In a small minority of states, local control is possible. A notable exception is Pennsylvania, where assessment ratios are set by the county

adequacy or equity. Adequacy-based reforms work to ensure that all districts have sufficient funding to provide an adequate education to their students. Equity-based reforms work to ensure that large disparities in spending across districts within the state do not exist. In practice, most reforms have some aspect of both adequacy and equity. Hoxby (2001) and Jackson, Johnson, and Persico (2014) provide a more extensive overview of the history of school finance reforms in the United States.

Today, most states have a system that at least partially equalizes spending across districts. Although specifics vary from state-to-state, the vast majority of states have a foundation formula which provides a guaranteed amount of funding for a district based on the number of enrolled students, sometimes weighted by their expected expense of education (i.e. English as a second language students may be worth more than native speakers in the formula). Local districts are then assigned a portion of this formula for which they are responsible ("local share") based off of their local property wealth (or less-commonly a formula including property wealth, income and other determinants of local wealth). In order to maintain equity, districts in many states also limit the tax rate districts can charge above their expected local share (and in some cases recapture any revenue above a certain amount). The strictness of these limits varies dramatically across states.

In this paper, we are concerned with the marginal impact of an increased dollar of industrial or utility tax base on available school revenues. This figure is shaped by two major characteristics of the school funding formula; upper and lower limits on taxes and the degree of crowd-out based on property wealth. For instance, a simplified funding formula common to many states is:

$$Rev_d = F_d + S_d - \tau^* V_d + \tau_d V_d \tag{2.1}$$

$F_d$ are federal transfers to the district (which are independent of the local tax base), $S_d$ is the state guaranteed funding to the district, which is typically depending on local student characteristics but is independent of local tax base, $\tau^*$ is the state-assigned tax rate used to determine a district's local share (typically this is uniform across all districts), $V_d$ is the total assessed value of a district's property and $\tau_d$ is the district's chosen tax rate where $\tau_d \in (\underline{\tau_d}, \bar{\tau_d})$, state-set limits on the taxes that can be charged.[7]

To understand the extent to which school districts can increase revenue in response to changes in local tax base we need to know both how a reform affects both $\tau$ (through new state limitations) and $\tau*$, the level of crowd-out. For our analyses in the latter-half of the paper, we qualitatively estimate both of these quantities for all states over the past half century using *Public Finance in Public Schools in the United States*, a report issued roughly

[7]In reality, these formulas will be much more complicated, but because we are only interested in the effects of changing tax-base on revenue, this simple illustration acts as a decent proxy.

every five years from 1952 to 2018 that describes the school finance system in use by each state as well as relevant taxation and spending limitations. Using these data in combination with the narrative descriptions, we identify major school finance reforms that create large shocks in the marginal value of an increase in tax base with respect to total revenue. This process is discussed in more detail in Section 4 and Appendix C.

# 3 Are Property Tax Payments from Power Plants Valued by Local Homeowners?

We begin by investigating the extent to which property tax payments from large externality-producing projects represent a benefit stream to their local jurisdictions. This question is important for two reasons. One, the value of property tax payments has important implications for how We think about who benefits and who is harmed from large, externality-producing capital projects. Second, in order to answer our second question—how changing jursidictions' ability to raise and retain property tax revenue affects local land use decisions—it is first necessary to establish that such revenue is indeed valued by local homeowners. We break this question into two parts—we first estimate the magnitude of these payments and show how they are used by local communities. We then estimate how homeowners value these payments as measured through changes in local home values.

## 3.1 Data and Sample Selection

The data to perform the analyses in this section come from four primary sources. First, we obtain power plant location, opening dates, energy source and nameplate capacity from Form EIA-860, published annually by the Energy Information Administration (EIA). We assign each power plant to its 2000 elementary or unified school district using coordinates provided by the EIA and shapefiles produced by the 2000 Census.

Second, we obtain data on taxable value and district property tax rates by collecting data from state Department of Education and Department of Revenue annual reports. This is to our knowledge a novel dataset of longitudinal district tax rates and assessed values in both its geographic scope and temporal coverage.[8] The left panel of Figure A2 shows coverage by state, while the right panel shows coverage over time. We have data for forty states and the vast majority of these states have data on both property tax rates and taxable value. The right panel shows coverage over time. By 1999, we have data for over 50% of

[8]Biasi (2019) and Miller (2018) both collect similar data, but their collection includes fewer states and is over a more limited time period.

the districts in our sample and this number increases to over 80% by 2015.[9] It is important to note that states use different assessment ratios (the proportion of the true market value of a property that is taxable) and therefore although rates and assessed values are generally comparable within states over time, cross-sectional interstate comparisons of these variables are generally not informative. In our primary analysis, we inflate taxable values (deflate property tax rates) according to reported assessment ratios, however such a conversion is imperfect as states sometimes report a summary taxable value, but have different assessment ratios for different classes of property. As a result, in all regressions we include state by year (or more restrictive) fixed-effects so all comparisons are only made within state-years.[10]

Third, we use district finance, staffing and demographic data from the National Center for Education Statistics (NCES) created by the Rutgers Graduate School of Education Education Law Center (Weber, Srikanth, and Baker, 2016). For all fiscal years between 1995 and 2015, we have detailed data on revenue sources, expenditure destinations, district staffing by occupation, student race/ethnicity and free-lunch eligibility. All financial data are inflation-adjusted and presented in 2014 dollars. Our sample consists of all elementary and unified school districts that existed in all years between 1995 and 2015, had greater than 200 students and fewer than 50,000 students in 1995[11] and never experienced a boundary change or a district type (i.e. elementary, unified, secondary) change over our 21 year time-period. This leaves us with 11,824 total school districts.[12]

Fourth, we use home transaction data from Zillow ZTRAX database. This nationwide database contains almost all home transactions between 2005 and 2017 with longer temporal coverage for some counties excluding 12 states for which home transaction data are not publicly available. Data include information on sales price, home attributes, home location and owner characteristics. Appendix B will provide much greater detail on how the ZTRAX data were processed for this project. We currently use home price data from 13 large states that have both comprehensive transaction coverage and a large number of plant openings.[13]

[9]Several states fund schools through county or municipal budgets. This includes Massachusetts, Connecticut, New Hampshire, Maryland, Virginia and North Carolina. For these states, we include municipal rather than school district tax rates.

[10]In all analyses involving taxable value or tax rates, we drop New York and Pennsylvania because for most years in our sample, assessment ratios were set by counties and so reinflation is not possible. We also drop Kentucky because reported tax rates were an order of magnitude higher than other states despite the state officially assessing properties at Fair Market Value. This is consistent with anecdotal evidence suggesting county assessment offices in Kentucky systematically undervalue local properties.

[11]Because our primary outcome variables are per student, we want to exclude very small districts where small changes in the student population could lead to large changes in the outcome variable and we believe very large districts are both unlikely to be affected by a single plant opening and will be unlikely to have good within-state counterfactuals in our data.

[12]We exclude districts that have undergone boundary changes to ensure that any observed changes are not simply arising from changes in composition within the district.

[13]Arizona, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina,

## 3.2 Effects on School District Budgets and Property Taxes

### 3.2.1 Empirical Strategy

We begin by estimating the effect of plant openings on school district fiscal outcomes. In an ideal world, we would randomly assign plants to some districts, but not others and examine the changes that ensued. Instead, we employ two separate but related strategies to approximate such a randomization. We first perform a nearest-neighbor propensity-score difference-in-difference analysis whereby we compare two observably similar districts in the same commuting zone and state before and after the "treated" district receives a plant. We define treatment as a district receiving a new utility-owned natural gas or renewable plant in any year between 1995 and 2015—our sample is roughly split between these two categories.[14] We are interested in the property tax effects of large projects—we define a project to be large if it is above 25MW in size—a common cut-off used by the EPA when determining eligibility for pollution control regulations.[15] There are 1,371 such plant openings in our data.

We define the treatment date as the year in which the plant first obtained regulatory approval or began construction as knowledge that a jurisdiction will receive a plant in the future may affect current taxing or borrowing behavior. For the 25% of plants for which this information is not available, we instead use the year of operation—note that to the extent that district's begin borrowing or raising revenue after approval this should bias our results downwards.[16] Because our outcomes are at the district level, we restrict our analysis to only the first plant opening in a district over our timeframe. In total 852 districts experienced an opening during this time period. Of these districts, 714 fit our district sample criteria and had a potential control district within their state by commuting zone by district type cell. [17]

To perform our matching strategy, we implement a fixed-effect logit regression of an indicator variable equal to 1 if a district received a plant and 0 otherwise on 1995 total, local

Ohio, Oklahoma and Pennslyvania.

[14]These are the plant types for which we can reliably estimate construction cost, a necessary component of our identification strategy. They also make up the vast majority of new openings during this time period.

[15]For reference, if running 100% of the time a 25MW plant could provide power for roughly 25,000 homes. Since most plant's capacity factor is far below 100%, these plants provide power for closer to 12,000-24,000 homes depending on the plant type.The vast majority of plant openings smaller than this level are very small (<10 MW) solar or landfill gas installations.

[16]We additionally drop 51 plants whose first year reporting to the EIA is more than 2 years after their stated operation date, 3 plants whose construction date is after their stated operation date and 28 plants whose operation date was more than 5 years after construction approval.

[17]33 districts did not match with district's in the NCES sample, 26 districts had fewer than 100 students in 1995, 18 students had greater than 100 students in 1995, 33 districts were not in operation for all 21 years, 2 districts had fewer than 3 grades, 2 districts had greater than 1 log point annual change in students and 24 districts had no control neighbor in their commuting zone.

and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. Table A1 shows the results of this regression. As expected from previous descriptive analyses of inequities in exposure to environmental harms, more populous and poorer districts in terms of home values, income and total school revenues per student are highly correlated with hosting a plant during this time period. Using the coefficients from this regression, we calculate for each district a probability of receiving a plant. We then identify for each treated district its nearest control neighbor (with replacement) that exists in the same state, commuting zone and district type (i.e. unified or elementary). Restricting matches to be within the same state and commuting zone is particularly important because it allows us to control for any labor-market wide economic shocks that might affect school funding and may also be correlated with changes in local generation capacity or other demand-shocks for plants. Using this sample of 714 matched pairs, we then implement the following difference-in-differences specification:

$$Y_{dpt} = \alpha_d + \tau_{pt} + \beta Post_{dpt} * Treat_d + \epsilon_{dpt} \tag{3.1}$$

where $Y_{dpt}$ is the outcome variable, $\alpha_d$ is a district fixed-effect, $\tau_{pt}$ is a matched pair by year fixed effect, $Post_{dpt} * Treat_d$ is our variable of interest, the interaction between the period following a pair's plant beginning construction and whether or not a district is the treated member of the pair, and $\epsilon_{dpt}$ is a mean-zero error term. Because our treatment year is defined as the year of approval, we separately examine effects for two post periods (years 1-3 which are the years over which most plants are built and years 4-10 when most plants are already in operation.) We also perform a similar analysis using indicators for years since approval to check for any pre-trends prior to approval. In our primary analysis, we show results for both all openings and "non-small" openings (defined as having fewer than $20,000 per student in expected tax base increase based on plant and district size—$< 20\%$ of sample), as these small openings are unlikely to produce sufficient tax revenue per student to make a meaningful impact on district finances.

Although this propensity-score matching approach attempts to control for differences that might drive plant selection into a given district, a disadvantage of this method is that we can only control for observables. To the extent that unobserved factors drive both plant locations and trends in relevant outcome variables over time, our coefficients will be estimated with bias. Thus, our second identification strategy leverages a third dimension of variation: the expected impact of a given plant on the local tax base.

We estimate the expected tax base impact of an opening by dividing a plant's estimated

construction costs (a proxy for the plant's value)[18] by the total number of students enrolled in the district in the year the plant received regulatory approval. Estimated construction costs are based off of fuel and prime-mover specific estimates of overnight construction costs per megawatt-hour in the EIA's Annual Energy Outlook from 1997-2018.[19] More details on these calculations are provided in Appendix A. The underlying assumption of this analysis is that plants that deliver more value per student should create correspondingly larger effects in their host district. We estimate:

$$Y_{dpt} = \alpha_d + \tau_{pt} + \beta Post_{dpt} * Treat_d + \gamma Post_{dpt} * Treat_d * NewBase/Stud_{dp} + \epsilon_{dpt} \quad (3.2)$$

where $NewBase/Stud_{dp}$ is the expected construction cost of the new plant divided by the number of students in the receiving district measured in \$100,000/student. This variable is assigned to both the treatment and control district in a given pair. The coefficient $\gamma$ is our coefficient of interest—it tells us the effect on local tax and spending variables of adding an additional expected \$100,000/student in local tax base. Note that for omitted variable bias to exist in this approach it cannot only be the case that receiving districts are systematically different from their control districts in ways that are correlated with time-trends in the outcome variables, rather it must instead be the case that differences between treatment districts receiving openings with larger expected fiscal impacts and their assigned control district are systematically different than the differences between treatment districts receiving openings with smaller fiscal impacts and their assigned control districts.[20] An important disadvantage of this specification is that if either the expected fiscal impact is incorrectly estimated or the relationship between fiscal impact and the outcome variable is misspecified, there will likely be attenuation bias in our results. For this reason, we provide both specifications throughout the paper—we believe they act as complements.

There are two major related challenges to identification in this setting. First, as discussed above, it is possible that treatment districts are fundamentally different from control districts and so even in the absence of treatment they would be expected to have different trajectories in the outcome variables. This could be true even in the treatment intensity approach if plants with larger expected fiscal impacts differ from their control districts in systematically

[18]The vast majority of states use original construction cost as the only or primary method of assessment. States that rely on other methods typically use either fair market value (which will be correlated with construction costs) or total production/income, both of which should be correlated with construction costs

[19]For years 1994-1996, the 1997 value was used.

[20]This is not simply the difference between districts receiving large and small plants, but is instead the interaction between plant size with the size of the receiving district. A small plant in a small district may have a similar expected fiscal impact as a large plant in a large district because the increased taxable value is split across fewer students.

different ways than districts with smaller expected openings. We address this potential problem in three ways. First, we examine whether pre-trends exist in major demographic and economic variables that we might expect to be correlated with both plant openings and changes in tax base—there is no evidence for any such trends. Second, we estimate dynamic versions of all models and examine trends prior to plants receiving regulatory approval for construction—we find no evidence for any violations. If unobserved differences between treatment and control districts were driving our results, we would expect for these trends to manifest themselves prior to opening. Finally, we show that results are robust to a number of different matching specifications, as well as both sets of identification strategies. Together, these results suggest that it is unlikely that differential trends between receiving districts and their control districts are driving our results.

A second and more challenging barrier to identification is the possibility that a jurisdiction's decision to open a plant is correlated with other factors that may be correlated with our outcome variables. Under this scenario, it could be true that control and treatment districts are ex-ante similar, but some event (i.e. the election of a development-minded mayor) leads to both the construction of a plant and other changes correlated with an increased tax base or education spending, which would lead us to estimate the plant's effects with bias. Incorporating treatment intensity into our estimating equation helps address this concern, but does not fully solve it—it could be the case that the districts that receive the largest fiscal impacts have the most developmentally-minded leaders. Thus as a second test, we examine if the plant opening is correlated with openings of other types of (non-utility) environmentally harmful plants using data from the EPA's Toxic Release Inventory.[21] If local governments are attempting to attract new facilities, we would expect to see such an increase. Figure A4 show the main results. There are no significant spikes in openings of non-utility toxic facilities following (or prior to) the beginning of the start of plant construction. This provides suggestive evidence that the construction of a plant is not a proxy for a larger development boom.

### 3.2.2 Results

We begin by estimating the effects of plant openings on the local tax base. Columns (1)-(3) of Table 1 show the main effects. Column 1 shows the difference-in-differences analysis, Column (2) shows the same analysis, but excluding very small openings and Column (3) shows the effect using the triple-difference analysis—here coefficients should be interpreted as the effect of an additional $100,000 in expected tax base increase per student. All three

[21] Although imperfect, the TRI provides the best publicly-available record of new plant openings. We say a plant has "opened" if it is the first year in which it appeared in the TRI.

columns tell a similar story—a new plant increases the local tax base per student by roughly 8-10% on average (recall that the average district receives an estimated increase in their tax base of $215,000 student) with effects increasing as the expected size of the opening grows. These results suggest that plant openings can have large effects on the fiscal capabilities of local districts. Figure 1 shows these effects in event study form for ten years before and after the plant receives approval for both specifications. There is no trend prior to approval and a large and sustained increase beginning 3 years after approval just when the average plant begins operation.

Table A2 shows that these results are robust to different matching and specification strategies. Column (1) shows that results are broadly similar if we examine the effect of openings with above median relative to below median expected fiscal impact rather than using the continuous measure of construction cost per student, suggesting that results are not driven by a small number of large openings. Columns (2)-(5) show that results do not change if we restrict control districts to being in either the same county (Columns (2)-(3)) or the same state (Columns (4)-(5)) as the receiving district. Columns (6)-(7) show that results are also robust to including very small and very large districts.

We next examine how districts utilize this increased tax base. Districts can respond on two margins: increasing local school revenues and/or reducing taxes. Columns (4)-(6) show the effect of an opening on local tax rates, while (7)-(9) show the effects on district revenue from local sources per student. Districts appear to respond on both margins, but with a greater emphasis on increasing revenues. Property tax rates fall by around 2.5% for the average opening, while local revenue per student increases by about 8%. Figure 2 shows these results in event study form. Reassuringly, there again appears to be no trend in the ten years prior to plant approval, with a significant decrease (for property tax rates) and increase (for local revenues) three to four years after approval.[22] Finally, Tables A4 and A3 shows that results are also robust to different matching strategies and samples using the same tests as in Table A2.

Together, the results from this subsection suggest that plant openings create a large increase in local tax base, which then leads to both more local money being spent on schools and lower taxes. However, the fiscal gains provided by local plants may in theory simply crowd-out transfers from the state and federal government creating little benefit for local communities. Indeed, because many state school funding formulas are explicitly based on

[22]It is important to note that districts' decisions to use this tax base increase to fund local public goods rather than reduce property tax rates only reflect local preferences subject to significant constraints over how new revenue may be used. As will be discussed in subsequent sections, because of state school funding programs—in many states, districts both face significant constraints in changing local property tax rates and increased incentives to take on debt in response to tax base changes, which leads to higher rates.

local property wealth, we may expect such an outcome to occur. Thus, we now turn to estimating the effect of these openings on total district revenues and expenditures.

Table 2 shows the effect of an opening on total revenues and expenditures. We see that an average district sees a 3.5% increase in expenditures per student or roughly $500/student. The revenue effects are similar, but slightly smaller.[23] Figure 3 shows these results in event study form. Again, there is little trend prior to approval and an increase beginning roughly three years post-approval. The smaller effect on total revenues and expenditures per student relative to the effect on local revenues is driven by two things. First, local revenues are only roughly 30%-50% of total school spending. Second, many state school funding formulas tie the level of state transfers to a district's property wealth—increases in the property wealth leads mechanically to fewer state transfers. We will discuss this in detail in the next section and provide evidence that openings in states with less strict equalization regimes produce larger increases in educational spending.

Table 3 shows where the additional revenues created by the plant is spent. We first see that openings lead districts to take a large amount of additional debt ($950-$1,200 per student or 15%-25% increase). This debt increase may occur for two reasons. One, because the plant opening increased the size of the tax base, the price (in additional tax rate increments) for any given sum has now fallen for a given household. Two, in many school funding formulas, debt is the only avenue accessible to local governments to access their additional tax base net of crowd out of state and local revenues.

In general, most school debt is used for capital expenditures. We observe a similar phenomenon here. Despite making up only 10% of total school spending, the vast majority of expenditure increases caused by the plant occur on capital projects. Specifically, by 4-10 years after approval, spending on capital projects increases by $165-$340/student (15%-25% increase). There is also evidence for smaller increases in non-instructional spending and instructional salaries. The disproportionate use of new revenue to fund capital expenditures is consistent with previous work examining responses to other forms of revenue shocks (i.e. (Davis and Ferreira, 2017)).

Lastly, Table 4 and Figure 4 examines whether these changes lead to large demographic shifts in the composition of students as well as whether there were any pre-trends in these variables prior to the start of construction. We focus on white share of enrollment, free or reduced lunch (FRL) share of enrollment and log total enrollment. There are no significant trends in any outcome prior to plant approval. There are no large changes after approvals—visually there is some evidence for slightly increased white share, but the effect is small

[23] As we will see, one response to plant openings is for districts to increase debt, which leads expenditures to exceed revenue.

and statistically insignificant. Together these results suggest that our observed effects are unlikely to be driven by differential trends across treatment and control districts nor are driven by sorting occurring after a plant opens.

## 3.3 Effect of Plant Openings on Home Prices: Empirical Strategy

The previous section showed that plant openings lead to appreciable increases in educational spending. However, in theory, such payments could be captured by local bureaucrats or simply spent in a way that was not valued by homeowners—this would imply that the payments are not effectively compensating those who are harmed by the plant. This is particularly true because we observed that the spending increase occurred almost exclusively among non-instructional expenditures. Accordingly, we now turn to estimating whether the fiscal benefits created by plant openings are valued by local residents.

### 3.3.1 Empirical Strategy

To obtain a valid estimate of the effect of plant's tax base increases on home values, we need to compare homes that are not only ex-ante similar, but also that are equally affected by the non-fiscal positive and negative effects of the plant (i.e. pollution).[24] The propensity score method used above cannot work in this setting—homes in the host district are exposed to both the positive and negative effects of the plants.[25] Instead to accomplish this goal, we create border pairs between all districts with a plant opening and neighbors with no openings. We then compare the relative change in housing values in the area just around the border between homes in the plant's district and homes in the neighboring district after the plant opening. In this set-up, both sets of homes will be exposed to similar economic and pollution shocks, but only homes on the plant's district side of the border will get the benefit of the expanded tax base—if the parallel trends assumption holds, we can attribute any changes in home prices to the fiscal effect of the plant.

One important caveat is that many school district boundaries are shared (or nearby) county and municipal boundaries. This has two important implications for our analysis. First, results should not be interpreted as the home price effect of the increased school district tax base alone, but as a weighted estimate of the increased tax base across all local government units that share the border. We test for robustness by excluding district boundaries that are shared with county boundaries and results are qualitatively similar, but

[24] An advantage of using power plant openings is that these plants have relatively small agglomeration and employment effects and so we are mostly concerned with pollution in this setting.

[25] No nationwide annual data on district home prices exist, which would be necessary to estimate these joint effects on housing prices. We are in the process of creating such an index using the ZTRAX data.

there are many other local government taxing units (i.e. municipalities, irrigation districts, sewage districts, etc) for which we lack granular enough geographic data to exclude. Further, even if we could exclude these districts, our remaining sample of school district boundaries would be too small to obtain valid statistical estimates. Second, because of these shared boundaries, we do not use the triple-differences approach above as our primary specification. Home values will respond to the expected fiscal impact on all relevant governmental units and there is no reason to believe there is a monotonic relationship between the expected impact on a district and the expected impact on the district's county (i.e. a small district could be in a very large county or city). Thus, we show these results as robustness checks, but given the boundary-sharing their interpretation is less clear.

Our primary specification is as follows:

$$Y_{idpt} = \alpha_d + \tau_{pt} + \beta Post_{dpt} * Treat_d + \epsilon_{dpt} \tag{3.3}$$

where $Y_{idpt}$ is logged home sale value of home $i$ in district $d$ in border pair $p$ and year $t$. The coefficient on $Post_{dpt} * Treat_d$ is our coefficient of interest. The vector $\alpha_d$ contains indicators for extremely granular spatial controls (border pair x district x distance to border bin (200 m) x distance to plant bin (200m) in our primary specification.) This ensures that we are comparing homes in very similar neighborhoods. The vector $\tau_{pt}$ contains indicators for border pair by year by month fixed-effects to control for any time-varying characteristics of homes within the border region. Our primary model uses a bandwidth of 1,600 meters, but we show robustness to alternate bandwidths (800 meters-3,200 meters).

Because our data is at the home transaction level, an unweighted regression will overweight those openings with more sales. This is advantageous because the average home price in each border region will be estimated more precisely, but disadvantageous because expected fiscal effect declines as student population increases, which are the exact areas in which we expect to see the most home sales. Therefore, this analysis will also overweight low expected fiscal impact districts—leading to a downwardly biased estimate. To address this, we exclude openings expected to have a very small (<$20,000/student) expected impact on the local tax base per student.[26]

We additionally estimate a model that weights each plant-year equally. While this specification allows for an estimate of the average plant effect, it also provides a large amount of weight on home transactions in low-transaction districts; when there are relatively few transactions, the true underlying home value of a given area is estimated with noise. To address this, we include only plants with at least 20 transactions in the border region in all

[26]These openings make up less than 20% of all openings, but more than 75% of all transactions.

years, but show robustness to other cut-offs. A potential problem with this approach is that the number of home sales may be endogenous to treatment. Although our weight is based on both treatment and control sales, if sales increase or decrease after plant approval this will endogenously lead some plants to enter or exit the analysis. Nonetheless, the fact that results are qualitatively similar in this analysis is reassuring.

The primary threats to identification using this approach are twofold. First, as with the fiscal effects analysis, homes outside may not be good counterfactual for homes inside the district. We test this assumption in several ways. First we employ a dynamic difference-in-difference analysis to test for pre-trends and find no evidence for any violations. Second, we control for a large number of major hedonic characteristics to ensure that we are comparing similar homes in both the treatment and control districts and find results are similar. Third, we show that effects are completely driven by plants with larger expected tax base impacts suggesting that there is nothing perform a placebo test examining effects on the borders of populous school districts and find no evidence for any effect.

An additional threat to identification is that the border design does not fully control for other positive or negative effects from the plant. For instance, if the plant increases nearby housing demand, this may increase home values closer to the plants, which will disproportionately be inside the plant district's border region. As power plants do not typically create large amount of jobs or have large agglomeration effects, positive effects are less of a concern. Thus unequal exposure to the plant would likely bias our results downnward. Nonetheless, We test this assumption in two primary ways. First, we show robustness to a large number of border bandwidths and results are qualitatively similar. Second, we show results from including only border regions that are "far" from the plant (where far is defined as at least 10km away) as these border regions should be "uncontaminated" by other effects of the plant. Again results do not change.

One other potential solution to this concern is to instead use a difference-in-discontinuity design. We use a difference-in-difference rather than a difference-in-discontinuity design for three reasons. First, different border pairs have very different spatial distribution of homes around the border, while similarity to homes across the border may differ according to baseline population density (i.e. a home three miles from a home across the border may be very "close" in a rural area, but not particularly "close" in an urban area.). This implies that using a discontinuity design here both overweights pairs that have a high density of homes near the border and more importantly, has no theoretically-grounded functional form to use for the running variable creating a high probability of mis-specification. Second, ZTRAX coordinates are accurate to the street block level, but there is a risk of misattribution for

homes very close to the border.[27] While this will have a small downward impact on difference-in-diffrence estimates because these are the highest leverage observations in the regression discontinuity they have the potential to create substantial bias. Third, we have only a limited number of openings and the use of the difference-in-discontinuity method requires significantly more power.

## 3.4 Effect of Plant Openings on Home Prices

We begin with an estimate of the home price capitalization of the increased tax base created by the plants. Figure 5 shows the main results for the unweighted analysis across a number of different distance bandwidths. Four to ten years following plant approval, home prices in the receiving district increase by 4%-6% depending upon the bandwidth. This result suggests that not only do plants increase local fiscal capacity, but that this increase is indeed valued by local homeowners. Figure 6 show these effects in event study form for our preferred bandwidth of 1 mile—there are no trends prior to the plant entering and a sustained increase beginning two years after approval.[28]

We next test the robustness of these results to various specifications, weighting schemes, and sample inclusion criteria. If these effects are truly driven by increases in the local tax base, we should expect a large effect as we restrict our sample to openings with larger and larger expected tax base impacts. To test this, Table 5 shows how effects vary for different estimated tax base cut-offs. There are two major takeaways from this table. First, when we estimate the effects for the full sample in an unweighted regression, both average expected tax base impact and effect sizes shrink dramatically, suggesting there is a minimal effect on home prices for areas with a small expected impact. Second, as we use more and more restrictive cut-offs, the average estimated tax base effect in the included sample increases, the housing price effect also increases.

To ensure that the above results are not driven by a single high-transaction opening, Figure 7 shows these same results after weighting all openings equally.[29] The top panel shows the results using the same opening size cut-off as in the unweighted approach and the bottom panel shows the results after including the full sample. The weighted results are very similar in magnitude to the unweighted results. Because weighting down-weights very large, low-impact openings, the effect on the full sample are also significant here; an opening in an

[27] We are in the process of geocoding all homes based on address rather than Zillow-provided coordinates.

[28] We only include 5 pre-years here because most districts begin home price coverage in the early 2000s. As a results we only have data on home-prices for less than half of districts six or more years prior to approval.

[29] We exclude openings that have fewer than 20 transactions in any year within a mile of the border because they will only estimate the annual home value within a given region with substantial noise.

average district regardless of expected impact size leads to a 3%-4% increase.[30]

Table A7 shows additional robustness tests at our preferred bandwidth of 1 mile. Columns (1) and (2) show that results are broadly similar regardless of if border regions are near or far from the plant, suggesting that other plant effects are not biasing our results. Columns (3) and (4) show that results are similar for boundaries that are and are not shared with a county providing suggestive evidence are not driven by other governmental units. Columns (5), and (6) show effects after restricting that border-regions be "similar."[31] Results are broadly similar, although shrink somewhat in our most expansive "similar" categorization. In Columns (7) and (8) we show that results are not driven by new construction by separately examining effects among homes built within the last 5 years and older homes. Lastly, Table A8 shows that results are not driven by our granular spatial controls. If, for instance, we only control for a school district or school district by border-pair, results remain largely unchanged. Similarly, if we include more or less granular distance bins, the effects remain the same.

The above analysis provides strong evidence that the plant opening leads to increased home prices. However in theory, the observed changes in price could be caused either by true underlying differences in home values or through a change in the composition of homes sold. To ensure that we are capturing only the change in home value, We attempt to control for any potential compositional differences in the types of homes sold in two ways. First, our outcome variable is adjusted for a vector of hedonic characteristics crossed with state indicators. In this way, observed changes in value cannot be driven simply by (observably) more expensive homes being sold. Second, we include extremely restrictive geographic fixed-effects (district x border pair x distance to border (200m bins) x distance to plant (200m bins). In this way, we are only comparing homes that are sold extremely nearby implying that results cannot be caused by homes in a different part of the border region being disproportionately likely to be sold after the opening.

Nevertheless, it is still instructive to examine if the opening is indeed causing changes in the types of homes that are being sold conditional on these geographic controls. Table A9 shows these results and reassuringly, we see no major differences before and after opening. Homes in the treated district in the post-period are slightly less old (.8 years) and smaller (55 square feet). Both of these differences are small, only marginally significant and unlikely to account for the price effects we observe here even in the absence of adjusting for hedonics

[30]Note that this is actually an underestimate because we exclude openings with relatively few transactions per year, but these are exactly the openings with large expected impacts.

[31]In Column (5) similar is defined as of the same district type, within 1.5 log points of baseline student enrollment and 50 percentage points of free lunch share. In Column 6, we restrict to same district type, within 1 log point of baseline student enrollment and 10 percentage points of free lunch share.

(which we employ in all our main specifications). All other variables (lot size, bedrooms, type of house, distance to border) have effect sizes near zero. Figure A5 shows the results when no hedonic controls are included—unsurprisingly given the results of Table A9, effects are similar, but of a slightly larger magnitude.

We also examine if the quantities of houses sold changes after the opening. Because the plant opening causes a large shock to local public good provision, we might expect that households will respond by re-optimizing leading to increased sales. Note that as long as the composition of homes is not changing, an increase in quantity will not bias our home price estimates. Table A10 presents some evidence that the opening does induce re-optimization; home sales in plant's district increase by about 6%-7% following an opening, although the effect is not significant at conventional levels. There is also little evidence that these sales increases are driven by new construction. Columns (1)-(3) show the effect weighting all openings equally, while columns (4)-(6) show the effect weighting by baseline student enrollment (we cannot weight by transactions as in the price analysis because we would be weighting on an outcome variable)—results are similar regardless of the weighting scheme used.

Finally, Table A11 shows the effects of the opening on school finance variables for the subset of openings studied here using the same border difference-in-differences specification as in the housing price regression. Despite the different counterfactual, results are very similar to the results obtained through the propensity score difference-in-difference approach. However, in this subset of openings, there are two major differences. First, despite the large effect of the opening on local revenue, crowd-out is nearly complete—after 4-10 years there is almost no effect on total revenues. Second, these districts respond to this crowd-out by taking on much more debt; by 4-10 years following an opening, host districts have more than $3,000/student in additional long-run debt, which leads to an additional $980/student in capital spending. There is no effect on any other type of spending. Despite this large increase in debt, we see no change in property tax rates suggesting that effective rates have fallen in response to the tax base increase. These results correspond to much anecdotal evidence from newspaper reports, which suggest that districts use the increased tax base from plants to help fund capital improvements (Samilton, 2018).

This increase in home values implies that the increased tax base caused by new plants is being used in ways that are valued by local and prospective homeowners. Such a result does not necessarily follow from more local spending; given rational voter inattention it is certainly plausible that local bureaucrats could capture this additional revenue through higher salaries or wasteful spending that bring no benefit to local homeowners. However, the increase in home values suggests that instead households value this new spending. As described above,

one possible mechanism through which this occurs is the construction of new schools and other capital improvements. We lack exogenous variation on school construction conditional on receiving a plant, but given previous work suggesting that new school construction has meaningful effects on local property values even when it is funded using increased taxes on existing properties (Cellini, Ferreira, and Rothstein, 2010), such a mechanism for the increase in housing prices appears plausible. Indeed, our home price results are extremely similar in magnitude to the effects (Cellini, Ferreira, and Rothstein, 2010) found for school bond passage in California.

The above analyses provided evidence that the tax base increase caused by an entrance of a plant leads to a meaningful increase in school district home values, all else held equal. But of course all else is not held equal—the plant opening brings with it significant negative externalities. We now estimate the effect of these negative externalities on nearby residents using a spatial difference-in-difference model. Table 6 shows the main results. Columns (1) and (5) show the results from our primary specifications with indicators for home distance to plant for an unweighted and weighted regression respectively. Relative to homes 10km-20km away, home prices fall by 3%-5% within 5km of the plant. We include county by year, city by year and school district by year fixed-effects in all regression so these estimates hold any fiscal effects constant. Column (2) and (6) show similar regressions, but for log distance. Moving one log point of distance closer to the plant leads to a 3%-5% decline in home prices. Columns (3)-(4) and (7)-(8) show results separately for natural gas and renewable openings. Effects seem to be largely driven by natural gas plants although the smaller number of transactions around renewable openings implies that these estimates are very imprecisely estimated.

Together, these results suggest that plants have the power to both increase and decrease home prices depending on a homes relative exposure to positive fiscal effects and distance to the plant. In general, houses within a receiving district that are far from a plant will benefit from the plant's entrance and the size of the benefit will depend upon the ratio between the plant's tax bill and the number of students in the district. Homes nearby the plant experience an ambiguous effect on home-values. If a plant is sufficiently small or the expected tax payment per student sufficiently large, these homeowners may see a net increase in home value. Conversely, if the plant is large and/or the expected tax payment per student sufficiently small, these homeowners will still see a decrease in home value though not as large as would be expected from their proximity to the plant alone. Finally, homes that are nearby a plant, but not in the same district experience an unambiguous decrease in home values.

However, these results also suggest that property tax payments from a plant create a

meaningful benefit to local homeowners. This result implies that districts' ability to raise and retain this revenue should have a meaningful impact on their willingness to accept such plants. We turn to this question in the next section.

# 4 Effects of Constraining Local Property Tax Revenues on Industrial Development

The previous section showed that property tax payments from large plants are valued by local communities as proxied by home-values. This suggests that restricting communities' access to such compensation may have large effects on their willingness to allow large externality-producing projects to enter. In this section, we test this question empirically by examining how negative shocks to the marginal value of an additional dollar of tax base with respect to school spending affects exposure to large plants. Below we describe our data and empirical strategy for answering this question and then show the primary results.

## 4.1 Data and Empirical Strategy

In this section, we use a series of school finance reforms that occurred across US states between 1970s and 1990s as a plausibly exogenous shock to the marginal value of an additional dollar of local tax base with respect to school spending. As described in Section 2, these reforms were generally aimed at equalizing state education systems and/or increasing the level of education provided by the state's poorest districts. These reforms often had large effects on the ability of local school districts to access their local tax base across two dimensions. One, many reforms greatly increased the degree to which state transfers were tied to a district's level of property wealth—in many states following the reform, increases in property wealth would lead to a corresponding decrease in state transfers, which limited (or in the extreme case, eliminated) any benefit districts would receive from an increased tax base. Two, reforms also often instituted tax ceilings and floors, which both limited the amount local districts could tax (and therefore, the amount of revenue they could obtain from their tax base) as well as their ability to cut rates.

Identifying the precise year and type of school finance reform is difficult. Many court-decisions lead to ostensibly large reforms that in reality had little effect on school finance, while other less-publicized legislative changes led to dramatic shifts in the way in which schools are financed hoxby2001all,lafortune2018school. We solve this problem in two ways. In our primary analysis, we identify reforms by analyzing state's school funding formulas going back to 1962 using *Public School Finance Programs of the United States*, a report series

published approximately every five years summarizing US states' school finance systems and formulas. Using both the funding formulas and narrative descriptions within these reports, we identified years in which there were large changes in either the marginal value of additional dollar of tax valuation on total revenue or on the level of taxes that a district could charge. Years with substantial shifts were defined as reforms—if a state had multiple major reforms we used the first reform as our event.

Figure A7 shows summary statistics related to these reforms. The left panel shows the cumulative number of reforms by year and the right panel shows a map of reform states by year of reform. There are two major takeaways. One, the majority of states (34) had major reforms and these states are geographically and demographically diverse. Two, reforms happened near continuously between 1970 and 1994—thus results are unlikely to be driven by a specific time-trend like the rapid decline in US manufacturing in the early 2000s.

Using these reform year estimates, we implement a contiguous border pair difference-in-differences analysis, adapting the methodology used by Dube, Lester, and Reich (2010) in their analysis of the employment and wage effects of minimum wage changes. Specifically, we stack all contiguous border pairs in the United States and assign each state the year of their first major reform. We define a state as treated if a year is after their reform date and 0 otherwise. States that did not experience a major reform have a value of 0 for all years. We restrict our sample to only twenty-four years prior to the reform and and twenty-four years following the reform to maintain a semi-balanced sample, but results are robust to using all years as well as a more restrictive cut-off. We then estimate

$$Y_{cpst} = \alpha_{cps} + \tau pt + Post_{st} + \epsilon_{cpst} \tag{4.1}$$

where $Y_{cpst}$ is the outcome variable, typically large manufacturing establishments or employment per capita, $\alpha_{cps}$ is a time-invariant fixed-effect for each county in a given county pair, $\tau_{pt}$ is a county-pair by year fixed-effect so that comparisons are only made between each set of contiguous counties, $Post_{st}$ is an indicator for whether or not a year is 1-10 years or more than ten years post-reform and $\epsilon_{cpst}$ is a mean-zero error term. We implement two-way clustered standard errors at the border pair and state level.

In this analysis, we use large manufacturing establishments and manufacturing employment as our proxy for local externality producing projects. We switch to manufacturing plants for two reasons. One, power plant openings are relatively rare and data for new openings with locational data attached during this period are sparse. By using manufacturing establishment and employment data, we cover one of the largest polluting industries in the United States and have full data coverage for all US counties. Two, power plant openings are largely a question of location because the US must meet power demand and so new plants

must go somewhere. This is not the case for manufacturing plants and so we believe they constitute a more interesting case.

Our data come from County Business Patterns, an annual measure of employment, establishments and payroll within a county by industry type. We have data from 1962 to 2013. For counties with low levels of employment, employment in certain industries is marked as 0 to maintain privacy. We therefore exclude any county-year in which the county has a positive number of establishments, but no employment reported. These are disproportionately small, rural counties and so in our primary analysis we restrict our sample to only counties with greater than 1,000 population in 1970 (prior to any reforms). However, results are robust to including all counties in our sample.

Implementing the regression above without weighting gives greater weight to states that have large number of border counties. If effects are homogeneous, this is unimportant, but if effects vary across regions this may lead to systematic bias in our estimate of the average effect. Accordingly, in our primary analysis we weight each state the same by giving each county a weight equal to the inverse of the number of border counties in the state. However, we show robustness to not weighting.

There are two major identifying assumption for this analysis to be interpreted as the causal effect of a shock to the marginal impact of an additional dollar of tax base with respect to school revenues. First, is that absent the reform event, manufacturing establishments and employments in the border counties would evolve on similar paths. This is a priori plausible because since these counties border one another, they are likely exposed to similar economic shocks. However, we additionally test this assumption in several ways. First, we show estimates in event study form to check for pre-trends. Second, we show also show results for manufacturing as a share of all employment and establishments. If effects were driven by broader economic forces then we would not expect there to be a disproportionate effect among manufacturing establishments. Third, we show that results are robust to a number of different specifications, baseline covariates by year controls and weighting schemes. Finally, we show that results are much larger in more urban counties where local control of land use and exposure to local externalities is typically stronger.

A second and more challenging identification assumption is that the event itself cannot affect our outcome variable through other channels. For instance, many school finance reform events restricted local jurisdictions' ability to raise property tax rates and instead instituted increases in the state sales tax to fund education. If manufacturing firms responded to the changed tax structure, this could bias our results in either direction. Second, these reforms could have been part of a broader push for progressive legislation including environmental legislation which may independently affect locational decisions of plants. Finally, these

reforms by design increased funding in poor areas—this may have led to changed household location decisions that could influence communities to allow plants to enter through an income effect rather than a price effect.

Although it is impossible to fully rule-out these explanations for our results, we address them in several ways. First, as described above, we interact the reform timing with urbancity, a proxy for local control over land use and exposure to local externalities. If it was indeed the case that other aspects of the reform were causing these effects, we should not expect to see any relationship with urbanicity. Similarly, we also observe if there is within state heterogeneity by local poverty rates—if the effect were driven by differential sorting induced by the reform, we should expect to see larger effects in low poverty areas, but we see no evidence for such a pattern. In the future, we will also perform a set of analyses on single reforms within several states that create large intra-state variation in incentives—these analyses will also help address these concerns.

A final concern is that our border county design may violate the stable-unit treated value assumption (SUTVA). If counties in reform states are now less receptive to industrial development, prospective plants may be more likely to instead open across the state border in the neighboring county. This is a common feature of all border designs and is unavoidable in our setting. However, we can bound the bias created by this violation under the assumption that the reform does not lead to an aggregate increase in the total number of plants across the border pair.[32] If we conservatively assume that every plant that would have opened in the reform county now instead opens in the county across the border, our estimates would be overstated by a factor of 2. Thus, assuming the reform does not increase the total number of plants in the pair, at most this violation of SUTVA inflates our estimates by 2. Therefore, the true effect will lie somewhere between our observed effect and one-half our observed effect.

## 4.2 Results

In this section, we examine the effects of shifting local jurisdictions' ability to raise and retain property tax revenue on changes in local industrial development. We examine this question in two ways. We first show that the school finance results obtained in the previous section vary based on their state's level of school finance equalization—this suggests descriptively that these reforms do indeed impact the localized benefit created by plant openings. We then examine in a more causal framework how changing local jurisdictions' ability to raise and retain local property tax revenue affect location patterns of large externality-producing

[32] Since the reform on net reduces incentives for plant location in the pair, this assumption seems reasonable.

plants using changes induced by school finance reform litigation and legislation.

### 4.2.1 Heterogenity in Effects of Plant Openings on School Finance Outcomes

In the previous section, we showed that tax payments produced by these types of projects were economically large and valued by local communities. However, many of the openings studied in the previous section occurred in states that had already undergone significant equalization reforms. Thus, it is likely that these effects are actually much smaller than they would have been in the past. To test this idea, we begin this section by showing how the school finance results differ by a state's marginal value of tax base (MVTB). We proxy for the MVTB by estimating the relationship between a district's total revenue per student and taxable-revenue per student over time by state conditional on district and year fixed-effects.[33] Of course, this relationship will be governed both by a state's level of crowd-out/tax limitations and by a state's average choice of using increased tax base to reduce rates relative to increasing spending. However, given that we saw a much larger spending response in Section 3 and because many states have rate floors for eligibility for state funds, we believe this measure is a reasonably good proxy for a district's ability to access its local tax base in a given state. We estimate this measure for years 2005 and 2017 to maximize the number of states for which we have data and then apply it to all years in our sample.

Figure A6 shows how the expected tax base impact of a plant differs by its state's MVTB in its year of approval. We would expect that this relationship would be positive; if local jurisdictions' can raise and retain a lot of revenue from a plant opening, then the size of a plant's expected fiscal impact should be an important consideration in siting. Conversely, if a jursidiction cannot raise or retain property tax revenue from a plant then it's expected impact is irrelevant. That is precisely the pattern we see here; locations with a higher MVTB see higher-impact plants locate there. Although descriptive, this provides suggestive evidence that a localities' ability to gain tax benefits from local industry is an important determinant of location choice. A second important piece of information from this plot is that our values of MVTB (on the x-axis) are of the magnitude we would expect; in general, states in our sample see a roughly .001 to .01 dollar increase for each dollar of tax base added. Given that the average school district tax rate is roughly .7% and almost all states now have some degree of crowd-out, this is precisely the range we would expect, increasing confidence that our measure is acting as a reasonable proxy.

[33] Our method for identifying reforms compares changes within states over time. However comparing the relative stringency of reforms across states is difficult because there is no obvious summary statistic to use to characterize this relationship. In the future, we are working on creating standardized measures of crowd-out and tax limitations for an average district and will use this as an alternate measure of MVTB.

We next examine how the school finance results from Section 3 vary based on a jurisdiction's estimated MVTB when a plant enters. Table A12 shows effects of an opening on total revenue and total expenditures for state-years with below median MVTB (1,4), above median MVTB (2,5) and interated with our MVTB measure in mills (one-thousandth of a dollar). The results are exactly as we would expect. Low equalization (high MVTB) states raise significantly more revenue from plant openings ($540/student for an average opening) . Indeed, there is essentially no effect of an opening in low MVTB states. We see a similar, but stronger pattern among total expenditures. States with below median MVTB see essentially no change in expenditures, while states with above average MVTB see a $600 increase per year on average. [34] Across both outcomes the pattern is clear: the less able jurisdictions are to raise and retain local property tax revenue the smaller the fiscal benefits of a plant opening.

### 4.2.2 Effect of School Finance Reforms on Local Industrial Development

Given that these reforms have large impacts on a plant's effect on a school district's local fiscal outcomes, the next obvious question is how these changes impacted local jurisdiction's willingness to allow large externality-producing plants to enter. We attempt to answer this question here in the context of large manufacturing plants. We begin by showing that our qualitatively-identified reforms did indeed have large effects on the ways schools in a state were funded. Table 7 shows the effect of the reforms on the local share of state and local revenue, By more than years after a reform, the local share of state and local funding fell by roughly 11 percentage points, a 30% decline. State revenues increased by $1,400 per student (28%), while property tax revenues fell by $1,000 per student (33%). Finally, the reforms were progressive; for each standard deviation in the share of a county's population beneath the poverty line, it saw a $600/student increase in funding. These results are reassuring; because reforms were identified from qualitative changes in a state's school funding formula, such a relationship is not mechanical. These large changes towards centralization and equalization suggest that such qualitatively-identified reforms did indeed have major effects on the way that state governments funded local schools.

We now investigate how this dramatic change in incentives affected exposure to large externality-producing projects, which we proxy for using local manufacturing employment and large manufacturing establishments (>500 employees). If localities are allowing polluting establishments to enter in large part for the local tax revenue then we should expect as this revenue becomes less available for local use, the number of polluting plants will decline. While we do not have historical data on the opening and closing of polluting plants going back to

[34] Throughout this section we use an opening size of $400,000/student as average.

the 1960s, we can use the presence of large manufacturing establishments and employment as a proxy for the intensity of exposure to these plants.

Finally, it is important to note that we are looking at changes in school district budgets, but it is cities and counties that control local land use decisions. However, as described in Section 2, in many parts of the country, there is substantial overlap between school, municipal and county boundaries Fischel (2010). Even if places where overlap is incomplete, as long as individuals near the plant within the host municipality and county are in the same school district, we would expect local leaders to internalize these benefits. To the extent that this jurisdictional mismatch leads local municipal and county leaders to discount any school funding benefits, our results would be an underestimates of the effects that would occur if there were shocks to municipal or county abilities to raise and retain local property tax revenue.

Table 8 shows the main results of this analysis using our contiguous border counties approach. Columns (1)-(3) show the results for manufacturing employment per 1,000 population, total employment per 1,000 population and manufacturing share of total employment. All estimates are weighted such that each state counts equally in the regression. Reforms lead to a large long-run decline in manufacturing. By 11-24 years following a reform, manufacturing employment has fallen by 10 workers per 1,000 population, a more than 15% decline. Total employment falls by a similar amount suggesting that these declines are not compensated by increases in other industries. Finally, manufacturing employment as a share of total employment falls by 2 percentage points (≈10% decline). This suggests that the observed decline in manufacturing is not caused by a more secular overall decline in employment, but instead is concentrated within the manufacturing industry. Columns (4)-(6) show analogous results for large manufacturing establishments (>500 employees). Again, we observe large declines in large manufacturing establishments, but not for other industry types. Large manufacturing establishments fall by .005 per 1,000 population by 11-20 years following the reform, or a roughly 25% decline.

Figure 8 shows these results in event study form. There appears to be little trend prior to the reform and a marked and sustained decline following the reform. Note that because we are measuring a stock, not a flow we would expect this declining pattern to occur until a new equilibrium is reached (i.e. all firms that entered under the old regime have closed), which is precisely the pattern we observe. Tables A13 and A14 show these analyses under various weights, covariates and sample criteria for manufacturing employment (Table A13) and large establishments (Table A14) per capita. Column (1) test the inclusion of demographic controls, Column (2) exclude states with no reform events to create a pure event study design, Column (3) only retains borders in which one state never experienced

a reform,[35] Column (4) show results with outliers and small population counties included, Column (5) show results with all years included and Column (6) show the unweighted results. Across all specifications, results remain qualitatively similar to the main analysis and highly statistically significant.

We can also separately examine the employment effects by industry type. Effects should be largest for externality producing industries that are likely to arouse community opposition. Indeed, as Table 10 demonstrates, the reforms had no negative economically or statistically significant effect on employment in any other industries. There is suggestive evidence for a decline in transportation and utility industries, but the effect is not significant at conventional levels.

The above event studies and robustness checks provide evidence that the reform caused manufacturing to decline in affected states. However, the mechanism remains unclear from these results—it is possible that the observed results are not driven by the change in local incentives, but instead are caused by some other aspect of the reform (such as a revenue shock to low-income districts) or a correlated piece of legislation (such as increased environmental controls.) Although we cannot fully rule out these alternative explanations, we test for there existence in several ways.

First, we hypothesized that these reforms reduce local government's incentive to allow externality-producing industries to locate within their jurisdictions by restricting their access to tax revenue. If the effect is truly being caused by the shift in incentives it should be larger in places where there is greater local control over land use and where the externality-related costs created by these projects are larger. While we do not observe either of these variables directly, it is likely that both quantities are higher in urban areas. In Columns (1)-(2) and (5)-(6) of Table 9, we see that indeed in more urban areas, the effects of the reforms on large manufacturing establishments and employment are significantly larger. While just correlational, these results are nonetheless reassuring as they are in exactly the direction that theory would predict.

Second, the studied reforms led to a large redistribution of resources of from high to low-income districts. A potential alternative explanation for the observed results is that this redistribution caused low-income districts to be less willing to host manufacturing plants either because the marginal value of the money these plants provides has declined (the district's school revenues have increased through state transfers), because higher-income household's with different preferences are increasingly moving into lower income districts or because the cost of land has increased. In each of these cases we would expect effects to be

[35] This addresses concerns about using a treated county as a control for a soon to be treated county as documented in **goodman2018difference.**

driven by low-income counties. In Columns (3)-(4) and (7)-(8) we examine this possibility by examining effects only among counties with below average 1970 poverty rates. Effects are similar to the full sample suggesting that the redistributive aspect of the reform is not driving the observed response.

We conclude by showing these same results using the reforms identified in Jackson, Johnson, and Persico (2014) analysis of the effect of school finance reform on educational and labor market achievement of students from low-income districts. We use the first reform identified in each state as an "event." Note that Jackson, Johnson, and Persico (2014) were searching for reforms aimed at increasing adequacy and equity within state school finance systems; therefore we should expect a smaller effect from these reforms as many may have had a minimal effect on the marginal value of tax base despite changing the distribution of school funding (i.e. by directing increased transfers to low-income districts without changing crowd-out or tax limitations). Table A15 shows the main results. The employment effects are broadly similar to our main analysis, but about half as large, while the effects on large manufacturing establishments are of a similar magnitude. The identification of a similarly-sized effect using reforms identified through a separate process provides reassuring evidence that our results do not depend on our qualitative method for the identification of reforms.

# 5 Conclusion

Large externality-producing projects create large benefits and costs. When these costs and benefits occur on different spatial scales in the presence of local control over land use, large inefficiencies can emerge. In theory, this problem could be solved through Coasean bargaining, but in practice high transaction and coordination costs make such a solution infeasible.

In this paper, we study how one practical ,already-existing compensation mechanism, local property taxation, affects this problem. We first show that in addition to the negative externalities imposed by plants, nearby residents also have the potential to experience significant gains from plant openings in the form of increased property tax payments. The average opening leads to a 10% increase in the tax base on average. This increased tax base further leads to increased educational spending, used largely on capital expenditures. There is also a small decrease in local property tax rates.

We next show that local homeowners value this increased educational spending. After the plant opens, homes within the receiving district increase by 4%-6% in value for an average opening relative to similar homes just across the border. This increase is of a similar magnitude to the decrease in home prices caused by the plant for nearby residents holding exposure to the plant's fiscal effects constant. These results suggest that property taxation of

large plants has important distributional consequences for who is helped and who is harmed by the construction of large.

We finally examine how changing local jurisdictions' ability to access these tax revenue benefits affects their openness to externality-producing projects. To investigate this question empirically, we use plausibly exogenous changes in crowd-out and tax limitations caused by school finance litigation and legislation. We show that manufacturing employment and large establishments fall by 10-15% following a reform. These results imply that property tax payments can be an important driver of local industrial development.

In this paper, we show that reforms that restrict local government's ability to raise revenue from their tax base may have significant unintended consequence for local land use. This is a feature of many common state-level policies including school finance reforms, municipal and county revenue sharing systems and property tax limitations. However, the welfare implications of this shift are not clear. If the jurisdictions hosting plants do not overlap with the geographic extent of a plant's costs, then such projects may be overproduced in the presence of property taxation—limiting taxation would increase efficiency. However, if jurisdictional boundaries and the extent of external costs line up closely then the opposite is true—in the absence of taxation too few plants will open as local jurisdictions are unable to be compensated for the costs the plants incur. Better understanding this trade-off is essential when considering the design and reform of state-level programs that infringe upon local property taxation. In future work, we will strive to both characterize the efficiency loss and gains created by this policy as well as investigate how changes to boundaries of taxing jurisdictions can affect this trade-off.

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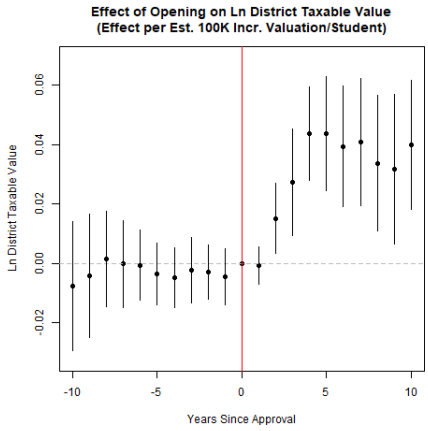
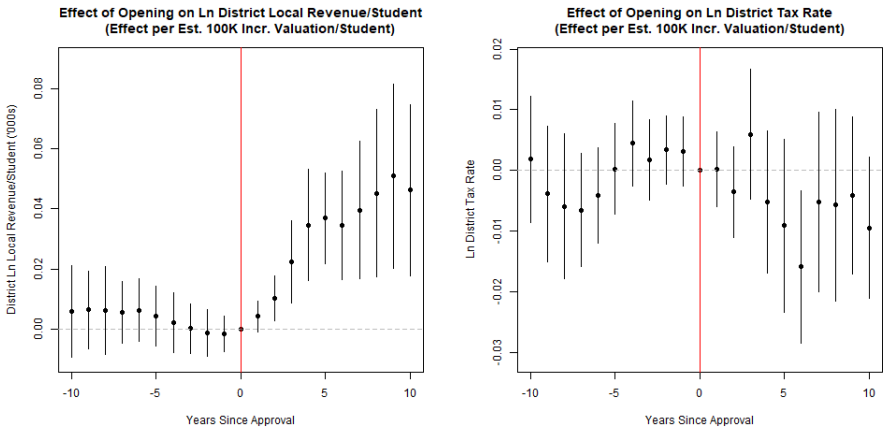
Figure 1: Effect of Opening on Taxable Value Per Student



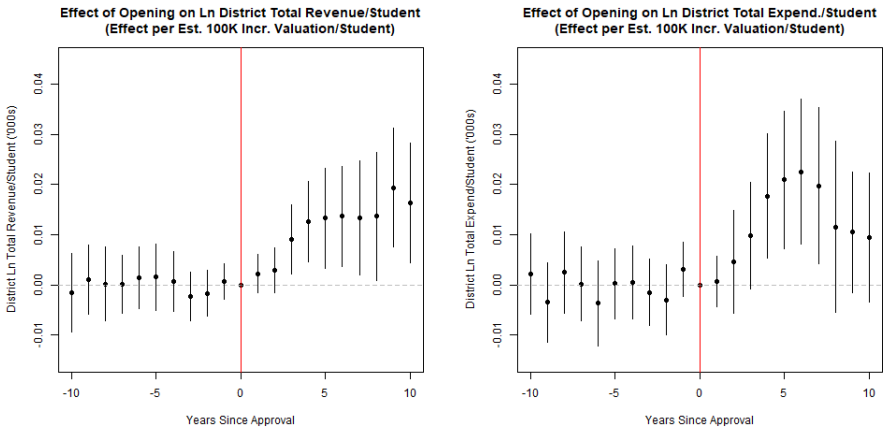

This figure shows the effect of a plant opening on the natural log of district taxable value per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the estimated effect of the plant opening on the local tax base. These estimates came from estimated construction costs using parameters provided in the EIA's Annual Energy Outlook divided by the number of students in a district at baseline. Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District taxable values were hand-collected from state Department of Education and Department of Revenue's annual reports. Plant opening data comes from the Energy Information Administration (EIA).

Figure 2: Effect of Opening on Property Tax Rates and Local Revenues per Student



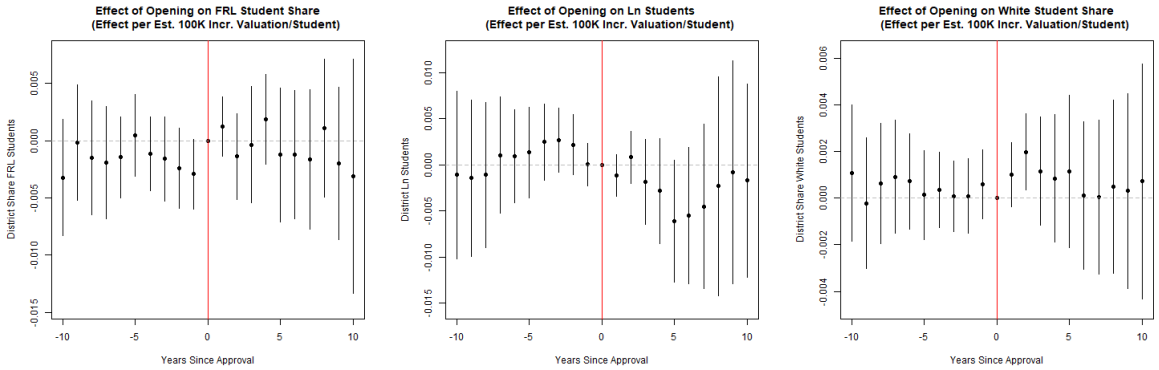

This figure shows the effect of a plant opening on district property tax rate and the natural log of locally-collected revenue per student. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the estimated effect of the plant opening on the local tax base. These estimates came from estimated construction costs using parameters provided in the EIA's Annnual Energy Outlook divided by the number of students in the district at baseline. Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Figure 3: Effect of Opening on Ln Total Revenues and Expenditures



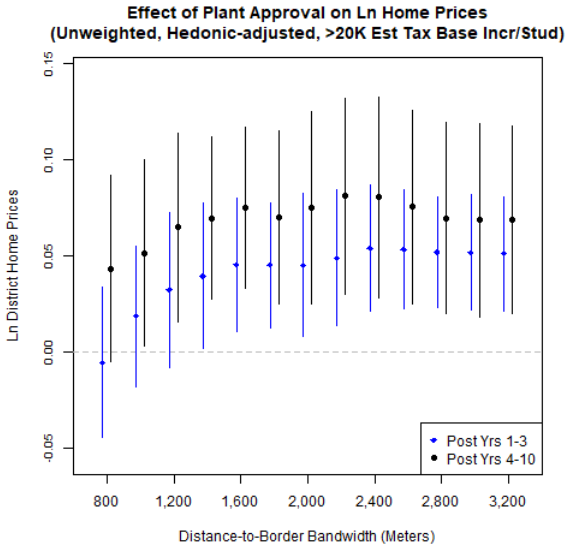

This figure shows the effect of a plant opening on the natural log of district revenues and expenditures per student. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the estimated effect of the plant opening on the local tax base. These estimates came from estimated construction costs using parameters provided in the EIA's Annual Energy Outlook divided by the number of students in a district at baseline. Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Figure 4: Differences in Key Demographic Groups Before and After Openings



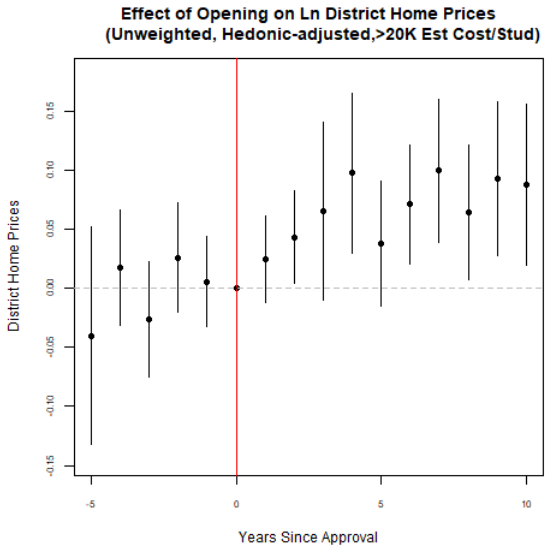

This figure shows the effect of a plant opening on share of students with free and reduced lunch(FRL, 1998-2018 only), log enrollment and share of White students. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the estimated effect of the plant opening on the local tax base. These estimates came from estimated construction costs using parameters provided in the EIA's Annual Energy Outlook divided by the number of students in a district at baseline .Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. Demographic data came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Figure 5: Effect of Opening on Host-District Home Prices: By Distance Bandwidth, Difference in Differences



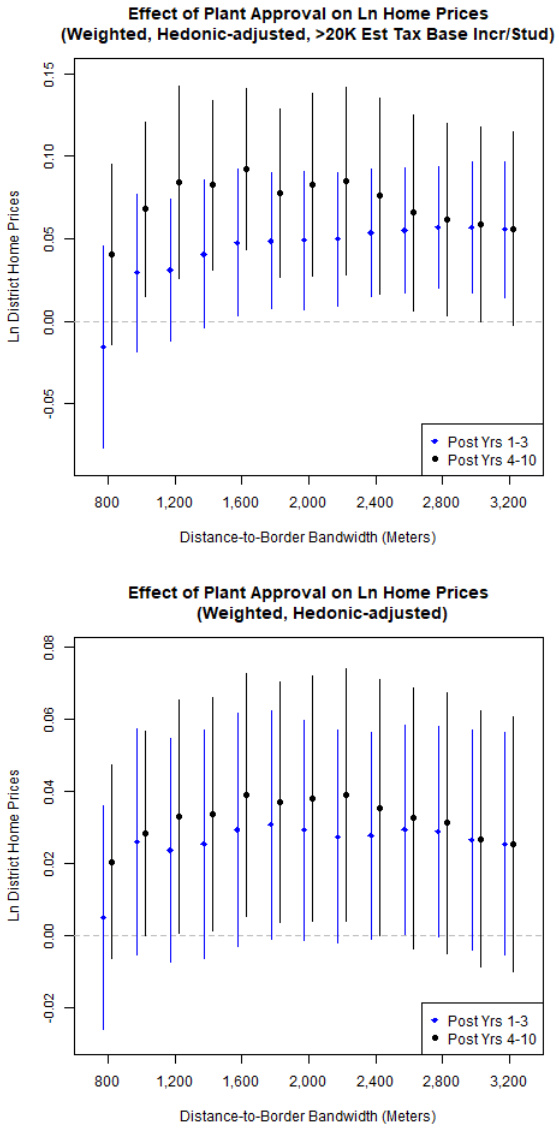

This figure shows the effect of a plant opening on local housing prices. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. Only openings with an expected tax base impact greater than $20,000 are included. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded.

Figure 6: Effect of Opening on Host-District Home Prices: By Distance Bandwidth, Event Study



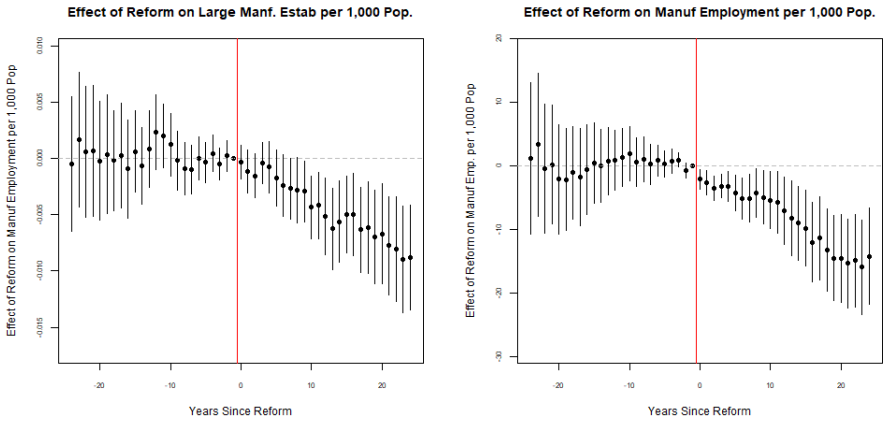

This figure shows the effect of a plant opening on local housing prices using a border difference-in-difference design in event study form. We use a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. Only openings with an expected tax base impact greater than $20,000 are included. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and $2,000,000 as likely outliers.

Figure 7: Effect of Opening on Host-District Home Prices: By Distance Bandwidth, Difference in Differences (Weighted)




This figure shows the effect of a plant opening on local housing prices. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. The top panel includes openings with an expected tax base impact greater than $20,000, while the bottom panel includes all openings. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded as are sales of less than $2,000 or above $2,000,000. Both panels are weighted by the inverse of the number of sales in a plant district and its bordering district in a given year. All plants with fewer than 20 transactions within 1 mile of the border in any year are dropped.

Figure 8: Effect of School Finance Reform on Manufacturing Share of Large Establishments and Employment




This figure shows the effect of a school finance reform on large local manufacturing establishments (>500 employees) and manufacturing employment per 1,000 population using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on indicators for years since the reform. Controls include border pair by year fixed effects and county fixed-effects. We cluster standard errors at the state and border pair level. County-years with outcomes greater than the 99th percentile were excluded as outliers. Counties with less than 5,000 population in 1970 were also excluded. All employment and establishment data come from County Business Patterns (CBP).

Table 1: Effects of Plant Opening on District Taxable Value, Local Rates and Local Education Revenue

| VARIABLES | (1) Ln Tval | (2) Ln Tval | (3) Ln Tval | (4) Ln Ptax | (5) Ln Ptax | (6) Ln Ptax | (7) Ln Loc Rev | (8) Ln Loc Rev | (9) Ln Loc Rev |
|---|---|---|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | 0.0622*** | 0.0684*** | 0.0302 | 0.00342 | 0.00331 | 0.00387 | 0.0403*** | 0.0433*** | 0.0181 |
| | (0.0157) | (0.0155) | (0.0215) | (0.0108) | (0.0119) | (0.0139) | (0.0120) | (0.0137) | (0.0128) |
| Treat x Post Yr 4-10 | 0.108*** | 0.130*** | 0.0208 | -0.0331** | -0.0410** | -0.0180 | 0.0745*** | 0.0879*** | 0.00729 |
| | (0.0229) | (0.0258) | (0.0264) | (0.0138) | (0.0161) | (0.0163) | (0.0164) | (0.0189) | (0.0182) |
| Treat x Post Yr 1-3 x $100K/Stud Est Tax Base Incr | | | 0.0129** | | | -0.000106 | | | 0.00873* |
| | | | (0.00569) | | | (0.00310) | | | (0.00453) |
| Treat x Post Yr 1-3 x $100K/Stud Est Tax Base Incr | | | 0.0422*** | | | -0.00862* | | | 0.0364*** |
| | | | (0.00768) | | | (0.00486) | | | (0.00904) |
| Observations | 11,580 | 9,604 | 11,366 | 11,940 | 9,826 | 11,700 | 20,500 | 16,972 | 20,308 |
| $R^2$ | 0.964 | 0.967 | 0.966 | 0.981 | 0.981 | 0.981 | 0.962 | 0.959 | 0.963 |
| Pair x Year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Sample | All | Non-Small | All | | Non-Small | | All | Non-Small | All |
| Dep. Var. Mean | 483780 | 484758 | 483780 | 0.0103 | 0.0107 | 0.0103 | 4.787 | 4.787 | 4.787 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on a district's taxable value per student, tax rate, and local revenue per student. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for whether a year is after an approval and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,6 and 9). Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level.District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Revenue data came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table 2: Effects of Plant Opening on Ln Total Revenue and Ln Total Expenditures Per Student

| VARIABLES | (1) Ln Tot Rev | (2) Ln Tot Rev | (3) Ln Tot Rev | (4) Ln Tot Rev | (5) Ln Tot Exp | (6) Ln Tot Exp | (7) Ln Tot Exp | (8) Ln Tot Exp |
|---|---|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 0.00879* | 0.00895 | -0.00186 | -0.0378 | 0.00600 | 0.00687 | -0.00601 | -0.0656 |
| | (0.00488) | (0.00559) | (0.00559) | (0.0759) | (0.00780) | (0.00888) | (0.00907) | (0.156) |
| Treat x Post Yrs 4-10 | 0.0178** | 0.0188** | -0.00730 | -0.112 | 0.0176* | 0.0218* | -0.0146 | -0.125 |
| | (0.00701) | (0.00828) | (0.00724) | (0.106) | (0.00967) | (0.0114) | (0.00919) | (0.205) |
| Treat x Post Yrs 1-3 x $100k/Stud Est Incr Tax Base | | | 0.00441** | 0.0674** | | | 0.00503 | 0.132 |
| | | | (0.00209) | (0.0289) | | | (0.00318) | (0.0806) |
| Treat x Post Yrs 4-10 x $100k/Stud Est Incr Tax Base | | | 0.0138*** | 0.212*** | | | 0.0176*** | 0.322*** |
| | | | (0.00356) | (0.0604) | | | (0.00465) | (0.109) |
| Observations | 20,464 | 16,990 | 20,272 | 20,272 | 21,212 | 17,612 | 21,018 | 21,018 |
| $R^2$ | 0.938 | 0.933 | 0.939 | 0.927 | 0.887 | 0.879 | 0.889 | 0.854 |
| Pair x Year FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Dep. Var. Mean | 11.66 | 11.77 | 11.66 | 11.66 | 11.97 | 12.09 | 11.97 | 11.97 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on revenue and expenditures per student outcomes. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for whether a year is after an approval and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3 and 6). Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Revenue and expenditure data came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table 3: Effects of Plant Opening on Debt and Expenditures by Type

| VARIABLES | (1) LTD/Stud | (2) LTD/Stud | (3) Instr Spend/Stud | (4) Instr Spend/Stud | (5) Cap Spend/Stud | (6) Cap Spend/Stud | (7) Oth Exp/Stud | (8) Oth Exp/Stud |
|---|---|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | 0.643* | 0.311 | -0.0252 | -0.0474 | 0.214 | -0.0151 | 0.0789 | -0.00310 |
| | (0.369) | (0.312) | (0.0309) | (0.0310) | (0.132) | (0.120) | (0.0719) | (0.0574) |
| Treat x Post Yr 4-10 | 0.880* | -0.201 | 0.0463 | -0.0697 | 0.166 | -0.137 | 0.333* | 0.0817 |
| | (0.461) | (0.420) | (0.0415) | (0.0460) | (0.130) | (0.110) | (0.176) | (0.137) |
| Treat x Post Yr 1-3 x $100K/Stud Est Tax Base Incr | | 0.0892 | | 0.0104 | | 0.0884 | | 0.0333 |
| | | (0.141) | | (0.0113) | | (0.0570) | | (0.0368) |
| Treat x Post Yr 4-10 x $100K/Stud Est Tax Base Incr | | 0.573*** | | 0.0481*** | | 0.163*** | | 0.111 |
| | | (0.206) | | (0.0168) | | (0.0556) | | (0.0748) |
| Observations | 17,074 | 20,440 | 17,612 | 21,018 | 17,612 | 21,018 | 17,612 | 21,018 |
| $R^2$ | 0.804 | 0.811 | 0.961 | 0.967 | 0.576 | 0.581 | 0.895 | 0.899 |
| Pair x Year FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Dep. Var. Mean | 5.647 | 5.647 | 6.053 | 6.053 | 1.254 | 1.254 | 4.660 | 4.660 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on district long term debt and expenditures per student. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for whether a year is after an approval and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 2,4,6, and 8). Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Revenue and expenditure data came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table 4: Effects of Plant Opening on Demographic Outcomes

| VARIABLES | (1) Ln Stud. | (2) Ln Stud. | (3) Share FRL | (4) Share FRL | (5) Share White | (6) Share White |
|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | -0.00187 | 0.00493 | -0.00153 | -0.00257 | 0.00690** | 0.00343 |
| | (0.00653) | (0.00683) | (0.00397) | (0.00434) | (0.00291) | (0.00305) |
| Treat x Post Yr 4-10 | -0.000474 | 0.0160 | -0.00774 | -0.00680 | 0.00947** | 0.00780 |
| | (0.0105) | (0.0129) | (0.00516) | (0.00572) | (0.00466) | (0.00523) |
| Treat x Post Yr 1-3 x $100K/Stud Est Tax Base Incr | | -0.00156 | | 0.00113 | | 0.00110 |
| | | (0.00207) | | (0.00135) | | (0.00104) |
| Treat x Post Yr 4-10 x $100K/Stud Est Tax Base Incr | | -0.00488 | | 0.000636 | | 0.000274 |
| | | (0.00406) | | (0.00213) | | (0.00161) |
| Observations | 17,612 | 21,018 | 16,320 | 19,498 | 17,052 | 20,404 |
| $R^2$ | 0.997 | 0.998 | 0.962 | 0.962 | 0.993 | 0.994 |
| Pair x Year FE | Y | Y | Y | Y | Y | Y |
| Dep. Var. Mean | 5.647 | 5.647 | 0.342 | 0.342 | 0.702 | 0.702 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on various student demographic outcomes. Coefficients come from a regression of the relevant outcome variable on district fixed-effects, year x matched pair fixed effects and interactions between indicators for whether a year is after an approval and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 2, 4 and 6). Matched pairs were created using nearest-neighbor matching based on a score derived from a fixed-effect logit model of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Revenue and expenditure data came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table 5: Effects of Plant Opening on Home Prices by Expected Impact Cut-off

| VARIABLES | (1) Ln Price | (2) Ln Price | (3) Ln Price | (4) Ln Price | (5) Ln Price | (6) Ln Price |
|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 0.0183 | 0.0455*** | 0.0393 | 0.0325 | 0.0848*** | 0.0711* |
| | (0.0118) | (0.0173) | (0.0248) | (0.0298) | (0.0214) | (0.0360) |
| Treat x Post Yrs 4-10 | 0.0282 | 0.0750*** | 0.0908*** | 0.0868*** | 0.116*** | 0.107*** |
| | (0.0240) | (0.0210) | (0.0220) | (0.0263) | (0.0224) | (0.0387) |
| Observations | 504,325 | 154,261 | 94,117 | 68,022 | 44,760 | 27,537 |
| $R^2$ | 0.607 | 0.552 | 0.559 | 0.581 | 0.520 | 0.610 |
| Cut-off | 0 | 20K/Stud | 40K/Stud | 60K/Stud | 80K/Stud | 100K/Stud |
| Avg Effect Size | 0.354 | 0.953 | 1.332 | 1.631 | 2.046 | 2.696 |

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on local housing prices using a border difference-in-difference design by different expected tax base impact cut-offs. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000 as likely outliers.

Table 6: Effects of Plant Opening on Nearby Home Prices: Spatial Difference-in-Differences

| VARIABLES | (1) Ln Price | (2) Ln Price | (3) Ln Price | (4) Ln Price | (5) Ln Price | (6) Ln Price | (7) Ln Price | (8) Ln Price |
|---|---|---|---|---|---|---|---|---|
| 5Km-10KM x Post Yrs 1-3 | -0.0168 | | -0.0224 | 0.00448 | 0.00292 | | 0.00300 | -0.000967 |
| | (0.0142) | | (0.0160) | (0.0261) | (0.0123) | | (0.0164) | (0.0185) |
| 5Km-10KM x Post Yrs 4-10 | -0.0128 | | -0.0146 | -0.0256 | 0.00675 | | 0.00761 | -0.00511 |
| | (0.0294) | | (0.0316) | (0.0477) | (0.0200) | | (0.0246) | (0.0361) |
| <5KM x Post Yrs 1-3 | -0.0322** | | -0.0376** | -0.0139 | -0.0127 | | -0.0188 | -0.00785 |
| | (0.0150) | | (0.0163) | (0.0331) | (0.0151) | | (0.0172) | (0.0291) |
| <5KM x Post Yrs 4-10 | -0.0487* | | -0.0529* | -0.00106 | -0.0324 | | -0.0425 | -0.00728 |
| | (0.0274) | | (0.0289) | (0.0603) | (0.0236) | | (0.0278) | (0.0464) |
| Ln Dist x Post Yrs 1-3 | | 0.0267** | | | | 0.00990 | | |
| | | (0.0123) | | | | (0.0110) | | |
| Ln Dist x Post Yrs 4-10 | | 0.0483** | | | | 0.0277 | | |
| | | (0.0202) | | | | (0.0203) | | |
| Observations | 3,491,212 | 3,491,239 | 3,034,538 | 456,583 | 3,397,457 | 3,397,457 | 3,008,049 | 389,399 |
| $R^2$ | 0.345 | 0.344 | 0.340 | 0.296 | 0.328 | 0.328 | 0.330 | 0.306 |
| Plant Dist Contr | Y | Y | Y | Y | Y | Y | Y | Y |
| Hedonics | Y | Y | Y | Y | Y | Y | Y | Y |
| Max Dist | 20km | 20km | 20km | 20km | 20km | 20km | 20km | 20km |
| Weighted | N | N | N | N | Y | Y | Y | Y |

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on local housing prices for nearby homes. All regressions include plant district by year, distance bin (or log distance) x plant x year, district x year, city by year and county by year fixed-effects. The outcome variable is adjusted for hedonic characteristics by state. These include include land use type, house age (5 year bins with 1 year bins for ages <5), bedrooms , square footage by plant district (500 sq ft bins) and lot size by state (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below 2.5th percentile and above the 97.5th percentile in a given plant district x year are excluded as outliers. Weighted specifications are weighted by the inverse of the number of sales within 20km of the plants in a given year. Plant-years with fewer than 200 sales are excluded.

Table 7: School Finance Reform and County School Revenue by Source: Border Pairs Design

| VARIABLES | (1) Loc Share | (2) St Rev/Stud | (3) St Rev/Stud | (4) Ptax/Stud | (5) Ptax/Stud | (6) St+Loc/Stud | (7) St+Loc/Stud |
|---|---|---|---|---|---|---|---|
| Post Yrs 1-10 | -0.0802*** | 0.869*** | 0.851*** | -0.675*** | -0.611** | 0.152 | 0.206 |
| | (0.0267) | (0.264) | (0.246) | (0.246) | (0.239) | (0.182) | (0.174) |
| Post Yrs 11-25 | -0.110*** | 1.435*** | 1.396*** | -1.008*** | -0.872*** | 0.175 | 0.291 |
| | (0.0324) | (0.436) | (0.386) | (0.330) | (0.291) | (0.327) | (0.337) |
| Post Yrs 1-10 x Std 1970 Pov Share | | | 0.207 | | 0.0590 | | 0.294*** |
| | | | (0.126) | | (0.161) | | (0.108) |
| Post Yrs 1-10 x Std 1970 Pov Share | | | 0.467** | | 0.0972 | | 0.616*** |
| | | | (0.196) | | (0.161) | | (0.162) |
| Observations | 28,214 | 28,216 | 24,778 | 28,216 | 24,778 | 28,216 | 24,778 |
| $R^2$ | 0.907 | 0.897 | 0.899 | 0.888 | 0.880 | 0.933 | 0.933 |
| Weight | Y | Y | Y | Y | Y | Y | Y |
| Dep. Var. Mean | 0.338 | 4.695 | 4.695 | 2.991 | 2.991 | 8.671 | 8.671 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on school finance outcomes using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is a reform state and a vector of indicators for periods relative to reform (pre-reform is the omitted category). Controls include border pair by year fixed effects and county fixed-effects. Standard errors are clustered at the state and border pair level. All school finance data come from the Census of Governments (COG) and National Center for Economic Statistics (NCES). All statistics are aggregated to the county level, with a district being assigned to its primary county as defined by COG/NCES. Only districts with data missing in fewer than 10% of years are included to insure constancy of the sample within each county over time. Counties with fewer than 1,000 population in 1970 are excluded. For reform states, we include only 24 years before and 24 years after the reform.

Table 8: School Finance Reform and Manufacturing Presence: Border Pairs Design

| VARIABLES | (1) Emp Manf /1000 Pop | (2) Emp All /1000 Pop | (3) Emp Manf /Emp All | (4) Large Manf Estb /1000 Pop | (5) Large Estb /1000 Pop | (6) Large Estb /Est All |
|---|---|---|---|---|---|---|
| Post Yrs 1-10 | -3.587** | -0.253 | -0.00836** | -0.00167* | 0.000962 | 5.64e-05 |
| | (1.484) | (3.111) | (0.00409) | (0.000898) | (0.00234) | (0.0171) |
| Post Yrs 11-25 | -10.13*** | -8.362 | -0.0276*** | -0.00554*** | -0.00535 | -0.0432* |
| | (2.555) | (5.645) | (0.00632) | (0.00141) | (0.00376) | (0.0230) |
| Observations | 75,196 | 75,186 | 75,116 | 97,488 | 97,464 | 30,612 |
| $R^2$ | 0.944 | 0.953 | 0.955 | 0.863 | 0.833 | 0.903 |
| Dep. Var Mean | 56.57 | 252.2 | 1.314 | 0.0196 | 0.0340 | 0.572 |

)

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on total large manufacturing establishments and manufacturing employment as a share of both county population and overall establishments and employment using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is a reform state and a vector of indicators for periods relative to reform (pre-reform is the omitted category). Controls include border pair by year fixed effects and county fixed-effect. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). County-years with outcomes greater than the 99th percentile were excluded as outliers. All counties with fewer than 1,000 population in 1970 were excluded. For reform states, we include only 24 years before and 24 years after the reform.

Table 9: School Finance Reform and Manufacturing Presence by Urbanicity and Poverty: Border Pairs Design

| VARIABLES | (1) EmpManf | (2) EmpManf | (3) EmpAll | (4) EmpAll | (5) LargeManf | (6) LargeManf | (7) LargeAll | (8) Large All |
|---|---|---|---|---|---|---|---|---|
| Post Yrs 1-10 | -0.951 | -0.00186 | -3.666* | -0.00962* | -0.000400 | 0.00250 | -0.00359** | -0.00199 |
| | (1.525) | (0.00543) | (1.967) | (0.00557) | (0.00107) | (0.00302) | (0.00147) | (0.00251) |
| Post Yrs 11-25 | -5.193* | -0.0189** | -11.78*** | -0.0333*** | -0.00344** | -0.00245 | -0.00785*** | -0.00355 |
| | (2.617) | (0.00851) | (3.397) | (0.00792) | (0.00144) | (0.00465) | (0.00239) | (0.00570) |
| Post Yrs 1-10 x > 50% Urban | -7.004*** | -0.0175*** | | | -0.00370** | -0.00446 | | |
| | (2.379) | (0.00647) | | | (0.00139) | (0.00282) | | |
| Post Yrs 11-125 x > 50% Urban | -12.72*** | -0.0226** | | | -0.00581*** | -0.00801 | | |
| | (3.862) | (0.0109) | | | (0.00167) | (0.00494) | | |
| Observations | 75,196 | 75,116 | 28,430 | 28,408 | 97,488 | 97,464 | 34,944 | 34,920 |
| $R^2$ | 0.945 | 0.955 | 0.951 | 0.963 | 0.864 | 0.834 | 0.879 | 0.873 |
| Dep. Var Mean | 59.41 | 250.7 | 59.41 | 250.7 | 0.0198 | 0.0377 | 0.0198 | 0.0377 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on total large manufacturing establishments and manufacturing employment as a share of both county population and overall establishments and employment using a contiguous border county difference-in-differences design. Columns (1)-(2) and (5)-(6) show the effects by 1970 urbanicity. Columns (3)-(4) and (7)-(8) restrict the sample to only counties that had below average poverty rates (relative to their state means) in 1970. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is a reform state and a vector of indicators for periods relative to reform (pre-reform is the omitted category). Controls include border pair by year fixed effects and county fixed-effect. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). Demographic variables are from the 1970 US Census. County-years with outcomes greater than the 99th percentile were excluded as outliers. All counties with fewer than 1,000 population in 1970 were excluded. For reform states, we include only 24 years before and 24 years after the reform.

Table 10: School Finance Reform and Employment by Industry Type

| VARIABLES | (1) AgEmp/1KPop | (2) MineEmp/1KPop | (3) ConstrEmp/1KPop | (4) RtlEmp/1KPopr | (5) TransUtilEmp/1KPop | (6) WholesaleEmp/1KPop | (7) OthInd/1KPop |
|---|---|---|---|---|---|---|---|
| Post Yr 1-10 | -0.0747 | 0.552 | 0.0648 | 0.752 | -0.0454 | 0.191 | 0.425 |
| | (0.132) | (1.539) | (0.265) | (0.456) | (0.315) | (0.348) | (3.622) |
| Post Yr 11-25 | 0.0921 | 2.256 | -0.268 | -0.236 | -0.597 | 0.612 | -0.0628 |
| | (0.188) | (2.774) | (0.477) | (0.744) | (0.541) | (0.564) | (5.281) |
| Observations | 23,936 | 16,830 | 82,652 | 95,880 | 67,146 | 82,684 | 45,678 |
| $R^2$ | 0.876 | 0.936 | 0.904 | 0.947 | 0.897 | 0.926 | 0.954 |
| Dep. Var Mean | 1.805 | 11.13 | 14.30 | 51.59 | 11.74 | 13.51 | 13.51 |

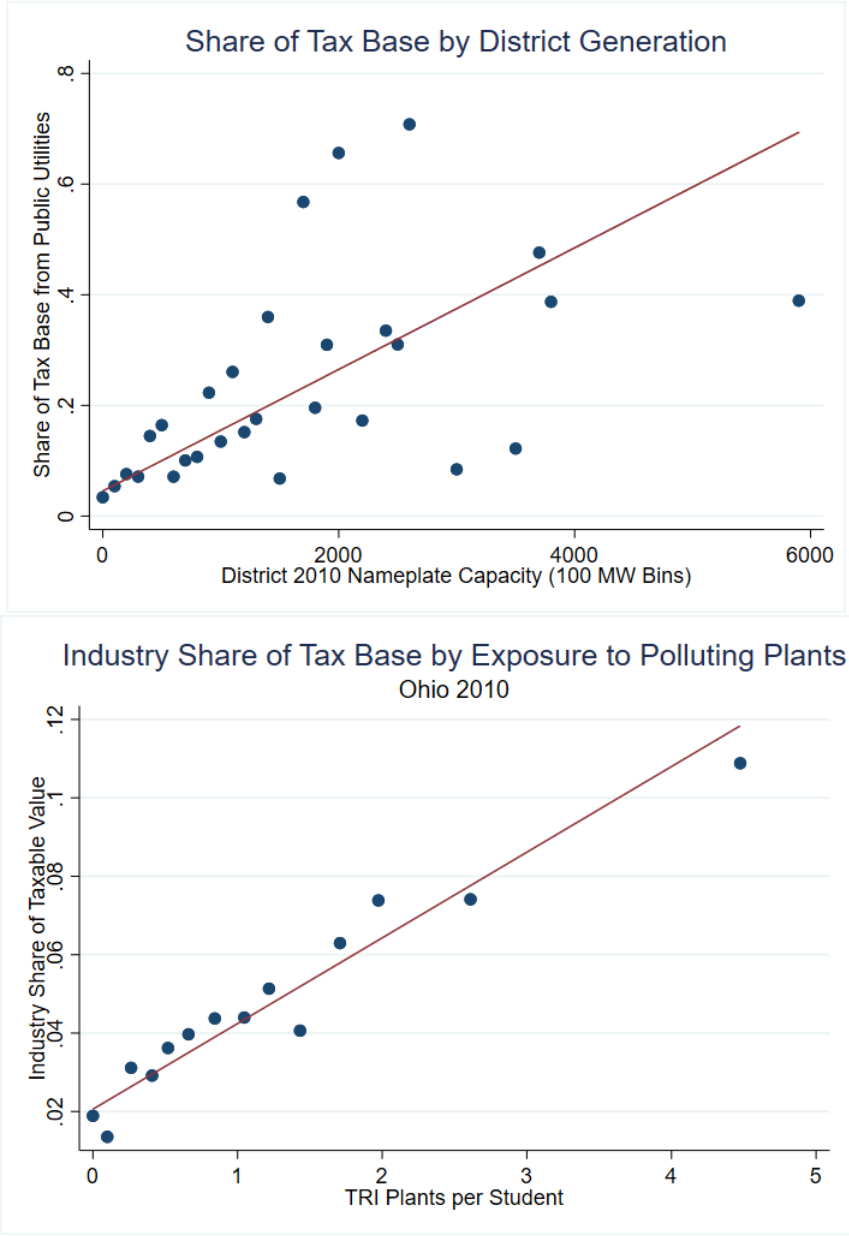
)

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

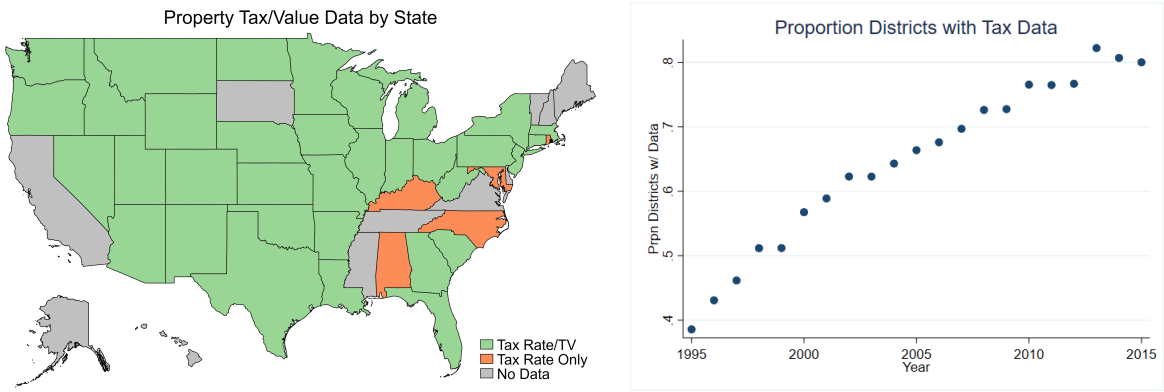
This table shows the effect of a school finance reform on employment by industry using a contiguous border county difference-in-differences design. Agricultural employment includes forestry and mining employment includes quarrying and oil/gas extraction. All county-years in which employment was reported as zero, but establishments were reported as non-zero were excluded. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is a reform state and a vector of indicators for periods relative to reform (pre-reform is the omitted category). Controls include border pair by year fixed effects and county fixed-effect. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). County-years with outcomes greater than the 99th percentile were excluded as outliers. All counties with fewer than 10,000 population in 1970 were excluded. For reform states, we include only 24 years before and 24 years after the reform.

Figure A1: Utility Share of School District Tax Base by District Generation Level



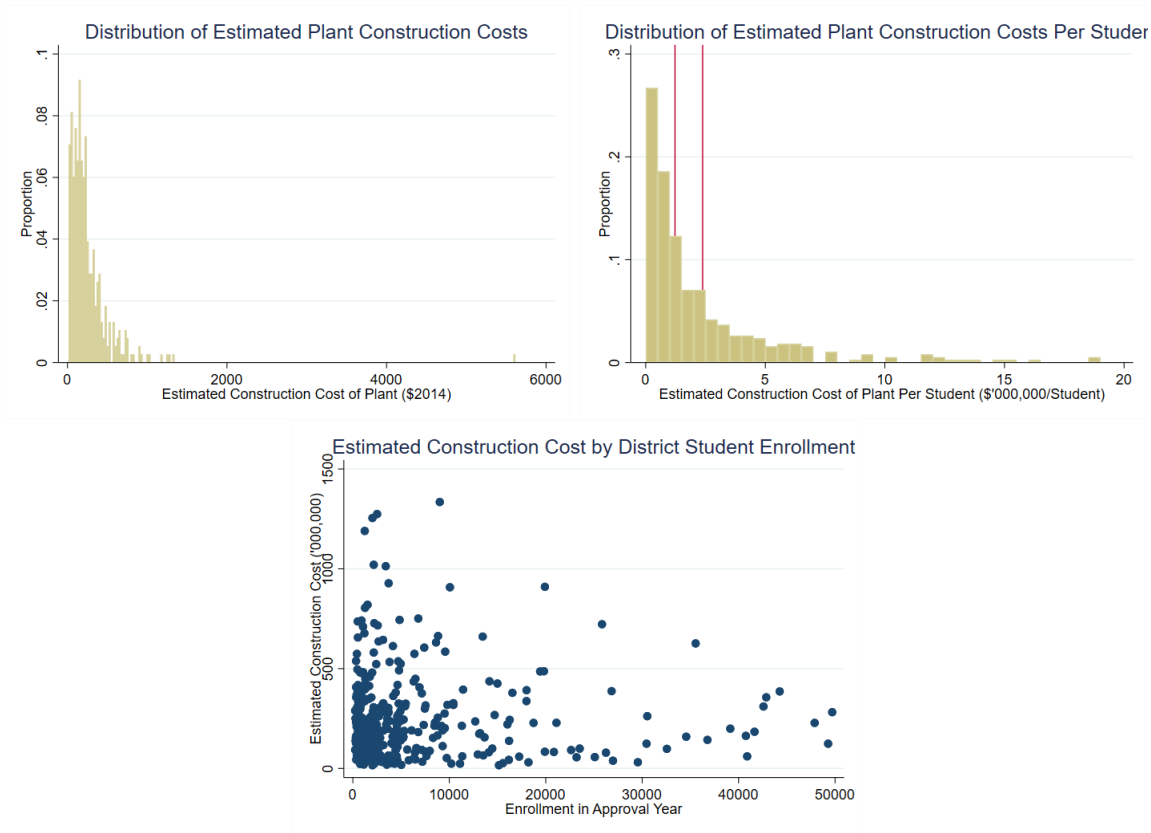

The top panel of this figure shows the proportion of a district's 2010 tax base that is made up of utility property as a function of the amount of generating capacity located in a district. All data are from 2010 and come from the 8 states with utility valuation data available: Connecticut, Georgia, Iowa, Minnesota, Ohio, Oklahoma, Oregon, and Washington. Hydroelectric generation is excluded as most dams are federally-owned and pay payments-in-lieu-of-taxes (PILOT) rather than property taxes. The bottom panel of this figure shows the proportion of a district's tax base that is made up by industry as a function of Toxic Release Inventory (TRI) plants per student within a district . Data is for Ohio only and from 2010.

Figure A2: Data Coverage of Property Tax Rates and Taxable Value



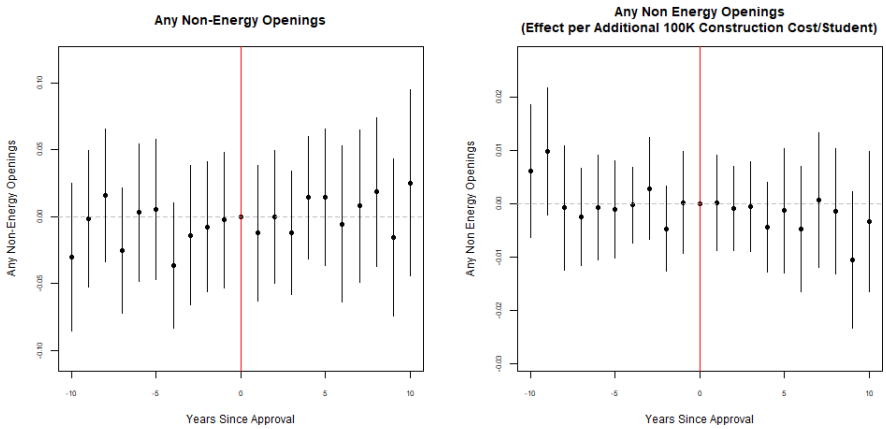

This figure shows coverage of district-level data on school district property tax rates and total taxable value. The figure on the left shows geographic coverage—"Tax Rate/TV" denotes that a state has both tax rate and taxable value coverage. The figure on the right shows the proportion of districts in our final sample that have property tax rate data in a given year. Data were hand collected from state Department of Revenue and Department of Education Annual Reports.

Figure A3: Estimated Fiscal Impact



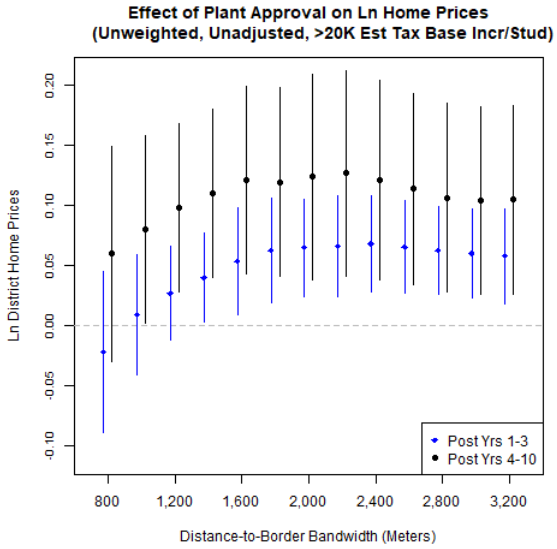

This figure shows various summary statistics on estimated plant construction costs, which we use as a proxy of plant valuation. The left and center figure on the top-left shows the distribution of estimated plant construction costs and construction costs per student (we exclude the small number of plant openings with grater than $2.5 million in construction costs per student. The figure on right shows the relationship between school district enrollment and the estimated construction cost of a plant locating in its borders. Construction cost data and plant opening data come from the EIA, while enrollment data comes from the NCES.

Figure A4: Opening of Non-Utility TRI Facilities



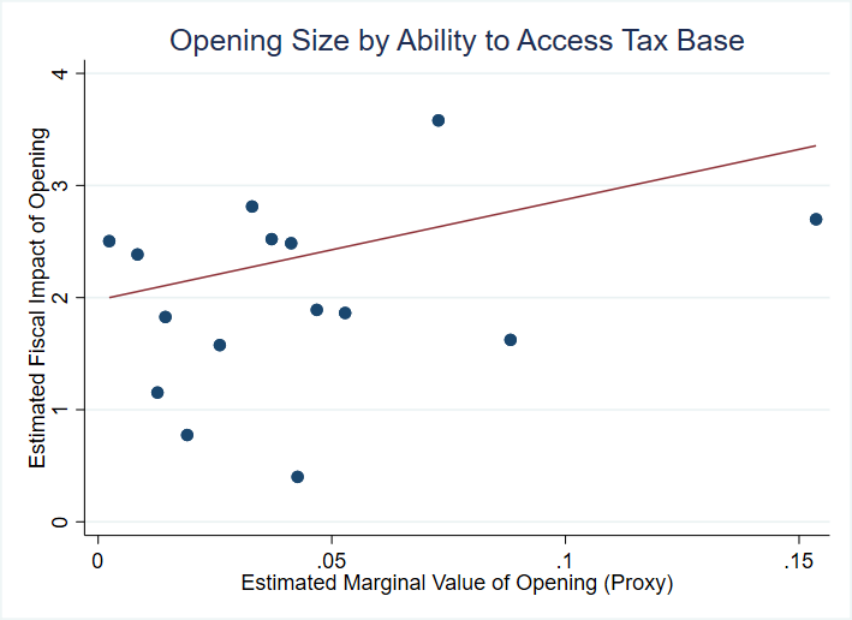

This figure shows the likelihood of a district having any facility opening in a year surrounding the start of construction on a power plant. Opening data comes from the Toxic Release Inventory (TRI) and is based on the first year that a facility appears in the data. Coefficients come from a regression of non-utility TRI plant openings on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the size of the expected increase in tax base per student (right panel only). Matched pairs were created using nearest-neighbor matching based on a score derived from a probit regression of treatment status on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. Matches are restricted to be in the same state and commuting zone. All standard errors are clustered at the matched pair level. Plant data comes from the Toxic Release Inventory run by the EPA. Plant opening data comes from the Energy Information Administration (EIA).

Figure A5: Effect of Opening on Host-District Home Prices: By Distance Bandwidth, No Hedonic Controls



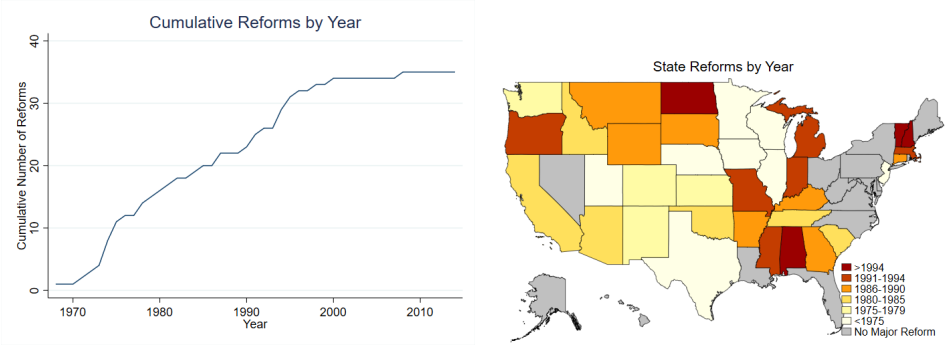

This figure shows the effect of a plant opening on local housing prices. Coefficients come from a regression of the outcome on an interaction between an indicator for whether a house is in a plant-receiving district and indicators for years since plant approval. Controls include a border pair by year fixed-effects and district by month fixed-effects. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and $2,000,000 as likely outliers. Only openings with an expected tax base impact of more than $20,000/student are included.

Figure A6: Estimated Tax Base Effect of Opening by Estimated Marginal Value of Tax Base in Opening State-Year




This figure shows the relationship between the expected marginal value of an additional dollar of tax base with respect to school spending and the estimated tax base impact of a plant. Estimated tax base impact of a plant is equal to the plant's estimated construction cost divided by the number of students in a district in the year of approval. Construction costs were estimated using parameters from the EIA's Annual Energy Outlook. We proxy for district's estimated marginal value of tax base using a coefficient dervied from a state-specific regression of state and local revenue per student on taxable value per student with district and year fixed-effects.

Figure A7: School Finance Reforms Geographic and Temporal Distribution




This figure shows the cumulative number of school finance reforms affecting the marginal value of an additional dollar of tax base (left panel) and their geographic distribution (right panel). Reforms were identified using funding formulas reported in the Public School Finance Programs of the United States series.

Table A1: Predictors of Plant Opening

| VARIABLES | (1)<br>treat2 |
|---|---|
| Total Rev/Student | -0.886*** |
| | (0.339) |
| State Rev/Student | 0.731** |
| | (0.351) |
| Local Rev/Student | 0.885*** |
| | (0.343) |
| 1990 Home-Value | -6.77e-06*** |
| | (1.79e-06) |
| 1990 Income | -1.69e-05** |
| | (7.87e-06) |
| Pct Free Lunch Share | 0.734 |
| | (0.530) |
| Share Asian | -5.473** |
| | (2.340) |
| Share Amer. Indian. | 0.921 |
| | (1.086) |
| Share Hispanic | 0.772 |
| | (0.520) |
| Share Black | 0.450 |
| | (0.506) |
| Land Area | -2.24e-05 |
| | (3.99e-05) |
| Students | 4.50e-05*** |
| | (7.61e-06) |
| Observations | 6,726 |
| Number of Commuting Zones | 343 |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the relationship between various economic, demographic and school funding variables and the probability that a district ever has a plant locate within it between 1995 and 2015 conditional on state x commuting zone using a fixed-effects logit model. All demographic characteristics come from the year 1995 and are taken from the National Center of Education Statistics (NCES). Observations reflect the number of districts in commuting zones with any openings.

Table A2: Effects of Plant Opening on District Taxable Value: Robustness Check

| VARIABLES | (1) Ln Tval | (2) Ln Tvale | (3) Ln Tval | (4) Ln Tval | (5) Ln Tval | (6) Ln Tval | (7) Ln Tval | (8) Ln Tval | (9) Ln Tval |
|---|---|---|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 0.0273 | 0.0361* | -0.00725 | 0.0493*** | 0.0396* | 0.0471*** | 0.0160 | | |
| | (0.0249) | (0.0197) | (0.0217) | (0.0177) | (0.0217) | (0.0171) | (0.0183) | | |
| Treat x Post Yrs 4-10 | 0.0366 | 0.0755** | 0.00533 | 0.117*** | 0.00682 | 0.0960*** | 0.0132 | | |
| | (0.0297) | (0.0305) | (0.0379) | (0.0260) | (0.0265) | (0.0266) | (0.0248) | | |
| Treat x Post Yrs 1-3 x >Med Est Tax Base Incr | 0.0646** | | | | | | | | |
| | (0.0317) | | | | | | | | |
| Treat x Post Yrs 4-10 x >Med Est Tax Base Incr | 0.139*** | | | | | | | | |
| | (0.0401) | | | | | | | | |
| Treat x Post Yrs 1-3 x $100K/Stud Est Tax Base Incr | | | 0.0151** | | 0.00307 | | 0.00720* | | |
| | | | (0.00634) | | (0.00626) | | (0.00419) | | |
| Treat x Post Yrs 4-10 x $100K/Stud Est Tax Base Incr | | | 0.0262 | | 0.0402*** | | 0.0271*** | | |
| | | | (0.0174) | | (0.00747) | | (0.00623) | | |
| Observations | 11,366 | 7,616 | 9,204 | 10,424 | 11,766 | 10,072 | 11,688 | | |
| $R^2$ | 0.965 | 0.976 | 0.978 | 0.968 | 0.965 | 0.968 | 0.969 | | |
| Spec | Base | Same Cnty | Same Cnty | State | State | +Small Dist | +Small Dist | | |
| Dep. Var. Mean | 552891 | 492500 | 492500 | 547513 | 547513 | 586645 | 586645 | | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the natural log of taxable value per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category) and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,5,7 and 9). Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. More controls refers to the inclusion of log 1990 median income, log land area, Title 1 expenditures per student, log total 1995 expenditures per student and local share of revenues. Including small districts drops any district size restrictions. The same county specification restricts matches to be within the same county, while the same state specification restricts matches to be within the same state only. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A3: Effects of Plant Opening on District Ln Local Revenue per Student: Robustness Check

| VARIABLES | (1) Ln LocRev | (2) Ln LocRev | (3) Ln LocRev | (4) Ln LocRev | (5) Ln LocRev | (6) Ln LocRev | (7) Ln LocRev | (8) Ln LocRev | (9) Ln LocRev |
|---|---|---|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 0.0163 | 0.0408*** | 0.0331** | 0.0374*** | 0.0200* | 0.0368*** | 0.00474 | | |
| | (0.0135) | (0.0133) | (0.0134) | (0.0124) | (0.0119) | (0.0127) | (0.0133) | | |
| Treat x Post Yrs 4-10 | 0.0184 | 0.0632*** | 0.0210 | 0.0776*** | 0.00358 | 0.0781*** | -0.0123 | | |
| | (0.0187) | (0.0184) | (0.0205) | (0.0182) | (0.0161) | (0.0190) | (0.0187) | | |
| Treat x Post Yrs 1-3 x >Med Est Tax Base Incr | 0.0414* | | | | | | | | |
| | (0.0212) | | | | | | | | |
| Treat x Post Yrs 4-10 x >Med Est Tax Base Incr | 0.112*** | | | | | | | | |
| | (0.0297) | | | | | | | | |
| Treat x Post Yrs 1-3 x $100K/Stud Est Tax Base Incr | | | 0.00314 | | 0.00797* | | 0.00867** | | |
| | | | (0.00423) | | (0.00409) | | (0.00415) | | |
| Treat x Post Yrs 4-10 x $100K/Stud Est Tax Base Incr | | | 0.0207* | | 0.0346*** | | 0.0356*** | | |
| | | | (0.0108) | | (0.00788) | | (0.00863) | | |
| Observations | 20,308 | 12,870 | 15,538 | 18,066 | 20,592 | 18,874 | 21,478 | | |
| $R^2$ | 0.963 | 0.960 | 0.964 | 0.955 | 0.959 | 0.956 | 0.963 | | |
| Spec | Base | Same Cnty | Same Cnty | Same State | Same State | +Small Dist | +Small Dist | | |
| Dep. Var. Mean | 4.787 | 4.806 | 4.806 | 4.732 | 4.732 | 4.953 | 4.953 | | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the natural log of local revenue per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category) and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,5,7 and 9). Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. More controls refers to the inclusion of log 1990 median income, log land area, Title 1 expenditures per student, log total 1995 expenditures per student and local share of revenues. Including small districts drops any district size restrictions. The same county specification restricts matches to be within the same county, while the same state specification restricts matches to be within the same state only. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A4: Effects of Plant Opening on District Local Tax Rate: Robustness Check

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | Ln Rate | Ln Rate | Ln Rate | Ln Rate | Ln Rate | Ln Rate | Ln Rate | Ln Rate | Ln Rate |
| Treat x Post Yr 1-3 | 0.00312 | 0.0174 | 0.0198 | 0.0120 | 0.0170 | 0.00621 | 0.0179 | | |
| | (0.0151) | (0.0169) | (0.0193) | (0.0132) | (0.0157) | (0.0115) | (0.0128) | | |
| Treat x Post Yr 4-10 | -0.0164 | -0.0246 | -0.0163 | -0.0211 | -0.00981 | -0.0355** | 0.00128 | | |
| | (0.0160) | (0.0243) | (0.0246) | (0.0170) | (0.0189) | (0.0161) | (0.0173) | | |
| Treat x Post Yr 1-3 x >Med Est Tax Base Incr | 0.000817 | | | | | | | | |
| | (0.0228) | | | | | | | | |
| Treat x Post Yr 4-10 x >Med Est Tax Base Incr | -0.0388 | | | | | | | | |
| | (0.0292) | | | | | | | | |
| Treat x Post Yrs 1-3 x $100K/Stud Est Tax Base Incr | | | -0.00133 | | -0.00406 | | -0.00260 | | |
| | | | (0.00364) | | (0.00289) | | (0.00177) | | |
| reat x Post Yrs 4-10 x $100K/Stud Est Tax Base Incr | | | -0.00215 | | -0.00375 | | -0.00990** | | |
| | | | (0.00599) | | (0.00489) | | (0.00482) | | |
| Observations | 11,700 | 7,594 | 9,160 | 10,712 | 12,114 | 10,444 | 12,168 | | |
| $R^2$ | 0.981 | 0.987 | 0.987 | 0.982 | 0.980 | 0.980 | 0.981 | | |
| Spec | Base | Same Cnty | Same Cnty | Same State | Same State | +Small Dist | +Small Dist | | |
| Dep. Var. Mean | 0.0103 | 0.0107 | 0.0107 | 0.0103 | 0.0103 | 0.0101 | 0.0101 | | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the natural log of property tax rate per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category) and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,5,7 and 9). Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. More controls refers to the inclusion of log 1990 median income, log land area, Title 1 expenditures per student, log total 1995 expenditures per student and local share of revenues. Including small districts drops any district size restrictions. The same county specification restricts matches to be within the same county, while the same state specification restricts matches to be within the same state only. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A5: Effects of Plant Opening on Total Revenue per Student: Robustness Check

| VARIABLES | (1) Ln Exp/Stud | (2) Ln Exp/Stud | (3) Ln Exp/Stud | (4) Ln Exp/Stud | (5) Ln Exp/Stud | (6) Ln Exp/Stud | (7) Ln Exp/Stud | (8) Ln Exp/Stud | (9) Ln Exp/Stud |
|---|---|---|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | -0.00160 | 0.00824 | 0.00660 | 0.00834 | 0.000252 | 0.000912 | -0.00457 | | |
| | (0.00592) | (0.00632) | (0.00637) | (0.00583) | (0.00636) | (0.00606) | (0.00625) | | |
| Treat x Post Yr 4-10 | 5.56e-05 | 0.0194** | 0.00798 | 0.0164** | -0.00168 | 0.0174** | -0.0117 | | |
| | (0.00747) | (0.00803) | (0.00865) | (0.00758) | (0.00783) | (0.00854) | (0.00809) | | |
| Treat x Post Yrs 1-3 x >Med Est Tax Base Incr | 0.0188* | | | | | | | | |
| | (0.00977) | | | | | | | | |
| Treat x Post Yrs 4-10 x >Med Est Tax Base Incr | 0.0363** | | | | | | | | |
| | (0.0143) | | | | | | | | |
| Treat x Post Yrs 1-3 x $100K Est Tax Base Incr/Stud | | | 0.000619 | | 0.00403** | | 0.00153 | | |
| | | | (0.00201) | | (0.00182) | | (0.00194) | | |
| Treat x Post Yrs 4-10 x $100K Est Tax Base Incr/Stud | | | 0.00502 | | 0.00946*** | | 0.0118*** | | |
| | | | (0.00426) | | (0.00296) | | (0.00381) | | |
| Observations | 20,272 | 12,840 | 15,522 | 18,182 | 20,678 | 18,908 | 21,416 | | |
| $R^2$ | 0.939 | 0.938 | 0.941 | 0.929 | 0.934 | 0.930 | 0.938 | | |
| Spec | Base | Same Cnty | Same Cnty | Same State | Same State | +Small Dist | +Small Dist | | |
| Dep. Var. Mean | 11.66 | 11.70 | 11.70 | 11.63 | 11.63 | 12 | 12 | | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the natural log of total revenue per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category) and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,5,7 and 9). Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. More controls refers to the inclusion of log 1990 median income, log land area, Title 1 expenditures per student, log total 1995 expenditures per student and local share of revenues. Including small districts drops any district size restrictions. The same county specification restricts matches to be within the same county, while the same state specification restricts matches to be within the same state only. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A6: Effects of Plant Opening on Expenditures per Student: Robustness Check

| VARIABLES | (1) Ln Exp/Stud | (2) Ln Exp/Stud | (3) Ln Exp/Stud | (4) Ln Exp/Stud | (5) Ln Exp/Stud | (6) Ln Exp/Stud | (7) Ln Exp/Stud | (8) Ln Exp/Stud | (9) Ln Exp/Stud |
|---|---|---|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | -0.00675 | 0.00680 | 0.00746 | 0.00808 | -0.000603 | 0.00218 | -0.00810 | | |
| | (0.00975) | (0.0104) | (0.0108) | (0.0102) | (0.0114) | (0.00787) | (0.00937) | | |
| Treat x Post Yr 4-10 | -0.00612 | 0.0285** | 0.00518 | 0.0296** | -0.00497 | 0.0143 | -0.0240** | | |
| | (0.00952) | (0.0111) | (0.0100) | (0.0122) | (0.0112) | (0.0134) | (0.0110) | | |
| Treat x Post Yrs 1-3 x >Med Est Tax Base Incr | 0.0235 | | | | | | | | |
| | (0.0153) | | | | | | | | |
| Treat x Post Yrs 4-10 x >Med Est Tax Base Incr | 0.0494** | | | | | | | | |
| | (0.0198) | | | | | | | | |
| Treat x Post Yrs 1-3 x $100K Est Tax Base Incr/Stud | | | 0.00116 | | 0.00383 | | 0.00203 | | |
| | | | (0.00345) | | (0.00435) | | (0.00344) | | |
| Treat x Post Yrs 4-10 x $100K Est Tax Base Incr/Stud | | | 0.00989** | | 0.0161*** | | 0.0136** | | |
| | | | (0.00476) | | (0.00444) | | (0.00552) | | |
| Observations | 21,018 | 13,278 | 16,044 | 18,826 | 21,420 | 19,628 | 22,172 | | |
| $R^2$ | 0.888 | 0.865 | 0.873 | 0.860 | 0.869 | 0.883 | 0.890 | | |
| Spec | Base | Same Cnty | Same Cnty | Same State | Same State | +Small Dist | +Small Dist | | |
| Dep. Var. Mean | 11.97 | 11.96 | 11.96 | 11.89 | 11.89 | 12.40 | 12.40 | | |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the natural log of total expenditures per student. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category) and whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide (Columns 3,5,7 and 9). Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. More controls refers to the inclusion of log 1990 median income, log land area, Title 1 expenditures per student, log total 1995 expenditures per student and local share of revenues. Including small districts drops any district size restrictions. The same county specification restricts matches to be within the same county, while the same state specification restricts matches to be within the same state only. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A7: Effects of Plant Opening on Home Prices: Robustness Check

| VARIABLES | (1) Ln Price | (2) Ln Price | (3) Ln Price | (4) Ln Price | (5) Ln Price | (6) Ln Price | (7) Ln Price | (8) Ln Price |
|---|---|---|---|---|---|---|---|---|
| Treat x Post Yr 1-3 | 0.0487** | 0.0445* | 0.0389** | 0.0551** | 0.0549** | 0.0145 | 0.0460 | 0.0337*** |
| | (0.0220) | (0.0227) | (0.0151) | (0.0270) | (0.0232) | (0.0304) | (0.0422) | (0.0110) |
| Treat x Post Yr 4-10 | 0.0710** | 0.0833*** | 0.0596** | 0.0904** | 0.0676*** | 0.0423** | 0.0834* | 0.0429*** |
| | (0.0282) | (0.0177) | (0.0248) | (0.0445) | (0.0244) | (0.0187) | (0.0458) | (0.00742) |
| Observations | 104,171 | 46,507 | 85,238 | 65,440 | 100,031 | 53,019 | 48,482 | 70,529 |
| $R^2$ | 0.504 | 0.619 | 0.571 | 0.493 | 0.536 | 0.592 | 0.432 | 0.684 |
| Bound Type | Non-County | County | All Dist Type | All | All Dist Type | All | All Dist Type | All |
| Distance Type | All | All | >5Km | <5km | All | All | All | All |
| Sample | All | All | All | All | Similar | Similar+ | All | All |
| House Age | All | All | | All | | All | $< 5Yr$ | >5yr |
| Dep. Var. Mean | 221162 | 221162 | 221162 | 221162 | 221162 | 221162 | 221162 | 221162 |

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on local housing prices using a border difference-in-difference design. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. Sample restrictions are as indicated. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000 as likely outliers. Only openings with an expected tax base impact of more than $20,000/student are included.

Table A8: Effects of Plant Openings on Home Prices: Different Fixed-Effect Specifications

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | Ln Price | Ln Price | Ln Price | Ln Price | Ln Price | Ln Price |
| Treat x Post Years 1-3 | 0.0274 | 0.0308 | 0.0405** | 0.0388** | 0.0404*** | 0.0502*** |
| | (0.0228) | (0.0199) | (0.0166) | (0.0176) | (0.0154) | (0.0170) |
| Treat x Post Years 4-10 | 0.0528* | 0.0612** | 0.0682*** | 0.0634*** | 0.0644*** | 0.0763*** |
| | (0.0314) | (0.0244) | (0.0211) | (0.0224) | (0.0192) | (0.0191) |
| Observations | 165,318 | 165,245 | 164,040 | 154,824 | 154,195 | 142,944 |
| $R^2$ | 0.335 | 0.354 | 0.392 | 0.497 | 0.503 | 0.586 |
| Time-Invarying | Dist. | Dist. x BP | Dist. x BP x Dist Bin | Dist x BP x Dist Bin + Dist x BP x Plnt Dist Bin | Dist x BP x Dist Bin (400m) | Dist. x BP x Dist Bin (100m) |
| Dep. Var. Mean | 221162 | 221162 | 221162 | 221162 | 221162 | 221162 |
| x Plant Dist Bin (100m | | | | | | ) |
| x Plant Dist Bin (400m | | | | | ) | |

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on local housing prices using a border difference-in-difference design. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and time-invariant fixed-effects as indicated. The outcome variable is residualized for hedonic by state controls. These include land use , home age by plant district (5 year bins with 1 year bins for ages <5, number of rooms , bedrooms , bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000 as likely outliers. Only openings with an expected tax base impact of more than $20,000/student are included.

Table A9: Effects of Plant Opening on Key Hedonic Variables

| VARIABLES | (1) Lot Size | (2) Bedroomss | (3) Age | (4) Sqft | (5) SFH | (6) Dist to Border |
|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | -0.00223 | 0.0234 | -0.569* | -19.56 | -0.00215 | 0.0535 |
| | (0.00688) | (0.0277) | (0.327) | (23.11) | (0.00828) | (0.0431) |
| Treat x Post Yrs 4-10 | 0.00394 | 0.00746 | -0.858* | -55.51* | -0.00644 | 0.0847 |
| | (0.00974) | (0.0332) | (0.506) | (29.28) | (0.00850) | (0.0640) |
| Observations | 117,744 | 77,406 | 124,862 | 106,935 | 154,263 | 163,405 |
| $R^2$ | 0.851 | 0.664 | 0.857 | 0.725 | 0.685 | 0.808 |
| Bandwidth | 1.6 km | 1.6 km | 1.6 km | 1.6 km | 1.6 km | 1.6 km |
| Weighted | N | N | N | N | N | N |
| Dep Var Mean. | 0.514 | 3.247 | 23.21 | 1786 | 0.881 | -0.133 |

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on hedonic characteristics of sold homes using a border difference-in-difference design. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. Outliers (above the 99th percentile or below the 1st percentile for each outcome variable) are excluded. Standard errors are clustered at the plant district level. Sample restrictions are as indicated. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000. Only openings with an expected tax base impact of more than $20,000/student are included.

Table A10: Effects of Plant Opening on Quantity of Homes Sold

| VARIABLES | (1) Ttl Sales | (2) Ln Sales | (3) New Constr | (4) Ttl Sales | (5) Ln Sales | (6) New Construct |
|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 2.109 | 0.0471 | 0.400 | 8.104 | 0.0298 | -0.927 |
| | (1.510) | (0.0537) | (1.158) | (4.978) | (0.0467) | (1.584) |
| Treat x Post Yrs 4-10 | 2.122 | 0.0715 | -0.716 | 6.812 | 0.0627 | -4.657 |
| | (2.009) | (0.0446) | (1.721) | (8.203) | (0.0415) | (3.418) |
| Observations | 8,016 | 4,746 | 8,016 | 8,016 | 4,746 | 8,016 |
| $R^2$ | 0.974 | 0.840 | 0.915 | 0.976 | 0.857 | 0.927 |
| Weighting | All Plant = | All Plant = | All Plant = | BL Stud | BL Stud | BL Stud |
| Dep. Var. Mean | 14.72 | 14.72 | 14.72 | 14.72 | 14.72 | 14.72 |

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on the quantity of homes sold using a border difference-in-difference design. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district fixed-effects. Standard errors are clustered at the plant district level. Sample restrictions are as indicated. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000 as likely outliers. Only openings with an expected tax base impact of more than $20,000/student are included.

Table A11: Effects of Plant Opening on Key School Finance Variables: Home Price Analysis

| VARIABLES | (1) Ln Loc Rev | (2) Ln Tot Rev | (3) Ln TtlExp | (4) CapEx | (5) OthEx | (6) LTD/Cap | (7) Ptax |
|---|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 | 0.127*** | 0.0319** | -0.0164 | -0.284 | -0.0283 | 1.025* | 0.000274 |
| | (0.0340) | (0.0157) | (0.0221) | (0.284) | (0.147) | (0.587) | (0.000363) |
| Treat x Post Yrs 4-10 | 0.131** | -0.00203 | 0.0505 | 0.983*** | -0.126 | 3.721*** | 0.000327 |
| | (0.0512) | (0.0236) | (0.0327) | (0.302) | (0.273) | (1.387) | (0.000341) |
| Observations | 145,998 | 148,799 | 147,189 | 147,189 | 147,189 | 144,262 | 74,793 |
| $R^2$ | 0.992 | 0.986 | 0.945 | 0.821 | 0.991 | 0.964 | 0.998 |
| Cut-off | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Weighted | N | N | Y | Y | Y | Y | Y |

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on key school finance variables using a border difference-in-difference design. We only include sales within a bandwidth of 1 mile from the border. Coefficients come from a regression of log home prices on an interaction between an indicator for whether a house is in a plant-receiving district and a vector of indicators for grouping of years since approval. Controls include border pair by year fixed-effects and border pair by district x distance to border (200 m bins) x distance to plant (200 m bins) fixed-effects. Standard errors are clustered at the plant district level. Sample restrictions are as indicated. All housing data come from the Zillow ZTRAX database—sales below the 2.5th percentile or 97.5th percentile in a given plant district and year are excluded. We further exclude any sales below $2,000 and above$2,000,000 as likely outliers. Only openings with an expected tax base impact of more than $20,000/student are included.

Table A12: Differential Effects of Plant Opening on Key School Finance Variables by State Equalization Status

| VARIABLES | (1) TotRev | (2) TotRev | (3) TotRev | (4) TtlExp | (5) TtlExp | (6) TtlExp |
|---|---|---|---|---|---|---|
| Treat x Post Yrs 1-3 x $100K/Std Est Tax Base Incr | 0.0128 | 0.0692* | 0.0391 | 0.0684 | 0.0347 | 0.0562 |
| | (0.0775) | (0.0407) | (0.0687) | (0.0957) | (0.0687) | (0.0848) |
| Treat x Post Yrs 1-3 x $100K/Std Est Tax Base Incr | 0.0869 | 0.238** | 0.0717 | -0.0342 | 0.279*** | -0.0388 |
| | (0.0795) | (0.0986) | (0.0878) | (0.0804) | (0.0924) | (0.0830) |
| Treat x Post Yrs 1-3 x $100K/Std Est Tax Base Incr | | | 0.00308 | | | -0.00102 |
| | | | (0.0100) | | | (0.0133) |
| Treat x Post Yrs 1-3 x $100K/Std Est Tax Base Incr x MVTB (Mills) | | | 0.0242 | | | 0.0439*** |
| | | | (0.0166) | | | (0.0156) |
| Observations | 3,972 | 7,364 | 11,336 | 3,980 | 7,338 | 11,318 |
| $R^2$ | 0.935 | 0.893 | 0.911 | 0.875 | 0.785 | 0.822 |
| Sample | Blw Med MVTB | Abv Med MVTB | All | Blw Med MVTB | Abv Med MVTB | All |
| Dep. Var. Mean | 11.66 | 11.66 | 11.66 | 11.68 | 11.68 | 11.68 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a plant opening on various district-level school finance outcomes. Coefficients come from a regression of log taxable value/student on district fixed-effects, year x matched pair fixed effects and interactions between indicators for years since approval (0 is the omitted category), whether or not a district receives a plant and the estimated size of the increase in tax base per student the plant will provide. Matched pairs were created using nearest-neighbor matching based on 1995 total, local and state revenue per student, 1990 income, 1990 housing, 1995 student population, 1995 district free lunch eligibility rate, 1995 Asian, American Indian, Black and Hispanic share of students, and commuting zone by state fixed effects. All matches were restricted to be in the same state, commuting zone and district type and control districts were sampled with replacement. We proxy for district's estimated marginal value of tax base using a coefficient dervied from a state-specific regression of state and local revenue per student on taxable value per student with district and year fixed-effects. All standard errors are clustered at the commuting zone level. District property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Local revenues per student came from the National Center for Education Statistics (NCES). Plant opening data comes from the Energy Information Administration (EIA).

Table A13: School Finance Reform and Employment: Border Pairs Design Robustness Check

| VARIABLES | (1) ManfEmp /1KPop | (2) ManfEmp /1KPop | (3) ManfEmp /1KPop | (4) ManfEmp /1KPop | (5) ManfEmp /1KPop | (6) ManfEmp /1KPop |
|---|---|---|---|---|---|---|
| Post Yr 1-10 | -3.747** | -3.555** | -4.723* | -4.832*** | -2.297 | -3.226** |
| | (1.559) | (1.528) | (2.556) | (1.634) | (1.762) | (1.554) |
| Post Yr 11-25 | -10.91*** | -8.251** | -11.44*** | -10.59*** | -8.405*** | -8.834*** |
| | (2.776) | (3.824) | (3.905) | (2.976) | (2.915) | (3.011) |
| Observations | 67,894 | 34,634 | 33,362 | 71,462 | 96,862 | 69,560 |
| $R^2$ | 0.946 | 0.947 | 0.950 | 0.942 | 0.930 | 0.936 |
| Sample | Base | Reform-Only | Pure DiD | Incl Outlier | Incl > \|24\|yr | All |
| Weight | All State = | All State = | All State = | All State = | All State = | N |
| Controls | Base+ Demo x Year | Base | Base | Base | Base | Base |
| Dep. Var Mean | 59.41 | 59.41 | 59.41 | 59.41 | 59.41 | 59.41 |

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on total manufacturing employment as a share of overall establishments and employment using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether or not a reform has yet occurred in a state. Controls include border pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). All counties with fewer than 1,000 population in 1970 were excluded unless otherwise noted. In reform states only years greater than 25 years prior and less than 25 years following the reform were included unless otherwise noted. County-years with outcomes greater than the 99th percentile were excluded unless otherwise noted. Reform years only means only states with reforms were included. Pure DiD means that only border pairs in which one member of the pair never had a reform were included. Demographic controls include povety, urbanicity, populatoin and white share of the county population.

Table A14: School Finance Reform and Establishments: Border Pairs Design Robustness Check

| VARIABLES | (1) LargeManfEst /1KPop | (2) LargeManfEst /1KPop | (3) LargeManfEst /1KPop | (4) LargeManfEst /1KPop | (5) LargeManfEst /1KPop | (6) LargeManfEst /1KPop |
|---|---|---|---|---|---|---|
| Post Yr 1-10 | -0.00205* | -0.00161 | -0.00198 | -0.00362** | -0.000863 | -0.00141 |
| | (0.00102) | (0.00105) | (0.00174) | (0.00136) | (0.00113) | (0.00108) |
| Post Yr 11-25 | -0.00560*** | -0.00457* | -0.00634** | -0.00727*** | -0.00465*** | -0.00543** |
| | (0.00165) | (0.00250) | (0.00233) | (0.00206) | (0.00159) | (0.00215) |
| Observations | 74,166 | 44,648 | 38,366 | 84,820 | 121,364 | 83,014 |
| $R^2$ | 0.876 | 0.862 | 0.869 | 0.858 | 0.837 | 0.849 |
| Sample | Base | Reform-Only | Pure DiD | Incl Outlier | Incl > \|24\|yr | Incl Outlier |
| Weight | All State = | All State = | All State = | All State = | All State = | N |
| Controls | Base+ Demo x Year | Base | Base | Base | Base | Base |
| Dep. Var Mean | 0.0198 | 0.0198 | 0.0198 | 0.0198 | 0.0198 | 0.0198 |

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on large manufacturing establishments as a share of overall establishments and employment using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether or not a reform has yet occurred in a state. Controls include border pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). All counties with fewer than 1,000 population in 1970 were excluded unless otherwise noted. In reform states only years greater than 25 years prior and less than 25 years following the reform were included unless otherwise noted. County-years with outcomes greater than the 99th percentile were excluded unless otherwise noted. Reform years only means only states with reforms were included. Pure DiD means that only border pairs in which one member of the pair never had a reform were included. Demographic controls include povety, urbanicity, populatoin and white share of the county population.

Table A15: School Finance Reform and Manufacturing Employment: Alternate Reform Identification Strategy

| VARIABLES | (1) ManfEmp/1KPop | (2) AllEmp/1KPop | (3) ManfEmp/AllEmp | (4) ManfBigEst/1KPop | (5) BigEst/1KPop | (6) ManfBigEst/TtlEst |
|---|---|---|---|---|---|---|
| Post Yrs 1-10 | -1.300 | -4.772** | -0.00329 | -0.00222** | -0.00117 | -0.0124 |
| | (1.157) | (2.182) | (0.00352) | (0.00101) | (0.00214) | (0.0113) |
| Post Yrs 11-25 | -4.482* | -9.780** | -0.0139* | -0.00570*** | -0.00628 | -0.0408** |
| | (2.571) | (4.618) | (0.00709) | (0.00181) | (0.00376) | (0.0180) |
| Observations | 67,790 | 66,664 | 66,602 | 89,188 | 89,188 | 26,846 |
| $R^2$ | 0.948 | 0.958 | 0.955 | 0.850 | 0.836 | 0.903 |
| Dep. Var Mean | 60.10 | 250.1 | 1.345 | 0.0205 | 0.0336 | 0.588 |

)

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on total manufacturing establishments and employment as a share of overall establishments and employment using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is a reform state and an indicators for years since the reform. Controls include border pair by year fixed effects and county fixed effects. County-years with outcomes greater than the 99th percentile were excluded as outliers. All counties with less than 5,000 population in 1970 were excluded. Standard errors are clustered at the state and border pair level. All employment and establishment data come from County Business Patterns (CBP). Reform events were the first event in each state identified by Jackson, Johnson, and Persico (2014).

# A ZTRAX Database

Home sales data come from Zillow ZTRAX and are merged to the assessment records by parcel ID. Sales are non-distressed sales with a deed type that does not reflect a transfer between family members, an inheritance, or another non-market transfer of property. These sample restrictions are designed to capture arm's length transactions. Foreclosures are transactions indicated by Zillow to be foreclosures, as well as tax deeds, foreclosure deeds, commissioner's deeds, redemption deeds, deeds in lieu of foreclosure, receiver's deeds, sheriff's deeds, beneficiary deeds, notices of sale, and notices of lease pendens. This is a liberal definition of foreclosure that includes the first notice of foreclosure.

For all sale types, we assume that a house will only transact once in a 93 day window.[36] We define a transaction event as beginning with the first time a parcel transacts. If another transaction is recorded within the next 93 days, that transaction is considered part of the initial transaction, and we check for another transaction within the following 93 days, until a 93 day period with no transaction activity passes. [37] The transaction date is coded as the date of the first event. The price is the maximum price observed over the transaction window.

Transaction and assessment data come from county governments. Because data are provided at the county level, the years in which counties enter our sample differ even within a state. We identify the starting year for each county in the following way. We cannot simply use the first year a county has a transaction in the data as the first case because many counties include a small minority of transactions ($< .1\%$ of housing units) for many years in the past. Accordingly, we use the following procedure to identify the years in which we observe the universe of transactions. We instead first identify all years in which a county had a greater than 300% increase in sales (off a minimum of 10 transaction base). This threshold is chosen because it is greater than any increase we would expect to observe in the course of normal annual fluctuations and therefore is likely driven by changes in reporting. Accordingly, we define a county's initial year as the most recent year in which there was a greater than 300% increase observed in our data (or the first year of a transaction if >300% increase never occurred.) We drop all transactions prior to our emprically-defined "start-date" from our analysis.

Finally, our home price analysis uses a border difference-in-differences design. Some border pairs overlap multiple counties. In these places to ensure sample consistency, we drop all

[36] Many transaction records only provide a month and year of sale. The 93 day window allows for any three month window regardless of month length.

[37] Many events have multiple transactions recorded in the ZTRAX database due to mortgage changes, adjustments, multiple foreclosure notices, etc.

transactions within the border-pair prior to the year in which the last county had data available.

# B Imputing Plant Value

To proxy for the effect of a plant opening on the local tax base, we use the estimated overnight construction cost of a power plant. Overnight construction cost is a term of art that reflects the estimated hypothetical cost of building a power plant overnight so as to abstract away from borrowing costs. To do this, we used annual estimates taken from the EIA's Annual Energy Outlook between 1997 and 2018. For years 1995 and 1996 we used the 1997 values. For combined-cycle gas turbines and combustion gas turbines, values for basic and advanced turbines were given. We averaged these two values for each year, but results are robust to picking one. All estimates were adjusted for inflation and are presented in $2014.

An important note is that estimates are presented for a power plant of a given size. We use the resulting cost per kwh for all power plants. If, as is likely, economies of scale exist then we are understating the costs of small plants and overstating the costs of large plants. This would bias our results toward zero and so to the extent that this affects our overall results they should be thought of as a lower-bound.

The left panel of Figure A3 shows the distribution of estimated plant construction costs, while the right panel shows the distribution of costs per student. In both cases the distribution is right-skewed; there is a very long right tail of expected impacts. To ensure results are not driven by behavior of extremely large impacts, we drop all impacts above 2.5 million dollars of construction costs per student (<2% of plants). Most districts have increases in expected tax base large enough to expect meaningful fiscal impact for local schools—the median opening has roughly $106,000 in estimated construction costs per student, while the mean opening has approximately $208,000 per student in estimated construction costs.[38] The bottom panel shows the relationship between district size in year of construction and estimated construction cost. We can see that among districts of a given size, there is wide variation in the types of plants they are exposed to. Similarly, among plants of a given expected cost, there is wide variation in the size of districts in which they are located. This identification strategy leverages exactly this variation; it implicitly compares effects of plant openings among similarly sized districts with different plant costs and plants with similar construction costs located in differently-sized districts.

[38] Most school district property taxes are between .3%-1%.

# C Identifying Reforms

In Section 4, we estimate how shocking a district's marginal value of tax base with respect to school spending affected local land use decisions. To do this, we identified school finance legislation, litigation and initiatives that affected this quantity within a given state between 1970 and 2015. To identify these reforms we used information from *Public School Finance Programs in the United States* 1962,1967, 1972, 1976, 1979, 1994, 1998, 2007, 2011, 2015, and 2018. Broadly speaking, changes to the marginal value of tax base are determined by the extent of crowd-out and the level of taxes a local district can charge. Accordingly, in each report year we attempted to quantify a state's school funding formula and tax limitations. We then looked for major changes in crowd-out or tax limits between report years and identified these changes as potential reforms. We then turned to the text of the report and online searches to identify the legislation, litigation or initiatives that led to these changes in order to ensure that such a change had indeed occurred and to identify the year in which the reform took place. If a reform took place in multiple years, we used the first reform-year only. Below, we summarize the year, reform type and changes of each reform used in our analysis.

Reforms are typically not amenable to simple summary statistics (except for example, where no crowd-out exists and so total crowd-out for all districts is 0). In the table below, we provide information on the level of crowd-out as a function of a district's property value ($P$). This is for an "average" district in a state, but should not be thought of as holding for all districts. For example, some districts with high property values may generate more money than state aid through local sources and so face no effective crowd-out when increasing the tax base. When necessary, we attempt to provide additional context. We also attempt to describe the tax limitations in place for each state. These should not be compared across states as assessment ratios (the ratio of assessed value to true market value) changed dramatically over time. Unfortunately, for earlier years we often lack information on the assessment ratio used and so cannot inflate these rates into a value common across all states.

Figure A8 shows the correlation between our qualitatively-identified reforms and the first major reform identified by Jackson, Johnson, and Persico (2014). Note that Jackson, Johnson, and Persico (2014) were searching for reforms that changed the distribution of school funding, not reforms that shocked the marginal value of tax base per se, and so therefore we would not necessarily expect to identify the same reforms. Each state in our reform sample also appeared in their sample and there is a fairly strong correlation. Roughly 3/5 of the reforms identified in our sample occurred within three years of reforms identified by Jackson, Johnson, and Persico (2014). There were a further ten states that had reforms as identified

by Jackson, Johnson, and Persico (2014), but for which we did not find a major change in their marginal value of tax base.

Figure A8: Correlation Between Qualitatively-Defined Reforms and Reforms Identified in Jackson, Johnson, and Persico (2014)

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| AL | 1995 | Response to litigation | .0025*(1938 Ttl State Property Value/Ttl State Property Value)*Property Value | .01*Property Value | 15 mills (exceed w/ vote) | 15 mills (exceed w/ vote) | Crowd-out |
| AZ | 1980 | Legislation | 0 | .0472*P | None | State-set expenditure limit | Crowd-out |
| AR | 1984 | Litigation | Equalization aid roughly 10% of state aid and distributed as function of AV/Teacher rank | .025*P | None | State-set expenditure limit | Crowd-out |
| CA | 1978 | Response to litigation+Prop 13 (property tax limitation) | .0387*P | .≈full crowd-out | No limit (w/ vote) | .01 (total taxes) | Crowd-out+Limitation |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| CO | 1974 | Legislation | min(.017,tax needed to raise $250/student). Tax needed to raise most common, effective crowd-out of 0 | Guaranteed Revenue Base of $29,620 (almost full crowd-out up to this limit—binds for most districts) | No limit (w/ vote) | No limit (w/vote) | Crowd-out |
| CT | 1990 | Legislation | Guaranteed Tax Base in which each district was guaranteed $\tau * P_85$, but b/c underfunded only received pro-rated share of available funding (which was often low) | $\tau * Inc_D/IncMax$ where $\tau*$ is the tax necessary to create the foundation amount at the state guaranteed wealth level (1.567 median wealth) | No limit | No limit | Crowd-out |
| DE | None | | | | | | |
| FL | None | | | | | | |
| GA | 1986 | Legislation | Tax rate necessary to raise 78,000,000 on all property in state (only 10% of state funding, so rate likely low) | .00825*P (if wealth <90th percentile), otherwise .005*P | | 20 (no limit w/ vote) | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| ID | 1979 | Initiative | .022*P | .0036*P | .027 (no limit with vote) | ..004 | Limitation |
| IL | 1973 | Legislation | 1.12*.0108*P | Guaranteed tax base of $42,000 (or more if elem or hs distr) for any tax rate (affects most >80% of districts) | No limit w/ vote | No limit w/ vote | Crowd-out |
| IN | 1973 | Legislation | .0215*P | .03*P | .047 | min(.03, tax to generate last year levy) | Crowd-out+Limitation |
| IA | 1972 | Legislation | 0 | 0054*P | No limit w/vote | 109% last years levy | Crowd-out+Limitation |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| KS | 1975 | Legislation | .005*County Valuation (apportioned by district share of county employees) | 017*P | No limit w/vote | 107% last years levy | Crowd-out |
| KY | 1991 | Litigation | 0 | .0036*P | No limit w/vote | Unclear | Unclear |
| Crowd-out | | | | | | | |
| LA | None | | | | | | |
| ME | None | | | | | | |
| MD | None | | | | | | |
| MA | 1993 | Legislation | Complicated, but >75% of districts (1979) in hold-harmless and therefore had effectively 0 crowd-out | .0094*P*$Inc_d$ but complicated so does not apply uniformly to all districts | .025 (all rates) | .025 (all rates | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| MI | 1993 | Initiative | $\tau$*P if value/student <$40K (up to first 30 mills) | 018*P | .05 (all taxes) | .021 | Crowd-out (Proportional) + Limitation |
| MN | 1971 | Litigation | .019*P (up to foundation leve, but foundation low—1/3 off formula) | .03*P (foundation level dramatically increased) | None | No limit w/ vote | Crowd-out + Limitation |
| MS | 1994 | Legislation | 0 | .028*P (up to 28% of program cost) | 10% increase | .055 | Crowd-out |
| MO | 1993 | Legislation | Complicated function in income, that leads to little crowd-out | $\tau$*P* Inc/AvgInc | No-limit w/ vote | No limit w/vote | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| MT | 1989 | Litigation | 0 | ($\tau$*)*P where $\tau * isthetaxratenecessarytoget40\%ofstatefundingon175\%xavg$ | No w/ vote | No w/ vote | Crowd-out |
| NE | 1990 | Litigation | Each districts share is .012*P and then receive pro-rated share from eq aid available (but typically aid quite low— i.e. 1979 $70/pupil on avg, so true crowd-out neglible ) | .0124*P (but bites, eq aid now 5x as large as 1979) | No w/ vote | Levy to collect 3%-5% > previous year | Crowd-Out + Limitation |
| NV | None | | . | | | | |
| NH | 1998 | Litigation | Funds 8% of edu cost adjusted by wealth (in reality funding much lower, so almost all districts face effective crowd-out of 0) | avg state tax rate*P for districts with <Avg Wealth (.0237 in 2007), 0 for >Avg Wealth | None | None | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| NJ | 1970 | Legislation | .0105*P | Guaranteed Tax Base of $30K, so full crowd-out if less and none if more | No-limit | No limit | Crowd-out |
| NM | 1974 | Legislation | .0005*P | .0089*P | .002 | Crowd-Out + Limitation | |
| NY | None | | | | | | |
| NC | None | | | | | | |
| ND | 2007 | Legislation | .035*P | Guaranteed tax base equal to 180 mills at 90% average wealth/stud and recapture above 150% avg wealth/stud | .185 | .185 | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| OH | None | | | | | | |
| OK | 1990 | .018*P (but many districts (≈50% held harmless) | Nearly full crowd-out for districts with <$55K/student | | | | |
| OR | 1991 | Initiative | $\tau^{**}$P where $\tau^*$ is tax rate chosen to distribute equalization dollars (typically quite low) | Nearly full crowd-out | No limit w/ vote | .005 (1991) | Crowd-out+Limitation |
| PA | None | | | | | | |
| RI | None | | | | | | |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| SC | 1977 | Legislation | None | $\tau$**P where $\tau^*$ is tax rate necessary to generate 30% of guaranteed funding | No | No | Crowd-out |
| SD | 1986 | .018*P | $\tau$**P where $\tau^*$ is avg. non-agri tax rate (came into effect in 1990) | .053 | .0186 | Crowd-out + Limitation | |
| TN | 1977 | Legislation | 0 (use index of econ. ability that does not include prop wealth) | $\tau$**P where $\tau^*$ is amount necessary to raise 10% of guaranteed funding in avg district | No limit | No limit | Crowd-out |
| TX | 1973 | Legislation | 0 (use index of econ. ability that does not include prop wealth) | .003*P | .015 | .015 | Crowd-out |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| UT | None | | | | | | |
| VT | 1997 | .01297*P (but many districts off formula) | Full crowd-out (began in 1997 and effectively full state funding in 2001) | no | NA | Crowd-out + Limitation | |
| VA | None | | | | | | |
| WA | 1974 | Legislation | .0119*P | All property tax rev sent to state w/ exception of small (25% of budget) local option levies | No limit w/ vote | .015 | Crowd-out + Limitation |

| State | Year | Type | Pre-Reform Crowd-out | Post-Reform Crowd-out | Pre-reform tax limit | Post reform tax limit | Shock Driver |
|---|---|---|---|---|---|---|---|
| WV | None | | | | | | |
| WI | 1997 | $\tau$**P where $\tau^*$ is tax producing equalization amt in district with 45k val/stud | $\tau$**P where $\tau^*$ is tax producing ttl cost in district with 98k val/stud | .025 | .025 | Crowd-out | |
| WY | 1983 | Litigation | .01*P | .025*P (recapture if above 109% foundation guarantee) | .028 | .028 | Crowd-out |