UNIVERSITY OF CALIFORNIA SAN DIEGO

Essays on the Provision of Local Public Goods

A dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Economics

by

Rebecca Libbin Cannon Fraenkel

Committee in charge:

Professor Julie Cullen, Chair
Professor Prashant Bharadwaj
Professor Jennifer Burney
Professor Joshua Graff Zivin
Professor Isaac Martin

2020

The dissertation of Rebecca Libbin Cannon Fraenkel is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

2020

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

Thank you to everyone who has supported and guided me in so many ways through this process. Thank you to my committee for your advice and patience, and to the members of the UCSD economics department—students and faculty—who have helped me in so many ways. A special thanks to Sam Krumholz who is a great coauthor and even better friend. Thank you to Paul Ippolito, Kristin Butcher, and Patrick McEwan for teaching me what economics is. Finally, thank you to my husband, Aaron, my parents, Chris and Anne, and my sister, Abby, for everything.

This material is based upon work supported by the National Science Foundation Graduate Research Fellowship under Grant No. DGE-1144086. Any opinion, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily reflect the views of the National Science Foundation.

For Chapter 1, I am grateful to seminar audiences at UCSD, as well as Kate Antonovics, Julian Betts, Julie Cullen, Catherine Weinberger, and two anonymous referees for helpful comments. I would also like to thank the NCES for providing the data for this chapter.

For Chapter 2, I would like to thank Prashant Bharadwaj, Jeffrey Clemens, Julie Cullen, Rodger Gordon, Michelle White, Josh Graff Zivin and the UCSD applied seminar participants for advice and feedback on this article. I would also like to thank the University of California, San Diego Social Sciences Computing Facility (SSCF) Team for their technical assistance and the use of the Social Sciences Research and Development Environment (SSRDE) cluster with special thanks to Todd Williams. Isaac Cotter provided excellent research assistance. Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

For Chapter 3, Sam acknowledges (and thanks) the Sloan Foundation for support during the writing of this article. We would also like to thank the University of California, San Diego Social Sciences Computing Facility (SSCF) Team for their technical assistance and the use of the Social Sciences Research and Development Environment (SSRDE) cluster. We would additionally like to thank Prashant Bharadwaj, Judd Boomhower, Julie Cullen, Gordon Dahl, Josh Graff Zivin, Karthik Muralidharan, members of the CU Environmental Workshop and UCSD applied micro lunch seminar participants for much useful feedback. We thank Tiffani Wu for excellent research assistance. All mistakes are our own.

Chapter 1, in full, is currently being prepared for submission for publication of the material. Fraenkel, Rebecca, ”Local Labor Markets and Job Match Quality: Teachers”. The dissertation author was the primary investigator and author of this material.

Chapter 2, in part, is currently being prepared for submission for publication of the material. Fraenkel, Rebecca. ”Property Tax-Induced Mobility and Redistribution: Evidence from Mass Reappraisals”. The dissertation author was the primary investigator and author of this material.

Chapter 3, in part, is currently being prepared for submission of publication of the material. Fraenkel, Rebecca; Krumholz, Samuel. ”Property Taxation as Compensation for Local Externalities: Evidence from Large Plants”. The dissertation author was a primary investigator and author of this material.

VITA

2011	B. A. in Economics *cum laude*, Wellesley College
2020	Ph. D. in Economics, University of California San Diego

ABSTRACT OF THE DISSERTATION

Essays on the Provision of Local Public Goods

by

Rebecca Libbin Cannon Fraenkel

Doctor of Philosophy in Economics

University of California San Diego, 2020

Professor Julie Cullen, Chair

Governments below the federal level provide many of the services with which citizens regularly interact. Education in the United States is primarily a local service and property taxes are a major source of education funding. This dissertation focuses on the local labor market for teachers and citizen responses to changes in education funding through property taxes. Chapter 1 studies how local labor market conditions affect who becomes a teacher. Chapter 2 investigates homeowner and community responses to changes in property taxes. Chapter 3 examines the role of property taxes in compensating homeowners for negative externalities generated by industrial facilities and how local control over tax revenue affects a community's willingness to accept externality-generating facilities.

Chapter 1

Local Labor Markets and Job Match Quality: Teachers

1.1 Introduction

How does the strength of the local labor market affect who chooses to teach? Does temporary labor market weakness make teaching more attractive? Teachers make up about 2.7% of the overall workforce (Provasnik and Dorfman, 2005) and are important to the development of their students' human capital, but schools have limited ability to adjust how and when they hire and recruit teachers. Unlike the private sector workforce, the size of the teacher labor force historically was unchanged by market conditions. I examine the effect of local labor market conditions on the academic talent, job satisfaction, demographics, and short-term retention of newly hired teachers.

Because teacher hiring does not vary with local labor market conditions in the period I study, ending with the 2007-08 school year, I can use realized hires to isolate the effect of the local labor market on changes in the supply of potential teachers. The Great Recession disrupted this pattern, and Evans et al. (2014), Leachman et al. (2016), and Jackson et al. (2018) all demonstrate associated cuts to school spending and teacher hiring. Prior to the Great Recession however, local labor market conditions provide plausibly exogenous variation in the quality and availability of jobs for potential teachers.

I show that teachers who start teaching during periods of higher local unemployment are more likely to have attended selective undergraduate institutions. College quality is a characteristic of teachers that hiring principals and private sector employers value, though evidence is more mixed on the implications for student achievement (Brewer et al., 1999, Boyd et al., 2013, Hinrichs, 2014, Jacob et al., 2016). This suggests that higher quality workers are drawn into teaching when they are presented with weaker outside options. Teachers hired during periods of higher local unemployment are also more likely to report that they are dissatisfied with their compensation, suggesting that they are aware that they would be able to earn more outside of teaching if the conditions in their local labor market were better. Despite their dissatisfaction, I find no evidence that teachers hired during weaker labor markets are more likely to leave teaching in the following year. I also find that local labor market conditions do not significantly alter the demographic, educational, or training characteristics of new teachers.

My findings complement those in a recent paper by Nagler et al. (2020) and reveal more about those drawn into teaching by weak labor markets. Nagler et al. (2020) find that Florida teachers who began their careers during national recessions have higher value added but are also more likely to exit the profession. I find, on a national level, that teachers drawn into teaching by weaker local labor markets come from more selective undergraduate institutions, a characteristic that that matters to both private and public sector employers (Black and Smith, 2006, Long, 2008). I also find that teachers drawn into teaching by weak labor markets report lower job satisfaction, a potential indicator of low job match quality for these teachers. I then look at short-run (primarily one year) retention of teachers and find no differential attrition for teachers hired when local labor markets are weaker.

The paper proceeds as follows: Section 1.2 reviews the relevant literature and describes key factors relevant to the occupational choice of would-be teachers. Section 1.3 discusses my empirical strategy and how I measure the strength of the local labor market. Section 1.4 describes the data and school hiring behavior. Section 2.7 reports results on the relationship between local labor markets and who chooses to teach. Section 2.8 concludes.

1.2 Teacher Labor Markets

As in Nagler et al. (2020), I consider a simplified Roy model of occupational choice where individuals choose to seek employment in either teaching or the private sector based on expected wages and job finding probabilities. In this model, which reflects the acyclic teacher labor market during the period I study, private sector wages and job finding probability vary with local labor market conditions while opportunities and wages in teaching are constant. Returns to worker quality exist only in the private sector.[1]

To test the model predictions, I test whether higher quality–as valued by the private sector–workers are drawn into teaching when the local labor market is weak. I measure worker quality using the selectivity of a teacher's undergraduate institution, which is a commonly used measure of quality, but is not strongly associated with teacher value added (Bacolod, 2007, Figlio, 2002, Hoxby and Leigh, 2004, Angrist and Guryan, 2005, Clotfelter et al., 2006, Harris and Sass, 2011). While those who attend higher quality colleges may not be better teachers, it is a characteristic that is valued by private sector employers, and to some extent schools (Brewer et al., 1999, Black and Smith, 2004, 2006, Hoekstra, 2009).[2]

[1]This model abstracts from differences in career salary structure and job security between teaching and the private sector. For example, Lang and Palacios (2018) find that teachers are more risk-averse on average. When finding a job is more difficult and the fluctuations in compensation that occur in the private sector are more apparent, the increase in perceived riskiness of the private sector may also push workers into teaching.

[2] Jacob et al. (2016) find that college quality is positively correlated with performance on in-person teacher evaluations, suggesting either that college quality is related to teacher skills not captured in value-added, or confirming that evaluators share the preferences of hiring principals seen in Boyd et al. (2011), Boyd et al. (2013), and Hinrichs (2014). Schools show a preference for hiring teachers who come from more selective colleges (Jacob et al., 2016, Boyd et al., 2011, 2013, Reback, 2006, Hinrichs, 2014). In contrast, Ballou (1996) finds applicants who went to selective colleges are no more likely to be hired.

Prior literature has shown that long run shifts in the availability of alternative and teaching job opportunities matter for who becomes a teacher. Several studies find that increasing labor market opportunities for women, historically the population of potential teachers, resulted in fewer highly skilled women choosing to enter teaching (Hoxby and Leigh, 2004, Bacolod, 2007, Corcoran et al., 2004a,b). Parallel to this, Murnane and Phillips (1981) describe 'vintage effects', where long run increases in demand for teachers due to large cohorts of children lead to lower quality hires.[3] Focusing on the role of wages, Loeb and Page (2000) show that student outcomes improve when teacher wages are higher relative to their outside options, suggesting that higher relative wages attract better teachers, and Britton and Propper (2016) find that school performance is negatively affected when teachers are paid below market rates.

In the short run, demand for teachers and teacher wages are relatively constant. In this analysis, I study how short-run changes in a potential teacher's outside option, measured with state-level unemployment rates, affect who chooses to teach. Consistent with set teacher salary schedules, I treat teacher wages as fixed (Podgursky, 2011, Dolton, 2006). Unlike teacher wages, private sector wages do change with the unemployment rate. Beaudry and DiNardo (1991), Genda et al. (2010), Kahn (2010), Altonji et al. (2016) and Oreopoulos et al. (2012) find that wages are lower for those who enter the labor market during a recession. Kahn (2010) is specific to college graduates, which almost all teachers are. Genda et al. (2010) and Oreopoulos et al. (2012) find that these wage effects are stronger for those who have only completed high school but also exist for college graduates. Similarly, the size of the teacher labor force is determined primarily by cohort sizes (Loeb and Béteille, 2008), and I show that teacher hiring is not determined by local labor market conditions. In the private sector, the probability of finding a job decreases as the unemployment rate increases (Shimer, 2008).[4] As shown in Figures 1.1 and 1.2, acyclic teacher

[3]Looking at a short-run shock to demand, Jepsen and Rivkin (2009) find that a sudden increase in hiring from a legally mandated reduction in class sizes in California led to the hiring of lower quality teachers.

[4]Supply constraints in the market for teachers are unlikely to have a large effect on whether openings are filled. On a national level, the supply of individuals completing teacher training programs is much larger than the number of new teacher openings reported by schools (Cowan et al., 2016).

supply is a feature of the period I study, but has not continued to today. I end my study period with the 2007-2008 school year because the Great Recession led to unprecedented cyclic changes to demand for teachers (Jackson et al., 2018, Leachman et al., 2016, Evans et al., 2014, Knight, 2017, Goldhaber and Theobald, 2013).[5]

With fixed wages and acyclic demand for teachers, the model predicts that short run increases in local unemployment rates should lead to higher quality workers being hired as teachers. Previous literature has suggested that a stronger labor market can pull the most qualified workers away from teaching and reduce teacher supply. Falch et al. (2009) find that teacher shortages are strongly procyclical with the local unemployment rate in Norway, resulting in schools hiring more uncertified teachers. Similarly, a weak labor market can push the more highly qualified into teaching. A more recent paper shows that Florida teachers hired during national recessions have higher value-added (Nagler et al., 2020). Because public sector hiring in general is less sensitive to cyclic labor market fluctuations, prior work has suggested that the quality of public sector job applicants and new hires increases in recessions (Zhang and de Figueiredo, 2018, Munnell et al., 2013, Borjas, 2002, Krueger, 1988).

Previous literature has also suggested that weak labor markets lead to worse job matches. Consistent with this, I examine whether teachers hired in weak labor markets are less well matched with their jobs as measured by self-reported job satisfaction. Bowlus (1995) and Kahn (2008) have show that job durations are shorter for job matches initiated during recessions. They argue that these shorter employment spells are the result of bad matches.[6] Kahn and McEntarfer (2014) find low upward mobility during recessions and that the distribution of hiring is shifted toward lower quality firms, implying a given worker ends up in a lower quality job on average. If teachers pulled in by weak labor markets are poorly matched with their jobs, Jackson (2013) suggests that this could lead to worse outcomes for students, as he finds that teachers who are

[5] I test indirectly for this effect in Appendix 1.B.

[6] Mustre-del Rio (2016) also looks at the effect of economic conditions on job duration but argues that duration is not a good proxy for match quality.

better matched with their schools have greater value-added. Consistent with this, Nagler et al. (2020) find that Florida teachers hired during national recessions are more likely to leave teaching. Retention is another important consideration in evaluating teacher quality because teachers are least effective early in their careers and improve over the first or first several years (Harris and Sass, 2011, Hanushek et al., 2005). Borjas (2002) finds that wage compression in the public sector makes it difficult to attract and retain high quality workers, further suggesting higher quality workers may leave when the economy improves.

1.3 Empirical Strategy

I seek to identify how short-run changes in outside options available to a potential teacher affect who enters teaching, how satisfied they are with their jobs, and their retention. I consider outcomes for quality of a teacher's undergraduate institution, teacher compensation and job satisfaction, teacher retention, and observable characteristics of teachers to examine how the teacher labor force changes with local labor market conditions. The key explanatory variable is state level unemployment U_{st} when a teacher enters teaching, and measures the strength of the outside option available to a potential teacher.

$$Q_{ijst} = \beta_0 + \beta_1 U_{st} + \gamma \mathbf{X}_j + S_s + T_t + \varepsilon_{ijst}$$

I consider unemployment in t and $t-1$ to allow for a longer decision making period given teacher training requirements. Q_{ijst} is the relevant characteristic of teacher i in school j in state s who was hired in year t. $\mathbf{X}_j$ are characteristics of a teaching job that do not vary with the unemployment rate. I use percent minority enrollment, urbanicity of a school, percent of students participating in the National School Lunch Program and fixed effects for grades taught by the teacher and offered at the school. I additionally include state by three level urbanicity fixed effects to account for the unique education environment of each state and year fixed effects to account for secular changes over time. How these variables are constructed is described in more detail in Section 1.4 and in Appendix 1.A. Standard errors are clustered at the state-year level.[7] Observations are weighted using teacher weights provided in the SASS. For the main specifications, I also run the model excluding school-level control variables to account for any unobserved correlation of the controls with the unemployment rate. I test for these correlations in Section 1.4 to confirm that they are valid controls.

I also examine whether conditions at entry affect a teacher's likelihood of remaining in the profession. The majority of this analysis only considers first-year teachers because teachers drawn into teaching by weak labor markets may be more likely to leave teaching when the labor market improves. As discussed further below, a subset of teachers are surveyed again one year later on whether or not they are still teaching. I can measure retention only in the limited sense of whether a teacher teaches in $t+1$ following survey year t. Because of this, retention for teachers past their first year is subject to sample selection. I first examine whether teachers are more likely to leave after their first year teaching. To look at a larger group of teachers, I then analyze leaving behavior among teachers pooled across the first five and ten years of experience. In the pooled regressions, I control for years of experience.

1.3.1 U_{st} and Potential Outside Options

U_{st} represents the strength of the labor market in which a teacher may search for a job if she does not become a teacher. I use the state-level unemployment rate for May of the year (one year before) a teacher began teaching. This means for a teacher who started teaching in August 2003, all analyses are done using the state-level unemployment rate from the BLS for May 2003 and May 2002. The unemployment rates in t and $t-1$ are very highly correlated, with a correlation coefficient of .93.

[7] To account for possible serial correlation within states over time, I also give results for the quality and job satisfaction regressions with standard errors clustered at the state level in the appendix.

Figure 1.3 shows why the May unemployment rate is particularly relevant to those considering entering teaching. It shows posted teaching job openings from the BLS *Job Openings and Labor Turnover Survey* for 2001-2010. There are many more teaching job openings listed in the late spring and summer, and in most years the jump in job openings occurs in May. May also coincides with the timing of college graduation, a relevant point of labor market entry for much of the potential population of teachers. The median first-year teacher in the sample is 1 year out of college.

I use the state level official unemployment rate, U-3, which is unemployment rate for all workers. This is primarily for data reasons, as the BLS generates a consistent monthly data by state for this measure of unemployment. While the college educated unemployment rate may also be relevant for potential teachers, it is not provided by the BLS at the state level. In the Appendix, I approximate the college educated unemployment rate at the state level from the CPS, but it yields a noisy measure of unemployment, especially in small states. Additionally, while a college degree is typically a requirement for teaching and 99% of the teachers in this sample have college degrees, their outside option may not be a job that requires a college degree. Abel et al. (2014) shows that many new college graduates work in jobs that do not require a college degree and, as shown in Figure 1.4, tend to have higher unemployment than the college educated overall.

I use the state level unemployment rate rather than a higher or lower level of geography because it best captures the labor market a potential teacher faces.[8] States have discretion in setting requirements for what it takes to become a teacher and an individual who can teach in one state may not be eligible to teach in another. Teachers in general are not a mobile population of workers. Reininger (2012) and Boyd et al. (2005) show that teachers have especially strong preferences for working in communities close to home. Schools also have a preference for hiring in-state teachers (Hinrichs, 2014). Consistent with this finding, 77% of the teachers in my sample

[8] In appendix table 1.D.4, I test the county unemployment rate. Because I do not observe the same counties in every year of the survey, I cannot control for county and this specification captures both temporal fluctuations in the unemployment rate and counties with persistently higher levels of unemployment. These counties are likely to attract a different type of worker in all years.

are teaching in the state in which they attended college.

1.4 Data and Sample

1.4.1 NCES-SASS

Teacher and school characteristics come from the *National Center for Education Statistics School and Staffing Survey* (NCES-SASS). I use the 1987-88, 1993-94, 1999-00, 2003-2004, and 2007-08 waves of the SASS.[9] The 1990 wave of the SASS is excluded because it does not identify where a teacher went to college. Retention data come from the Teacher Follow-Up Survey (TFS). In the TFS, the NCES follows up with a subset of teachers one year later to determine whether they are still teaching.

As discussed in Section 1.3, I use school and community characteristics as controls to account for the ways these characteristics influence hiring independent of fluctuations in the state unemployment rate. Summary statistics for these characteristics from the full sample of schools are shown in Table 1.1. Across the included waves of the survey, the SASS surveyed approximately 37,610 schools, approximately 7,610 of which had a first-year teacher surveyed. The SASS is designed to provide a nationally representative picture of school characteristics. Larger schools with more teachers are more likely to be included in the sample. Teachers are then sampled from within a school at a rate that makes selection probability approximately constant within strata. Thus, there are schools in the sample who hired a first-year teacher that we do not observe. Column 2 in Table 1.1 gives school summary statistics for the included schools. Unsurprisingly, larger schools are more likely to have a new teacher.

[9] Complete information on the design of the SASS and accompanying Teacher Follow-Up Survey (TFS) can be found at https://nces.ed.gov/surveys/sass/methods.asp

1.4.2 Sample Selection

I restrict the sample of teachers to full time first-year teachers over 18 at public schools and I end my analysis with the 2007 SASS wave. In additional specifications looking at retention, I consider teachers with 5 or fewer and 10 or fewer years of experience. I calculate experience as survey year minus year a teacher began teaching, so any missed years of teaching are not captured in this variable. The retention sample for teachers beyond their first year only includes those teachers who remained in the profession up to that point. I also present results on a restricted sample of teachers to those who are within 3 years of their college graduation because those with less labor market experience may be more likely to be drawn into teaching by labor market conditions and may be more able to adjust their training.

Table 1.2 gives summary statistics for teachers in the full sample, the sample of recent college graduates, and the teachers who appear in the Teacher Follow-Up Survey (TFS). First-year teachers are mostly female, have an average age of 30, and are 3.8 years out of college. Almost all (99%) have college degrees, but only 14% start teaching with a master's degree. The teachers in the TFS are very similar, but the sample of new grads is younger, closer to their college graduation, and less likely to have a master's degree. The experienced TFS sample is older, more educated, and has stronger certifications.

Figures 1.1, 1.2, and 1.3 show why the 2011 SASS wave is excluded from the sample. Figure 1.1 also shows that the Great Recession was a break from the historical independence of teacher demand and labor market conditions. Figure 1.2 shows the persistent effect of the 2007 recession on the size of the teacher labor force in contrast with the three prior recessions and Figure 1.3 shows that fewer teaching jobs were listed in the the Great Recession. The literature describes the school funding and staffing changes in the great recession as "unprecedented" (Knight, 2017) and "the first significant teacher layoffs in recent times" (Goldhaber and Theobald, 2013). These layoffs led to increased teacher churn (Goldhaber et al., 2016) and high poverty schools were disproportionately affected (Knight, 2017). In Appendix 1.B, I show empirically that realized teacher employment, hiring, and selection into the sample does not change with the local unemployment rate during the study period.

I demonstrate the validity of the control variables within the sample period by showing that which schools are able to hire does not change with the unemployment rate. Table 1.3 examines whether there are compositional changes in which schools are hiring along the dimension of the control variables. It shows that, within the sample period, there was no effect on hiring of the interaction between minority enrollment and the state unemployment rate or the interaction of National School Lunch Program Participation rate and the unemployment rate. These control variables also do not vary with the unemployment rate, as shown in Appendix 1.B.[10]

1.4.3 Outcomes

I consider outcomes on a teacher's college selectivity, job satisfaction and match quality, retention, and demographic and training characteristics. Table 1.2 gives summary statistics on the outcomes considered. The main outcome is worker quality as measured by the selectivity of where the teacher attended college. The selectivity rankings come from the 2015 Carnegie classification of undergraduate institutions.[11] College selectivity is observed for 96% of the sample. In alternate specifications, I impute colleges for which selectivity is not observed as not selective. This is consistent with the approach used by Angrist and Guryan (2005) and Clotfelter et al. (2006).

Teacher match quality is measured by responses to survey questions. The two survey questions considered are "I am satisfied with my teaching salary" and "If I could get a higher paying job I'd leave teaching as soon as possible".[12] Both of these measures of satisfaction

[10] Consistent with the findings in Knight (2017), Appendix Table 1.C.5 shows that when the 2011 wave of the SASS is included we do see school characteristics affecting who is able to hire. When 2011 is included, schools with higher minority enrollment are overall more likely to have first year teachers, but among high minority schools, those experiencing high unemployment are less likely to hire a first year teacher. This may reflect the budget and staffing cuts that disproportionately affected higher minority schools during the Great Recession.

[11] I use the 2015 classification because it is the first year for which complete data were available. Schools are coded as selective if their selectivity score is 12 or higher. This corresponds to schools coded as "Four-year, full-time, selective, lower transfer-in".

[12] Available responses are Strongly Agree, Somewhat Agree, Somewhat Disagree, and Strongly Disagree. Some-

relate to teacher compensation, so I also test for whether teacher compensation, as measured by self-reported academic year base teaching salary in thousands of 2016 dollars, is related to local labor market conditions.

In addition to measures of worker quality and job satisfaction, I consider how the composition of the population of newly hired teachers changes with local economic conditions. I examine their academic training and their method of entry into the profession. Most teacher training programs have not been shown to be related to value added, but college major is related to job finding in the labor market as a whole (Harris and Sass (2011) and Abel et al. (2014)). I look at whether a new teacher was an education major or a STEM major. I also consider whether a teacher is certified, what type of certificate she has, and if she entered teaching via an alternative certification program. A full description of how these variables are coded is available in Appendix 1.A.

1.5 Results

I first present results on college selectivity for newly hired teachers. The top portion of Table 1.4 shows the preferred specification with controls included. Newly hired teachers are about 2.5 percentage points more likely to have attended a selective college or university for each 1 point increase in the local unemployment rate and a one standard deviation increase in the unemployment rate increases the likelihood that a newly hired teacher came from a selective college by about 6 percent. Results are similar whether the unemployment rate in the year of hire (Panel A) or the unemployment rate one year before (Panel B) is used. Columns 2 and 3 confirm that these results are not driven by selection into whether I observe college quality. In Column 2, results are similar when teachers for whom quality is not observed are coded as not having attended a selective college. Column 3 shows that the unemployment rate does not predict

what Disagree and Strongly Disagree are coded as dissatisfaction with salary in an indicator variable. Strongly Agree and Somewhat Agree are coded as willing to leave for salary in an indicator variable.

whether college selectivity is observed.[13] Results in the bottom portion of the table show the same analyses without control variables included. The results are very similar, though statistically insignificantly smaller than, those with controls. The effects are similar when the sample is restricted to teachers within 3 years of college graduation (Table 1.5). These results on college selectivity are consistent with prior literature on who enters the public sector during recessions. Teachers who enter teaching during weak labor markets appear to have attended more selective undergraduate institutions. While we might expect higher quality workers to make better teachers, characteristic and education-based measures of quality are only weakly related to value-added (Hanushek and Rivkin, 2006). The results here provide suggestive evidence that the high value added teachers induced to teach in Nagler et al. (2020) also have characteristics that the private sector would value.

Table 1.6 shows results for job match quality.[14] Teachers hired under higher unemployment rates are 1.6 to 2.2 percentage points more likely to disagree with the statement "I am satisfied with my teaching salary" (Column 1).This result is dependent on the inclusion of controls for unemployment in t, but results are consistent for unemployment in $t-1$. This effect grows to 3.5 to 4.1 percentage points when I restrict the sample to recent college graduates (Table 1.7), despite average dissatisfaction that is almost identical at 44%. Though the local unemployment rate does not significantly predict teacher compensation (Column 5), Column 2 includes compensation as an explanatory variable to ensure this finding is not driven by changes in teacher pay. The effect sizes are similar, though the unemployment rate in the year a teacher begins teaching is only significant at the ten percent level with compensation included. A one point increase in the unemployment rate has a similar effect on the likelihood that a teacher is dissatisfied as a \$1000 (2016 \$) decrease in compensation. The estimates on the effect the unemployment rate on compensation (Column 3) are not statistically significant and would suggest an approximately

[13] Table 1.D.1 presents the same specifications with standard errors clustered at the state level. The standard errors are slightly larger but the main results remain significant at the 5% level.

[14] Table 1.D.2 gives results with standard errors clustered at the state level. Results remain significant at the 5% level.

$160 decrease in compensation for a one point increase in the unemployment rate. This suggests that the effect on satisfaction occurs through a channel other than decreased pay. Teachers are about 4% more likely to report being dissatisfied with their compensation for a one point increase in the unemployment rate. This suggests that teachers drawn in during weaker labor markets are more likely to feel as though they are in a bad or underpaid job match. The results on whether a teacher agrees with "If I could get a higher paying job I'd leave teaching as soon as possible" are not precisely estimated (Leave for Salary, Column 2). This question is only asked in two waves of the survey and replies are less balanced (16% agree), making it harder to detect an effect, but it may also suggest that these teachers do not have plans to act on their dissatisfaction. Results looking at teacher retention confirm this, as I find no effect of the unemployment rate at time of hire on teacher retention. While weak labor markets draw more highly qualified, and higher value added (Nagler et al., 2020) teachers into teaching, this benefit to students comes at the cost of teacher job match quality.

Teachers hired when local labor market conditions are weaker are more likely to have attended a selective college and less likely to be satisfied with their salary, but they are similar in other observable ways. As shown in Table 1.8, their demographic characteristics are not detectably different, including time since college graduation and likelihood they are teaching in the state in which they attended college. This makes it less likely that those becoming teachers in weaker labor market conditions are choosing to do so after substantially longer and more geographically varied periods of job searching. They also enter teaching through similar certification channels and are equally likely to have studied education and STEM fields as undergraduates (Table 1.8). I do find a marginally significant effect on STEM major in one specification but this may be the result of multiple comparisons. This suggests teachers drawn into teaching during periods of lower local labor demand are not individuals who chose to attend an alternative certification program due to job finding difficulties.

1.5.1 Retention

I find no overall effects of the local unemployment rate on the likelihood that a teacher continues teaching. Table 1.10 gives results on whether teachers continue to teach. Because teachers are only followed for one year after they are first interviewed, only the results for first year teachers are not subject to bias due to sample attrition.[15] There is no significant effect for first-year teachers or when looking at teachers in the first 5 and 10 years of their careers. Because new teachers are less effective, this result suggests that the lower quality job matches described above are not harming students through teacher turnover.

I break down retention effects further by method of entry into teaching and type of certification. Some methods of entry into teaching, including alternative and temporary certifications, allow workers to become teachers with lower training costs relative to the time it takes to earn a standard teaching certificate. Because a teacher would know when seeking this training that the training costs can be recouped more quickly, we may expect those who entered via lower cost routes to be more likely to leave or view teaching as a temporary job. Tables 1.10 provides suggestive evidence that who entered through an alternative certification program are somewhat *more* likely to remain in teaching if they started their teaching career in a weaker local labor market, but the sample of teachers available here is too small to draw strong conclusions. I explore patterns of retention by certification and years of experience in more detail in Appendix 1.E. Overall, I do not find any strong effects on retention, but I am only able to look at short-term effects.

[15] Table 1.E.2 shows results for each year of experience separately to address attrition concerns. Teachers who start teaching during weaker labor markets may be more likely to leave after their 5th year, but this result could be due to multiple inference. Teachers hired during weaker labor markets may also be less willing to pay training costs for continuing certification. In 2006, 43 states required additional training in five year intervals (Loeb and Miller, 2006).

1.6 Conclusion

I find that the strength of a teacher's outside option affects who selects into teaching on a national level. This results in more academically qualified workers becoming teachers, consistent with the findings of higher quality teachers in Nagler et al. (2020) for Florida. This may come at a private cost to the teachers, as they report higher levels of dissatisfaction with their compensation. However, we cannot directly observe the teacher's outside option or previous labor market experience so it is unclear if their dissatisfaction represents true lost earnings.

Despite their dissatisfaction, I do not observe that these teachers are not more likely to leave teaching in the first year in which they are observed teaching. Weaker local economies then represent an opportunity for schools to hire academically talented, and possibly higher quality, teachers. Unfortunately, schools were not able to take full advantage of this in the most recent large economic downturn. As shown in Jackson et al. (2018), schools adjusted their hiring and spending and experienced declines in student performance. Consistent with prior work on public sector workers, state and local governments and schools may benefit from continuing to hire during downturns, but job satisfaction for these workers may be an issue.

Chapter 1, in full, is currently being prepared for submission for publication of the material. Fraenkel, Rebecca, ”Local Labor Markets and Job Match Quality: Teachers”. The dissertation author was the primary investigator and author of this material.

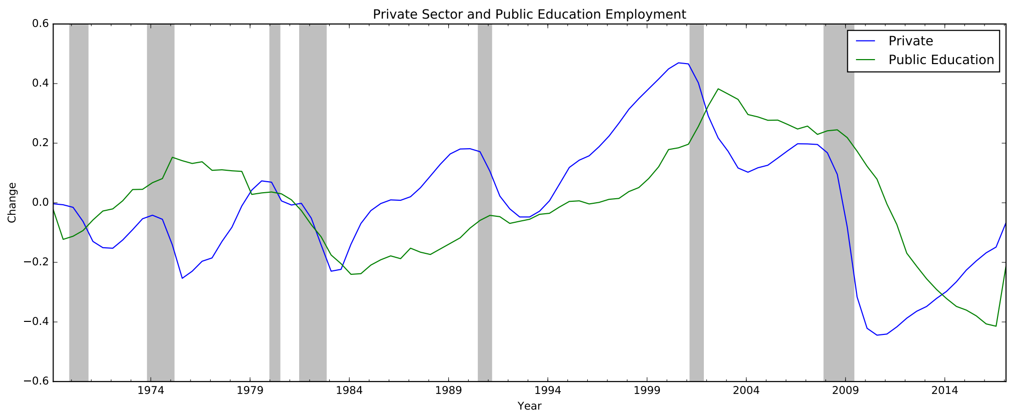


Figure 1.1: Private Sector vs. Public Education Employment

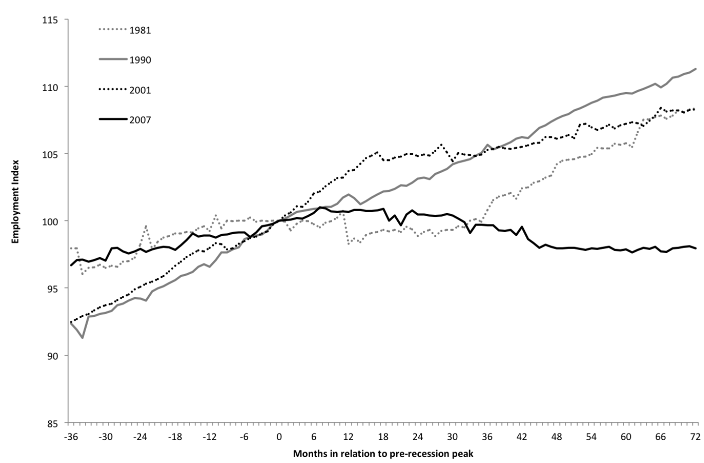


Figure 1.2: Public Education Employment Index over Last Four Recessions

BLS Current Employment Statistics (100=Start of Recession). Adapted from Evans et al. (2014).

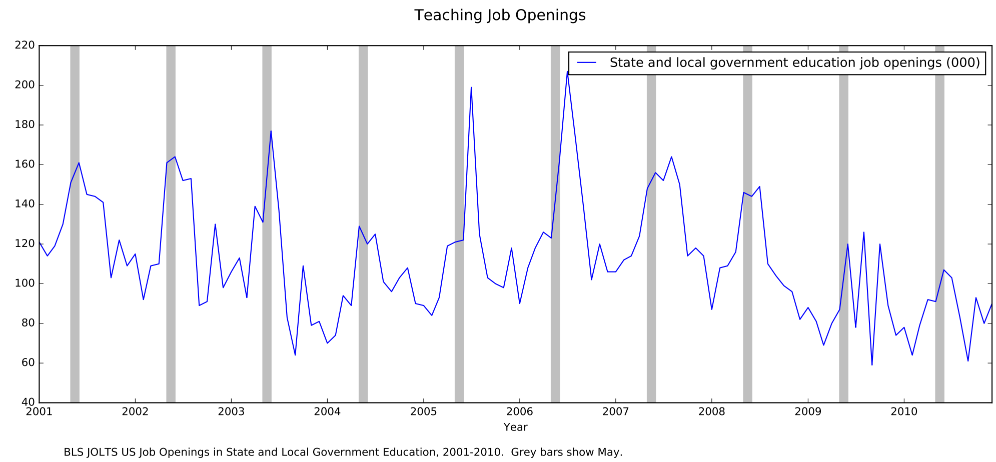


Figure shows posted teacher job openings, most of which occur in the summer. 2001 is the first year of data availability for the survey.

Figure 1.3: Teacher Job Openings

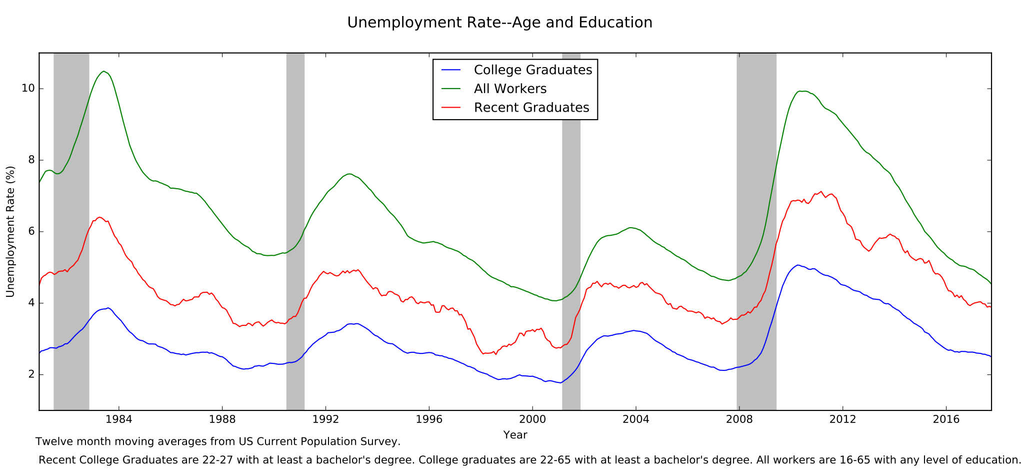


Figure 1.4: Unemployment for Recent College Graduates

Table 1.1: School Summary Statistics

	All Schools	Schools with a New Teacher
N 1st Yr Teachers	0.207	1.138
	(0.47)	(0.41)
Number of Teachers	33.436	39.097
	(24.38)	(28.84)
Enrollment	530.684	630.128
	(418.69)	(505.09)
Unemployment May t-1	5.913	6.004
	(1.79)	(1.78)
Unemployment May t	5.621	5.674
	(1.64)	(1.65)
Pct. Minority Enrollment	32.309	37.189
	(32.84)	(34.51)
Pct NSLP	40.750	42.983
	(29.00)	(29.72)
Participate in NSLP	0.960	0.966
	(0.20)	(0.18)
Observations	37600	7610

Schools that do not participate in the National School Lunch Program are coded as 0% NSLP in the later regressions. An indicator variable for participation is also included. In the 2003 and 2007 waves of the SASS, percent participation in NSLP is reported directly. In the 1987-1999 waves of the SASS, I calculate percent participation in NSLP as number of participants divided by total enrollment. All schools are included in tests for sample selection. Only teachers from schools with a new teacher are considered in the teacher hiring analysis. All counts are rounded to the nearest 10 for data privacy.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) “Public School Data File” and “Public School District Data File” 1987–2012

Table 1.2: Teacher Summary Statistics

	Full Sample	New Grads	TFS Sample	TFS 10 Year
Quality Measures				
CC Selective	0.647	0.637	0.643	0.663
Selectivity Observed	0.958	0.973	0.962	0.954
Satisfaction and Compensation				
Leave For Salary	0.161	0.147	0.156	0.219
Dissat w/ Salary	0.452	0.440	0.430	0.572
Compensation $(000) 2016	41.06	40.01	41.43	49.22
	(7.23)	(6.57)	(7.04)	(12.52)
Certification and Education				
Teaching in College State	0.775	0.832	0.766	0.752
Certified	0.946	0.954	0.954	0.980
Temp Cert	0.255	0.220	0.252	0.114
Alternative Cert	0.204	0.151	0.195	0.166
Full Cert	0.532	0.562	0.540	0.793
Probationary	0.173	0.178	0.173	0.079
BA	0.987	1.000	0.982	0.991
MA	0.141	0.053	0.112	0.307
STEM Major	0.133	0.114	0.127	0.116
Ed Major	0.519	0.620	0.543	0.575
Teacher Characteristics				
Male	0.235	0.224	0.209	0.230
Age	30.34	27.50	30.17	32.74
	(8.27)	(6.31)	(8.54)	(8.35)
Yrs BA to Teaching	3.80	0.64	3.81	3.40
	(6.53)	(0.74)	(6.80)	(6.07)
Year Began	1999.13	1999.35	2001.24	1996.76
	(6.76)	(6.63)	(6.80)	(7.20)
Local Unemployment rates				
Unemployment May t	0.054	0.054	0.052	0.056
	(0.015)	(0.015)	(0.015)	(0.016)
Unemployment May t-1	0.057	0.056	0.055	0.057
	(0.016)	(0.016)	(0.015)	(0.016)
BA Unemp t-1	0.022	0.022	0.022	0.023
	(0.010)	(0.010)	(0.009)	(0.009)
County Unemp. May t-1	0.054	0.054	0.052	0.053
	(0.025)	(0.025)	(0.022)	(0.025)
Observations	7760	5070	2800	9000

All counts are rounded to the nearest 10 for data privacy.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Teacher Data File,” “Public School Data File,” and “District Data File” 1987–2008

Table 1.3: School Characteristics and Teacher Hiring

	(1) 1st Yr Teacher	(2) 1st Yr Teacher	(3) N 1st Yr Teachers	(4) N 1st Yr Teachers	(5) Log # of Teachers	(6) Log # of Teachers
Panel A						
Unemployment May t	0.00248	0.000875	0.00197	−0.00191	0.00341	0.0544
	(0.00362)	(0.00690)	(0.00485)	(0.00656)	(0.00849)	(0.0369)
Pct. Minority Enrollment	0.00147***		0.00165**		0.00166*	
	(0.000491)		(0.000745)		(0.000961)	
Unemployment May t × Pct. Minority Enrollment	−0.000128		−0.0000683		0.0000145	
	(0.0000842)		(0.000129)		(0.000152)	
Pct NSLP		0.0000941		−0.000182		−0.00448***
		(0.000460)		(0.000736)		(0.00109)
Unemployment May t × Pct NSLP		0.0000269		0.000135		0.000172
		(0.0000839)		(0.000140)		(0.000182)
Panel B						
Unemployment May t-1	0.00613	0.00199	0.00598	−0.000936	−0.00396	0.0611
	(0.00390)	(0.00639)	(0.00516)	(0.00630)	(0.00795)	(0.0379)
Pct. Minority Enrollment	0.00143***		0.00163**		0.000852	
	(0.000458)		(0.000691)		(0.000870)	
Unemployment May t-1 × Pct. Minority Enrollment	−0.000116		−0.0000631		0.000148	
	(0.0000764)		(0.000120)		(0.000133)	
Pct NSLP		−0.0000337		−0.000293		−0.00492***
		(0.000443)		(0.000681)		(0.00101)
Unemployment May t-1 × Pct NSLP		0.0000466		0.000147		0.000236
		(0.0000759)		(0.000125)		(0.000157)
Rounded Observations	37600	37600	37600	37600	37600	37600
Number of Clusters	255	255	255	255	255	255
Dependent Variable Mean	0.182	0.182	0.207	0.207	3.259	3.259

Each panel represents a separate regression. Fixed effects for state and year. Standard errors clustered at the state-year level. Regressions test whether the unemployment rate interacts with the control variables to affect teacher hiring and employment in the sample. Observations weighted by school weights. Table 1.C.5 shows these results with the 2011 SASS wave included.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.4: Local Unemployment Rate and Worker Quality

	(1) CC Selective	(2) Selective (Imputed)	(3) Selectivity Observed
With Controls			
Panel A			
Unemployment May t	2.690***	2.419**	−0.0284
	(0.984)	(0.996)	(0.305)
Panel B			
Unemployment May t-1	2.905***	2.694***	0.0489
	(0.851)	(0.861)	(0.324)
Without Controls			
Panel A			
Unemployment May t	2.007**	1.937**	0.0734
	(0.922)	(0.919)	(0.287)
Panel B			
Unemployment May t-1	2.454***	2.427***	0.101
	(0.796)	(0.800)	(0.312)
Rounded Observations	7440	7760	7760
Number of Clusters	301	302	302
Dep Var Mean	0.646	0.619	0.958

Each cell represents a separate regression. Controls are for percent minority enrollment and free lunch program participation. Regressions with controls include fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Regressions without controls include fixed effects for state and year. Standard errors clustered at the state-year level. Regressions without controls have up to 130 fewer observations where controls are not observed. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. Regressions test the effect of the state unemployment rate on the quality of newly hired teachers. See Table 1.3 for how the controls interact with the unemployment rate. Table 1.C.6 shows these results with the 2011 SASS wave included.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.5: Local Unemployment Rate and Worker Quality—Recent College Graduates

	(1) CC Selective	(2) Selective (Imputed)	(3) Selectivity Observed
Panel A			
Unemployment May t	2.601**	2.201**	−0.311
	(1.048)	(1.069)	(0.399)
Panel B			
Unemployment May t-1	3.373***	3.063***	−0.0961
	(0.970)	(0.962)	(0.390)
Rounded Observations	4860	4980	4980
Number of Clusters	277	277	277
Dep Var Mean	0.640	0.623	0.973

Sample restricted to teachers within 3 years of college graduation. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.6: Local Unemployment Rate and Compensation

	(1) Dissat w/ Salary	(2) Dissat w/ Salary	(3) Leave For Salary	(4) Leave For Salary	(5) Compensation $(000) 2016
With Controls					
Panel A					
Unemployment May t	1.780**	1.598*	2.932**	2.835**	−16.18
	(0.903)	(0.819)	(1.185)	(1.244)	(14.46)
Compensation $(000) 2016		−0.0115***		−0.000952	
		(0.00147)		(0.00156)	
Panel B					
Unemployment May t-1	2.239**	2.061**	0.532	0.435	−16.43
	(0.909)	(0.843)	(1.694)	(1.690)	(12.44)
Compensation $(000) 2016		−0.0115***		−0.00107	
		(0.00148)		(0.00155)	
Without Controls					
Panel A					
Unemployment May t	1.172	0.908	5.077***	5.016***	−26.06
	(0.965)	(0.870)	(1.483)	(1.490)	(17.77)
Compensation $(000) 2016		−0.0103***		−0.000646	
		(0.00150)		(0.00161)	
Panel B					
Unemployment May t-1	1.881**	1.673**	1.938	1.869	−21.35
	(0.897)	(0.835)	(1.415)	(1.406)	(14.20)
Compensation $(000) 2016		−0.0103***		−0.000784	
		(0.00151)		(0.00161)	
Rounded Observations	7760	7760	2980	2980	7760
Number of Clusters	302	302	106	106	302
Dep Var Mean	0.453	0.453	0.161	0.161	41.06

Each panel-by-column cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Regressions with controls include fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Regressions without controls include fixed effects for state and year. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. Columns 1 and 2 have 30 fewer observations in regressions with controls due to missing control variables. Regressions test the effect of the state unemployment rate on the job satisfaction of newly hired teachers unconditional on school characteristics. See Table 1.3 for how the controls interact with the unemployment rate.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Teacher Data File,” “Public School Data File,” and “District Data File” 1987–2008

Table 1.7: Local Unemployment Rate and Compensation—Recent College Graduates

	(1) Dissat w/ Salary	(2) Dissat w/ Salary	(3) Leave For Salary	(4) Leave For Salary	(5) Compensation $(000) 2016
Panel A					
Unemployment May t	3.747***	3.501***	4.392	3.787	−14.42
	(1.002)	(0.931)	(2.746)	(2.661)	(16.51)
Compensation $(000) 2016		−0.0174***		−0.00629**	
		(0.00262)		(0.00297)	
Panel B					
Unemployment May t-1	4.139***	3.961***	−0.409	−1.007	−11.16
	(1.004)	(0.955)	(2.148)	(2.194)	(14.12)
Compensation $(000) 2016		−0.0174***		−0.00656**	
		(0.00264)		(0.00302)	
Rounded Observations	4980	4980	1960	1960	4980
Number of Clusters	277	277	103	103	277
Dep Var Mean	0.441	0.441	0.148	0.148	40.02

Sample restricted to teachers within 3 years of college graduation. Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.8: Local Unemployment Rate and Teacher Characteristics and Certification Methods

	Unemployment May t	Unemployment May t-1	Dep Var Mean	N / N Clusters
Teacher Characteristics				
Age	−7.796	3.455	30.25	7630
	(10.46)	(10.48)		301
Male	0.301	0.250	0.238	7630
	(0.801)	(0.806)		301
Yrs BA to Teaching	5.218	9.703	3.814	7470
	(8.968)	(8.252)		299
Recent Graduate	−0.703	−0.381	0.668	7630
	(0.704)	(0.669)		301
Teaching in College State	0.644	0.123	0.774	7160
	(0.586)	(0.687)		298
Certification Types				
Alternative Cert	−0.469	−2.634	0.204	5870
	(1.959)	(1.780)		226
Certified	0.309	−0.154	0.945	7630
	(0.510)	(0.517)		301
Not Fully Cert	−0.954	−0.0431	0.502	7630
	(1.225)	(1.284)		301
Full Cert	0.864	0.0257	0.527	7140
	(1.300)	(1.316)		298
Temp Cert	−1.116	0.296	0.258	7140
	(1.145)	(1.073)		298
Probationary	0.381	−0.496	0.174	7140
	(1.029)	(0.982)		298
Ed Major	−1.024	−0.0277	0.518	7630
	(1.114)	(0.981)		301
STEM Major	1.059*	−0.135	0.134	7630
	(0.539)	(0.522)		301

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. Regressions examine the extent to which the characteristics and certification status of newly hired teachers changes with the local unemployment rate.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Teacher Data File,” “Public School Data File,” and “District Data File” 1987–2008

Table 1.9: Local Unemployment Rate and Teacher Characteristics and Certification Methods–Recent College Graduates

	Unemployment May t	Unemployment May t-1	Dep Var Mean	N / N Clusters
Teacher Characteristics				
Age	−3.475	2.474	27.45	4980
	(10.26)	(11.56)		277
Male	1.541	1.234	0.224	4980
	(0.972)	(0.986)		277
Yrs BA to Teaching	1.693	1.837	0.634	4980
	(1.642)	(1.614)		277
Teaching in College State	0.402	0.289	0.831	4850
	(0.845)	(0.930)		277
Certification Types				
Alternative Cert	−1.335	−3.117*	0.149	3970
	(1.728)	(1.661)		212
Certified	0.231	−0.391	0.954	4980
	(0.545)	(0.575)		277
Not Fully Cert	−1.560	−0.630	0.463	4980
	(1.341)	(1.526)		277
Full Cert	1.623	0.976	0.563	4730
	(1.355)	(1.513)		274
Temp Cert	−1.265	0.461	0.221	4730
	(1.498)	(1.417)		274
Probationary	−0.196	−1.430	0.176	4730
	(1.202)	(1.133)		274
Ed Major	−0.424	0.420	0.621	4980
	(1.579)	(1.352)		277
STEM Major	0.740	0.0533	0.114	4980
	(0.684)	(0.589)		277

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. Regressions examine the extent to which the characteristics and certification status of newly hired teachers changes with the local unemployment rate.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.10: Retention

	Unemployment		Dep Var	N
	May t	May t-1	Mean	N Clusters
All Teachers				
First Year	0.702	0.62	0.918	2750
	(1.052)	(0.894)		246
Pooled 5	−0.175	−0.218	0.925	6610
	(0.585)	(0.496)		1029
Pooled 10	−0.221	−0.246	0.929	8880
	(0.432)	(0.386)		1325
Alternative Certification Teachers				
First Year	−2.028	1.623	0.908	480
	(4.016)	(6.340)		118
Pooled 5	4.324*	5.446**	0.919	950
	(2.568)	(2.222)		346
Pooled 10	2.252	3.898**	0.930	1140
	(1.545)	(1.795)		415
Non-Fully Certified Teachers				
First Year	−0.524	0.323	0.900	1340
	(2.265)	(1.581)		205
Pooled 5	0.614	0.415	0.917	2400
	(1.202)	(1.138)		635
Pooled 10	0.330	−0.0810	0.920	2630
	(0.905)	(0.835)		732
Fully Certified Teachers				
First Year	0.430	0.182	0.936	1410
	(0.892)	(1.062)		233
Pooled 5	−0.680	−0.609	0.929	4210
	(0.684)	(0.590)		973
Pooled 10	−0.251	−0.262	0.932	6250
	(0.511)	(0.458)		1272

Each cell represents a separate regression. Controls for percent minority enrollment, experience, and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Teacher Data File,” “Public School Data File,” and “District Data File” 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Appendix

This appendix contains additional detail on how the variables used are coded and robustness checks. I test several alternate specifications and sample restrictions, and present additional results including the 2011 SASS.

1.A Variable Descriptions

This list gives additional detail on how variables are coded for consistency across survey years.

- Number of Teachers: The number of full time equivalent teachers at a school is recorded directly from the NCES in every year but 1993. In 1993 I calculate it as the number of full time teachers plus .5*the number of part time teachers.
- STEM major / Education Major: Education major and STEM major are indicator variables based on a teacher's recorded major or minor. Education fields are listed in the SASS. STEM fields are Mathematics, Natural Sciences, Computer science, and engineering.
- Certified: A teacher is coded as certified if the teacher holds any type of certification for the state in which she is teaching, including an emergency or temporary credential. In 1993 and 1999, certification is only recorded in fields in which the teacher is teaching.

- Alternative Certification: An indicator equal to one if a teacher entered teaching through an alternative certification program. Not recorded in the 1987 SASS wave.

- Temporary certification: An indicator variable equal to one if the teacher currently holds a temporary certificate requiring additional coursework or student teaching, including work on the way toward an alternative certification, or an emergency certificate or waiver that requires additional training. A teacher can be coded as both "alternative" and "temporary" if she is teaching while still working toward a certificate in an alternative certification program.

1.B Demand for Teachers and Sample Selection

The model predicts that higher quality workers will be pushed into teaching when local labor markets are weak because it becomes easier to find a job in teaching relative to the private sector. Here I show that teacher demand and teacher hiring does not change with the local labor market during the sample period.

I am able to analyze teacher hiring directly only at the district level. Table 1.B.1 shows summary statistics for these school districts. Table 1.B.2 tests whether hiring behavior is correlated with the unemployment rate and I find no evidence of an effect on hiring or staffing at the district level. The SASS does not record hiring at the school level, but in I can test for selection into the sample of *surveyed* first-year teachers. I run this test using the full set of controls as well as in a model with only state and year fixed effects. This tests for both non-random hiring and non-random selection into the surveyed population of teachers. The included controls are percent minority enrollment, percent of students receiving free or reduced price lunch, grades offered at the school, fixed effects for the year a teacher began teaching, and state by three-level urbanicity fixed effects. Table 1.B.3 shows that the local unemployment rate, both in the year a new teacher would start teaching and with a one year lead, does not predict whether a school has a surveyed first-year teacher or the number of first-year teachers surveyed at a school, regardless of the inclusion of controls.

Table 1.B.4 looks at whether school characteristics, including those school characteristics I use as controls, change with the unemployment rate. Consistent with the national-level evidence discussed above, the unemployment rate does not predict the number of teachers employed at a school. During the sample period, the local unemployment rate is uncorrelated with the school-level control variables I include at both the school and district level. This is true both in the preferred sample period excluding the Great Recession as well as when the 2011 wave of the SASS is included (Table 1.C.4). I also test the effect of the local unemployment rate on total number of teachers employed and log enrollment. While enrollments are the major driver of teacher hiring, the increase in enrollments that accompanies a weaker local labor market is small enough that it does not detectably pass through to teacher hiring.

Table 1.B.1: District Summary Statistics

	All Districts	Districts with a New Hire
# Teachers Hired	18.303	20.538
	(74.44)	(78.57)
% Newly Hired Teachers	8.313	9.285
	(17.40)	(18.19)
Number of Teachers	189.784	208.789
	(857.79)	(904.82)
Enrollment	3014.294	3317.810
	(13753.37)	(14521.01)
Unemployment May t-1	5.794	5.774
	(1.75)	(1.73)
Unemployment May t	5.537	5.524
	(1.59)	(1.58)
Pct. Minority Enrollment	20.065	20.359
	(26.32)	(26.27)
Pct NSLP	0.378	0.373
	(0.26)	(0.25)
Participate in NSLP	0.936	0.951
	(0.25)	(0.22)
Observations	23370	22050

All counts are rounded to the nearest 10 for data privacy.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) “Public School District Data File” 1987–2012

Table 1.B.2: Unemployment Rate and Teacher Hiring—Districts

	(1) Teacher Hired	(2) Teacher Hired	(3) % Newly Hired Teachers	(4) % Newly Hired Teachers	(5) Log(# of Teachers)	(6) Log(# of Teachers)
Panel A						
Unemployment May t	0.00325	−0.00143	−0.254	0.0450	0.00396	−0.00483
	(0.00600)	(0.00481)	(0.325)	(0.310)	(0.0169)	(0.0165)
Panel B						
Unemployment May t-1	0.00131	−0.00303	−0.0203	0.242	−0.00388	−0.0135
	(0.00578)	(0.00495)	(0.301)	(0.289)	(0.0184)	(0.0169)
2011 Included	No	Yes	No	Yes	No	Yes
Rounded Observations	23370	27770	23370	27770	22860	26860
Number of Clusters	255	305	255	305	255	305
Dependent Variable Mean	0.891	0.887	8.313	6.860	4.249	4.323

Each cell represents a separate regression. Fixed effects for state and year. Standard errors clustered at the state-year level. Observations weighted by district weights. All counts are rounded to the nearest 10 for data privacy. This table shows that the unemployment rate does not affect the number of teachers hired or the proportion of teachers who are newly hired at the district level. Total hiring is only recorded at the district level. District summary statistics are in Table 1.B.1.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) "Public School District Data File" 1987–2012

Table 1.B.3: Unemployment Rate and Teacher Hiring—Schools

	(1) 1st Yr Teacher	(2) 1st Yr Teacher	(3) N 1st Yr Teachers	(4) N 1st Yr Teachers
Panel A				
Unemployment May t	−0.00124	−0.00145	0.000580	0.000368
	(0.00259)	(0.00266)	(0.00348)	(0.00354)
Panel B				
Unemployment May t-1	0.00286	0.00286	0.00471	0.00467
	(0.00334)	(0.00325)	(0.00441)	(0.00427)
Controls	Yes	No	Yes	No
Rounded Observations	37600	37600	37600	37600
Number of Clusters	255	255	255	255
Dependent Variable Mean	0.182	0.182	0.207	0.207

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by school weights. All counts are rounded to the nearest 10 for data privacy. This table shows that the number of 1st year teachers in the sample not statistically significantly affected affected by unemployment rate. Table 1.C.1 shows these results with the 2011 SASS wave included.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) “Public School Data File” and “Public School District Data File” 1987–2008

1.C 2011 SASS wave

Here I repeat several of the analyses with the 2011 wave of the SASS included. As shown in Figures 1 and 2, teacher hiring moved with the business cycle in the great recession, so the assumptions of the Roy model are not met in this period. The exclusion of 2011 is also practical. The coding of urbanicity in the NCES-SASS changed between 2007 and 2011 and the regressions including 2011 do not include urbanicity controls.

First I test for sample selection when 2011 is included. The effect of the unemployment rate on teacher employment and hiring at the district level is shown in the main results in Table 1.B.2. The effect of the unemployment rate on selection of schools into the sample is shown in Table 1.C.1. Table 1.C.3 shows the effect of the unemployment rate on teacher employment and enrollments. Tables 1.C.2 and 1.C.4 shows the effect of the unemployment rate on the control variables and Table 1.C.5 shows the effect of the control variables interacted with the local unemployment rate on teacher hiring. Table 1.C.5 in particular suggests that the 2011 wave of the SASS should not be included in the sample. Possibly due to these changes in demand, Table 1.C.6 shows that teacher quality is no longer statistically significantly related to the state unemployment rate when 2011 is included.

Table 1.B.4: Local Unemployment Rate and School Characteristics

	Unemployment May t	Unemployment May t-1	Dep Var Mean	N / N Clusters	Controls
Teachers and Enrollment					
Log # of Teachers	0.00946	0.00693	3.259	37600	Yes
	(0.00618)	(0.00637)		255	
Log # of Teachers	0.00493	0.00167	3.259	37600	No
	(0.00659)	(0.00665)		255	
Log Enrollment	0.0219***	0.0181***	5.970	37600	Yes
	(0.00638)	(0.00686)		255	
Log Enrollment	0.0164**	0.0120*	5.970	37600	No
	(0.00676)	(0.00715)		255	
School Characteristics					
Pct. Minority Enrollment	0.583	0.543*	32.31	37600	No
	(0.368)	(0.311)		255	
Pct NSLP	0.183	0.333	40.17	37600	No
	(0.362)	(0.292)		255	
Participate in NSLP	−0.00183	−0.00213	0.960	37600	No
	(0.00208)	(0.00197)		255	
District Characteristics					
Pct. Minority Enrollment	0.212	0.116	20.07	23360	No
	(0.337)	(0.286)		255	
Pct NSLP	−0.00852	−0.00951*	0.378	22640	No
	(0.00573)	(0.00499)		255	
Participate in NSLP	0.00123	0.000482	0.936	23370	No
	(0.00538)	(0.00580)		255	

Each cell represents a separate regression. Controls are for percent minority enrollment, free lunch program participation, and fixed effects for grades offered at the school, year, and stateXurbanicity. Regressions without controls include state and year FE. Standard errors clustered at the state-year level. School regressions are weighted by school weights and district regressions are weighted by district weights. All counts are rounded to the nearest 10 for data privacy. The employment and enrollment regressions look at the effects of the unemployment rate on enrollments and teacher hiring. The school and district characteristic regressions test whether the control variables move with the unemployment rate at the school and district level. Tables 1.C.2-1.C.4 show these results with the 2011 SASS wave included.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) "Public School Data File" and "Public School District Data File" 1987–2008

Table 1.C.1: Unemployment Rate and Teacher Hiring—Schools—2011 Included

	(1) 1st Yr Teacher	(2) 1st Yr Teacher	(3) N 1st Yr Teachers	(4) N 1st Yr Teachers
Panel A				
Unemployment May t	−0.00364	−0.00335	−0.00433	−0.00384
	(0.00300)	(0.00300)	(0.00416)	(0.00411)
Panel B				
Unemployment May t-1	−0.000456	−0.000102	−0.000855	−0.000247
	(0.00310)	(0.00311)	(0.00406)	(0.00406)
Controls	Yes	No	Yes	No
Rounded Observations	50150	50150	50150	50150
Number of Clusters	305	305	305	305
Dependent Variable Mean	0.170	0.170	0.191	0.191

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, year, and state. Standard errors clustered at the state-year level. Observations weighted by school weights. All counts are rounded to the nearest 10 for data privacy. This table shows that the number of 1st year teachers in the sample not statistically significantly affected affected by unemployment rate.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.
SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Data File," and "District Data File" 1987–2012

Table 1.C.2: Unemployment Rate and District Characteristics–2011 Included

	(1) Pct. Minority Enrollment	(2) Pct NSLP	(3) Participate in NSLP
Panel A			
Unemployment May t	0.410	0.000242	−0.00221
	(0.311)	(0.00415)	(0.00456)
Panel B			
Unemployment May t-1	0.263	0.00135	−0.00242
	(0.261)	(0.00357)	(0.00455)
Rounded Observations	27750	26920	27770
Number of Clusters	305	305	305
Dependent Variable Mean	21.33	0.397	0.938

Fixed effects for state and year. Standard errors clustered at the state-year level. Observations weighted by district weights. All counts are rounded to the nearest 10 for data privacy. This table tests whether the control variables included in the teacher regressions are affected by the unemployment rate at the district level.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS) “Public School District Data File” 1987–2012

Table 1.C.3: Unemployment Rate and School Characteristics—2011 Included

	(1) Log # of Teachers	(2) Log # of Teachers	(3) Log Enrollment	(4) Log Enrollment
Panel A				
Unemployment May t	−0.0106	−0.0117	−0.0231**	−0.0239**
	(0.00818)	(0.00798)	(0.0101)	(0.00977)
Panel B				
Unemployment May t-1	−0.00961	−0.0126*	−0.0183**	−0.0212**
	(0.00744)	(0.00745)	(0.00928)	(0.00905)
Controls	Yes	No	Yes	No
Rounded Observations	50150	50150	50150	50150
Number of Clusters	305	305	305	305
Dependent Variable Mean	3.290	3.290	4.796	4.796

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by school weights. All counts are rounded to the nearest 10 for data privacy.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Data File,” and “District Data File” 1987–2012

Table 1.C.4: Unemployment Rate and School-Level Control Variables—2011 included

	(1) Pct. Minority Enrollment	(2) Pct NSLP	(3) Participate in NSLP
Panel A			
Unemployment May t	0.341	0.222	−0.000935
	(0.304)	(0.261)	(0.00142)
Panel B			
Unemployment May t-1	0.291	0.402	−0.000975
	(0.268)	(0.248)	(0.00138)
Rounded Observations	50150	50150	50150
Number of Clusters	305	305	305
Dependent Variable Mean	34.90	42.78	0.961

Fixed effects for state and year. Standard errors clustered at the state-year level. Observations weighted by school weights. All counts are rounded to the nearest 10 for data privacy. This table shows that the control variables are not determined by the unemployment rate, though less strongly when 2011 is included.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Data File,” and “District Data File” 1987–2012

Table 1.C.5: School Characteristics and Teacher Hiring—2011 Included

	(1) 1st Yr Teacher	(2) 1st Yr Teacher	(3) N 1st Yr Teachers	(4) N 1st Yr Teachers	(5) Log # of Teachers	(6) Log # of Teachers
Panel A						
Unemployment May t	0.00192	0.00672	0.00320	0.00630	−0.0140	−0.0250
	(0.00331)	(0.00608)	(0.00407)	(0.00634)	(0.0157)	(0.0314)
Pct. Minority Enrollment	0.00147***		0.00217***		0.00162	
	(0.000340)		(0.000533)		(0.00129)	
Unemployment May t × Pct. Minority Enrollment	−0.000124***		−0.000167**		0.0000366	
	(0.0000449)		(0.0000679)		(0.000222)	
Pct NSLP		0.000383		0.000645		−0.00445***
		(0.000297)		(0.000451)		(0.000933)
Unemployment May t × Pct NSLP		−0.0000241		−0.0000219		0.000156
		(0.0000430)		(0.0000666)		(0.000144)
Panel B						
Unemployment May t-1	0.00463	0.00915	0.00611	0.00896	−0.0153	−0.0201
	(0.00347)	(0.00620)	(0.00425)	(0.00662)	(0.0127)	(0.0295)
Pct. Minority Enrollment	0.00150***		0.00222***		0.00149	
	(0.000320)		(0.000487)		(0.00108)	
Unemployment May t-1 × Pct. Minority Enrollment	−0.000121***		−0.000164***		0.0000534	
	(0.0000396)		(0.0000587)		(0.000177)	
Pct NSLP		0.000319		0.000591		−0.00454***
		(0.000295)		(0.000425)		(0.000863)
Unemployment May t-1 × Pct NSLP		−0.0000139		−0.0000133		0.000161
		(0.0000400)		(0.0000589)		(0.000124)
Rounded Observations	50150	50150	50150	50150	50150	50150
Number of Clusters	305	305	305	305	305	305
Dependent Variable Mean	0.170	0.170	0.191	0.191	3.290	3.290

Fixed effects for state and year. Standard errors clustered at the state-year level. Observations weighted by school weights. All counts are rounded to the nearest 10 for data privacy.This table tests whether the control variables included in the teacher regressions are affected by the unemployment rate at the district level.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Data File," and "District Data File" 1987–2012

Table 1.C.6: Local Unemployment Rate and Worker Quality (2011 Included)

	(1) CC Selective	(2) CC Selective	(3) Selective (Imputed)	(4) Selective (Imputed)	(5) Selectivity Observed	(6) Selectivity Observed
Panel A						
Unemployment May t	1.379	1.189	1.189	0.984	−0.0954	−0.194
	(0.932)	(0.906)	(0.962)	(0.957)	(0.280)	(0.319)
Panel B						
Unemployment May t-1	1.501*	1.213	1.398*	1.092	0.0152	−0.104
	(0.796)	(0.785)	(0.814)	(0.826)	(0.279)	(0.303)
Controls	Yes	No	Yes	No	Yes	No
Rounded Observations	8490	8610	8840	8970	8840	8970
Number of Clusters	350	351	351	352	351	352
Dep Var Mean	0.645	0.644	0.618	0.618	0.958	0.958

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and state. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2012

1.D Additional Robustness Checks

Table 1.D.1 shows that the results remain statistically significant when standard errors are clustered at the state level to further account for correlations within the education system in a state over time. Table 1.D.4 shows results using the county unemployment rate. The county unemployment rate is associated with about a 1.3 percentage point *decrease* in the likelihood of a new teacher having attended a selective college. Higher poverty schools have a more difficult time attracting higher quality teachers (Clotfelter et al., 2006, Peske and Haycock, 2006) and higher poverty areas also have higher unemployment. Because the sample of schools and thus counties changes over time, it is difficult to fully control for the persistent effect of a county's availability to attract quality teachers. The effect seen here is likely the combined effect of a county's persistent labor market characteristics as well as macroeconomic fluctuations.

Table 1.D.1 and 1.D.2 estimates the same model as Table 1.4 and 1.6 but presents standard errors clustered at the state (as opposed to state-year) level. The standard errors are slightly larger but the interpretation is largely unchanged.

Table 1.D.3 estimates the main model using the state-level college educated unemployment rate as calculated from the CPS. As discussed in Section 1.3.1, this unemployment rate cannot be precisely calculated, especially in smaller states. While the results are directionally similar to those in the preferred specification, they are statistically insignificant. Table 1.D.4 estimates the main model using the county instead of the state employment rate. The county unemployment rate is estimated from the CPS and is not available for all counties. This model is estimated without county fixed effects due to counties entering and leaving the sample. The county unemployment rate is associated with about a 1.3 percentage point *decrease* in the likelihood of a new teacher having attended a selective college. Higher poverty schools have a more difficult time attracting higher quality teachers (Clotfelter et al., 2006, Peske and Haycock, 2006) and higher poverty areas also have higher unemployment. Because the sample of schools and thus counties changes

over time, it is difficult to fully control for the persistent effect of a county's availability to attract quality teachers. The effect seen here is likely the combined effect of a county's persistent labor market characteristics as well as macroeconomic fluctuations. These effects along with larger migration concerns at the county level relative to state make these results more likely to reflect underlying and persistent correlated trends in county unemployment and worker and school quality. Consistent with this, the results here suggest that counties with high unemployment hire lower quality teachers.

Table 1.D.1: Local Unemployment Rate and Worker Quality, State Cluster

	(1) CC Selective	(2) Selective (Imputed)	(3) Selectivity Observed
Panel A			
Unemployment May t	2.690**	2.419**	−0.0284
	(1.022)	(1.007)	(0.311)
Panel B			
Unemployment May t-1	2.905**	2.694**	0.0489
	(1.158)	(1.154)	(0.360)
Rounded Observations	7320	7630	7630
Number of Clusters	51	51	51
Dep Var Mean	0.648	0.620	0.957

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.D.2: Local Unemployment Rate and Compensation, State Cluster

	(1) Dissat w/ Salary	(2) Dissat w/ Salary	(3) Leave For Salary	(4) Leave For Salary	(5) Compensation \$(000) 2016
Panel A					
Unemployment May t	1.780**	1.598**	2.932**	2.835**	−16.18
	(0.823)	(0.746)	(1.377)	(1.405)	(16.96)
Compensation \$(000) 2016		−0.0115***		−0.000952	
		(0.00135)		(0.00138)	
Panel B					
Unemployment May t-1	2.239**	2.061**	0.532	0.435	−16.43
	(0.900)	(0.857)	(2.070)	(2.055)	(13.96)
Compensation \$(000) 2016		−0.0115***		−0.00107	
		(0.00137)		(0.00139)	
Rounded Observations	7630	7630	2980	2980	7630
Number of Clusters	51	51	51	51	51
Dep Var Mean	0.452	0.452	0.161	0.161	41.08

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.D.3: Local Unemployment Rate and Worker Quality—College Educated Unemployment Rate

	(1) CC Selective	(2) Selective (Imputed)	(3) Selectivity Observed
BA Unemp t-1	1.688	1.411	−0.230
	(1.035)	(0.988)	(0.363)
Rounded Observations	7320	7630	7630
Number of Clusters	300	301	301
Dep Var Mean	0.648	0.620	0.957

Each cell represents a separate regression. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. Regressions test the effect of the state unemployment rate on the quality of newly hired teachers. State-level college educated unemployment is calculated from a small sample. The BLS does not provide college educated unemployment by state and when it is calculated from the CPS the rate appears as zero for some states in some periods and measurement error is a concern.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

Table 1.D.4: Local Unemployment Rate and Worker Quality—County Unemployment Rate

	(1) CC Selective	(2) Selective (Imputed)	(3) Selectivity Observed
County Unemp. May t-1	−1.286**	−1.163**	0.202
	(0.544)	(0.516)	(0.192)
Rounded Observations	5810	6070	6070
Number of Clusters	227	228	228
Dep Var Mean	0.644	0.616	0.956

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher weights. All counts are rounded to the nearest 10 for data privacy. County unemployment is unavailable before 1990 and a small number of schools cannot be matched with their county.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008

1.E Retention

Table 1.E.1 shows retention results for new college graduates. As in Table 1.10 I do not detect an effect of the local unemployment rate at the time of hire on the likelihood that a teacher continues teaching.

I further break apart the retention sample to test whether pooling across years of experience led to sample selection bias. Table 1.E.2 tests for differential leaving behavior by years of experience and Tables 1.E.3 – 1.E.5 break this differential leaving behavior apart by type of certification to see, for instance, if teachers are more likely to exit the profession when their credential expires if they entered in a weaker labor market. While there is no obvious pattern, there is some evidence that fully certified teachers who enter during weak labor markets are less likely to leave after their fifth year. I do not find an overall retention effect for this group in the full sample in any specification, but this effect in year 5 may bias the 10 year pooled results for fully certified teachers. Tables 1.E.3 and 1.E.5 only extend to year 5 due to sample size constraints.

Table 1.E.1: Local Unemployment Rate and Teacher Retention—Recent College Graduates

	(1) First Year	(2) Pooled 5	(3) Pooled 10
Panel A			
Unemployment May t	0.0127	0.251	0.0411
	(1.153)	(0.607)	(0.516)
Panel B			
Unemployment May t-1	−0.130	0.179	−0.0263
	(1.074)	(0.551)	(0.480)
Rounded Observations	1840	4570	6160
Number of Clusters	236	963	1239
Dep Var Mean	0.924	0.933	0.929

Sample restricted to teachers within 3 years of college graduation. Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), “Public School Teacher Data File,” “Public School Data File,” and “District Data File” 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Table 1.E.2: Local Unemployment Rate and Teacher Retention Year by Year

Retention | y years, $y =$	1	2	3	4	5	6	7	8	9	10
Panel A										
Unemployment May t	0.702	1.652	−0.584	−0.899	−4.686**	−1.655*	−0.454	2.456	−2.261	−1.667
	(1.052)	(1.067)	(1.343)	(1.285)	(1.805)	(0.987)	(1.335)	(1.489)	(5.461)	(2.633)
Panel B										
Unemployment May t-1	0.620	1.510	−0.528	−0.783	−4.350***	−0.697	−1.651	3.439**	−0.598	−1.598
	(0.894)	(1.048)	(1.103)	(0.927)	(1.302)	(0.914)	(1.473)	(1.527)	(3.109)	(2.050)
Rounded Observations	2750	1330	1170	740	610	540	500	460	410	360
Number of Clusters	246	227	217	205	198	186	204	180	163	165
Dep Var Mean	0.918	0.939	0.921	0.924	0.924	0.956	0.905	0.946	0.924	0.943

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Table 1.E.3: Local Unemployment Rate and Teacher Retention—Alternative Certification Teachers

Retention | y years, $y =$	1	2	3	4	5
Panel A					
Unemployment May t	−2.028	30.43	2.871	50.52	−77.25
	(4.016)	(20.14)	(13.59)	(48.88)	(105.6)
Panel B					
Unemployment May t-1	1.623	30.61***	−7.144	−6.016	−88.00
	(6.340)	(9.425)	(14.07)	(40.80)	(140.7)
Rounded Observations	480	170	130	90	80
Number of Clusters	118	81	68	55	43
Dep Var Mean	0.908	0.924	0.926	0.914	0.924

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Table 1.E.4: Local Unemployment Rate and Teacher Retention—Fully Certified Teachers

Retention | y years, $y =$	1	2	3	4	5	6	7	8	9	10
Panel A										
Unemployment May t	0.430	−0.287	1.841	−1.820	−5.753***	−2.061*	0.798	2.879*	−2.198	−3.666
	(0.892)	(1.301)	(1.745)	(1.430)	(2.188)	(1.153)	(1.548)	(1.552)	(5.394)	(3.863)
Panel B										
Unemployment May t-1	0.182	−0.143	1.582	−1.288	−5.524***	−1.584	−0.600	4.444**	−0.957	−2.398
	(1.062)	(1.427)	(1.466)	(1.020)	(1.474)	(1.118)	(1.619)	(1.807)	(3.084)	(2.467)
Rounded Observations	1410	850	850	580	510	470	450	420	370	330
Number of Clusters	233	207	209	196	192	179	195	177	156	158
Dep Var Mean	0.936	0.943	0.919	0.929	0.923	0.954	0.907	0.950	0.917	0.945

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Table 1.E.5: Local Unemployment Rate and Teacher Retention—Not Fully Certified Teachers

Retention | y years, $y =$	1	2	3	4	5
Panel A					
Unemployment May t	−0.524	5.257	−0.251	2.979	32.15
	(2.265)	(3.266)	(4.555)	(16.01)	(19.75)
Panel B					
Unemployment May t-1	0.323	4.832*	−1.647	−0.182	32.59*
	(1.581)	(2.820)	(5.493)	(10.83)	(16.49)
Rounded Observations	1340	480	310	150	110
Number of Clusters	205	168	129	85	66
Dep Var Mean	0.900	0.934	0.924	0.907	0.932

Controls for percent minority enrollment and free lunch program participation. Fixed effects for grades offered at the school, grades taught by teacher, year, and stateXurbanicity. Standard errors clustered at the state-year level. Observations weighted by teacher followup weights. All counts are rounded to the nearest 10 for data privacy.

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

SOURCE: U.S. Department of Education, National Center for Education Statistics, Schools and Staffing Survey (SASS), "Public School Teacher Data File," "Public School Data File," and "District Data File" 1987–2008; Teacher Follow-up Survey TFS, 1987–2009

Chapter 2

Property Tax-Induced Mobility and Redistribution: Evidence from Mass Reappraisals

2.1 Introduction

US state and local governments collected $488 billion in property taxes in 2015, and property taxes represent 12% of annual housing costs for mortgage-holding US households (Urban Institute-Brookings Institution Tax Policy Center, 2017, Bradley, 2017). A long literature started by Tiebout (1956) describes how we expect property taxes and the public goods they provide to shape communities.[1] However, it is difficult for existing homeowners to respond to changes in their property taxes. Selling a home is costly and capitalization of property taxes into home values means that homeowners may be unable to escape wealth losses from property tax increases.

Because property taxes fund local public goods, changes in property taxes typically

[1] See Ross and Yinger (1999) and Banzhaf (2013) for a review of this literature.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

accompany a change in services provided. It is difficult to then determine whether mobility responses are due to the change in taxes or the change in services. Further, homes transact relatively infrequently and property tax changes are often small, so identifying changes in homeowner mobility from changes in taxes requires a large sample.

In this paper, I explore mobility and voting responses to changes in property taxes using unique features of the Ohio property tax collection and assessment system which allow me to overcome these obstacles. Ohio cyclically updates the taxable value of individual homes to redistribute tax burden within jurisdictions. Additionally, the majority of local taxes are approved through time-limited referenda on spending for specific projects and services. These voted levies can only collect a fixed amount of money. These two features combine to provide a change in a homeowner's property tax burden with no mechanical change in public good provision.

I first examine the size of homeowner responses to changes in their property taxes. Under full capitalization, homes sales in response to changes property taxes holding all else equal are either a reallocation of consumption following a wealth shock or a way to gain access to housing wealth that liquidity or borrowing constraints prevent the homeowner from accessing through borrowing against housing wealth.[2] If services do not change and the change in taxes is perceived as permanent, housing wealth would decrease by the present discounted value of the additional stream of future taxes. Recent literature has explored the size of overall consumption responses to changes in housing wealth, particularly in the wake of the financial crisis, but the direct effect on housing consumption is less studied (Chan, 2001, Ferreira et al., 2010, Mian and Sufi, 2011, Mian et al., 2013, Aladangady, 2017, Cloyne et al., 2019). With full capitalization, any large change in property taxes, positive or negative, should increase the likelihood of sale so that homeowners can reoptimize consumption in a new home that better matches the portion of their wealth they would like to spend on housing. If this is the only channel through which property taxes cause sales, we

[2]See Oates and Fischel (2016) for a summary of the debate about how to characterize property taxes. See Hamilton (1976) and Caplan (2001) for a full description of how capitalization can prevent Tiebout sorting from exerting pressure on public good provision.

should expect very small effects because property taxes are low compared to the transaction costs associated with selling a house and moving (Hardman and Ioannides, 1995).

If capitalization is incomplete or misunderstood by homeowners, or if liquidity constraints limit the ability of homeowners to access their housing wealth and pay their property taxes, tax increases may be more likely to drive mobility. In a literature survey, Sirmans et al. (2008) reveals that and the most common finding in studies of property tax capitalization is partial capitalization, however several studies have found full capitalization and identification challenges are common.[3] There is also evidence that homeowners misunderstand property tax systems and overpay for property tax savings (Cabral and Hoxby, 2012, Bradley, 2017). In this scenario, homeowners may be responding to an increase in their property taxes as an increase in the price of living in their home that they can escape paying by selling.[4]

To measure homeowner responses to changes in their property taxes, I study changes in their likelihood of moving in the three year period following a change in taxes. To control for endogenous house and neighborhood characteristics that may be correlated both with relative home price appreciation and likelihood of sale, I forecast cyclic changes to the assessed value of individual homes. I use this forecast to estimate the change in each homeowner's tax burden arising from changes in relative assessed value due to statewide shifts in demand. I then examine how these changes in a homeowner's forecast tax burden affects their likelihood of selling their home. I find that homeowners are 5% more likely to sell their home when they experience a 1 standard deviation increase in the flow price of public goods and no more likely to experience a foreclosure. This corresponds to about $700 in additional property taxes on an average house over the next three years.

[3] Related to the setting in this paper, Livy (2018) finds full capitalization of property tax rates in Franklin County, Ohio at a discount rate of 3.5%. Borge and Rattsø (2014) find full capitalization in Norway and Palmon and Smith (1998) find full capitalization in Houston, Texas suburbs.

[4] Literature on mobility responses to property tax levels has looked to populations with low demand for public goods to disentangle the effects of varying supply. Johnson and Walsh (2009) study vacation home purchase decisions while Shan (2010) and Farnham and Sevak (2006) study retirees, and all find that these groups are sensitive to property taxes when making locational choices.

My estimate of the magnitude of mobility responses to changes in property taxes provides an important data point for evaluating the benefits of property tax limitations. Concern about the effect of tax increases on cash-poor households have motivated property tax limitations, which have large effects on local government budgets (Martin, 2008).[5] Assessment limitations hold taxes below the market rate, but only for as long as a homeowner stays in their home. Wasi and White (2005) and Ihlanfeldt (2011) have demonstrated a "lock-in" effect, where assessment growth limitations cause homeowners to stay in their homes longer. Under the types of policies that lead to lock-in, however, sale of a home that has appreciated reduces the wealth of the seller. This discount for infrequent movers generated by property tax limitations may drive people to stay in homes that no longer match their preferences for housing. Politically, the lock-in effect has been described as allowing people to afford to remain in their homes, but OSullivan et al. (1995) builds a model where these assessment growth limitations generate additional distortions and excess burden relative to a tax based on current home values. This is consistent with prior evidence that homeowners are unlikely to be priced out of their homes due to property tax increases (Martin and Beck, 2018). I estimate large voluntary increases in mobility in response to property tax increases, suggesting that while property taxes are not forcing people out through foreclosures, property tax limitations do reduce tax-motivated sales.

I next explore the other channel through which homeowners can change their property taxes: voting. Changes in property tax burden may change the demand for public goods of homeowners who remain in their homes. Most property taxes in Ohio are approved through voted levies, providing another channel through which existing homeowners can register their preferences for public good provision. Tiebout (1956) predicts that people will choose com-

[5] One of the most famous, and most restrictive, property tax limitations is California's Proposition 13. The Supreme Court ruled in favor of Proposition 13 saying, "...the existing owner, already saddled with his purchase, does not have the option of deciding not to buy his home if taxes become prohibitively high. To meet his tax obligations, he might be forced to sell his home or to divert his income away from the purchase of food, clothing, and other necessities."(*Nordlinger v. Hahn*, 505 U.S. 1) These limitations are largely the result of property tax revolts that resulted in policies like California's Proposition 13. See Cabral and Hoxby (2012), Martin (2009), Fischel (1998) for a discussion of property tax revolts.

munities according to their preferences for public goods and this will result in communities where all residents have similar preferences (and would thus vote similarly). Hamilton (1975) demonstrates that when local revenues are collected through property taxes, preference-based sorting should lead to communities with homogeneous home values. Without this homogeneity, there is within-community variation in the price each homeowner is paying for their public goods through their property taxes. We know from Pack and Pack (1977) and Rhode and Strumpf (2003) that in practice communities are not homogeneous, and the communities I study also exhibit substantial variation in home prices.[6]

Given heterogeneous communities, residents must find an agreeable level of public good provision. Economic theory from Meltzer and Richard (1981) suggest that in a right skewed distribution, greater skew in wealth leading to a larger subsidy to the median voter should lead to higher voted tax rates.[7] However, Benabou (1996), Benabou (2000), and Alesina et al. (1999) build a model where community heterogeneity leads to lower tax rates due to social preferences. Empirical results that directly test this hypothesis have been mixed. Boustan et al. (2013) and Corcoran and Evans (2010) finds that rising income inequality leads to growth in tax revenues and public expenditures in the US, and Borge and Rattsø (2004) find that increasing inequality in municipalities in Norway leads to increased reliance on redistributive forms of taxation, but several studies have not found this association (Alesina et al., 1999, Kenworthy and McCall, 2007, Georgiadis and Manning, 2012). I test how changes in the skewness of the distribution of home values as measured by the ratio of the median to mean forecast home value affects voted tax rates and find suggestive evidence of increases in taxes when this ratio decreases consistent with Meltzer and Richard (1981).

This paper proceeds as follows. Section 2.2 describes the Ohio property tax collection and

[6] For Tiebout sorting to efficiently provide public goods, communities must be homogeneous both in the value of their housing stock and in the public good preferences of their residents (Calabrese et al., 2011, Brueckner, 2000). Barseghyan and Coate (2016) discuss how households with high demand for both housing and public goods may lead to heterogeneous communities.

[7] Epple and Romano (1996) describe how the availability of private education options can break this association for the median income voter. Such a pattern is seen in California by Brunner and Ross (2010).

assessment system. Section 2.3 describes how I calculate changes in taxes. Section 2.4 describes the Zillow ZTRAX data and my procedure to instrument for tax changes through forecast assessed values. Section 2.5 documents the variation in taxes across Ohio communities and how it diverges from the predictions of Tiebout (1956). Section 2.6 describes the empirical approach, Section 2.7 presents the findings, and Section 2.8 concludes.

2.2 Background

In this paper, I investigate how changes in property taxes affect the mobility of homeowners and the change in the level of voted taxes in Ohio. Property taxes are strongly disliked by those who pay them but are also seen as an efficient way to raise revenue because they are difficult to avoid (Norregaard, 2013, Chamberlain, 2007). Mobility responses to changes in property taxes provide insight into the extent to which agents attempt to avoid higher property taxes. Property taxes are also an important source of revenue, both in this context and throughout the US. As of 2015, 30% of US school funding came from property taxes (U.S. Census Bureau, 2017). Ohio collected \$16.2 billion in property taxes in 2017 from \$680.7 billion of property, while the state collected \$25.7 billion of tax revenue, mostly through sales and income taxes (Testa, 2018).

Two features of the Ohio property tax system allow me to study the effects of changing property taxes. First, home values for tax purposes change on set three year cycles for each county. Second, the majority of Ohio property taxes are collected through what is known as "outside" millage—voted levies that are authorized to collect only a fixed dollar amount regardless of changes to the value of the existing tax base. Collection through "outside" millage means that the changes to property taxes that occur at reassessment are largely redistributive. With outside millage, any moves caused by these redistributive tax changes are in response to changes in individual taxes, not changes in the current level of public good provision in a community.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

2.2.1 Ohio Reassessment Cycles

Homes in Ohio are reappraised for tax purposes every six years, with an update to value three years following the appraisal. In the years between reappraisals and updates, a homeowner's assessed value for tax purposes does not change. For the purposes of this paper, I will largely treat updates and reassessments as the same event.[8] Ohio's 88 counties are divided into three groups, and each group reassesses on its own fixed schedule. Figure 2.1 shows the reassessment schedule for each Ohio county and for the subset of counties included in the analysis.

Figure 2.2 shows a joint histogram of the the accuracy of assessments for homes that are sold. Assessments are a very strong predictor of future sale price. Tables 2.1 and 2.B.1 show this in regression form. Without controls it appears that assessments underestimate future sale price, particularly in logs, but with hedonic, time, and place controls, assessments are much more accurate and may even overestimate sales price. An important caveat to this measure of assessment accuracy is that I can only observe the accuracy of assessments for the subset of houses that sell.

Homeowners are notified of their new assessed value in the summer of the reappraisal year.[9] The value assigned is the value as of January 1 of the reappraisal year will be used to assign tax liability for three years. Homeowners do not receive their new tax bill until December of the reappraisal year. As many county auditor websites point out, it is difficult for homeowners to know how their reassessment will change their taxes until they receive their bill.[10]

[8]In an appraisal, an assessor visits, but does not enter, each property. The procedure for updates varies by county but is often model-based. Figure 2.A.1 shows a map of the six year reappraisal schedules.

[9]Most counties notify in July. Some are as late as September.

[10]For example, the Cuyahoga County reappraisal FAQ says: "Q: How will the value change impact my taxes? A: We will be unable to determine the tax impact until tax rates are certified by the State of Ohio in November." https://treasurer.cuyahogacounty.us/en-US/real-estate-taxes.aspx

2.2.2 Outside Millage

Most property taxes in Ohio are in the form of voted levies earmarked for specific purposes and projects. These levies are time-limited and not indexed to inflation and are known in Ohio as "outside" millage.[11] "Outside" is in reference to those collections that are outside of the restrictions set forth in HB 920, which restricts property tax collections to 1% of assessed value. Ohio uses a 35% assessment ratio statewide, so this restricts collections to .35% of appraised value. All levies outside of this limit must be approved by voters and can only collect what is essentially a fixed amount of revenue (Testa, 2018, Rink, 1981).

Outside millage levies are put to voters and, if approved, allow a taxing authority to collect "the amount that would have been levied if the full rate thereof had been imposed against the total taxable value of such property in the preceding tax year."[12] This means that while a gross tax rate appears on the ballot, homeowners are in fact voting on a pre-set dollar amount to be collected. In the following years, outside millage levies can only change the amount of money they collect through new construction or if property is reclassified into their taxing authority.[13] Since 1980, these adjustment factors have been calculated separately for Class I (Residential and Agricultural) and Class II (commercial) property (Rink, 1981).

Figure 2.3 shows the share of collections that are from outside millage by year. Aggregate collections have increased over time primarily through newly voted levies and construction of new homes. Inside millage levies change total collections proportionally with home price changes, however they are a relatively small share of collections ($\approx$ 15%) and, as shown in Figure 2.4, house price growth exhibited volatility characterized by the housing boom and subsequent collapse, but ended the period roughly where it started.

[11]In this period, the average age of an existing school district levy is 13.5 years and the median age is 12 years.

[12]Ohio Revised Code 319.302 (D)(1) `http://codes.ohio.gov/orc/319.301`

[13]For all tax levies, the tax commissioner must, "Determine by what percentage, if any, the sums levied by such tax against the carryover property in each class would have to be reduced for the tax to levy the same number of dollars against such property in that class in the current year as were charged against such property by such tax in the preceding year." Ohio Revised Code 319.302 (D)(1) `http://codes.ohio.gov/orc/319.301`

Figure 2.5 shows compliance with HB 920. Change in tax collections on carryover property from continuing levies should be zero. While there are some very small ($< 1\%$ in every year but 2007) changes in collections, collections largely do not change with reassessment. I then test that the reassessment cycle does not drive changes in the level of school funding. Table 2.2 tests this relationship in a regression of an indicator for reassessment on school spending and tax collections. As discussed in the next section, school districts will be the primary taxing entity considered in this analysis because they are the largest collector of property taxes. Here, it appears that collections are less than 1% lower in reassessment years but this effect does not pass through to spending. Legally, the reassessment cycle should not cause a mechanical change in tax collections. Empirically, the relationship between tax collections and the reassessment cycle is economically small.

2.3 Flow Price of Public Goods

To examine how property tax changes affect mobility separately from changes in the quantity of public goods, I consider changes in the cost to an individual household of each dollar of local collections. I call the cost of a dollar of spending to an individual homeowner that household's "flow price of public goods." It is a "flow" price because if the change is unanticipated and fully capitalized, the wealth shock is absorbed either way and selling only changes how the homeowner pays for the change.

While many types of jurisdictions, including counties, library districts, and municipalities, can levy taxes, school districts are by far the largest collector of property taxes. Figure 2.6 shows that school districts have consistently received about 60% of property tax collections.[14]

In this analysis, I use school districts as the taxing unit. Because education scales with number of students, I consider its per-household cost. If a school district has many childless

[14]Another 20% of collections goes to counties, and, as discussed in Section 2.4 my measure of tax share does respect county boundaries.

households, this measure will suggest a low "price" of education services. Because I am primarily concerned with *changes* in taxes, this will only be a problem if the number of students in a district changes drastically and there is an accompanying change to property tax rates. The per-household cost of education through property taxes is $\bar{T}$, which is simply total revenue divided by the number of households.

Due to the HB920 restrictions described above, total collections must stay consistent over time. So for homes with assessed value A and district tax rate r[15]

$$\bar{T}_t = \frac{\sum A_t r_t}{N} \approx \frac{\sum A_{t-3} r_{t-3}}{N} = \bar{T}_{t-3}$$

For each household, its assessment share, $\frac{A_{it}}{\bar{A}_t}$, is its flow price of public goods or its price per dollar of collections. The change in assessment share then gives the change in a household's flow price of public goods.

$$\Delta T_{it} \approx \frac{A_{it}}{\bar{A}_t} - \frac{A_{it-3}}{\bar{A}_{t-3}}$$

Column 1 of Table 2.3 shows the relationship between change in assessment share and change in tax share. The actual change in assessment share is very similar to the actual change in tax share.

2.4 Data

The data for this paper come from Zillow ZTRAX and the Ohio Department of Taxation.[16] Zillow ZTRAX contains historical assessment records for 2002-2014 and home sales, foreclosure, and loan records through 2017. I place homes in their school districts using the coordinates provided by Zillow and the Census Bureau's TIGER database for school district boundaries for

[15] The following relationship is approximate because new levies come in and old levies expire.

[16] `https://www.tax.ohio.gov/research/property_tax_statistics.aspx`,`http://www.zillow.com/ztrax`

the year 2000.[17] Figure 2.7 shows Ohio school districts and Figure 2.1 shows the counties included in the sample.[18]

The taxing unit I consider is the school district. While jurisdictions other than school districts collect taxes, school districts are the largest recipient of property tax revenue (Figure 2.6) and school quality is valued by home buyers (Black, 1999a, Ries and Somerville, 2010, Bayer et al., 2007). School districts are included in the analysis if at least 90% of the observed homes in the district are in one county. The portion of the district that is outside of the majority county is excluded. This leaves me with a sample of 544 school districts.[19] I include only those houses that exist in both their assessment year and $t-3$ because that is the sample for which I can calculate change in tax share. Because voted levies that existed in $t-3$ collect revenue on "continuation property", this is the relevant population for the redistribution of tax liability. Figure 2.8 shows the number of assessment records available in each year.For many properties and counties, assessment records are only available in assessment or update years.

The assessment records contain data on home characteristics. Summary statistics for continuous hedonic characteristics of the homes are shown in Table 2.4. Additional categorical controls are building condition and quality grade, heat type, AC type, and land use code. For all variables, indicators for missings are added.

As described in Section 2.2, taxes for an individual home are relatively stable for the three years between assessment cycles. Figure 2.10 shows that changes in taxes are much larger in assessment years. Each graph is a histogram of the percent change in taxes on a house-year observation. The upper figure is for non-update or reassessment years while the lower figure is for update years. As shown in the upper figure, there are small changes in collections in off-cycle years. These most commonly occur due to new and expiring levies. The lower figure confirms that the much larger driver of changes in taxes occurs in update years. As expected due to the

[17]The coordinates provided by Zillow are enhanced Tiger coordinates and are accurate to the block segment level.

[18]Excluded counties are Coshocton, Defiance, Fayette, Harrison, Meigs, Noble, Pike, Vinton, and Wyandot. They are excluded because ZTRAX does not provide a panel of assessment records.

[19]As of 2018, Ohio had 608 total school districts.

redistributive nature of reassessments, the median house sees no change in tax liability, however there is large variability around this median. (The median house sees a 5.4% change in assessment share in absolute value.)

Home sales, foreclosure, and home equity loan data also 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 definitions are designed to capture arm's length transactions. Foreclosures are transactions coded 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. Some homeowners coded as foreclosed under this measure find ways to remain in their homes. Home equity loans are loans coded by Zillow as a HELOC.

For all sale types, I assume that a house will only have one transaction of each type within a 93 day window.[20] I 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 I check for another transaction within the following 93 days, until a 93 day period with no transaction activity passes.[21] The transaction date is coded as the date of the first event in the transaction window. The price is the maximum price observed over the transaction window.

Data on gross rates, effective rates, and total collections come from the Ohio Department of Taxation. Table 2.5 shows summary statistics for average effective tax rate, gross tax rate, and total tax collections on residential property. Effective rates are lower than gross rates due to HB 920 adjustments.

[20]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.

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

2.4.1 Assessments Forecast

We may be concerned that relative assessments within a school district and probability of sale move together for non-tax-driven reasons. For instance, suppose one neighborhood within a school district experiences an increase in crime while patterns in the rest of the school district are stable. This may cause property values within that neighborhood to fall relative to the rest of the school district while also increasing out-migration from the neighborhood.[22] Because this neighborhood has depreciated relative to the rest of the school district and experienced an increase in out-migration, simple OLS in this context would suggest that tax decreases increase the likelihood of sale.

To address this endogeneity problem, I generate a leave-one-out by county forecast of assessed values. This forecast uses market valuation from the rest of Ohio, excluding the county containing the home, to predict the assessed value of each home. The prediction uses home and neighborhood characteristics from the pre period to forecast assessed value. This means that if homes with four bedrooms are in higher demand, I will forecast a higher assessed value for four bedroom homes. However, if a home is remodeled from two to four bedrooms I will forecast its value as though it were still a two bedroom house. The same is true of neighborhood characteristics. If urban neighborhoods appreciate I will forecast a higher value for houses that were in urban places in 2000. If a neighborhood has become more urban, I will forecast values for homes within it at its prior density. Changes to a home can be remodeling in preparation for sale and changes to a neighborhood can accelerate mobility through sorting. The forecast controls for shocks to homes and neighborhoods that change valuation and mobility through channels other than taxes.

I use a random forest regression to generate a non-parametric forecast of assessed values.[23]

[22]In this example, as with any home sale, a willing buyer must be found. See Kirk and Laub (2010) for a discussion of how crime affects neighborhoods and property values.

[23]See Hastie et al. (2005) for a discussion of random forest regression. Random forest regression has also been suggested as a technique for performing mass appraisals (Antipov and Pokryshevskaya, 2012).

For each county, I generate a forecast from 20% of the assessment records from the rest of the state, not including the forecasted county. The random forest has 50 trees with a maximum tree depth of 20. From Zillow, I include home characteristics from the prior assessment period and the assessed value from the prior assessment period.[24] I also attach tract characteristics from the 2000 Census, county employment characteristics from the 2000 QCEW, and school district characteristics from the 2000 SAIPE.[25]

Figure 2.9 shows a joint histogram of the forecast and true values for assessed values below $100,000 (this corresponds to houses with a value below $350,000, which is approximately the 95th percentile of the home value distribution). The R^2 of the forecast is 0.94. Table 2.6 shows the relationship between the assessment share and the forecast. Column 1 confirms that the school district level assessment share is a very good predictor of the true school district tax share (the coefficient is statistically indistinguishable from 1). I will be using changes in assessments as a proxy for changes in taxes and Table 2.3 looks at changes. Column 1 shows that actual assessment share changes strongly predict actual tax share changes, where again the coefficient is statistically indistinguishable from 1. Column 2 shows the performance of the forecast change in assessment share. The forecast captures about 25% of the true change in assessment share.

2.5 Documenting Variation in Tax Share within Communities

A central prediction of Tiebout (1956) and the literature that follows is that homeowners will form communities of homogeneous preferences. When public goods are funded through property taxes, Hamilton (1975) points out that restrictions on home sizes are needed to prevent

[24]Included home characteristics are Lot Size, Number of Units, Property Type, Year Built, Total Rooms, Total Bedrooms, Total bathrooms, Building Quality, Building Condition, Architectural Style, Roof Type, Heating Type, AC Type, Water Type, Sewer Type, Lot Site Appeal, and Year.

[25]Included census characteristics are: Fraction HS plus, Fraction College Plus, Fraction Poor, Tract Population, fraction urban, fraction rural, fraction white, fraction black, fraction non-hispanic white, fraction under 18, fraction over 65, fraction of housing that is owner-occupied. School district characteristics are school district size and fraction of children in poverty. County employment characteristics are annual average pay and employment location quotient for the the service industry.

a game of "musical suburbs, with the poor following the rich in a never-ending quest for tax base." He suggests that these restrictions will take the form of zoning such that there is no cross-subsidisation of public goods.[26] We know that there is heterogeneity in home values within districts, and in this paper I first demonstrate the heterogeneity in within-district taxes and home values that exist in this context. This is consistent with findings by Pack and Pack (1977) and Rhode and Strumpf (2003) that demonstrate the heterogeneity in tax prices of public goods within jurisdictions.

Figure 2.11 shows the distribution of public goods prices that individual households in the sample face. Those to the right of one are cross-subsidizing education for those to the left. In a frictionless Tiebout-Hamilton setting, we would see a single spike at one. This shows that heterogeneity in preferences for housing and public goods leads to communities that do not match the Tiebout-Hamilton framework. Tiebout-Hamilton predicts that those to the right of 1 should be induced to move to a community in which they are not cross-subsidizing other residents. Instead, the median assessment share is 0.9, meaning most residents are receiving some subsidy and there is a wide distribution of assessment shares.

Figure 2.12 shows that there is also substantial variation in the ratio of median to mean home value across communities. Again, communities are concentrated at 0.9, but there is variation in levels of subsidisation of the median voter, especially to the left of the peak, with some communities having a median subsidy of 20-30%.

[26]There has been substantial debate on whether zoning restrictions in practice achieve the necessary stringency. See, for example, Oates and Fischel (2016), Fischel (2013), Mieszkowski and Zodrow (1989).

2.6 Empirical Strategy

2.6.1 Flow Price of Public Goods

In the mobility analysis, the variable of interest is the individual change in tax share. The tax share approximates the flow price of public goods each homeowner faces. As described in Section 2.3, the change in tax share for each house is approximated by the change in assessment share:

$$\Delta T_{it} \approx \frac{A_{it}}{\bar{A}_{td}} - \frac{A_{it-3}}{\bar{A}_{t-3d}}$$

To control for endogenous changes to local assessments coming from neighborhood improvement or remodeling at the house level, in most specifications I estimate the change in tax share as

$$\Delta \hat{T}_{it} \approx \frac{\hat{A}_{it}}{\bar{\hat{A}}_{td}} - \frac{A_{it-3}}{\bar{A}_{t-3d}}$$

Table 2.3 shows the relationship between change in assessment share and change in tax share. The actual change in assessment share is very similar to the actual change in tax share, but the forecast only captures about 30% of this relationship.

As discussed in Section 2.2, taxes for individual homeowners are redistributed based on assessed values every three years. Taxes are based on the share of total residential value that belongs to each homeowner. I examine how this change in taxes based on relative home value appreciation affects mobility, foreclosure, and home equity loan origination. The identifying assumption here is that (forecast) assessment share, conditional on a rich set of controls including the (forecast) wealth change, influences homeowner mobility only through its effect on a homeowner's tax share.[27]

Homeowners estimate their own home values imprecisely and know even less precisely how their home's value has changed relative to other homes in their school district (Benítez-Silva

[27] If residents have a preference for the position of their home value in their community, it could confound findings here (Fligstein et al., 2017).

et al., 2015). In this way, the change in taxes that comes through reassessments is a shock. It is a shock to present income because it must be paid today, and, if the change is persistent, the future payments are a shock to housing wealth to the extent that the tax change is capitalized into home values. Further, if mobility is driven by liquidity constraints, homeowners will not face these constraints until their new taxes come due. As discussed in Section 2.4.1 I use a leave-one-out by county forecast of assessed values to identify how market forces external to the neighborhood of the home itself have changed the value of each home and the share of value in the district held by that home. Columns 1 & 3 of Table 2.6 show the relationship between (forecast) assessment share and tax share. As expected due to tax collection formulas, the coefficient on assessment share is close to 1 for both the actual and the forecast assessment share.[28]

In the voting analysis, I use forecast assessments to examine whether changes in the distribution of assessed values change public good provision. Specifically, I test whether changes in the ratio of median to mean home values predicts changes in the level of spending on school district service provision as predicted by Meltzer and Richard (1981).

2.6.2 Effect of Change in Tax Share on Mobility

I first estimate the effect of change in tax share on homeowner mobility and home equity loan origination. I estimate mobility changes as cumulative mobility over the three year period following reassessment. This means that for a 2004 reassessment, I look at the likelihood that a homeowner moves in any of 2005, 2006, or 2007. I use this timing because, as described in Section 2.2 homeowners reassessed in 2004 do not learn their new tax liability until December 2004. I consider sales over the next three years because this is the full period over which I can analyze the effects of the tax change before another reassessment occurs.

[28] Some levies are collected and reallocated among entities other than the school district. This causes some of the slippage between assessment share and tax share. Figure 2.3 shows that about 60% of levies are collected by the school district.

I estimate a linear probability model of the following form:

$$Sale_{it+[1,2,3]} = \beta_0 + \beta_1 \Delta T_{it} + f(\frac{A_{it-3}}{\bar{A}_{t-3d}}) + g([A_{it} - A_{it-3}]) + h(A_{it-3}) + \mathbf{X}_i + \mathbf{T}_t \times C + \varepsilon_{it} \quad (2.1)$$

where i indexes home assessments and t indexes years. $Sale_{it+[1,2,3]}$ is an indicator for whether a house is sold in the 3 years following its reassessment, ΔT_{it} is represented by $\Delta \mathrm{A}_{it}$ and is as described in Section 2.3, $\frac{A_{it-3}}{\bar{A}_{t-3d}}$ is the initial assessment ratio, $A_{it} - A_{it-3}$ is the change in housing wealth by assessed value, A_{it-3} is initial assessed value, and f, g, h are quartic polynomials to flexibly control for changes and levels of relative and absolute housing wealth. These controls capture the direct effect on mobility of the change in wealth that the homeowner has experienced. Additionally, housing wealth evolves smoothly in the period leading up to the reappraisal while taxes are revealed only at the time of billing. X_i is a vector of home and census tract characteristics and $\mathbf{T}_t \times C$ are county-by-tax year fixed effects to control for county-level trends in the housing market.

The identifying assumption is that the mobility behavior of homeowners who live in observably similar houses, including in terms of absolute assessment growth, but face different appreciation of their home value relative to the mean home in their district is driven only by the induced relative changes in taxes they face. A threat to identification is that a third factor, for instance an increase in crime as described in Section 2.4.1, will drive both mobility and relative home price appreciation. To address this concern, I use the forecast of relative home price growth described in Section 2.4.1.

When using forecasts, all instances of A_{it} are replaced with $\hat{A}_{it}$:

$$Sale_{it+[1,2,3]} = \beta_0 + \beta_1 \Delta \hat{T}_{it} + f(\frac{A_{it-3}}{\bar{A}_{t-3d}}) + g([\hat{A}_{it} - A_{it-3}]) + h(A_{it-3}) + \mathbf{X}_i + \mathbf{T}_t \times C + \varepsilon_{it} \quad (2.2)$$

To test effects on foreclosures and home equity loan origination, I replace $Sale_{it+[1,2,3]}$ with

$Foreclosure_{it+[1,2,3]}$ and $HELOC_{it+[1,2,3]}$ in equations 2.1 and 2.2. The coefficient of interest in each regression is β_1 which tells us the effect of a change in tax share on the likelihood that a homeowner moves in the three years following the reassessment and accompanying tax shock.

2.6.3 Voted Tax Rates

Changes to property taxes may also change the way homeowners vote for public spending. If reassessment changes the distribution of tax liability within a community, the preferred level of spending may change. Meltzer and Richard (1981) observe that when the median voter has less than the mean voter, the decisive median voter will desire higher taxes to be spent on redistribution. In Ohio (as is usually true for the distribuion of home values), the median home value is almost always less than the mean. Figure 2.12 shows the distribution of the ratio of the median to mean home value that I observe for the school districts in my sample at the time of reassessment from 2002-2014.

Extending this hypothesis, it should be the case that as the ratio of median to mean increases, the median voter should prefer less redistribution, and tax rates should fall. Alternatively, if the model of redistribution proposed by Benabou (1996) operates, an increase in this ratio may increase voted tax rates, as in this model voters are more likely to support redistribution in homogeneous communities and an increase in this ratio implies movement toward homogeneity. Figure 2.13 shows the distribution of ratio changes. It shows that changes in the ratio of median to mean assessments are centered around zero, making this an interesting contest in which to test how changes in inequality affect redistribution.

To test these theories, I look at the effect of changes in the ratio of median to mean forecast assessments within a school district on changes in tax rates and collections. To address the association between wealth and tax growth, I control for the ratio of forecast median assessment to median assessment in the pre period. I also control for initial wealth and include county by year fixed effects in the following regression:

$$\Delta Rate_{ds(t)} = \beta_0 + \beta_1 \Delta \frac{Med\hat{A}_{dt}}{\bar{\hat{A}}_{dt}} + \frac{Med\hat{A}_{dt}}{MedA_{dt-3}} + log(MedA_{dt-3}) + \mathbf{C} \times \mathbf{T} + \varepsilon_{dt} \tag{2.3}$$

where d indexes districts, t indexes years and s indexes years since reappraisal. Change in the ratio, $\Delta \frac{Med\hat{A}_{dt}}{\bar{\hat{A}}_{dt}}$, is observed from three years before reappraisal to the year of reappraisal and constructed using the forecast described in Section 2.4.1. The outcome variable, $\Delta Rate_{ds(t)}$ is change in voted or effective rate from $t-1$ to t, t to $t+1$, or $t+1$ to $t+2$. Elections after homeowners observe their new tax bill first occur in $t+1$. I test the effect of changes in the ratio of the forecast median to mean assessment on gross tax rates, effective tax rates, and percent change in total collections. Effective tax rates move mechanically with total assessed value in a district due to HB 920, as discussed in Section 2.2. Gross tax rates adjust through newly voted levies. Collections change with new construction and new levies.

The sign of β_1 tells us whether the framework proposed by Meltzer and Richard (1981) or by Benabou (1996) dominates in this environment. If β_1 is negative, rates decrease when the ratio of the median to mean home price increases, suggesting residents are voting based on their preferred level of own subsidy. If β_1 is positive, it suggests that residents are voting based on their preferences for redistribution as described by Benabou (1996).

2.7 Results

Table 2.7 shows the effect of tax changes on sales decisions by homeowners as described in Section 2.6.2. Columns 1 and 2 show results for using actual assessments (Equation 2.1), while 3 and 4 show results for forecast assessed values (Equation 2.2). The results for forecast assessments are larger but statistically indistinguishable from those using actual assessments. These results suggest that tax increases increase the likelihood of home sales. A one standard deviation increase in forecast tax share leads to an approximately 5% (0.34 percentage point)

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

increase in the likelihood a home is sold.[29] In absolute terms this is a very small change, but given that for an average home a one standard deviation increase in tax share increases taxes $230, or about 0.175% of the total value of an average home, and selling a home costs at least 5% of the home's value the size of this response is large.

Table 2.8 looks at the effect of tax changes on foreclosures. Recall that the foreclosure variable I use here is an indicator for the first foreclosure filing with the county, and not all of these proceed to evictions. The baseline rate is fairly high, with about 4% of homes receiving a notice of foreclosure every three years during this period.[30] While the tax changes faced by homeowners are small in dollar terms relative to home values, policymakers have long been concerned about property taxes leading to homeowner displacement, and foreclosures are one way to measure displacement due to financial distress.[31] Columns 1 & 2 show that for true assessment changes there is no significant effect of tax increases on the likelihood of foreclosure. Columns 3 & 4 using forecast assessment changes suggest that tax increases lead to a decrease in the likelihood of foreclosure. This result emphasizes the importance of forecast assessments for causal identification. As described in Mallach (2009), disinvestiment often precedes foreclosure, thus lowering true assessments. The assessment forecast addresses this reverse causality.

Taken together, the results in Tables 2.7 and 2.8 suggest that property tax increases do induce a small number of homeowners to move, but homeowners are at least able to sell their homes on the market and are not facing foreclosure as a result of their tax increases. While it does not appear that tax increases from assessment growth are causing these homeowners to "lose their homes" in the most stringent sense of facing foreclosure, the welfare implications of these sales due to tax increases are unclear.

I check that the results I see for sales are driven by tax changes and not wealth changes or

[29]Standard deviation of forecast assessment share change is 0.061, baseline sale probability is 0.071. 0.061*0.0562/0.071

[30]This is consistent with other reports on foreclosure rates in Ohio. In 2008 almost 4% of mortgages were in foreclosure (Mallach, 2009).

[31]See Martin and Beck (2017) for a discussion of the rhetoric surrounding homeowner displacement.

other trends not absorbed by the controls and instrument by testing the effect of the change in assessment ratio in the year of the reassessment on the likelihood of sale in the two years *before* the new tax bill is released to the homeowners. Results of this regression are shown in Table 2.10. As expected, changes in taxes have no effect on the likelihood of sale before they are released to homeowners.

I then test whether the effects I see are consistent with what we would expect if the change in taxes operates as a wealth shock through tax capitalization. Under full capitalization, an increase in tax share is a decrease in wealth, and this result suggests that some homeowners are induced to shift some of their consumption away from housing following a decrease in housing wealth coming from an increase in tax shares. Unfortunately, I am unable to observe where those who sell end up at this stage, so I cannot confirm that their new residence is less expensive and so reduces housing consumption. However, if these sales operate only through preferences for the consumption of housing as a share of total wealth, we would expect that wealth increases from tax reductions might also drive some sales decisions. In this case, decreases in taxes might also increase sales. Tax decreases may both increase home values and increase the amount of money homeowners have to spend on things other than housing. If they wish to spend some of this money on housing services they may be induced to move. To test this, I interact the variable of interest $\Delta\widehat{Assess_i}/\overline{\widehat{Assess}}$ with an indicator equal to one when the variable of interest is positive, and an indicator equal to one for positive home price increases.[32] Table 2.11 shows this analysis. For sales, there is no statistically significant difference in the coefficient on $\Delta\widehat{Assess_i}/\overline{\widehat{Assess}}$ regardless of the sign of the change or the sign of the change in housing values. Instead, homeowners are more likely to move if they experience a smaller tax decrease or a larger tax increase. The effect of the tax change on the likelihood of foreclosure is only statistically significant when the direct change in housing wealth and the indirect change in housing wealth through tax capitalization

[32]While the quartic polynomials for wealth should absorb any effect of assessed value change on likelihood of moving, in theory we may expect effects to be larger if the wealth effect from taxes and home value appreciation move in the same direction.

have opposite signs. Again, the effect of tax increases on foreclosures is either zero or negative. These interaction effects help to confirm that homeowner mobility increases with tax increases throughout the distribution of tax change. The results suggests that homeowners are moving away from an increase in the price of public goods.

2.7.1 Home Equity Loans

If homeowners are selling due to liquidity constraints caused by tax increases, we may expect that they would first attempt to access their home equity. Selling and buying homes are both costly transactions, with at least 7% of the value of the house paid in these costs (Chatterjee and Eyigungor, 2015). The average effective tax rate is less than 2%. This suggests that homeowners are unlikely to sell their homes in response to tax changes unless their housing consumption is already out of equilibrium. Instead homeowners may choose to take out a home equity loan as a less costly way to pay higher-than-expected taxes.

Cloyne et al. (2019), Aladangady (2017), and Mian and Sufi (2011) have all found that households borrow against their homes to consume out of housing wealth shocks. Further, households consume less when their borrowing is constrained due to decreases in home value (Mian et al., 2013). Here, I examine whether a shock to housing wealth through tax capitalization affects the propensity of households to take out home equity loans. In this context, households face two opposing forces: If taxes are fully capitalized, an increase in taxes (controlling for home price appreciation) should decrease home equity loans because homeowners are now poorer and less able to consume out of their housing wealth. If homeowners are income constrained such that they are not able to pay for the increase in taxes they face out of current income, an increase in taxes may increase the likelihood of taking out a home equity loan and using the loan to pay for the property tax increase.

Table 2.9 shows how tax changes affect home equity loan origination. New home equity loan origination is an imperfect measure of the object we would like to study: the additional home equity extracted as a result of the tax change. Homeowners must have a line of credit to extract additional housing wealth, so new loans at least capture one step of the process. However, if these homeowners already had an active credit line, I will miss any additional borrowing they do. Households are less likely to take out a new home equity loan when their assessment share increases, increasing their taxes. This suggests that households are not using home equity loans to pay their property taxes. Unfortunately, I cannot tell if this is because they do not face binding liquidity constraints or because they lose access to this margin for borrowing when their taxes increase. This is true across the distribution of tax and wealth changes, with the exception of those households who see both their assessment and their taxes fall (Table 2.11 Column 3).

2.7.2 Voted Tax Rates

I now turn to the question of how homeowners respond to changes in their property taxes through voting. Tables 2.12, 2.13, and 2.14 present results from estimating equation 2.3. I find some evidence that an increase in the price of a dollar of public goods to the median voter lowers tax rates in the first year after homeowners experience their new tax bill, with no effect in other years.

In the first and third year, increasing median assessments are associated with a decrease in effective rates (confirming the mechanical relationship from HB 920 for the first year) and a small decrease in gross rates.

The coefficient on Δ Median $\hat{A}_t/\bar{\hat{A}}_t$ tests whether changes in rates and collections are consistent with the predictions of the Meltzer-Richard model. I find some suggestive evidence of the Meltzer Richard type redistribution only in year 1, the second year in which homeowners are paying taxes on their new assessed value. For effective rates, a decrease of .01 in the ratio of the median to the mean, which is about the 25th percentile, is associated with a .6 mil increase in the effective tax rate. This is a very small movement relative to the average tax rate of 156 mils, but about 10% of the average movement in effective tax rates. Results for gross rates and effective rates are very similar, which is consistent with a change in rates through newly-voted levies. The sign of the result for percent change in collections (Table 2.14) in year 1 is consistent with the results on voted rates but is not statistically significant.

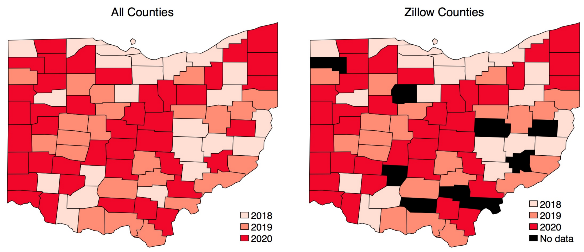
Overall, I find suggestive evidence of small changes in voted tax rates consistent with the median voter seeking higher tax rates when their level of redistribution increases.

2.8 Conclusion

Homeowners do respond to changes in their property taxes, both by moving and by voting. It appears that, consistent with the predictions of Tiebout (1956), homeowners attempt to move away from increases in taxes, with a one standard deviation increase in taxes leading to a 5% increase in the likelihood of selling. Tax increases do not increase the likelihood that a home is foreclosed upon. One of the policy goals of property tax limitations is to keep residents from being forced from their homes. The results here suggest that homeowners are not being forced from their homes through property taxes in a strict sense, as they are no more (or possibly less) likely to face a foreclosure following an increase in property taxes. However, if policymakers wish to prevent a larger set of tax-motivated moves, for instance because homeowners invest in their neighborhoods, property tax limitations may be an effective tool as homeowners who face a tax increase are more likely to sell their homes (DiPasquale and Glaeser, 1999). I cannot determine whether these additional sales are due to financial constraints, though results for home equity loan origination suggests that those who stay are not taking out new loans in order to pay their property taxes. Homeowners may be responding to the flow price they see and attempting to escape their increase in taxes. These types of sales reflect either homeowners failing to understand how their taxes are capitalized or incomplete capitalization. I also find that homeowners who stay may be attentive to their tax shares in their voting choices. I find suggestive evidence that increases in the subsidy to the median voter lead to increases in voted taxes.

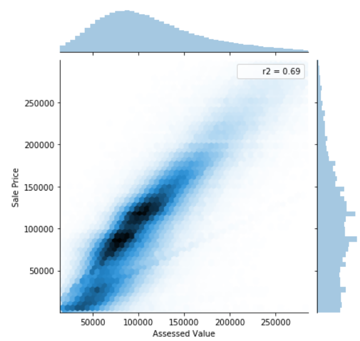
Chapter 2, in part, is currently being prepared for submission for publication of the material. Fraenkel, Rebecca. ”Property Tax-Induced Mobility and Redistribution: Evidence from Mass Reappraisals”. The dissertation author was the primary investigator and author of this material.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows Ohio counties by their reassessment cycle. Coshocton, Defiance, Fayette, Harrison, Meigs, Noble, Pike, Vinton, and Wyandot counties are excluded from the analysis due to missing assessment records. There is geographic variation in which county reassesses when.

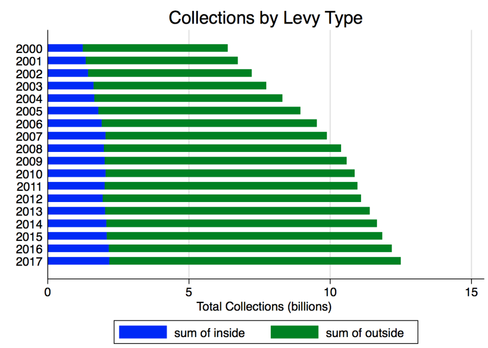
Figure 2.1: Reassessment Cycle



This figure is a joint histogram of assessed value and sale price for market sales as recorded in the Zilow ZTRAX historical assessment and sales data.

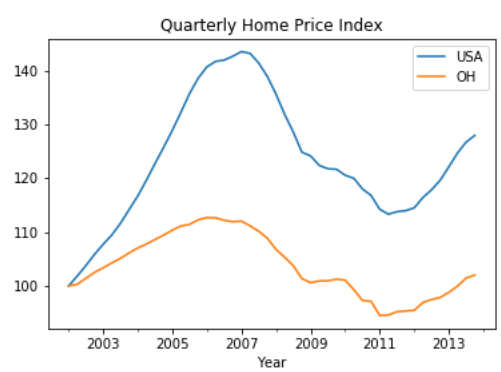
Figure 2.2: Assessment Accuracy

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows nominal total property taxes in Ohio from 2000-2017. Collections through inside millage appreciate through new construction and appreciation of existing properties. Collections through outside millage increase due to newly voted levies and new construction. Data is from the Ohio Department of Taxation.

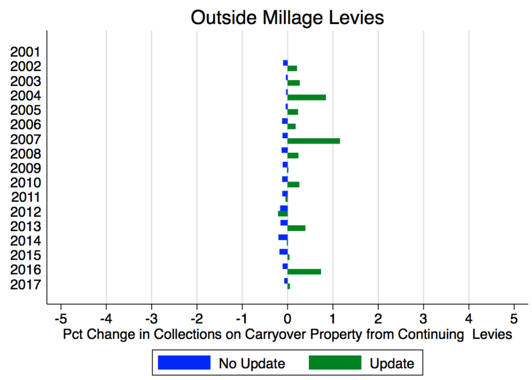
Figure 2.3: Collections by Millage Type



Seasonally adjusted quarterly home price indicies from the FHFA for Ohio and the US from 2002-2014 (Bogin et al., 2019).

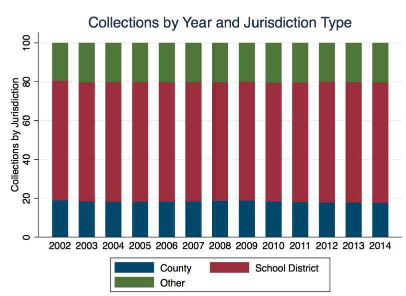
Figure 2.4: Ohio Home Price Index

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows the percent change in collections at the county level on carryover property from outside levies that exist in both t and $t-1$. The total change in collections is summed across all reassessing (updating) and non-reassessing counties. Under HB 920, this change should be zero. Data is from the Ohio department of taxation.

Figure 2.5: Change in Collections from Continuing Outside Millage Levies on Carryover Property



This table shows the share of total collections that goes to each type of jurisdiction that levies taxes. School districts are the largest collector of revenues. Other includes municipalities, library districts, etc.

Figure 2.6: Share of Taxes Collected by Taxing Entity

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

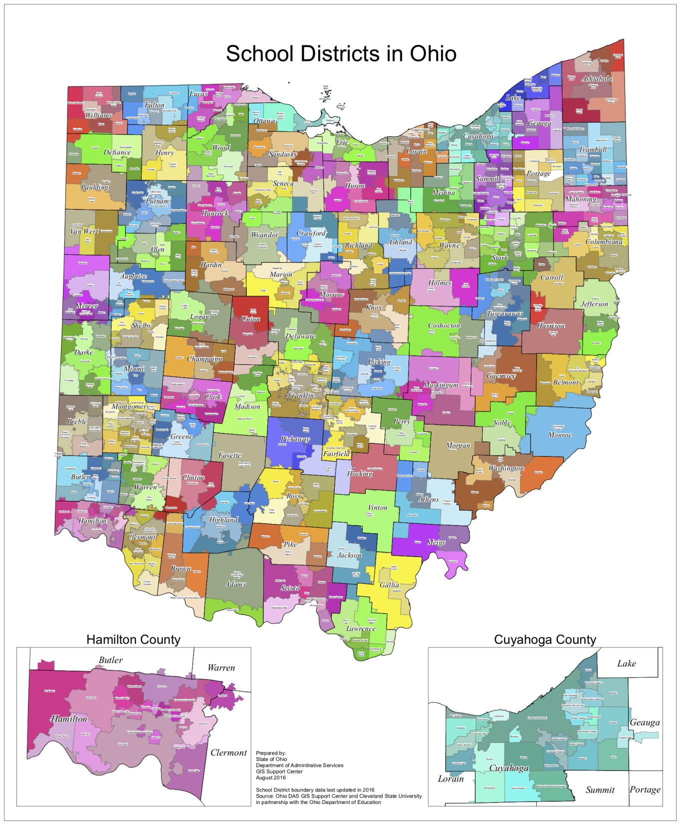
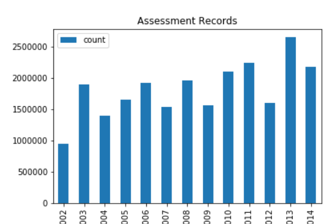


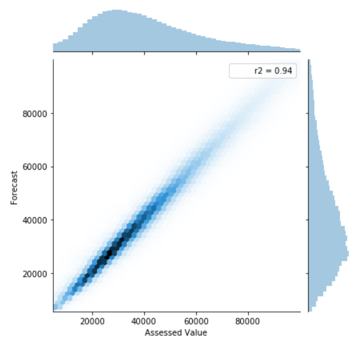
Figure 2.7: Ohio School Districts



This figure shows the number of assessment records available in each year of the sample. Some counties only provide assessment records in assessment years, which gives the three year cyclic pattern in number of records.

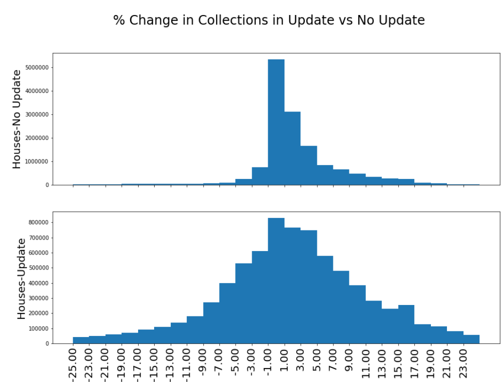
Figure 2.8: Assessment Records

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure is a joint histogram of true and forecast assessed values.

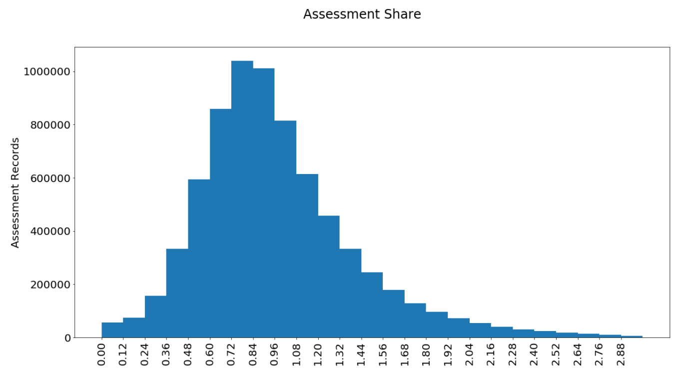
Figure 2.9: Forecast Quality



The top panel of this figure is a histogram of the percent change in year-to-year tax collections for houses with recorded collections two years in a row when the second year is not an assessment year. Collections in non-assessment years change very little because the value of the property against which taxes are levied does not change. There are some small upward and downward increases in taxes primarily due to new and expiring levies. This is in contrast with the bottom panel, which shows the distribution of changes in collections when the second year is an update year. Changes to the assessed value of the underlying property in generates variance in change in collections.

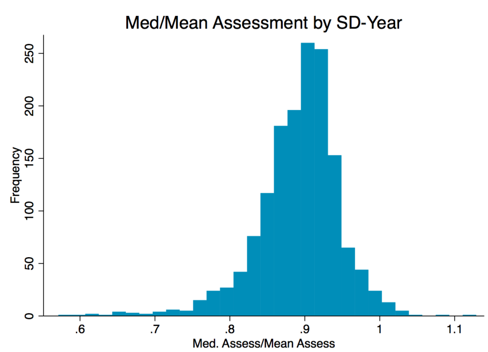
Figure 2.10: Change in Taxes

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This is a histogram of the ratio of home price to mean home price within a home's district for each time an assessment of a home is observed in an update year. The median ratio is .9, so the majority of residents are receiving a "subsidized" public goods relative to the per-household expenditure in their district.

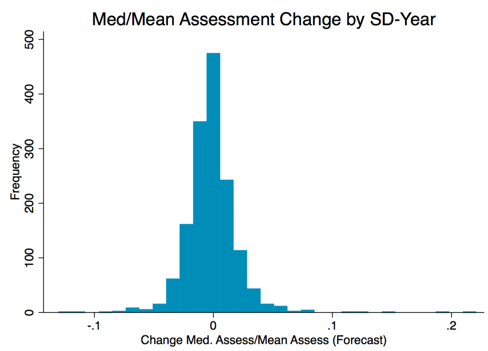
Figure 2.11: Household Tax Shares



This is the distribution of median to mean household assessment ratios by school district-year. Districts appear in this sample only in update years between 2002 and 2014.

Figure 2.12: Distribution of Assessed Value Ratios

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This shows the assessment to assessment change at the school district level of of the ratios plotted in Figure 2.12. Most communities see a relatively small change in this ratio. Districts appear in this sample only in update years between 2002 and 2014.

Figure 2.13: Distribution of Change in Assessed Value Ratios

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.1: Price vs. Assessment

	(1) Price	(2) Price	(3) Price	(4) Price	(5) Price	(6) Price
Assessed Value/.35	1.033***	1.027***	0.956***			
	(0.00103)	(0.00146)	(0.00270)			
Forecast Assessed Value/.35				1.036***	1.030***	0.958***
				(0.00105)	(0.00150)	(0.00278)
Year One Only		X	X		X	X
Controls			X			X
N	563153	218682	218175	563153	218682	218175
Adjusted R2	0.642	0.693	0.725	0.632	0.683	0.719

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls are month-of-year by school district and year by school district fixed effects and hedonic controls. This table shows the relationship between assessed values and realized prices for houses that sell. The sample is restricted to single family homes. Assessed values and forecast assessed values are divided by .35 because Ohio uses a 35% assessment ratio.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.2: Cyclic Change in Expenditures and Revenue

	(1) Exp./Stud	(2) Expenditure	(3) Prop. Tax/Student	(4) Prop. Tax Revenue
Update	0.457	–0.665	–0.917***	–0.884***
	(0.507)	(1.088)	(0.311)	(0.258)
r2	0.049	0.079	0.111	0.148
N	7339	8635	7340	7944

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

FE for School District and Year. Standard Errors clustered at the county level. All outcomes measured in percent change. Data from *Rutgers Graduate School of Education/Education Law Center: School Funding Fairness Data System*. Table shows the extent to which the property reassessment cycle in Ohio predicts spending and revenue collected at the school district level.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.3: Δ Assessment Share and Δ Tax Share

	(1) Tax Share Change	(2) $\Delta \text{Assess}_i / \overline{\text{Assess}}$	(3) Tax Share Change
$\Delta \text{Assess}_i / \overline{\text{Assess}}$	1.038***		
	(0.0312)		
$\Delta \widehat{\text{Assess}}_i / \overline{\widehat{\text{Assess}}}$		0.263***	0.317***
		(0.0303)	(0.0619)
N	8952580	8952580	8952580
Number of Clusters	79	79	79
Adj R2	0.191	0.0494	0.0121

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. This table shows the relationship of changes in assessment share and forecast assessment share with tax share and forecast assessment share. Controls are those that will be used in later mobility regressions.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.4: Assessment Summary Statistics

	count	mean	sd	p50
Rooms	8413780	6.225	1.695	6.00
Bedrooms	8633639	3.019	0.811	3.00
Baths	4375835	1.968	0.764	1.50
age	8533890	49.892	29.544	48.00
Lot Size (Sq. Ft.)	7879039	53317.288	1238194.936	10542.00
Year Built	8533890	1958.375	29.447	1960.00
Move	8952580	0.114	0.318	0.00
Trad. Sale	8952580	0.072	0.258	0.00
Foreclosure	8952580	0.043	0.202	0.00
Home Equity Loan	8952580	0.052	0.222	0.00
Sale Yr 1	8952580	0.028	0.165	0.00
Sale Yr 2	8952580	0.025	0.157	0.00
Sale Yr 3	8952580	0.024	0.153	0.00
Price	805273	118791.340	121296.768	92667.00
Assessed Value	8952580	46427.892	34200.589	38606.00
Assessment T-3 (000)	8952580	45.911	33.709	38.10
Tax T-3	8952580	2441.507	2077.986	1915.28
$\Delta \text{Assess}_i / \overline{\text{Assess}}$	8952580	-0.004	0.095	-0.00
$\Delta \widehat{\text{Assess}}_i / \overline{\widehat{\text{Assess}}}$	8952580	-0.003	0.061	-0.01
$\lvert \Delta \text{Assess}_i / \overline{\text{Assess}} \rvert$	8952580	0.054	0.078	0.03
$\lvert \Delta \widehat{\text{Assess}}_i / \overline{\widehat{\text{Assess}}} \rvert$	8952580	0.032	0.052	0.02
$A_{t-3} / \bar{A}_{t-3}$	8952580	0.997	0.517	0.90
Observations	8952580			

Table shows summary statistics for assessment records in the ZTRAX sample. A house will appear each time it appears in the sample between 2002 and 2014 in an update year.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.5: School District Summary Statistics

	count	mean	sd	p50
Δ Median A_t / $\bar{A}_t$	1160	-0.003	0.016	-0.00
Δ Median $\hat{A}_t$ / $\bar{\hat{A}}_t$	1160	-0.002	0.022	-0.00
$\bar{\hat{A}}_t$	1160	46169.880	19643.929	42613.52
$\bar{A}_t$	1160	45629.825	19437.424	42038.56
Median A_t	1160	40581.233	16219.851	37466.50
Median $\hat{A}_t$	1160	41154.162	16764.771	38122.69
Median A_{t-3}	1160	40544.743	16444.608	37677.50
$\bar{A}_{t-3}$	1160	45440.213	19727.029	41789.83
Effective Tax Rate	4941	157.167	81.360	143.33
Δ Effective Tax Rate	4941	2.656	12.800	0.01
Gross Tax Rate	4941	250.963	128.877	232.80
Δ Gross Tax Rate	4941	2.731	14.830	0.00
Collections	4941	9955004.421	12605863.679	5085717.97
%Δ Collections	4941	0.021	0.052	0.01
Observations	4941			

This table shows school district by year summary statistics for tax collections and assessed home values. Rate variables come from the Ohio department of taxation. Home value variables are from Zillow ZTRAX. Home value variables are available only in update years.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.6: Assessment Share and Tax Share

	(1) Tax Share	(2) $\text{Assess}_i / \overline{\text{Assess}}$	(3) Tax Share
$\text{Assess}_i / \overline{\text{Assess}}$	0.990*** (0.00607)		
$\widehat{\text{Assess}} / \overline{\widehat{\text{Assess}}}$		0.981*** (0.00524)	0.972*** (0.00751)
N	8952580	8952580	8952580
Number of Clusters	79	79	79
Adj R2	0.877	0.960	0.843

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. FE for tax year X county. Standard errors clustered at county level. This table shows the relationship between assessment share and tax share, forecast assessment share and assessment share, and forecast assessment share and tax share. Controls are those that will be used in later mobility regressions.

Table 2.7: Traditional Sales

	(1) Trad. Sale	(2) Trad. Sale	(3) Trad. Sale	(4) Trad. Sale
$\Delta\text{Assess}_i / \overline{\text{Assess}}$	0.0394*** (0.0139)	0.0369*** (0.0126)		
$\Delta\widehat{\text{Assess}}_i / \overline{\widehat{\text{Assess}}}$			0.0562*** (0.00855)	0.0585*** (0.00920)
Sample	Full	Single Family	Full	Single Family
N	8952580	8342381	8952580	8342381
Number of Clusters	79	79	79	79
Adjusted R2	0.0131	0.0118	0.0132	0.0119

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. Table shows how true and forecast change in assessment share affect the likelihood of sale.

Table 2.8: Foreclosures

	(1) Foreclosure	(2) Foreclosure	(3) Foreclosure	(4) Foreclosure
$\Delta\text{Assess}_i/\overline{\text{Assess}}$	0.00557	0.00746		
	(0.00982)	(0.0104)		
$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$			–0.0178***	–0.0156***
			(0.00464)	(0.00495)
Sample	Full	Single Family	Full	Single Family
N	8952580	8342381	8952580	8342381
Number of Clusters	79	79	79	79
Adjusted R2	0.0243	0.0245	0.0243	0.0245

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. Table shows how true and forecast change in assessment share affect the likelihood of foreclosure.

Table 2.9: Home Equity Loans

	(1) Home Equity Loan	(2) Home Equity Loan	(3) Home Equity Loan	(4) Home Equity Loan
$\Delta\text{Assess}_i/\overline{\text{Assess}}$	–0.0520***	–0.0498***		
	(0.0117)	(0.0122)		
$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$			–0.0964***	–0.0951***
			(0.0110)	(0.0116)
Sample	Full	Single Family	Full	Single Family
N	8952580	8342381	8952580	8342381
Number of Clusters	79	79	79	79
Adjusted R2	0.0444	0.0450	0.0444	0.0450

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. Table shows how true and forecast change in assessment share affect the likelihood of new home equity loan origination.

Table 2.10: Sale Before Reassessment

	(1)	(2)
	Sale t–$1,t$	Sale t–$1,t$
$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$	0.00349	–0.00209
	(0.00755)	(0.00877)
Sample	Full	Single Family
N	8952580	8342381
Number of Clusters	79	79
Adjusted R2	0.0182	0.0189

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. Table shows how forecast change in assessment share affect the likelihood of sale before tax change takes place.

Table 2.11: Change in Assessment and Change in Tax

	(1) Trad. Sale	(2) Foreclosure	(3) Home Equity Loan
Home Value DecreaseXTax DecreaseX$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$	0.0513***	–0.0347**	–0.108***
	(0.0149)	(0.0147)	(0.0158)
Home Value DecreaseXTax IncreaseX$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$	0.0743**	–0.0254	–0.0365
	(0.0309)	(0.0299)	(0.0223)
Home Value IncreaseXTax DecreaseX$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$	0.0488***	0.00686	–0.0983***
	(0.0127)	(0.0166)	(0.0250)
Home Value IncreaseXTax IncreaseX$\Delta\widehat{\text{Assess}}_i/\overline{\widehat{\text{Assess}}}$	0.0625***	–0.0156**	–0.0992***
	(0.0134)	(0.00605)	(0.0186)
Sample	Single Family	Single Family	Single Family
N	8342381	8342381	8342381
Number of Clusters	79	79	79
Adjusted R2	0.0119	0.0245	0.0450

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics and census tract characteristics. Includes quartic polynomial of initial assessment, assessment change, and initial assessment ratio. FE for tax year X county. Standard errors clustered at county level. Regression estimates the effect of change in assessment share separately for homes that decreased in value and saw a tax decrease, homes that decreased in value and saw a tax increase, and the opposite.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.12: Δ Gross Tax Rate

	(1) [t]-[t-1]	(2) [t+1]-[t]	(3) [t+2]-[t+1]
Δ Median $\hat{A}_t$ / $\bar{\hat{A}}_t$	3.016	–52.177*	–18.547
	(18.259)	(28.670)	(24.957)
Med $\hat{A}_t$ / Med A_{t-3}	–22.357**	–2.040	–49.833***
	(11.265)	(12.810)	(16.041)
Log(Med. A_{t-3})	0.940	2.036	4.058*
	(1.291)	(1.659)	(2.104)
Constant	14.471	–17.321	10.419
	(14.675)	(18.759)	(24.358)
r2	0.202	0.123	0.180
N	1153	983	903

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

FE for County by Year. Standard Errors clustered at the school district level. Rates are in mils This table shows how changes in the ratio of median to mean home value within a school district affect voted gross tax rates for school districts.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.13: Δ Effective Tax Rate

	(1) [t]-[t-1]	(2) [t+1]-[t]	(3) [t+2]-[t+1]
Δ Median $\hat{A}_t$ / $\bar{\hat{A}}_t$	10.043	–59.730**	–25.127
	(17.210)	(27.679)	(21.847)
Med $\hat{A}_t$ / Med A_{t-3}	–69.983***	–8.232	–47.007***
	(10.898)	(9.833)	(15.797)
Log(Med. A_{t-3})	0.236	2.060	3.860**
	(1.243)	(1.395)	(1.829)
Constant	69.716***	–11.094	9.715
	(15.324)	(13.805)	(18.644)
r2	0.327	0.133	0.196
N	1153	983	903

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

FE for County by Year. Standard Errors clustered at the school district level. Rates are in mils. This table shows how changes in the ratio of median to mean home value within a school district affect effective tax rates for school districts.

Table 2.14: % Δ Collections

	(1) [t]-[t-1]	(2) [t+1]-[t]	(3) [t+2]-[t+1]
Δ Median $\hat{A}_t$ / $\bar{\hat{A}}_t$	0.012	–0.005	0.016
	(0.063)	(0.017)	(0.022)
Med $\hat{A}_t$ / Med A_{t-3}	0.739***	0.013**	0.013
	(0.049)	(0.006)	(0.008)
Log(Med. A_{t-3})	0.011**	0.012***	0.010***
	(0.004)	(0.001)	(0.001)
Constant	–0.819***	–0.135***	–0.111***
	(0.059)	(0.014)	(0.014)
r2	0.869	0.696	0.483
N	1153	983	903

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

FE for County by Year. Standard Errors clustered at the school district level. Outcome in percent change. This table shows how changes in the ratio of median to mean home value within a school district following reassessment lead to changes in collections from school district levies in subsequent years.

Appendix

2.A Appendix Figures

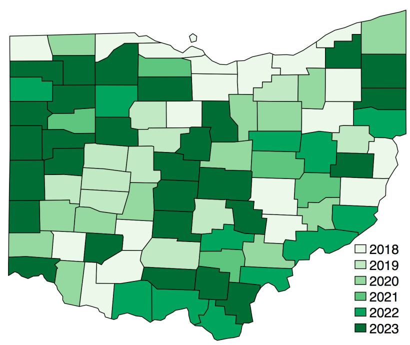


Figure 2.A.1: Reappraisal Schedule

2.B Appendix Tables

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.B.1: Price vs. Assessment

	(1) Log Price	(2) Log Price	(3) Log Price	(4) Log Price	(5) Log Price	(6) Log Price
Log(Assessed Value/.35)	1.172***	1.193***	0.983***			
	(0.00122)	(0.00198)	(0.00399)			
Log(Forecast Assessed Value/.35)				1.201***	1.227***	1.050***
				(0.00128)	(0.00208)	(0.00429)
Year One Only		X	X		X	X
Controls			X			X
N	563153	218682	218175	563153	218682	218175
Adjusted R2	0.621	0.623	0.706	0.611	0.614	0.705

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls are month-of-year by school district and year by school district fixed effects and hedonic controls. This table shows the relationship between assessed values and realized prices for houses that sell in logs. The sample is restricted to single family homes. Assessed values and forecast assessed values are divided by .35 because Ohio uses a 35% assessment ratio.

Table 2.B.2: Linear Controls

	(1) Trad. Sale	(2) Trad. Sale	(3) Foreclosure	(4) Foreclosure	(5) Home Equity Loan	(6) Home Equity Loan
$\Delta \text{Assess}_i / \overline{\text{Assess}}$	0.0361***		0.0129		–0.0512***	
	(0.0121)		(0.00959)		(0.0131)	
$\Delta \widehat{\text{Assess}}_i / \overline{\widehat{\text{Assess}}}$		0.0596***		–0.00172		–0.118***
		(0.00830)		(0.00513)		(0.0119)
Assess-A[t-3] (000)	–0.000871***		–0.000221*		0.00149***	
	(0.000301)		(0.000123)		(0.000397)	
Hat Assess -A[t-3] (000)		–0.00152***		–0.000100		0.00227***
		(0.000272)		(0.0000660)		(0.000333)
Ratio t-3	–0.00198	–0.000571	–0.000595	–0.000635	0.00654***	0.00409**
	(0.00202)	(0.00171)	(0.00172)	(0.00165)	(0.00222)	(0.00156)
Assessment T-3 (000)	–0.0000546	–0.0000669***	–0.000189***	–0.000196***	0.000218***	0.000223***
	(0.0000360)	(0.0000219)	(0.0000416)	(0.0000424)	(0.0000649)	(0.0000516)
N	8952580	8952580	8952580	8952580	8952580	8952580
Number of Clusters	79	79	79	79	79	79
Adjusted R2	0.0130	0.0131	0.0235	0.0235	0.0427	0.0430

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics, census tract characteristics, and quartic polynomial of initial assessment. FE for tax year X county. Standard errors clustered at county level.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 2.B.3: Forecast Error

	(1) Trad. Sale	(2) Foreclosure	(3) Home Equity Loan
Forecast Error	0.0236**	0.00962	–0.0187**
	(0.0100)	(0.00675)	(0.00744)
N	8952580	8952580	8952580
Number of Clusters	79	79	79
Adjusted R2	0.0131	0.0243	0.0442

Standard errors in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Controls for hedonics, census tract characteristics, and quartic polynomial of initial assessment. FE for tax year X county. Standard errors clustered at county level.

Chapter 3

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

(Co-author: Samuel Krumholz)

3.1 Introduction

When local jurisdictions have control over land use, proposed projects must create net benefits for the host community in order to be approved. However, many projects create external costs and benefits on vastly different spatial scales. For example, a large manufacturing plant may simultaneously increase local exposure to pollution and contribute to the global risk of climate change, while also boosting regional productivity and employment. This dynamic creates the potential for substantial inefficiencies; communities will refuse to approve projects that decrease local welfare even if they benefit society as a whole (e.g. a nuclear power plant that powers a region cleanly, but significantly lowers nearby home values), while approving projects that are

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

locally beneficial, but socially costly (e.g. a plant with large local employment effects that poisons the drinking water of down-river jurisdictions). Because local control of land use is very common in the United States (Gyourko et al., 2008), these types of inefficiencies likely have significant impacts on overall well-being.

In this paper, we study how local government control over the revenues created from property taxation impact these types of land-use decisions. Local property tax payments can act as a transfer from the externality-producing entity to the jurisdictions responsible for land use. If local governments spend tax payments efficiently, the payments should enhance the value of living within jurisdictions that have these types of projects, increasing their likelihood of approval. Conversely, state and federal policies that constrain the ability of jurisdictions to raise and retain local property tax revenues should mute this effect with potentially large implications for local industrial development and environmental quality.

Our goal in this paper is to evaluate this hypothesis empirically. We begin by testing the extent to which property tax payments from large projects are valued by local homeowners as measured through changes in home prices. This question is important not only as a necessary precursor to the second half of our analysis, but also because these benefits have the potential to change the distribution of groups that gain and lose from the construction of new capital projects; depending on the income and demographic characteristics of individuals inside and outside of the taxing jurisdiction, property tax revenues could either significantly dampen or amplify existing inequities in exposure to the projects' negative local externalities.

Our specific empirical context is the effect of power plant openings on school districts. Power plants exemplify the types of projects that create spatially divergent external costs and benefits,[1] while school districts are the majority recipient of property tax dollars and a major determinant of home prices and locational choice across the United States. We first look at the effects of these openings on a school district's tax base, property tax rates, revenues and

[1] An additional benefit of power plants from an identification standpoint is that they are relatively free of large positive local externalities such as agglomeration or employment effects.

expenditures in order to understand the magnitude of the plant's tax base effect and how this increase in fiscal capacity is used. We then examine the extent to which these changes are capitalized into home values.

To estimate the effects of these openings, we use a border difference-in-differences design, in which we compare outcomes before and after an opening in neighboring districts that did or did not receive a plant. To test the effect of this new tax base on home values, we use Zillow ZTRAX home transaction data and the same border difference-in-differences design, but restrict our sample to only home transactions within a mile of the border between the plant's school district and all neighboring districts to ensure that treated and control homes are similarly exposed to other positive and negative effects of the plant. In both cases, we show that control districts are both observably similar to and have similar trends in both outcomes and covariates to treated districts prior to the plant opening.

We find that property tax payments from these plants are both economically large and highly valued by homeowners. On average, an opening increases a host district's tax base per student by 10%. This tax base increase leads to both a small decrease in property tax rates and a larger increase in educational spending concentrated on capital expenditures. We further find that these changes are valued by local homeowners; home prices increase by 4-5% following a plant opening on the plant's side of the border relative to similar homes directly across the district boundary. Both sets of results are robust to a number of different specifications, sample definitions and an additional identification strategy leveraging variations in treatment intensity based on the size of the opening plant. These effects suggest that property tax payments by large plants act as a substantial local benefit for homeowners within the plant's jurisdiction.

We next test the second half of our hypothesis: restricting jurisdictions' ability to access property tax revenue should reduce their willingness to be exposed to externality-producing plants. To examine this question empirically, we use a series of state-level school finance reforms over the past half century that dramatically increased the magnitude of state education transfers tied to the level of local property wealth and/or imposed strict property tax limitations, both of which had the effect of reducing the value of tax base increases to local jurisdictions.

To estimate these effects, we employ a geographically-proximate county pair difference-in-differences design in which we compare counties in states that had a reform to nearby counties in states that did not have a reform (or whose reform would occur in the future). Because of data limitations surrounding the timing and location of old power plant openings, we use large manufacturing establishments and manufacturing employment per capita at the county level as our primary outcome of interest.

We show that these reforms led to meaningful (10%) declines in large manufacturing establishments and manufacturing employment per capita in the fifteen years following enactment both in absolute and relative terms. These findings suggests that reducing the tax benefits from large plants has a significant negative impact on local industrial development. Results are robust to a variety of specifications, covariates and weighting schemes and show no evidence of any pre-trends.

This paper makes several contributions to the existing literature. By providing new evidence that property tax payments produced by large externality-producing projects are highly valued by local homeowners, we build on previous work that has estimated other costs and benefits of large plants including decreased health and human capital among individuals affected by pollution (Luechinger, 2014, Barrows et al., 2018, Persico and Venator, 2018), lower home values near the plant (Davis, 2011, Currie et al., 2015, Gibbons, 2015) and agglomeration and employment benefits (Greenstone et al., 2010). Because these benefits accrue to many of the same individuals affected by the plant's negative externalities, they have the potential to act as partial compensation for these costs, which has significant implications for our understanding of income and racial disparities in who is helped and who is harmed by large, essential infrastructure projects (Boer et al., 1997, Banzhaf et al., 2019).[2] Finally, by showing that a shock to inputs

[2] Indeed, in this way, the local fiscal benefits they provide are very similar to those created by natural resource windfalls as shown in Marchand et al. (2015), Martinez (2016), Sances and You (2017), Bartik et al. (2018).

(non-residential tax base) of public goods provision leads to home price increases, we contribute to a broad public finance literature focused on the capitalization of local public goods (Oates, 1969, Black, 1999b, Anderson, 2006, Bayer et al., 2007, Nguyen-Hoang and Yinger, 2011) as well as providing new evidence that local politicians use these tax base shocks for the benefit of local homeowners rather than engaging in capture (Martinez, 2016).

By demonstrating that shifts in local governments' ability to retain property tax revenue significantly affect non-residential land-use decisions and industrial development, we build on a literature examining the development incentives embedded in fiscal decentralization. Previous theoretical work established the importance of local governmental incentives in encouraging economic growth (Weingast, 2009). While empirical work focused largely outside the United States has found support for the idea that local government's share of local (non-property) taxation influences local public good provision and economic development (Han and Kung, 2015, Careaga and Weingast, 2003, Burnes et al., 2011, Zhuravskaya, 2000), since reforms are often nationwide and come with large income and political consequences, well-identified studies of these effects are scarce (Gadenne and Singhal, 2014). Our results build on this work by presenting novel evidence from a large developed economy that fiscal centralization can have large impacts on local development. This finding is particularly important because in a federal system many higher-level policies aimed at other economic and social goals affect local control over property taxation. Our results imply that these policies may have significant unintended consequences.

Finally, these results contribute to a growing literature on the effects of centralizing and equalizing school finance reforms in the United States. These reforms have been shown to greatly increase low-income students' long-run educational and earnings prospects (Biasi, 2019, Miller, 2018, Lafortune et al., 2018, Jackson et al., 2015, Card and Payne, 2002), while also affecting local housing values by diluting the value of local tax dollars (Hoxby, 2001, Hoxby and Kuziemko, 2004) and changing local property tax burdens (Lutz, 2015, Ross, 2013). Our paper is the first to show an additional major unintended consequence of these reforms—by divorcing the size of the

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

local tax base from available revenue for schools, these reforms affected local non-residential land-use decisions and, in particular, the development of local industry.

The remainder of this paper proceeds as follows. Section 2 provides background on our institutional setting. Sections 3 and 4 describe our empirical strategies and main results. Section 5 concludes.

3.2 Background

In this paper, we first investigate the extent to which local property taxation from large capital projects are valued by local homeowners and then test how limiting this benefit stream affects jurisdictions' willingness to be exposed to these projects. To answer these questions, we undertake a number of separate analyses that rely upon institutional details in plant siting, local public finance, and state school finance systems. In this section, we provide some necessary background information in each of these areas to allow the reader to better understand the validity of the assumptions behind our identification strategies and the plausibility of our observed effects.

3.2.1 Plant Siting

Power plant siting is a complex process governed by a large web of state and local regulations.[3] Utilities take into account a number of factors when siting including access to transportation and energy infrastructure, construction costs and environmental concerns (Cirillo et al., 1977). There is typically a significant trade-off between the low-cost and low-environmental impact of locating in rural areas and increased electricity transmission costs (Davis, 2011).

Utilities also face significant 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

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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

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 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.

Local land-use decisions are typically governed by the local city council (in incorporated areas) or county commissioners (in unincorporated areas). In most states, school districts, the focus of our empirical study, have no control over local land-use.[4] 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 within the home municipality/county are concentrated among individuals within the same school district, we would again expect the relevant municipal leaders to internalize their preferences.

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

3.2.2 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.[5] 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-lieu 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).[6] Public schools receive the majority of property tax revenue and about 40% of state and local education funding on average comes from property taxes. Additionally, local property taxes are often the only source of funding over which school districts have direct control (Oates and Fischel, 2016).

In Figure 3.A.1 we show the importance of utility and industrial property to the tax bases of districts with plants. The top panel 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.[7] Among districts with no generating capacity, utility property typically makes up 5% of the total tax base (from infrastructure such as transmission lines and pipelines). However, this proportion rises quickly as generation capacity increases; in districts with 1,000 MW of generating

[5]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.

[6]Similarly, a large threatened downward reassessment of pipelines in Northern Minnesota was reported as being potentially disastrous for local municipalities and schools.

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

capacity, utilities make up 15% of the local tax base and in districts with 2,500 MW capacity they make up over 30% of the tax base.

In the bottom panel of Figure 3.A.1, we perform similar analysis for industrial plants. The more large polluting plants in a school district (a proxy for exposure to industry), the larger the share of industry as a proportion of total taxable value (bottom panel of Figure 3.A.1).[8] In both of these cases, because increases are driven by only a small number of plants, it suggests that these facilities are major contributors to the local tax base.

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 ("assessment ratio").[9] In some states, utility and industrial property have a different assessment ratio than other types of property leading to a higher or lower effective tax 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 limits 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.

3.2.3 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 fifty years (Jackson et al., 2015), moving from primarily locally-financed systems to systems with greater levels of state support.

[8]We use plants that report to the EPA's Toxic Release Inventory (TRI) as a proxy for large polluting plants.

[9]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.

These reforms have typically centered around ensuring some combination of 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 effect on both adequacy and equity. Hoxby (2001), Jackson et al. (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 fiscal capacity). In order to maintain equity, districts in many states also limit the tax rate that districts can charge above the amount that will provide their expected local share (and in some cases, the state can recapture any revenue above a certain threshold). The strictness of these limits varies dramatically across states.

In this paper, we are interested in understanding how changes in the marginal value of a locality's tax base affects industrial development. We study school finance reforms because in many cases they acted as a significant shock to this value. They both by change the upper and lower limits on taxes that can be charged and shift the degree of crowd-out of state revenues based on property wealth. To see this, note that a simplified funding formula common to many states is:

$$Rev_d = F_d + S_d - \tau^* V_d + \tau_d V_d \tag{3.1}$$

where F_d are federal transfers to the district (which are independent of the local tax base), S_d is the

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

state guaranteed funding to the district, which typically depends on local student characteristics but is independent of local tax base, τ^* is the state-assigned tax rate used to determine a district's local share (typically this is uniform across most districts within a state), V_d is the total assessed value of a district's property and τ_d is the district's chosen tax rate where $\tau_d \in (\underline{\tau}, \bar{\tau})$, state-set limits on the taxes that can be charged. In reality, these formulas are much more complicated, but because we are only interested in the effects of changing tax-base on revenue, this simple illustration acts as a good representation.

All else equal, increasing τ^* implies that growing a district's tax base will lead to a larger reduction in state funding, making any tax base increase less valuable. Similarly, creating a more stringent limitation on the taxes a district can charge also makes new tax base less valuable because districts are unable to fully access the tax base's potential revenue and put it towards their preferred use (assuming that the tax rate constraint binds).

Thus, to understand how a reform affects the marginal value of new tax base, we need to know both how a reform affects $\bar{\tau}$ (through new state limitations) and τ^*. In general, there is no simple summary statistic for either of these terms as funding formulas are written in such a way that the exact level of crowd-out and tax limitations will vary by district. For our analyses, we qualitatively describe these quantities for all states over the past half-century using *Public Finance in Public Schools in the United States*, a report issued roughly 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 formula in combination with the narrative descriptions in the report, we then identify major school finance reforms, in which large changes to crowd-out or tax limitations occurred for most districts within a state. Our process for identifying these reforms is discussed in much more detail in Section 4 and Appendix C.

3.3 Are Property Tax Payments from Large Capital Projects 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 who bears the cost and benefits of these types of plants—an issue of large policy interest and an area of active debate in the environmental justice literature (Banzhaf et al., 2019). Two, in order to answer our second question—how changing jurisdictions' ability to raise and retain property tax revenue affects local land-use decisions—it is first necessary to establish that such revenue is indeed economically significant and valued by local homeowners. We break our analysis 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.

3.3.1 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 information 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 and temporal scope.[10] The left panel of Figure 3.A.2 shows our data coverage

[10] Biasi (2019) and Miller (2018) both collect similar data, but their collection includes fewer states and is over a

by state. 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 the districts in our sample and this number increases to over 80% by 2015.[11] 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 during our sample period, 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.[12]

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 et al., 2016). For all fiscal years between 1995 and 2016, we have detailed data on revenue sources, expenditures by type, 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 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[13] and never underwent a boundary change or a district type (i.e. elementary, unified, secondary) change over

more limited time period.

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

[12] 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.

[13] 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. Because most metro areas typically have a single large center-city school district, large districts are unlikely to be a good counterfactual for neighboring districts.

our 22 year time period.[14] This leaves us with 11,824 total school districts.

Fourth, we use home transaction data from the Zillow ZTRAX database. This nearly-nationwide database contains almost all home transactions between 2005 and 2017 with much longer temporal coverage for some counties. There are 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 A provides greater detail on how the ZTRAX data were processed for this project. We currently use home price data from 14 states that have both comprehensive transaction coverage and a large number of plant openings.

3.3.2 Effects on School District Budgets and Property Taxes

Empirical Strategy

We begin by estimating the effect of power plant openings on school district fiscal outcomes. We focus on power plants because, although they create significant negative local externalities, they do not cause many of the other positive local externalities such as large employment or agglomeration effects created by other types of capital-intensive projects. This simplifies our empirical efforts to estimate the property taxation benefits provided by these plants because it is less plausible that any observed effects are being driven simply by greater economic activity within the district.

In an ideal world, we would randomly assign plants to some districts but not others and examine the changes in fiscal outcomes that ensued. Because such random assignment of plants to localities does not exist in the real world, we instead attempt to causally identify the effects of a plant opening using a border difference-in-differences design in which we compare tax base and school finance outcomes in districts experiencing an opening between 1995 and 2015 relative to neighboring districts that never experience an opening during this time period. We use

[14] 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.

neighboring districts as controls because they are exposed to many of the same local economic and school funding shocks as treated districts and so plausibly would be expected to have similar outcomes to the treated districts were they not to have received a plant—in subsequent subsections, we provide extensive evidence supporting this assumption.

We define a treated district as a district receiving a new utility-owned natural gas or wind turbine in any year between 1995 and 2015—our sample is made up of 55% natural gas plants and 45% wind plants.[15] In this paper, we are interested in the property tax effects of large projects—accordingly, we only include plants that are above 25MW in size, a common cutoff used by the EPA when determining eligibility for pollution control regulations.[16,17] There are 1,297 such plant openings in our data.[18]

We define the treatment year as the year in which the plant first obtained regulatory approval or began construction because 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 if districts begin borrowing or raising revenue after approval, using year of approval would bias our results towards zero. 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.[19] Of these districts, 675 fit our district sample criteria.[20]

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

[16] For example, only coal units greater than 25MW are required to participate in the Continuous Emissions Monitoring System (CEMS).

[17] For reference, if running 100% of the time a 25MW plant could provide power for roughly 25,000 homes. Since most plants' 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.

[18] We additionally drop 50 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 29 plants whose operation date was more than 5 years after construction approval.

[19] 75% of these districts only experienced a single opening. Results are robust to restricting our sample to only these districts.

[20] 59 districts had fewer than 200 students in 1995, 16 districts had greater than 50,000 students in 1995, 81 districts were not in operation for all 22 years or experienced a boundary change, 3 districts had fewer than 3 grades, 2 districts had greater than 1 log point annual change in students.

In the primary analysis, we restrict our sample to only those plants approved between 2001 and 2007 in order to create a fully balanced panel with six years of pre-data and 8 years of post-data. These cutoffs are data-driven; a large number of plants received approval in both 2001 and 2007 and our data begins in 1995 and ends in 2015, so choosing these cutoffs allows us to maximize our sample while maintaining sufficient pre-approval data to look for pre-trends and post-opening data to identify medium-run effects. Using a balanced panel addresses concerns raised by Goodman-Bacon (2018) that a difference-in-differences model with time-varying treatment and an unbalanced panel can place a negative weight on some years, producing an estimand of different magnitude (and potentially sign) from the average treatment effect. However, we also show that all results are robust to using the full unbalanced panel.

For each of the treated districts in our sample, we identified all neighboring districts within the same state that did not also experience an opening in our sample period and fit our sample inclusion criteria (i.e. in existence over the full period with greater than 200 students or fewer than 50,000 students at baseline). If control districts border multiple treatment districts then they will enter the sample multiple times. To adjust for this, we use two-way clustering at the plant district (all bordering control districts attached to a treated district) and district level. Different districts have different numbers of border pairs and so an unweighted regression would overweight districts with high numbers of pairs. To adjust for this, we weight all districts attached to a given opening by the inverse of the total number of border pairs the opening has in a given sample year. Using this sample, we then implement the following difference-in-differences specification:

$$Y_{dpt} = \alpha_d + \tau_{pt} + \beta Post_{dpt} * Treat_d + \varepsilon_{dpt} \tag{3.2}$$

where Y_{dpt} is the outcome variable in district d, in border pair p and time t, α_d is a district fixed effect, τ_{pt} is a 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 ε_{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 0–2 which are the years over which most plants are built and years 3–8 when most plants are already in operation). In our primary analysis, we show results for both all openings and "non-small" openings defined as having fewer than \$10,000 per student in expected tax base increase based on plant and district size (excludes about 10% of sample). Districts receive on average 0.5% of tax base per student in revenue, so openings of this size are expected to produce less than \$50/student in additional revenue. As a result, "non-small" openings are our preferred sample because they exclude the small proportion of openings that are unlikely to produce sufficient tax revenue per student to make a meaningful impact on district finances.

Table 3.A.1 shows differences in baseline covariates between treated and control districts. Unsurprisingly, given previous work in the environmental justice literature, there is a significant 2.2 percentage point difference in the proportion of underrepresented minorities (Black and Hispanic students) in the treated district relative to the bordering controls. However, there are no economically or statistically significant differences between treatment and control districts in number of students in a district, student free and reduced lunch status, school revenues or local home values, providing reassuring evidence that treatment and control districts are relatively similar. Of course baseline differences would not invalidate our design—what matters for identification is parallel trends—but nonetheless the fact that differences across most key covariates are minimal increases confidence in the validity of this strategy.

There are two major related challenges to identification in this setting. First, 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. We address this possibility in several ways. First, we examine whether pre-trends exist in our outcome variables as well as 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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

such trends. We additionally show that results are robust to increasingly restrictive, time-varying covariates as well as different specification and sample restriction choices, suggesting that these factors are not driving our results.

Second, we estimate an alternate identification strategy as an additional robustness check. We leverage the fact that the expected revenue per student effect of a plant opening in a district is a function of both the size of the plant's estimated effect on the tax base and the number of students that this new revenue will be split across. Accordingly, 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)[21] by the total number of students enrolled in the district in the year the plant received regulatory approval and include this variable in a triple difference framework. The underlying assumption of this analysis is that plants that have a higher value per student should create correspondingly larger tax base effects in their host district. 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 expected fiscal impacts and their assigned control districts.[22] Reassuringly, this design identified off of a completely different source of variation produces very similar results to our base strategy.

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 associated with our

[21] 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 or income, both of which should be correlated with construction costs. 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. More details on these calculations are provided in Appendix 3.C.

[22] Note that this is not simply the difference between districts receiving large and small plants, but is instead the interaction between plant value 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.

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 developmentally-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 model helps address this concern, but does not fully solve it—for instance, 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.[23] If local governments are attempting to attract new facilities, we would expect to see such an increase. Figure 3.A.3 shows 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 at least suggestive evidence that the construction of a plant is not a proxy for a larger development boom.

Results

Table 3.1 show the main effects of a plant opening on major tax base and school finance outcomes using our primary specification. A new plant increases the local tax base per student by 11% on average suggesting that plant openings can have large effects on the fiscal capabilities of local districts. This increase is used primarily to increase local school revenues; there is no economically or statistically significant effects on the local property tax rate. Conversely, a plant opening increases locally-raised district revenues by $500/student (10%) and total revenues per student by $409 (3.5%). This gap between local and total revenues is explained by a reduction in state funding. Many state school funding formulas tie the level of state transfers to a district's property wealth, so increases in the property wealth should leads mechanically to lower levels of state transfers for education. Finally, Column 5 shows that expenditures increase by $770/student

[23]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.

or more than 5% following an opening. These results demonstrate that having a plant enter a school district has important effects on local educational spending.

Figures 3.1 and 3.2 show these results in event study form. Reassuringly, across all outcomes there are no trends in the six years prior to plant approval. We then observe a rapid increase in tax base per student, local revenues per student, total revenues per student and total expenditures per student beginning two to three years following plant approval, after which these outcomes plateau at a significantly higher level. The opposite pattern is true for property tax rates, although the decrease is not statistically significant. This timeline makes sense intuitively; construction was typically completed two to three years after final approval and so this is precisely when we would expect to see the change in tax base (and by extension district revenues) appear in the data.

Table 3.3 shows where the additional revenues created by the plant are spent. We first see that openings lead districts to take a large amount of additional debt ($750-$1,100 per student or 15%-25% increase). This increase in debt explains why plant openings appear to have a larger effect on expenditures than on revenues. 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 allows local governments to use their additional tax base to increase school budgets when any other increase in local revenue would simply crowd out of state transfers.

In general, most school district debt is used to fund capital expenditures. We observe a similar phenomenon here. Despite making up only 10% of total school spending, the 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 $400/student (33% 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 school district responses to other forms of revenue shocks (i.e. Davis and Ferreira

(2017)).

We finally examine changes in school district revenues by source. If these changes are driven by plant openings, we would expect several dynamics to hold. First, changes in local revenue should be driven by property tax revenue, increased parent government contributions for districts that are not financially independent[24], or payment-in-leiu of taxation (PILOT)[25] We should expect no change (or a compensatory decrease) in other sources of local funding such as sales and income taxes. Second, we should expect a decrease in state aid from the state's school finance funding formula as almost all states now tie formula aid to a district's level of property wealth. Finally, we should expect no changes in other state funding or federal funding.

Table 3.2 shows the main results of this analysis and they accord exactly with the predictions above. The bulk of the increase in local revenues comes from increased property tax revenues. However, we also see significant increases in parent government contributions and income from unspecified sources, which likely comes from PILOT payments. There is no change in local revenue from other sources, which largely consists of sales and income tax revenue. We also observe a significant decrease in state transfers through formula aid; this is precisely what we would expect given that formula aid in most states is inversely proportional to a district's level of property wealth. Finally, just as we would expect, there is no change in other sources of state funding or federal revenue. These results provide additional suggestive evidence that the observed changes are indeed driven by the plant opening and not other correlated trends in district school finance.

We next turn to examining the robustness of our results to different specification and sample exclusion restrictions. Table 3.A.2 shows results after controlling for baseline covariate

[24] In some states (primarily states in the Northeast and Mid Atlantic, as well as some large cities), school districts are funded through municipal budgets rather than having independent budgetary and taxing authority. In these states, increased local property tax revenue would be classified as parental government contributions rather than property tax revenue since the district's revenue is technically coming from the parent government.

[25] Some plants may negotiate tax abatements with local school districts that lead them to make transfers to local district outside of the tax system. Further, municipal, state and federal utilities are often mandated by law to pay PILOT because they are exempted from local property taxation.

by state by year fixed effects. If baseline differences between treated and control districts were driving results, we might expect to see effects diminish once these trends are controlled for. However, results remain nearly identical to the full specification. Table 3.A.4 shows effects when including all districts instead of excluding those with a very small expected tax base per student impact. Unsurprisingly, effect sizes fall slightly but remain highly economically and statistically significant. Table 3.A.3 shows results with an unbalanced panel including ten years of data before and after an opening. Results again remain largely unchanged. Lastly, Table 3.A.5 shows effects separately for natural gas openings and wind turbine openings. In both cases, results are highly economically and statistically significant suggesting that effects are not driven by one type of opening. Effects are about twice as large for wind turbine openings than natural gas openings—this is likely because wind turbines open in largely rural areas with little property wealth per student where the expected tax base effect per student is much larger.

Table 3.A.6 shows the results of our triple difference design. We use the same sample as in our primary analysis, but interact the post by treat term with logged expected tax base per student. All outcome variables are also logged to create an elasticity.[26] Results are highly consistent with the primary analysis despite being identified off of an entirely different source of variation (opening vs no opening relative to the slope based on size of opening). A 10% increase in expected tax base per student leads to a 0.4% increase in tax base per student, a 0.6% increase in local revenue per student, a 0.13% increase in total revenue per student and a 0.2% increase in expenditures for student. For context, moving from an opening at the 25th percentile of expected tax base per student to an opening at the 75th percentile results in an increase of 2 log points, or a 8% increase in tax base per student, a 12% increase in local revenue, a 2.5% increase in local revenue and a 4% increase in total expenditures or roughly similar to the effect sizes found in our difference-in-differences approach.

Interestingly, one exception appears to be the effect on property tax rates. These rates

[26] We use the log of expected tax base per student because this variable is heavily right skewed and results are otherwise sensitive to how we treat and define outliers.

increase with expected tax base size—a 10% increase in tax base leads to a 2.6 mill increase in tax rate. Likely, this is because increased tax base has two competing effects on the tax rate. An increased tax base makes districts richer, which through an income effect should decrease rates. However, at the same time an increased tax rate lowers the price of debt (which is financed through tax rate increases to repay bonds), which can lead to tax rate increases. In this case, it appears that larger tax base increases are more likely to induce districts to undertake large capital projects and therefore higher tax rates.

As a final robustness check, Figure 3.3 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 black and Hispanic share of enrollment, the proportion of students eligible for free lunch (a proxy for poverty) and log total enrollment. There are no significant trends in any outcome prior to plant approval increasing confidence that any changes in school finance outcomes were not driven by underlying trends in district demographics in treatment relative to control districts. We also do not observe any large changes after approvals—there is some evidence for a slight decline in free lunch students seven to eight years after approval, which suggests some sorting may be beginning to occur in the long-run, but this effect is not statistically significant. Together these results suggest that our observed effects are unlikely to be driven by differential trends across treatment and control districts nor are they driven by sorting occurring after a plant opens.

In sum, the results from this subsection suggest that plant openings can have large impacts on the finances of host school districts. Through their direct tax base effect, these plants lead to meaningful increases in total revenue per student and expenditures per student, with a particularly large effect on capital expenditures. We now turn to estimating how households values these changes.

3.3.3 Effect of Plant Openings on Home Prices: Empirical Strategy

The previous section showed that property tax payments arising from plant openings lead to appreciable increases in education spending centered on capital expenditures. However, it is unclear if such spending actually increases the well-being of local homeowners. Instead, such payments could be captured by local bureaucrats or simply spent in a well-intentioned way that was not valued by local homeowners. In this subsection, we estimate empirically if the fiscal benefits created by plant openings are valued by local residents as measured through changes in home values.

Empirical Strategy

In this section, we want to estimate the hedonic value of the increase in tax base caused by a plant opening. To obtain a valid estimate of the effect of tax base increases on home values, we need to compare homes whose values are both expected to evolve similarly in the absence of a plant opening *and* are similarly affected by the non-fiscal positive and negative effects of the opening. In other words, we want to hold exposure to all other positive and negative plant effects constant and just estimate the home price effect of the tax base increase. Our results from this section should not be interpreted as the net home price effect of a plant opening, but instead as the component of the net effect caused by its effect on local school district fiscal capacity.

As described above, one advantage of using power plant openings as our setting is that these plants have relatively small agglomeration and employment effects and so we are mostly concerned with differential exposure to these plant's negative externalities. Accordingly, to estimate the home price effects of the property tax shock alone, we use the same border difference-in-differences approach as above, but instead of comparing whole neighboring districts, we restrict our sample to only a narrow bandwidth around the border. Specifically, we create border pairs between all homes that are in bordering districts with a plant opening and neighbors with no openings and are within two kilometers of the border. We then compare the relative change

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

in housing values on either side of the opening after plant approval.[27] We assume both sets of homes are exposed to similar economic and pollution shocks from the plant, but only homes on the plant-district's side of the border will receive the benefit of the expanded tax base—we test this assumption empirically in great detail and find no evidence for any violation. If this parallel trends assumption holds, we can then attribute any changes in home prices to the fiscal effect of the plant. We perform this analysis for openings in fourteen states that have comprehensive home price data and the largest number of openings.[28]

One important caveat is that many school district boundaries are shared (or nearby) county and municipal boundaries. This implies that 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 same border. We test for robustness by excluding district boundaries that are shared with county boundaries and results are qualitatively similar, but there are many other local government taxing units (i.e. municipalities, irrigation districts, sewage districts, etc) 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 likely be too small to obtain valid statistical estimates. Because of these shared boundaries, we do not use a triple-differences approach as a robustness check, since 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 or other governmental units (i.e. a plant opening in a small district could be in a very large county or city, while a plant opening in a moderately sized district may be in a very small municipality). However, we do show robustness to using different expected tax base per student cutoffs and results increase with the expected tax base per student cutoff, just as we would expect.

Our primary specification is as follows:

[27] If a home is near multiple borders we assign it to its nearest border.

[28] Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

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

where Y_{idpt} is the logged sales price of home i in district d in border pair p and year t. The vector α_d contains indicators for extremely granular spatial controls. In the primary specification we use border pair x district x .004 degree x .004 degree latitude and longitude cells to ensure that we are comparing homes in very similar neighborhoods, but show robustness to more or less granular controls including a parcel indicator in which our estimand is completely identified off of multiple sales of the same parcel. The vector τ_{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. The coefficient on $Post_{dpt} * Treat_d$ is our coefficient of interest. Our primary model uses a bandwidth of 2,000 meters, but we show robustness to alternate bandwidths (1,000 meters-3,000 meters).

As with the analysis above, we weight observations such that each plant-opening year counts equally. One challenge with that approach in this setting is that some openings in rural districts have very few (i.e. < 2) home sales transactions per year. Because a single transaction per year provides a very noisy estimate of how local home values are changing, using these openings in our analysis would decrease our power considerably. As such, we restrict our sample to only bordering districts that each have a population of at least 10,000, while showing robustness to including all openings. As above, we restrict our primary sample to only openings that have an expected tax base impact larger than $10,000 per student.

An alternate approach is to run the regression at the home transaction level. This implicitly gives more weight to openings whose border regions have a higher number of transactions. The challenge here is that transaction-dense areas typically are part of school districts with large numbers of students—as such, these are exactly the openings where we expect the fiscal impact per student (and by extension, the effect on home prices) to be lowest. We do show robustness to using this unweighted approach with different expected tax base increase per student impact

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

thresholds and as the threshold increases, results look increasingly similar to the weighted approach.

The Zillow home transaction data used here does not go back as far as the school finance data for many counties. As a result, we cannot use a fully balanced panel here as we will have too few openings to have sufficient power to detect reasonably-sized effects (27 openings). Thus, our primary specification uses an unbalanced panel in which all openings have data at least two years prior and two years subsequent to the plant approval year. We show in robustness checks that coefficients when using a balanced panel look similar, but are estimated very imprecisely.

The primary threats to identification using this empirical approach are twofold. First, as with the school finance analysis, homes outside the district may not be good counterfactuals for homes inside the district. We test this assumption in several ways. First we employ a dynamic difference-in-differences 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; there is a minimal and insignificant effect among districts with a small expected tax base impact. This result suggests that there is nothing specific about the types of districts receiving plants that are driving our results.

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, these types of positive effects are less of a concern. Instead, it is more likely that as the bandwidth of included sales increases, there will be unequal exposure to the plant's pollution, which would likely bias our results downward. We test for this possibility in two ways. First, we show robustness to a large number of border bandwidths and

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

results are qualitatively similar. Second, we show results when including only border regions that are "far" from the plant (where far is defined as the nearest home to the plant being at least 5 km away) as these border regions should be "uncontaminated" by other effects of the plant. Again results do not exhibit large changes.

A final identification check leverages the unique school finance system in California. California's school funding formula is very strongly tied to a locality's property wealth; outside of taking on debt, it is almost impossible for a school district to increase its revenue flow from an increase in the tax base because any revenue increase will be crowded out one-for-one by a decrease in state funding.[29] Thus, in addition to showing the pooled results, we show results separately for our California and non-California samples. If the observed home price effects are truly driven by the local fiscal effects caused by a plant opening, we would expect that the results should be larger for the non-California states—indeed that is precisely what we see in the data.

Before moving to the results, it is important to reiterate here that this specification attempts to estimate the effect of the tax base increase on home prices alone, *not* the net effect of a plant opening. Previous research has shown both wind turbines and natural gas plants have negative effects on the values of nearby homes. Because our goal here is to understand the extent to which the tax base increase from these plants is valued by local homeowners, our aim is to hold these negative distance-based effects constant and estimate only the effect of the tax base increase itself. Observing positive effects here does not contradict these earlier results—previous work has estimated something akin to the slope of home price changes with respect to distance to the plant, while we are estimating how the intercept of being in a given district that receives a plant changes after the plant enters. Both effects are possible and their relative magnitudes will dictate both the average net effect of the plant on local home values, as well as which households gains and which lose from a plant entering.

[29]Technically, the formula is even more extreme. Most school districts in California do not receive their base local funding from the tax base in their district, but rather as a share of the total county tax base. The exception to this is taxes raised to repay bonds, which are levied only on the tax base within a district's borders.

3.3.4 Effect of Plant Openings on Home Prices

We begin with an estimate of the home price capitalization of the increased tax base created by plant openings. Table 3.4 shows the main effects. Column (1) show the results for our full geographic sample when restricting to districts with greater than 10,000 population. By 3–8 years following approval, home prices increase by 4.8% suggesting that homeowners meaningfully value the increased tax base created by the plant. In Columns (2) and (3) we can see that as expected these results are much larger when excluding California, which has a very strong school finance equalization system, from our sample. Home prices increase by 7% outside of California, but only by 2.8% within California and the effect is not statistically significant. In Columns (4)–(6), we show the same specifications, but including all districts, even those with very low populations. Results remain very similar although standard errors increase.

The left panel in Figure 3.4 shows these results in event study form for our primary specification. Because we are identified off of 71 openings, individual year estimates are somewhat imprecise. Nevertheless, there do not appear to be any trends prior to plant approval followed by a sustained increase which plateaus three to four years after approval. This provides some evidence that the observed effects are not simply driven by differential trends between the border regions. The right panel in Figure 3.4 shows the home price effects across a number of different distance bandwidths. Coefficients are extremely similar for any bandwidth between 1 kilometer and 3 kilometers suggesting that our results are not driven by bandwidth choice.

Table 3.5 shows results using different expected tax base cutoffs. Columns (1)–(3) show results weighting all plant opening years equally, while Columns (4)–(6) show these same results, but weighting by transaction, which implicitly gives more weight to more populous districts. Several trends are immediately apparent. First, even when including openings with less than \$10,000/student in expected tax base increase, results in the weighted specification remain economically and statistically significant. Second, as we restrict the sample to a higher expected tax base cutoff, effect sizes increase significantly. For districts with an expected increase of greater than \$75,000/student, home prices increase by 7% and for those with an expected increase of greater than \$150,000/student, home prices increase by 10.5% in the weighted specification and by 5% and 7% respectively in the unweighted specification where the average expected tax base effect is lower because districts with large numbers of students receive more weight. This is precisely what we would expect if effects are indeed driven by the tax base effect of a plant and not other underlying district characteristics. Finally, effects disappear when we include the full sample in the unweighted specification because the average expected tax base per student shrinks significantly (\$38,000/student compared to \$108,000/student in the weighted specification). Again, this is consistent with effects driven by the size of the fiscal impact.

One potential confounder of our results would be a change in the composition of homes sold in treated districts after the plant opens. We attempt to control for this in our main specification by controlling for hedonic variables and very granular geographic fixed effects, but it is of course possible that other, unobserved home characteristics are driving the results. We address this concern in two ways. First, Table 3.A.7 tests whether key hedonic characteristics differentially change in homes sold in treated districts after the plant receives approval. There are no economically or statistically significant differences in lot size, home age, square footage or whether or not a home is single-family home. There is a marginally significant decrease in the number of bedrooms in homes sold, but this effects is very small (.05 bedrooms) and unlikely to explain the large changes in home prices observed here. While we cannot observe changes in unobserved characteristics, the lack of large changes among the variables we do see increases confidence that our results are not driven by higher-quality homes being sold in the treated district during the post period. Second, Table 3.A.8 shows results using our main specification, but including only repeat sales, which hold all time-invariant characteristics of a house constant. Standard errors increase as our estimates are only identified off of homes that have multiple sales within our sample period, but results are very similar to our primary analysis suggesting that compositional changes in the underlying types of houses sold are unlikely to be driving our results.

We can also examine if the quantity of homes sold changes after an opening. Because a plant opening causes a large shock to local public good provision, we might expect that households will respond by reoptimizing, increasing home sales, Further, if, as we showed above, the plant causes the tax price of public good provision to fall and there are few supply constraints, we may also expect that districts receiving a plant will see an increase in new construction as in Lutz (2015). Note that as long as the composition of homes is not changing conditional on our covariates (which includes controls for new homes), an increase in quantity will not bias our home price estimates. Table 3.A.9 presents some evidence that the opening does indeed induce reoptimization; home sales in plant's district increase by 22 sales/year (25%) following an opening, although the effect is only marginally significant. This increase is driven by both sales of old homes and new construction—the probability that a plant district has any new construction increase by 8 percentage points or 20% ($p<.05$) suggesting that builders are responding to the decreased tax price of public goods created by the plant opening. To ensure that our price results are not driven by new construction, Table 3.A.10 shows our main specification excluding newly constructed homes. Results are similar with new homes excluded from the sample.

Table 3.A.11 shows a number of additional robustness tests that aim to rule out alternative explanations for these effects. Column (1) shows results when restricting to border pairs whose closest house is at least 5 km away from the newly-opened plant, while Column (2) shows results when dropping boundaries that are also county borders. If anything, home price effects are larger with both restrictions suggesting results are unlikely to be driven by other direct effects from the plant (i.e. land payments or increased housing demand), nor by increases in county instead of school district fiscal capacity. Column (3) shows results using a fully-balanced panel. Standard errors increase as our sample size shrinks, but effect sizes remain similar suggesting that our unbalanced panel is not driving results. Columns (4)–(7) show results with different levels of time and geographic fixed effects. Regardless of whether we use district fixed effects, .004 degree, or

.001 degree as our geographic region fixed effects, results remain extremely similar. The same is true whether we use border by year by month fixed effects as in our main specification, or border by year fixed effects. Together, these results provide additional reassurance that the observed home price effects are not driven by a single specification choice.

Table 3.A.12 shows results by plant type. As with the school finance results, effects are significant for both natural gas plants and wind farms, but much larger for wind farms. Districts receiving a natural gas opening have to a 3% increase in home prices, while those receiving wind turbines have a 12% increase. As above, this difference between the two is likely because wind farms are disproportionately located in rural areas with few students so they lead to larger increases in tax base per student. That we see the energy source with a greater school finance impact cause a larger home price effect adds additional reassurance that our observed results are indeed causal.

Finally, Table 3.A.13 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. The odd columns show results for the full sample and the even columns show results without California. When including California, results are sightly smaller in magnitude than were observed in Section 3.3.2 ($\approx$ \$300/student increase in local revenue/student, \$250/student increase in total expenditures per student) and statistically insignificant as standard errors increase relative to 3.3.2 given our smaller sample size. When excluding California, effect sizes more than double and become statistically significant reflecting California's strong school funding equalization program. That we see correspondingly larger home price effects when excluding California from our analysis again increases confidence that we are uncovering the causal effect of a plant's tax base effect.

Together, the results in this section imply 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, but this does not appear to be occurring in practice. One mechanism through which this capitalization likely occurs is the construction of new schools and other capital improvements. We lack exogenous variation on school construction conditional on receiving a plant, but previous work has suggested that school construction leads to a roughly 6% increase in home prices, even when it is funded using increased taxes on existing properties (Cellini et al., 2010, Lafortune and Schonholzer, 2019). This effect size is very similar in magnitude to our observed results.

3.3.5 Valuation of Negative Externalities

The above analyses provide 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 also brings with it significant negative externalities. In order to better understand the relative magnitudes of the positive effects identified above to homeowners, we attempt to benchmark these effects by estimating the effect of these negative externalities on nearby residents.

To do this, we use a spatial difference-in-differences model comparing homes that are nearby the plant relative to those that are further away before and after a plant opens. In particular, our design follows previous work by Davis (2011) and Gibbons (2015) who estimated the effect of natural gas plant openings and wind turbine openings respectively on home prices. The main innovation in our analysis is that we include school district by year fixed effects in order to control for any positive fiscal effects the plants may have on home values. In this way, we are ensured of estimating the negative effect of the plant's non-fiscal externalities alone and can hold constant the benefits of the district's increased tax base. Specifically, we estimate:

$$Y_{idpt} = \alpha_{dpt} + Dist_{idp} * Post_{dpt} + Z_{idpt} + \varepsilon_{idpt} \tag{3.4}$$

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

where Y_{idpt} is the log sales price of home i in district d near plant p in month-year t, α_{dpt} is a plant by year by month fixed effect, $Dist_{idp}$ is a variable capturing distance to the plant (either 5 kilometer bins or log distance), $Post_{dpt}$ is a vector of indicators for various time periods following the plant opening, Z_{idpt} is a .004 latitude x .004 longitude fixed effect, which compares homes within the same .25 mile by .25 mile grid cells and ε_{idpt} is a mean-zero error term. We restrict our sample to only plants that are larger than 100 megawatts in order to ensure that we are considering plants large enough to have a negative impact on the nearby community. We also perform a robustness test in which we interact our post by treat indicator with plant size under the assumption that effects should increase with the size of the plant. As before, we weight all plant openings equally and therefore restrict our analysis to only plant openings in districts with greater than 10,000 population. Our primary analysis considers homes within 20 kilometers of the plant, but we show robustness to other cutoffs. We also exclude homes within 500 meters of the plant because there is a concern that these parcels could have been purchased in the construction of the plant themselves, which may bias our results.

Table 3.6 shows the main results. Columns (1) and (2) show the results with indicators for home distance to plant for an unweighted and weighted regression respectively. Relative to homes 10–20 km away, home prices fall by 4%–6% within 5 km of the plant. Columns (3) and (4) show that this result is not simply a function of the distance bins used; here we interact log distance with plant opening and show that for each log point closer to a plant a house is, its price falls by 3–4% following an opening. Finally, Columns (5)–(8) provide an additional robustness check by interacting our post x distance variable with the size of the plant—if the effect is truly driven by the plant opening, we would expect the negative effect to be larger for bigger plants. Indeed, that is precisely what we see here; relative to homes 10–20 km from the plant, for each 100 MW increase in plant size, the home price effect of the plant decreases by an additional 1.5%. We see a similar result when using log distance instead of distance bins.

Figure 3.5 shows these results in event study form with the log distance specification on the left and the distance bin specification on the right. In both cases there appears to be no trend prior to approval and then a sharp decrease in prices for homes closer to the plant[30] beginning two to three years after approval, which then plateaus at the new, lower level. This is precisely the temporal pattern we would expect if the price change was caused by the plant opening. Table 3.A.14 tests the robustness of our main specification to different sample restrictions and covariates. Columns (1)–(2) show the results when restricting to homes within 15 km of the plant instead of 20 km—if anything effect sizes increase. Columns (3)–(4) restrict our sample to be fully balanced—only 27 plants meet this criteria so our standard errors increase considerably, but coefficients are qualitatively similar to those of the main analysis, albeit statistically insignificant. Finally, Columns (5)–(6) include parcel fixed effects and so are identified off of the same parcel transacting at different times. Results are very similar to the main specification suggesting that changes in the composition of homes sold following the plant opening are not driving the observed effects.

3.3.6 Implications of Home Price Analysis

These results provide evidence that households negatively value proximity to natural gas plants and wind turbines and that the average magnitude of this distaste for households nearby the plant (at least as proxied through home values) is roughly similar to the average district-wide gains following a plant opening. This finding has three important implications that are worth discussing in more depth. First, it suggests that hedonic estimates purporting to capture the disamenity of living near power plants and other large industrial plants must be sure to adjust for jurisdiction by year fixed effects in order to produce unbiased results. If not properly controlled for, estimates may be comparing homes nearby the plant that receive both the benefits of the increased tax base and the disamenity value from the plant to homes far from the plant that receive less of the tax

[30]Note that for log distance this appears as a positive coefficient because it is the effect of being an additional log point away from the plant site.

base benefit (assuming some homes are outside the plant's district) and less of the disamenity. Estimating this joint effect would lead to an underestimate of the true disamenity value caused by the negative externalities created from these types of projects. We can see that in our setting by estimating the same model as above, but excluding school district by year fixed effects. Table 3.A.15 shows that when implementing this specification effect sizes decrease by nearly two-thirds since we are now capturing the joint effect of a larger tax base and increased exposure to negative externalities caused by the plant. In other words, had we not controlled for the fiscal impact of these projects on local communities, we would have significantly underestimated the negative value created by these plants' pollution, noise and other negative externalities for nearby residents.

An related point is that even if hedonic estimates do properly control for district effects (or are estimating values in places where both the control and treatment groups are in the same district), our results suggest that these estimates cannot be used to back out the change in welfare or even the net change in home prices experienced by a given household. These types of hedonic estimates capture the negative gradient with respect to distance to a plant, but our results suggest that these plants also create a positive shift up in home prices for all homes within the plant's district. If the size of the home price increase is larger than the negative price effect caused by a plant's negative externalities, it's possible even a home nearby a plant could experience a net increase in home value, albeit an increase that would be less than that of its neighbor who was further from the plant.

Second, these results have important implications for the distributional consequences of power and other large industrial plant openings. In general, homeowners within a district receiving a plant that are far from a plant itself will benefit from the plant's entrance—these households receive the benefit of increased tax base, but face none of the negative externalities created by the plant. Homeowners both nearby the plant and in the plant's district will experience an ambiguous effect on home values as they both gain the benefits of increased tax base, but also face the costs of these plants local disamenities. Finally, homes nearby, but not in, the same district as the plant will experience an unambiguous decrease in home values.

Table 3.A.16 shows the demographics of census tracts within these three location categories. All coefficients are relative to being in the plant's district, but more than 15 km from the plant. We can see that tracts that are within 5 km of the plant, but outside the plant's district—in other words tracts that see an unambiguous loss from the plant—have 7% lower median incomes, 9% lower home values, 5 percentage points fewer homeowners and 4 percentage points fewer white residents than tracts that gain an unambiguous benefit (inside the plant's district but more than 15 km away). Homes within the plant's district but near the plant are also much worse off than those far away, although their median income and median home value are slightly higher than nearby tracts outside the plant's district. These results suggest that despite the fact that plants benefit host communities by increasing the local tax base, the net effect of these plants remains highly unequal. Indeed, because households nearby the plant, particularly those near the plant but outside the plant's district, are much poorer and more likely to be nonwhite than households inside the plant's district but far from the plant, these tax base increases may actually serve to exacerbate rather than dampen the inequalities already created through these plant's disparate negative impact.

Lastly, these results imply that property tax payments from a plant create a meaningful benefit to local homeowners within the receiving district of a similar magnitude to the negative value created by a plant's pollution and other negative externalities (i.e. noise and shadow flicker for wind turbines). Given the size of this effect, we might expect that access to these revenues could be an important driver for incentivizing local jurisdictions to allow these types of negative-externality producing developments to go forward. In the next section we investigate this question explicitly by exploiting cross-sectional and temporal changes in local jurisdictions' ability to access the local tax revenue generated by large industrial plants and examining how these changes affect local exposure to these projects.

3.4 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 homeowners. This finding suggests that restricting communities' access to such payments may have large effects on their willingness to allow large negative externality-producing projects to enter into their communities. In this section, we test this question empirically by examining how negative shocks to jurisdictions' ability to use their local tax base to fund local public goods affects their exposure to large manufacturing and power plants.

3.4.1 Data and Empirical Strategy

In this section, we use a series of school finance reforms that occurred across US states between the 1970s and 2000s 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 3.2.3, 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 affected 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. Following the reform, increases in property wealth would lead to an offsetting 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 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 reforms are difficult (Hoxby, 2001). Many court decisions led 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 were financed (Lafortune et al., 2018). In our analysis, we identify reforms by isolating major shifts in the amount of crowd-out or tax limitations embedded within state school funding formulas. In order to find these shocks, we first document state school funding formulas going back to 1962 using *Public School Finance Programs of the United States*, a report 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 crowd-out caused by an additional dollar of tax base or on the level of taxes that a district could charge. Years with substantial shifts in either of these variables were defined as reforms—if a state had multiple major reforms we used the first reform as our event.

Figure 3.A.5 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 nearly 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 years, we implement a geographically-proximate border pair difference-in-differences analysis, adapting the methodology used by Dube et al. (2010) in their analysis of the employment and wage effects of minimum wage changes. Specifically, we stack all cross-state county pairs whose centroids are within an x mile radius in the United States. In our primary analysis, we use x=60, but show robustness to larger and smaller bandwidths. Because pairs increase exponentially with x, we restrict each county to its twenty closest pairs to ensure results are not driven by more geographically distant counties. Again we show that results are robust to using fewer or all possible pairs.

We define as treated the county in the pair whose state received a reform first (i.e. if one county in the pair had a reform in 1975 and the second county in the pair had a reform in

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

1990, the 1975 county would be treated). We define the year of the event for the pair as the year of the earliest reform event occurring in either of the pair's states. We drop all years after the second county experiences a reform so as to create an uncontaminated control group. In our primary analysis we maintain a balanced panel by keeping only pairs that have at least 8 years of data before the event and more than 14 years of data after the event, but we show robustness to estimating effects for an unbalanced panel over a broader window. Our estimating equation is as follows:

$$Y_{cpst} = \alpha_{cps} + \tau_{pt} + Post_{pst} * Treat_{cst} + \varepsilon_{cpst} \tag{3.5}$$

where Y_{cpst} is the outcome variable in county c, as a part of county pair p, in state s and year t, α_{cps} is a time-invariant fixed effect for each county in a given county pair, τ_{pt} is a county-pair by year fixed effect so that all time shocks common to both counties in a pair are differenced out, $Post_{pst}$ is an indicator for whether or not a reform has occurred, $Treat_{cst}$ is an indicator for whether or not a given county is "treated," and ε_{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, manufacturing employment and power plants as our proxy for local externality producing projects. We view manufacturing plants and establishments as our primary outcome measure for three reasons. First, there are data limitations in the location and timing of power plant openings going back to the early 1960s. We are able to reconstruct the presence of plants that were owned by all utilities still in operation in 1990 and that were still in operation or had retired subsequent to 1975. However, data on any plants that may have been owned by utilities that went out of business prior to this period as well as plants or units that retired prior to 1975 are unavailable. Conversely, County Business Patterns provides comprehensive data on the stock of manufacturing employment and establishments by county going back to 1964.[31] Second, power plant openings and closings are relatively rare events,

[31] We are in the process of digitizing data to extend our sample back to 1948.

while changes in large manufacturing establishments although still rare are far more common. Thus, using manufacturing as an outcome greatly increases our power to detect an effect. Finally, because manufacturing plants bring large regional employment and agglomeration effects, we believe the effect on manufacturing location likely has larger economic implications and is thus of greater interest than the effect on power plants.[32]

As described above, our manufacturing data come from County Business Patterns, an annual measure of employment, establishments and payroll within a county by industry type. For this reason, we perform our analysis at the county, not district, level. We have data from 1964 to 2013 for all two-digit NAICS industries.[33] 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 choose to restrict our sample to only counties with greater than 1,000 population in 1970 (prior to any reforms). Results are robust to including all counties in our sample as well as restricting our sample to only counties with greater than 10,000 population in 1970 for which employment is almost universally available for the entire time period of the sample.

We show results from regressions weighted both such that all counties count equally and such that all reforms count equally. Our preferred specification weights all counties equally as this maximizes power, but results are similar (or larger) when weighting all reforms equally. We also show further robustness to not weighting or weighting by population. To account for county duplicates across pairs we cluster all standard errors at the state and border region.

There are two major identifying assumptions for this analysis to be interpreted as the

[32]We cannot perform a parallel analysis to Section 3 with manufacturing plants because data on opening date and location are not publicly available. Further, they would be a less desirable subject for such analysis because their large regional employment and agglomeration effects would make estimating the home price and school finance effects of the tax base changes more challenging. However, given the similarity between large manufacturing plants and power plants, it is likely that a strong relationship between manufacturing plant openings and the local tax base also exists.

[33]Earlier data is provided for SIC industry codes, which we then convert to NAICS codes.

causal effect of a shock to reducing a local jurisdiction's ability to raise and/or retain property tax revenue. First, we must assume that absent the reform event manufacturing and power plants in the border counties would evolve on similar paths. This is a priori plausible because these counties are likely exposed to similar geographic 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 results for manufacturing as a share of all employment and establishments. If effects were driven by broader economic forces we would not necessarily expect there to be a disproportionate effect among manufacturing establishments, a large producer of negative local externalities. Third, we show that results are robust to a number of different specifications, baseline covariates by year controls and weighting schemes.

For identification, we must also assume that no other reforms coinciding with the event itself influence our outcome variables and that the reform itself does not 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 this changed tax structure, it would change the interpretation of our results. Alternatively, these reforms could have been part of a broader push for progressive legislation that may independently affect locational decisions of plants (i.e. environmental legislation), which would bias our estimates. Finally, these reforms by design increased funding in poor areas—this may have led to changed household location decisions that could influence communities' decisions to allow plants to enter through an income effect rather than a price effect.

Although it is difficult to fully rule out these alternative explanations—and for this reason we see these results as less well identified than those from the first part of the paper—we attempt to address these concerns in several ways. First, we test if there is within-state heterogeneity by local poverty rates—if the observed effects were driven by by the reform increasing funding to poorer communities, we should expect to see larger effects in areas that were low income prior to

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

the reform. Second, we show suggestive evidence that effects are larger in counties where there is greater overlap between school districts and zoning jurisdictions exactly as theory would predict if effects were driven by the school finance reforms and not other correlated reforms. Lastly, examining pre-trends can also help identify if effects are being driven by correlated legislation—if so we might expect effects to appear prior to the reforms as not all correlated reforms would be expected to occur in the exact year of the change. In the future, we will perform a set of analyses on single reforms within several states that create large intra-state variation in incentives, which will further help address these concerns.

A final worry may be that our border county design will violate the stable-unit treatment 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. We address this concern in two ways. First, we might expect that the SUTVA concern would be larger when comparing treated counties to control counties that are very nearby the state border. Accordingly, we estimate if results vary based on a control county's proximity to the treated state and find no evidence for any such effect. Second, we can bound any bias created by this SUTVA violation under the assumption that the reform does not lead to an aggregate increase in the total number of plants across the border pair.[34] 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. Therefore, one-half our observed effect can be thought of as a lower bound of the true effect.

Finally, it is important to note that although we are looking at changes in school district budgets, it is cities and counties that control local land-use decisions. However, as described in Section 2, in most of the country there is substantial overlap between school, municipal and county boundaries (Fischel, 2010).[35] Even in places where overlap is incomplete, as long as

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

[35]Using 2000 population data, we similarly estimate that in the average county there is an 85% probability that

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 underestimate of the effects that would occur if there were shocks to the municipal or county capacity to raise and retain local property tax revenue.

3.4.2 Results

In this section, we examine the effects of shifting local jurisdictions' ability to raise and retain property tax revenue on exposure to externality-producing plants. 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 property tax revenue affect location patterns of large externality-producing plants using changes induced by school finance reform litigation and legislation.

Heterogeneity 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 meaningful and valued by local homeowners. However, many of these openings occurred in states that had already undergone significant equalization reforms, making it likely that these effects are actually much smaller than they would have been in the past, before the reforms were enacted. To test this idea, we examine how the school finance effects of an opening differ by a state's marginal value of tax base (MVTB) with respect to school spending. We proxy for the MVTB in each state by estimating the relationship between a district's total revenue per student and taxable value per student over time conditional on district and year fixed

two randomly selected individuals within the same zoning jurisdiction will also be in the same school district.

effects.[36]

The coefficient on taxable value represents the association between a district's tax base per student and the total revenue per student available to the district. The higher the value, the more district tax base increases translate into revenue increases on average. Tax limitations and crowd-out will both make this value smaller. Of course, this relationship will also be determined by the extent to which the average district in a state chooses to respond to a tax base shock by shifting property tax rates relative to changing spending. However, given that we saw a much larger spending response than property tax 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. 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 3.A.4 shows how the expected tax base impact of a plant differs by its state's MVTB. We would expect this relationship to 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 jurisdiction cannot retain property tax revenue from a plant then its expected impact is irrelevant. That is precisely the pattern we see here; states with a higher MVTB see location patterns that create higher tax base impacts per student. Although descriptive, this plot provides suggestive evidence that a locality's ability to gain tax benefits from local industry is an important determinant of location choice. This plot also reassuringly shows that our estimated 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 1% and almost all states have some degree of crowd-out, this is precisely the range we would expect.

[36]Our qualitative 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 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 3.7 shows effects of an opening on total revenue and total expenditures for state-years by above/below median MVTB (Columns 1,2) and estimated MVTB as measured in mills (Columns 3,4). The results are exactly as we would expect. Low equalization (high MVTB) states raise significantly more revenue from plant openings. In low equalization states moving from 25th to 75th percentile in expected tax base impacts (2 log points) leads to a 7% increase in total revenues per student and an 13% increase in total expenditures per student, while in a below median state such an increase leads to no change in revenue and a 2% increase in expenditures. Similarly, a state with an estimated MVTB of 0 sees no change in revenues when moving from a opening with an expected tax base impact per student at the 25th percentile to one at the 75th percentile and only a 3% increase in expenditures. Conversely, moving from the 25th percentile to the 75th percentile in MVTB (4.4 mills) leads to a 3.2% increase in revenue and a 8% increase in expenditures. In general, we see that, just as we would expect, the less able jurisdictions are to raise and retain local property tax revenue the smaller the fiscal benefits of a plant opening.

Effect of School Finance Reforms on Local Industrial Development

In this subsection, we examine in a causal framework how changes in incentives created by school finance reforms impacted local jurisdictions' exposure to large externality-producing plants. We answer this question in the context of school finance reforms and large manufacturing and power plants. We begin by testing whether our qualitatively-identified reforms had meaningful effects on the way schools in a state were funded. Table 3.8 shows these reforms increased the state share of school funding by 9 percentage points, increased state revenue per student by $1,000 and decreased property tax revenue per student by $700, suggesting a massive change in how a state's schools were funded. Further, these reforms were highly progressive; counties with higher poverty rates at baseline saw a much larger increase in state aid. These results provide confirmatory evidence that our qualitatively-identified reforms did indeed lead to a sharp change in incentives for local districts.

We now investigate how this dramatic change in incentives affected exposure to large capital projects. We examine the effect of reform events on both large manufacturing establishments/employment and exposure to large power plants. Table 3.9 shows the main results for the manufacturing analysis. The odd columns show the results using an unbalanced panel of 14 years before and 14 years after the reform and the even columns show the results using a balanced panel of 8 years prior and 14 years after the reform.[37] Because we are measuring the effects of the reform on stocks not flows, we show the effect of being within 5 years of the reform as well as being more than 5 years after a reform as we expect the treatment effect to increase over time. On average, manufacturing employment per capita falls by roughly 6-7 workers (10%) per 1,000 population in reform counties. We also observe a 2 percentage point decline in the share of total employees that work in the manufacturing sector suggesting that this result is not simply driven by a secular economic decline. Finally, there is a meaningful decline in large manufacturing establishments (as measured by number of employees). Establishments with greater than 250 employees fall by .009 per 1,000 population (20%). Establishments with greater than 500 workers fall by .004 per 1,000 population (20%).

Table 3.10 show results for the power plants analysis. As described above, we do not have reliable retirement data prior to 1975 so our estimates of generating capacity are somewhat incomplete. With this caveat in mind, the results do provide suggestive evidence of a decline in large plants in reform counties relative to their control neighbors. Specifically, the probability of having a plant of any meaningful size (generating capacity greater than 50 MW) falls by 3 percentage points off of a base of 23% and the probability of having a large plant (generating capacity greater than 250 MW) falls by 2.3 percentage points off of a base of 15%. Using an inverse-hyperbolic sine transformation we can further see that the total amount of generating

[37]Our data begin in 1964 and a large number of reforms occurred in the early 1970s so we cannot include as many pre years in the balanced panel.

capacity within a county falls by 17-21%. All results are marginally statistically significant.

Table 3.A.17 shows the effect of the reforms on plant openings and retirements. Openings and retirements are extremely rare events—they occur in less than 0.5% of county-years and we therefore lack sufficient power to estimate these effects with any kind of precision. Nonetheless, the coefficients all go in the expected directions; reforms lead to a large decrease in openings (in proportional terms) and a somewhat smaller increase in retirements but with the lack of precision results are generally statistically insignificant.

Together, these results provide evidence that shocks to local jurisdictions' ability to access their local tax bases lead to large changes in siting behavior. However, there are a number of significant identification concerns that may preclude us from interpreting these results causally. Below, we attempt to test for violations of our identifying assumption across four different domains: omitted variable bias, results driven by other aspects of the reform/correlated reforms, SUTVA violations and weighting/specification/bandwidth choices. While we are unable to fully rule out many of these violations our results do provide suggestive evidence that it is indeed the change in incentives embedded in the reform that are driving our results.

Omitted Variable Bias

One major identification concern is omitted variable bias; we may be worried that even in the absence of a reform, treated counties would have had different trends in manufacturing employment or power generation than their control neighbors. We begin testing for omitted variable bias by examining whether treatment and control counties have different pre-trends in our outcomes of interest using an event study design. Figure 3.6 shows results dynamically for our manufacturing outcomes, while Figure 3.7 shows dynamic results for our power plant outcomes. In all cases, there are no pre-trends prior to the reform and then a decline following the reform's onset. Because we are examining stocks as an outcome, we would expect the effect to increase in magnitude over time until a new equilibrium is reached; indeed that is precisely the pattern we observe.

As a second check, we examine whether treatment and control counties differ on baseline characteristics prior to the reform. We are implementing a differences-in-differences analysis and so differences across groups would not be a violation of our identifying assumption per se, but a lack of large differences would still be reassuring that absent a reform these groups of counties would remain on similar trajectories.

Table 3.A.18 test for differences across key demographic and economic characteristics in the Decennial Census preceding the reform. There are no economically or statistically significant differences across any covariates. These results suggest that pre-deterimined differences in baseline characteristics are unlikely to be driving our results. Table 3.A.19 shows differences across outcome variables between treated and control counties in the year of the reform. There are no economically or statistically significant differences in power generation, but treated counties do appear to have a greater number of manufacturing employees and large manufacturing establishments per capita at baseline. Although we did not see any pre-trends in our dynamic analysis, we may still be worried that that this difference in levels between treatment and control counties may be driving our results.

To evaluate this possibility, we perform several tests of the robustness of our results in Tables 3.A.20 (manufacturing employment) and 3.A.21 (large manufacturing establishments). In Columns (1)–(2), we show the results after logging the outcome variable. If results were driven by reforms being correlated with a secular proportional decline in manufacturing, the difference in levels could mechanically create our observed results. Such a scenario is unlikely given that events happened at different times and we did not see any pre-trends in our dynamic analysis, but even more reassuringly we see that results are extremely similar to our main analysis when using a logged dependent variable. Columns (3)–(4) restrict the sample to be only years prior to the start of the massive decline in US manufacturing jobs (in 1997) to ensure that nothing about correlations between this decline and baseline levels of manufacturing are driving our results.

Again, results remain largely the same. Columns (5)–(6) restrict our sample to only county pairs that have manufacturing levels per capita within 0.5 log points in 1964 (the first year of our sample), while Columns (7)–(8) use a 0.25 log point cutoff. Despite significantly reducing our sample size, results remain qualitatively similar. Finally, Columns (9)–(10) exclude border regions whose pre-reform differences in manufacturing employment are large positive outliers. In this sample, there are no economically or statistically significant baseline differences in manufacturing employment and yet results persist suggesting again that these baseline differences are very unlikely to be driving our main results.

Finally, if results were driven by other trends we might also expect changes to occur in other areas of the economy aside from manufacturing. We expect the effects of these reforms to be largest among large polluting plants for two reasons. One, these plants are often the most valuable pieces of non-residential real estate in a given district. While properties like warehouses or office buildings also create tax revenue because they are typically less capital-intensive than large plant, each individual property contributes much less to the local tax base and therefore the tax benefits are less likely to be an important reason for approval. Two, these plants create large local externalities that are likely to induce substantial local opposition to the project in the absence of compensation.

We test this supposition in several ways. Table 3.A.22 shows the effects of the reforms on non-manufacturing employment and industries, while Table 3.A.23 breaks down the effects of the reform by industry type. There are no significant effects of the reform across non-manufacturing industries, providing further evidence that results are not driven by a secular economic decline in treated counties. Further, there is suggestive evidence for employment declines in other sectors that may have large externality producing projects such as the mining/extraction or transportation/utility sectors although these effects are not statistically significant. Together, these results suggest that differential trends among treated and control counties are not likely to be driving our results.

Simultaneous Shocks

A second and harder to rule out identification concern is that results are driven by shocks that occurred simultaneous to the change in incentives. We can divide this concern into two parts: other aspects of the school finance reform are driving the results and reforms correlated with the adoption of school finance are causing the results. We address each of these possibilities in turn.

School finance reforms typically increased spending in the poorest districts and weakly decreased spending in the richest districts. Such changes in spending could induce sorting, increase land values in poor districts or the marginal value of additional tax base because of diminishing marginal returns all of which would lead polluting industries to decline in low-income areas of reform states for reasons other then the change in incentives. However, if these dynamics were driving our observed results, we would expect effects of the reforms to be much larger in high-poverty relative to low-poverty districts. We test this hypothesis empirically in Table 3.11. This table shows that if anything, effects are larger in low-poverty relative to high-poverty counties suggesting that these other aspects of the reforms are unlikely to be driving the observed results.

An additional concern is that these reforms may have occurred as part of a suite of progressive legislation that may independently have had effects on the location of polluting industries (i.e. laws requiring strict environmental or community impact assessments prior to development). We think this is unlikely to be the cause of the observed results for several reasons. First, many school finance reforms were the results of lawsuits alleging that existing funding structures violated the state constitution (Jackson et al., 2014) and not part of a broader legislative push. Second, reforms happened across a diverse range of states and over a long time period. Third, we do not observe major changes outside of the manufacturing and power generation industries—thus, such reforms would have to be highly targeted to achieve such an effect. The most likely type of reforms that could lead to this pattern are environmental reforms, but in the vast majority of states most major environmental initiatives have been federal. However, we are in the process of collecting information on major environmental reforms across US states to empirically assess this possibility.

In Table 3.A.24 we provide a further empirical check of this assumption. As discussed above, the studied reforms affected school districts, but the jurisdictions typically making zoning decisions are cities and counties. Thus, we might expect that the more closely school districts align with zoning jurisdictions, the stronger the effect a reform should have. School districts and zoning jurisdictions can be mismatched on two dimensions; a single zoning jurisdiction can be spread over multiple school districts and similarly a single school district can be spread over multiple zoning jurisdictions. Both dimensions should matter for the effect of the reform. If a zoning jurisdiction is spread over multiple school districts fewer residents will benefit from a given plant opening. If a school district is spread over multiple zoning jurisdictions, the amount of benefit will be diluted within each zoning jurisdiction. Thus, our variable of interest is an interaction term between the average probability that two given residents of a county's zoning jurisdiction will in the same school district and the average probability that two given residents of a school district will be in the same zoning jurisdiction.

Table 3.A.24 shows some suggestive evidence that effects are larger in areas with greater overlap between school districts and zoning jurisdictions, although effects are only significant in the case of employment. The results in this table include state by year fixed effects and so are identified wholly off of differences in effect sizes within treated counties in the same reform state. Because no other major state environmental laws are likely to target counties in states with disproportionate overlap between school and zoning jurisdictions, we believe this pattern provides further suggestive evidence for the change in incentives as the main mechanism driving these results.

Stable Unit Treatment Value Assumption Violation

An additional concern may be that our analysis may violate the stable unit treatment value assumption. The idea here would be that if a plant's previously optimal location was in a given county, its next most optimal county after the reform may be directly across state lines in the control county. We address this concern in two ways. First, we can attempt to put a lower bound on the treatment effect if SUTVA is operative. We first assume that the treatment does not increase the total number of plants locating in a given pair; this assumption seems plausible as the treatment is making it less likely on average that the pair will want a plant. Under this assumption, a violation of SUTVA could overstate our result by at most a factor of 2 if all plants from the treated county moved to the control county. All results would remain highly economically significant under this assumption.

In addition to bounding, we attempt to test this supposition empirically in Table 3.A.25. Specifically, we interact our difference-in-differences estimator with a variable equal to the distance of the control county to the treated state border (odd columns) or exclude control counties that are within 45 miles of the border (even columns). We expand our sample to include all county pairs that are within 90 miles of each other to provide sufficient power to test these hypotheses. We find no evidence that effects differ by distance to border or are significantly smaller after excluding counties nearby the border. These results provide some supportive evidence that SUTVA violations are not a major determinant of our observed results.

Weighting/Specification/Bandwidth

Finally, we perform several checks to ensure that our weighting, sample selection, or other specification choices are not driving our results. Table 3.A.26 and 3.A.27 show results when weighting by reform event or population. We see that results for both manufacturing and power generation are if anything larger when weighting by population. When weighting by reform state results are broadly similar although standard errors increase as the effects of several states are estimated imprecisely. Table 3.A.28, 3.A.30 and 3.A.29 show results using different distance bandwidths. Results are broadly similar regardless of whether we restrict the bandwidth to 30 miles or increase it to 90 miles.

We might also be concerned that something in our reform identification procedure itself is driving results. To address this, we perform the same analysis using the first school finance reform identified for each state in Jackson et al. (2014) analysis of school finance reform's effects on low-income educational achievement and labor market outcomes. We would expect the effects using these reforms to be smaller because not all reforms used by Jackson et al. (2014) change property tax incentives—some simply equalize spending through lump-sum transfers or other techniques, which do not change local control over property taxation. The observed results are consistent with this pattern; using the Jackson et al. (2014) reforms we see directionally similar results that are also statistically significant, but the magnitudes are often smaller than those found when using our qualitatively identified reforms. These results provide additional reassurance that nothing particular to our qualitative reform-identification process is causing our results.

Finally, we can identify the effects separately for each reform state to ensure that no one state is behind our results. Figure 3.A.7 shows the distribution of effects across states. This figure shows that almost all states have negative treatment effects suggesting that results are not driven by one or two states. Treatment effect sizes are not correlated with reform timing nor geographic region of the country. In sum, these results suggest that the observed effects are unlikely to be driven by specification, bandwidth, weighting or other similar factors.

3.5 Conclusion

Large capital projects create substantial external benefits and costs. When these costs and benefits occur on different spatial scales in the presence of local control over land use, inefficiencies can emerge. In this paper, we study how access to local property taxation may change local jurisdictions' willingness to allow externality-producing projects to enter their communities. 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 caused 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%-5% 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 suggesting that property taxation of large plants has important distributional consequences for who is helped and who is harmed by their construction.

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 following a reform, manufacturing employment and large establishments fall by 10-15% suggesting that the benefits provided by property tax payments from these entities can be an important driver of local industrial development.

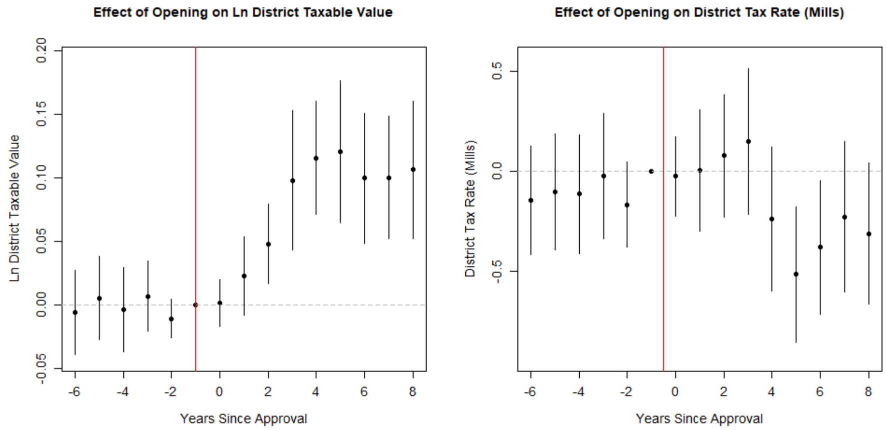
These results also suggest that reforms that restrict a local government's ability to raise revenue from their tax base may have significant unintended consequence for local land-use. This is a feature of many common state-level policies including school finance reforms, municipal and county revenue sharing systems and property tax limitations. However, the welfare implications of this shift are not clear. Depending on the relative distribution of local and social costs and benefits, limiting the property taxation benefits of these projects may either increase or decrease efficiency. Better understanding this trade-off is essential when considering the design and reform

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

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.

Chapter 3, in part, is currently being prepared for submission of publication of the material. Fraenkel, Rebecca; Krumholz, Samuel. “Property Taxation as Compensation for Local Externalities: Evidence from Large Plants”. The dissertation author was a primary investigator and author of this material.

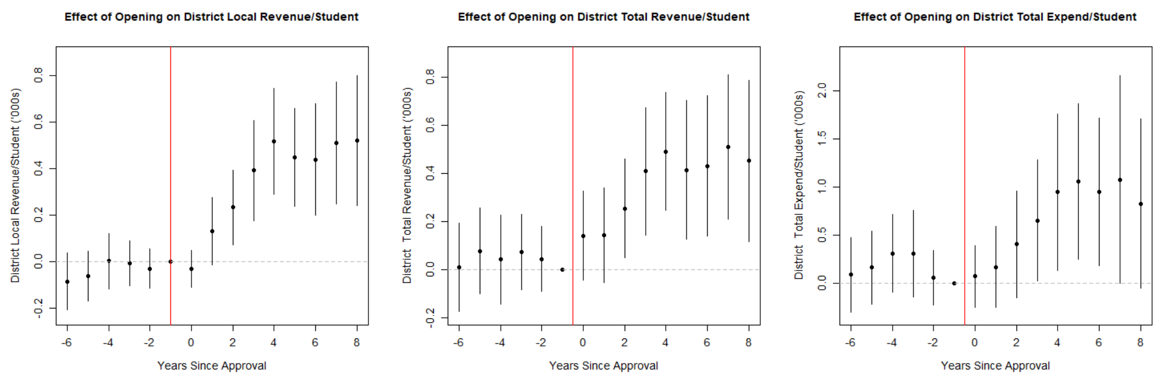
Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows the effect of a plant opening on the natural log of district taxable value per student and property tax rates measured in mills. Coefficients come from a regression of the outcome variable on district fixed effects (property tax rates), year x border pair fixed effects and interactions between indicators for years since approval (-1 is the omitted category) and whether or not a district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. District taxable values and property tax rates were hand-collected from state Department of Education and Department of Revenue's annual reports. Plant opening data come from the Energy Information Administration (EIA).

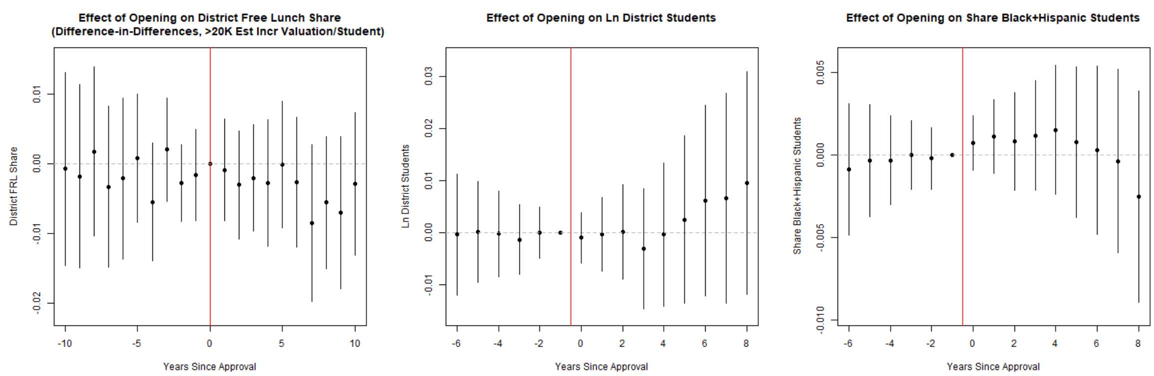
Figure 3.1: Effect of Opening on Taxable Value Per Student and Property Tax Rates

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



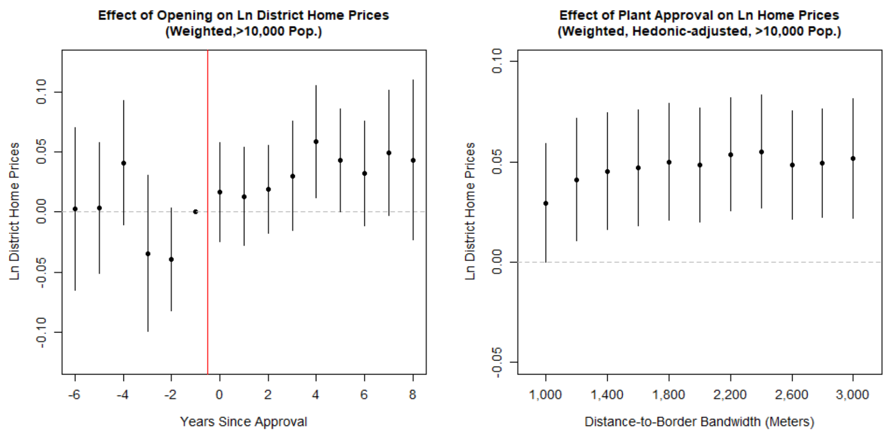
This figure shows the effect of a plant opening on district revenues and expenditures per student. Coefficients come from a regression of the outcome variable on district fixed effects (property tax rates), year x border pair fixed effects and interactions between indicators for years since approval (-1 is the omitted category) and whether or not a district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included. Only openings with greater than $10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. Revenue and expenditure data came from the National Center for Education Statistics (NCES). Plant opening data come from the Energy Information Administration (EIA).

Figure 3.2: Effect of Opening on Local Revenues/Student, Total Revenues/Student and Total Expenditure/Student



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 black and Hispanic students. Coefficients come from a regression of the outcome variable on district fixed effects (property tax rates), year x border pair fixed effects and interactions between indicators for years since approval (-1 is the omitted category) and whether or not a district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. Demographic data came from the National Center for Education Statistics (NCES). Plant opening data come from the Energy Information Administration (EIA).

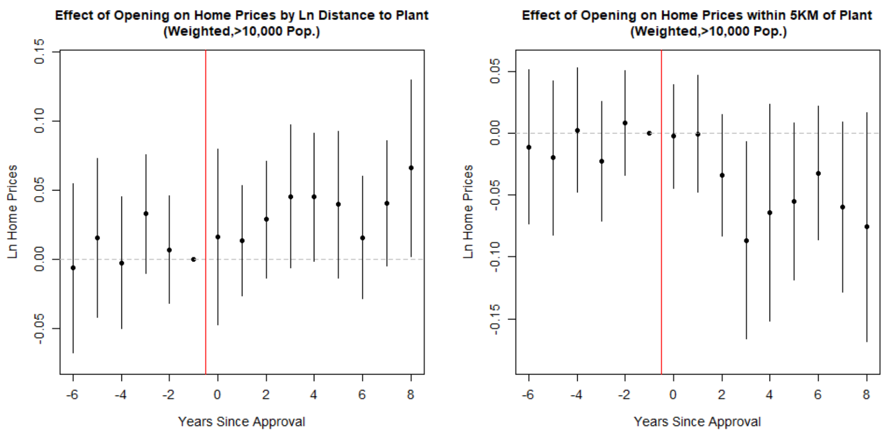
Figure 3.3: Differences in Key Demographic Groups Before and After Openings



This figure shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. In the left panel, we only include sales within a bandwidth of 2 km 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 years since approval. In the right panel, we show the coefficient of a regression of log prices on an indicator for whether a house was in a plant receiving district with an indicator for years 3–8 since plant approval using different border bandwidths. Controls include border pair by year by month fixed effects and border pair x district x .004 degree x .004 degree latitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below $5,000 or greater than $1,500,000 are excluded as outliers. Only pairs of districts in which both district have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than $10,000 are excluded. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Figure 3.4: Effect of Opening on Host-District Home Prices

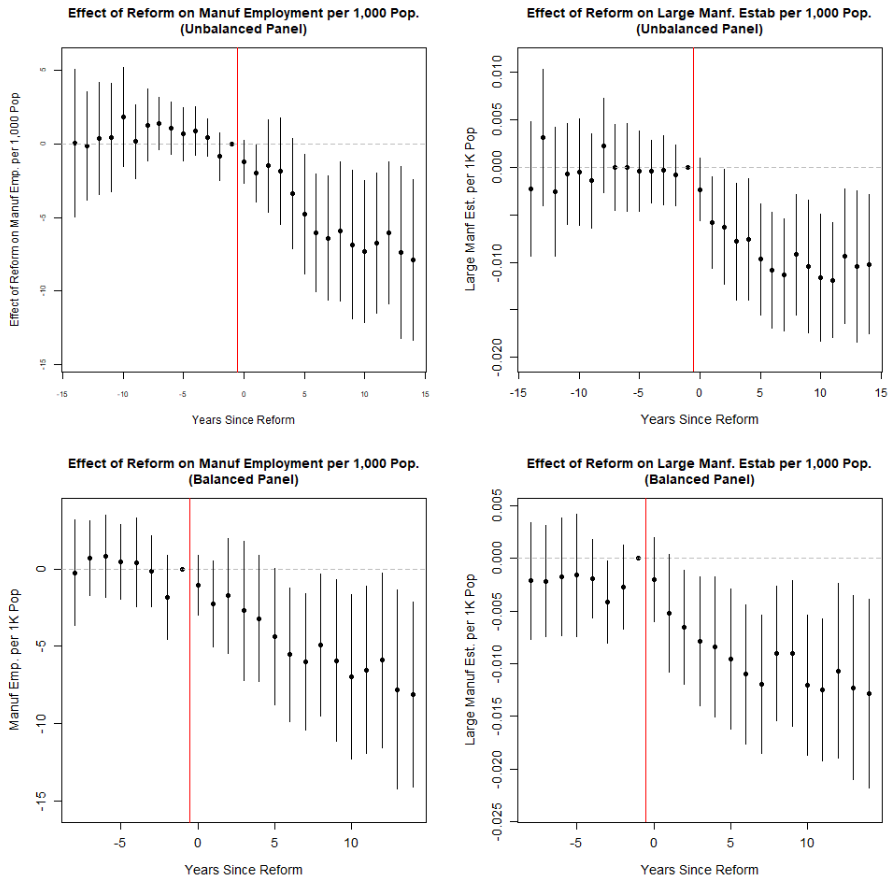
Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows the effect of a plant opening on log local housing prices for nearby homes. All regressions include controls for plant by year by month fixed effects and .004 degree latitude by .004 degree longitude cell fixed effects. Coefficients are the interaction between an indicator for years since plant approval (-1 is the omitted variable) and two distance metrics: ln distance from plant (on left) and an indicator for being less than 5km from the plant (on right). Homes more than 20 km away and closer than .5km from the plant are dropped and the regression on the right also includes indicators for being 5km-10km from the plant x years since approval. Plants in districts with fewer than 10,000 population are excluded as there are too few annual transactions to create consistent home price estimates. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation is weighted by the inverse of the number of sales attached to a given plant in each year. Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below \$5,000 or greater than \$1,500,000 are excluded as outliers. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

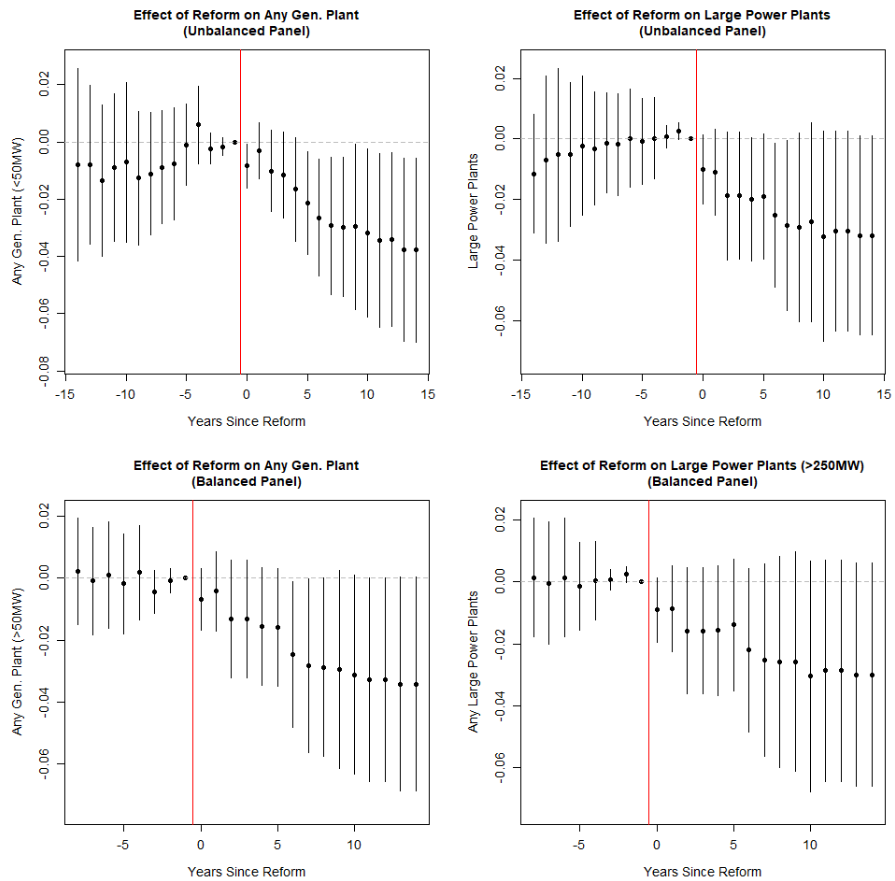
Figure 3.5: Effect of Opening on Nearby Home Prices

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



This figure shows the effect of a school finance reform on manufacturing outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. In the case where both members of a pair were treated, all years after the second event occurred were dropped. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is in a reform state and a vector of indicators for periods relative to reform (the year prior to reform is the omitted category). Controls include county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing employment data come from County Business Patterns. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. We exclude outcome values greater than the 99th percentile as outliers.

Figure 3.6: Effect of School Finance Reform on Large Manufacturing Establishments and Manufacturing Employment



This figure shows the effect of a school finance reform on power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. In the case where both members of a pair were treated, all years after the second event occurred were dropped. 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 (the year prior to reform is the omitted category). Controls include county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. County-level power generation data come from EIA Form 860. We lack generation data on generators that were not owned by a utility that remained in business in 1990 or which retired before 1975. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Figure 3.7: Effect of School Finance Reform on Power Plant Openings

Table 3.1: Effects of Plant Opening on District Tax Base and School Finance Outcomes

VARIABLES	(1) Ln Tax Value /Stud	(2) Rate	(3) Loc Rev /Stud	(4) Tot Rev /Stud	(5) Tot Exp /Stud
Treat x Post Yrs 0-2	0.0260**	0.110	0.141**	0.138*	0.0602
	(0.0130)	(0.113)	(0.0599)	(0.0792)	(0.175)
Treat x Post Yrs 3-8	0.108***	-0.167	0.501***	0.409***	0.765**
	(0.0236)	(0.134)	(0.111)	(0.114)	(0.344)
Observations	21,240	21,092	38,984	38,962	39,882
R^2	0.966	0.973	0.955	0.937	0.853
Pair x Year FE	Y	Y	Y	Y	Y
Sample	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small
Dep. Var. Mean	580887	10.56	4.991	11.79	12.15

Clustered 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 log taxable value per student, tax rate, local revenue per student ('000s/student), total revenue per student ('000s/student) and total expenditures per student ('000s/student). Coefficients come from a regression of the relevant outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Only openings with greater than $10,000 in expected tax base per student are included (≈ 10% of openings are dropped). We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. District property tax rates and tax bases 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 come from the Energy Information Administration (EIA).

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.2: Effects of Plant Opening on School Finance Outcomes by Revenue Source

VARIABLES	(1) Ptax /Stud	(2) PrntGov /Stud	(3) UnspecLoc /Stud	(4) OthLoc Stud	(5) StFormAid /Stud	(6) OthSt /Stud	(7) Fed /Stud
Treat x Post Yr 0-2	0.0697*	0.0315*	0.0287	0.0108	-0.0636	0.00417	-0.00616
	(0.0418)	(0.0175)	(0.0191)	(0.0175)	(0.0421)	(0.0349)	(0.0127)
Treat x Post Yr 3-8	0.338***	0.0666**	0.0714***	0.0270	-0.134***	0.0110	-0.0224
	(0.0913)	(0.0269)	(0.0249)	(0.0267)	(0.0504)	(0.0282)	(0.0157)
Observations	38,818	38,818	38,818	38,818	39,262	39,262	39,056
R^2	0.961	0.983	0.677	0.944	0.964	0.906	0.937
Pair x Year FE	Y	Y	Y	Y	Y	Y	Y
Sample	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small
Dep. Var. Mean	3.473	0.565	0.210	1.201	4.294	1.537	0.874

Clustered 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 revenue streams measured on a per-student basis. Uncategorized local and state payments represent payments that do not fit into the NCES categories and often encompass payments in lieu of taxation (PILOT). "Other local payments" are largely made up of sales and income taxes. "Other state revenues" are transfers for state-mandated programs like transportation, special education or English language learners. Coefficients come from a regression of the relevant outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. Revenue data came from the National Center for Education Statistics (NCES). Plant opening data come from the Energy Information Administration (EIA).

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.3: Effects of Plant Opening on Debt and Expenditures by Type

VARIABLES	(1) LTD /Stud	(2) Instr Sal. /Stud	(3) Cap /Stud	(4) Oth /Stud
Treat x Post Yrs 0-2	0.248	-0.0134	0.0920	-0.0184
	(0.279)	(0.0258)	(0.129)	(0.110)
Treat x Post Yrs 3-8	0.863*	0.0430	0.402***	0.319
	(0.462)	(0.0381)	(0.142)	(0.247)
Observations	39,712	39,882	39,882	39,882
R^2	0.829	0.974	0.597	0.878
Pair x Year FE	Y	Y	Y	Y
Dep. Var. Mean	5.819	6.103	1.351	4.796

Clustered 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 outstanding long-term debt per student, total instructional personnel salaries per student, total capital expenditures per student and all other expenditures per student. Coefficients come from a regression of the relevant outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. Schol finance data came from the National Center for Education Statistics (NCES). Plant opening data come from the Energy Information Administration (EIA).

Table 3.4: Effects of Plant Opening on Home Prices

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price	(5) Ln Price	(6) Ln Price
Treat x Post Yrs 0-2	0.0220	0.0370	0.00696	0.0290*	0.0552**	-0.00175
	(0.0145)	(0.0227)	(0.0166)	(0.0152)	(0.0247)	(0.0152)
Treat x Post Yrs 3-8	0.0485***	0.0700***	0.0275	0.0508***	0.0718**	0.0285
	(0.0143)	(0.0207)	(0.0185)	(0.0175)	(0.0291)	(0.0168)
Observations	501,699	198,576	303,123	538,353	223,992	314,361
R^2	0.708	0.639	0.804	0.727	0.685	0.802
Weighted	Y	Y	Y	Y	Y	Y
Sample Pop Excl	>10K Pop	>10k Pop	>10k Pop	All	All	All
Sample Geo Excl	All	No CA	CA-Only	All	No-CA	CA-only

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and border pair x district x .004 degree x .004 degree latitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant-district level. All housing data come from the Zillow ZTRAX database—sales below \$5,000 or greater than \$1,500,000 are excluded as outliers. Population exclusion of greater than 10,000 means that only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than \$10,000 are excluded. Data are from fourteen states in total: Arizona, California (columns 1,3,4, and 6), Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.5: Effects of Plant Opening on Home Prices: Different Expected Tax Base Per Student Cutoffs

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price	(5) Ln Price	(6) Ln Price
Treat x Post Yrs 0-2	0.00945	0.0403**	0.0611**	-0.00189	0.0208*	0.0364***
	(0.00929)	(0.0183)	(0.0289)	(0.00752)	(0.0104)	(0.00943)
Treat x Post Yrs 3-8	0.0309***	0.0708***	0.105**	0.00650	0.0498**	0.0687***
	(0.0104)	(0.0241)	(0.0430)	(0.00967)	(0.0207)	(0.0171)
Observations	1,107,048	143,277	61,487	1,156,284	165,509	75,216
R^2	0.715	0.701	0.704	0.716	0.639	0.640
Weighted	Y	Y	Y	N	N	N
Sample	>10k Pop	>10k Pop	>10k Pop	>10k Pop	>10k Pop	>10k Pop
Exp Tax Base Cut-off	All	75K/Stud	150K/Stud	All	75K/Stud	150K/Stud
Avg Exp Tax Base/Stud	1.076	2.685	4.130	0.344	1.871	2.878

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and border pair x district x .004 degree x .004 degree latitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation in Columns (1)-(3) are weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below $5,000 or greater than $1,500,000 are excluded as outliers. Only pairs of districts in which both district have a 2000 population greater than 10,000 are included in the regression. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.6: 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
5-10kM Away x Post Yrs 0-2	-0.00546	-0.00377				
	(0.0118)	(0.0108)				
5-10 Km Away x Post Yrs 3-8	-0.0288*	-0.0185				
	(0.0168)	(0.0119)				
<5km away x Post Yrs 0-2	-0.00633	-0.01277				
	(0.0130)	(0.0108)				
<5km away x Post Yrs 3-8	-0.0579**	-0.0329*				
	(0.0252)	(0.0168)				
Ln Dist x Post Yrs 0-2			0.00864	0.00863		
			(0.0126)	(0.00980)		
Ln Dist x Post Yrs 3-8			0.0313*	0.0240**		
			(0.0157)	(0.0115)		
5-10Km Away x Post Yrs 0-2 x Nameplate ('00 MW)					-0.00284	-0.00393**
					(0.00219)	(0.00173)
5-10Km Away x Post Yrs 0-2 x Nameplate ('00 MW)					-0.00895***	-0.00878***
					(0.00309)	(0.00123)
< 5Km Away x Post Yrs 0-2 x Nameplate ('00 MW)					-0.00620***	-0.00520***
					(0.00226)	(0.00117)
< 5Km Away x Post Yrs 3-8 x Nameplate ('00 MW)					-0.0150***	-0.00920***
					(0.00486)	(0.00190)
Observations	2,228,378	2,352,691	2,228,378	2,352,691	3,964,489	4,107,232
R^2	0.683	0.705	0.683	0.701	0.690	0.698
Size Cutoff	>100MW	>100MW	>100MW	>100MW	N	N
Hedonics	Y	Y	Y	Y	Y	Y
Max Dist	20 km	20 km	20 km	20 km	20 km	20 km
Weighted	Y	N	Y	N	Y	N

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, .004 degree longitude x .004 degree latitude bins, and district x year 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), bedrooms, square footage (500 sq ft bins), and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Only homes within 20 km of the opening plant are included. Only plants who are in districts with a population greater than 10,000 are included in weighted analysis. Standard errors are clustered at the plant district level. Only openings with at least two years of pre and two years of post data are included. All housing data come from the Zillow ZTRAX database—sales that are less than $5,000 or greater than $1,500,000 are excluded as outliers. Weighted specifications are weighted by the inverse of the number of sales within 20 km of the plants in a given year.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.7: Differential Effects of Plant Opening on Key School Finance Variables by State Equalization Status

VARIABLES	(1) Ln TotRev	(2) LnTotRev	(3) LnTotExp	(4) LnTotExp
Treat x Post Yrs 0-2 x Ln Exp Tax Base Incr/Stud	-0.00229	-0.00256	-0.00900	-0.00811
	(0.00458)	(0.00487)	(0.00672)	(0.00694)
Treat x Post Yrs 3-8 x Ln Exp Tax Base Incr/Stud	-0.00173	0.00367	0.0108*	0.0147**
	(0.00431)	(0.00460)	(0.00609)	(0.00678)
Treat x Post Yrs 0-2 x Ln Exp Tax Base Incr/Stud x > Med MVTB	0.0234***		0.0351***	
	(0.00682)		(0.0109)	
Treat x Post Yrs 3-8 x Ln Exp Tax Base Incr/Stud x > Med MVTB	0.0384***		0.0543***	
	(0.0110)		(0.0158)	
Treat x Post Yrs 0-2 x Ln Exp Tax Base Incr/Stud x MVTB (Mills)		0.00305***		0.00447***
		(0.000966)		(0.00154)
Treat x Post Yrs 3-8 x Ln Exp Tax Base Incr/Stud x MVTB (Mills)		0.00399***		0.00650***
		(0.00150)		(0.00219)
Observations	25,106	25,106	25,708	25,708
R^2	0.939	0.938	0.886	0.886
Sample	All	All	All	All
Dep. Var. Mean	11.58	11.65	11.89	11.89

Clustered 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 the relevant outcome variable on district fixed effects, year x border pair fixed effects and interactions between indicators for whether a year is after an approval and whether or not a district receives a plant, log expected tax base per student and various quantiles of estimated size of the increase in tax base per student the plant will provide. Border pairs are any two districts that share a border within the same state where only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. We proxy for district's estimated marginal value of tax base (MVTB) using a coefficient derived from a state-specific regression of state and local revenue per student on taxable value per student with district and year fixed effects. District property tax rates were hand-collected from state Department of Education and Department of Revenue annual reports. Local revenues per student come from the National Center for Education Statistics (NCES). Plant opening data come from the Energy Information Administration (EIA).

Table 3.8: School Finance Reform and County School Revenue by Source: County Pairs Design

VARIABLES	(1) StShare	(2) StRev /Stud	(3) StRev /Stud	(4) PtaxRev /Stud	(5) PtaxRev /Stud
Treat x Post Yr $\leq$5	0.0605***	0.794***		-0.373	
	(0.0176)	(0.199)		(0.232)	
Treat x Post Yr >5	0.0849***	1.079***		-0.701*	
	(0.0213)	(0.247)		(0.357)	
Treat x Post Yr $\leq$5 x BL Poverty Rate			3.344***		-0.394
			(1.023)		(0.844)
Treat x Post Yr >5 x BL Poverty Rate			3.510***		-1.091
			(0.953)		(1.087)
Observations	40,296	40,296	40,102	40,580	40,388
R^2	0.949	0.924	0.969	0.914	0.970
Unbalanced	Y	Y	Y	Y	Y
Dep. Var. Mean	0.369	3.507	3.507	2.688	2.688

Two-way clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of school finance reforms on school finance outcomes using a county border pair difference-in-differences design. All revenue outcomes are measured in '000s of dollars. Counties whose geographic centroids were less than 60 miles apart were included. 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). Baseline poverty rate is the 1970 county poverty rate. Controls include county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state 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. Our unbalanced sample consists of 14 years before and after the reform year.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.9: School Finance Reform and Manufacturing Presence: County Pairs Design

VARIABLES	(1) EmpManf /1K Pop	(2) EmpManf /1K Pop	(3) EmpManf /TtlEmp	(4) EmpManf /TtlEmp	(5) >250 Emp Est /1K Pop	(6) >250 Emp Est /1K Pop	(7) >500 Emp Est /1K Pop	(8) >500 Emp Est /1K Pop
Treat x Post $\leq$5 Years	-3.159**	-2.529	-0.00996*	-0.0101*	-0.00673**	-0.00520*	-0.000869	0.000595
	(1.500)	(1.595)	(0.00514)	(0.00501)	(0.00265)	(0.00258)	(0.00122)	(0.00123)
Treat x Post >5 Years	-7.315***	-6.434***	-0.0206***	-0.0185***	-0.0108***	-0.00938***	-0.00442**	-0.00333*
	(2.096)	(2.263)	(0.00643)	(0.00641)	(0.00314)	(0.00317)	(0.00167)	(0.00175)
Observations	164,626	91,218	165,546	91,644	206,702	119,582	205,986	119,586
R^2	0.955	0.959	0.957	0.964	0.902	0.913	0.865	0.874
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	74.09	73.16	0.324	0.301	0.0649	0.0619	0.0206	0.0237

Two-way clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on manufacturing outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing employment data come from County Business Patterns. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. We exclude outcome values greater than the 99th percentile as outliers.

Table 3.10: School Finance Reform and Power Plants: County Pairs Design

VARIABLES	(1) Any >50MW	(2) Any >50MW	(3) Ttl >50MW	(4) Ttl >50MW	(5) Any >250MW	(6) Any >250MW	(7) IHS(MW)	(8) IHS(MW)
Treat x Post Yr ≤5	-0.00583	-0.0110	-0.00306	-0.0167	-0.00370	-0.0137	-0.0207	-0.0981
	(0.00892)	(0.00979)	(0.0102)	(0.0126)	(0.00645)	(0.00854)	(0.0612)	(0.0709)
Post x Treat Yr >5	-0.0281*	-0.0302*	-0.0276	-0.0379	-0.0230*	-0.0279*	-0.172*	-0.214*
	(0.0141)	(0.0164)	(0.0218)	(0.0252)	(0.0134)	(0.0158)	(0.0957)	(0.112)
Observations	212,448	122,938	212,448	122,938	212,448	122,938	212,448	122,938
R^2	0.965	0.969	0.975	0.977	0.960	0.963	0.965	0.969
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Unbalanced	Unbalanced	Unbalanced	Unbalanced	Unbalanced
Dep. Var Mean	0.226	0.223	0.326	0.356	0.145	0.150	215.4	229.5

Two-way clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of school finance reforms on power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. Coefficients come from a regression of the outcome variable on the interaction between an indicator for whether a district is in a reform state and a vector of indicators for periods relative to reform (pre-reform is the omitted category). Controls include county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. County-level power generation data come from EIA Form 860. We lack generation data on generators that were not owned by a utility that remained in business in 1990 or which retired before 1975. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.11: School Finance Reform, Manufacturing Presence and Power Plants by Baseline Poverty

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	EmpManf	EmpManf	LrgManfEst	LrgManfEst	AnyGen(50MW)	AnyGen(50MW)	AnyGen(250MW)	AnyGen(250MW)
Pov Status	> Med	< Med	> Med	< Med	> Med	< Med	> Med	< Med
Treat x Post Yr ≤5	-1.527	-4.215*	-0.00560	-0.00525*	-0.0110	-0.0111	-0.00578	-0.0218
	(2.598)	(2.151)	(0.00403)	(0.00304)	(0.00981)	(0.0171)	(0.00712)	(0.0147)
Treat x Post Yr >5	-4.103	-8.350**	-0.00984**	-0.00793**	-0.0166	-0.0443	-0.00760	-0.0489
	(3.729)	(3.277)	(0.00444)	(0.00361)	(0.0108)	(0.0311)	(0.00748)	(0.0293)
Observations	46,760	47,278	63,706	58,170	64,506	58,432	64,506	58,432
R^2	0.956	0.965	0.913	0.927	0.982	0.958	0.979	0.950
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	0.226	0.223	0.326	0.356	0.145	0.150	215.4	229.5

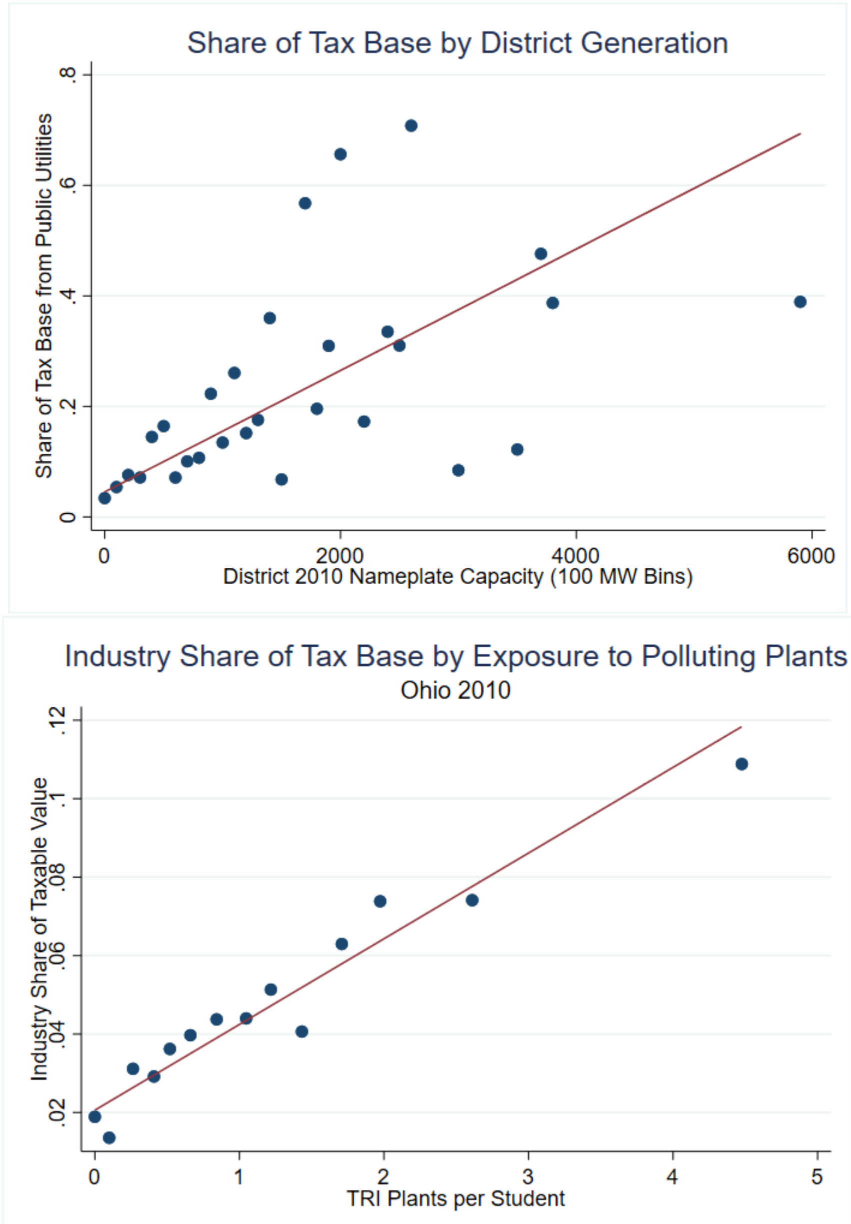
Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of school finance reforms on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. For plants retiring prior to 1990, only plants that were owned by utilities still in operation in 1990 were included. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Appendix

3.A Appendix Figures and Tables

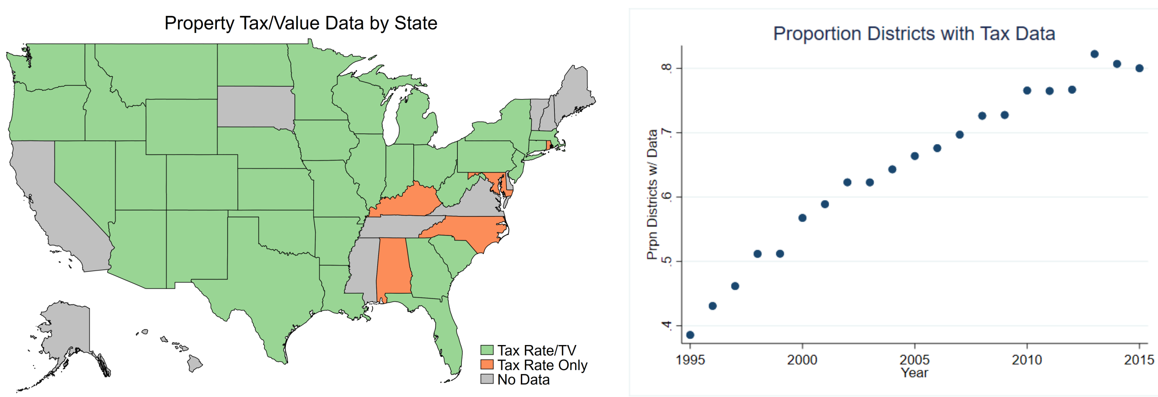
Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



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 3.A.1: Utility Share of School District Tax Base by District Generation Level

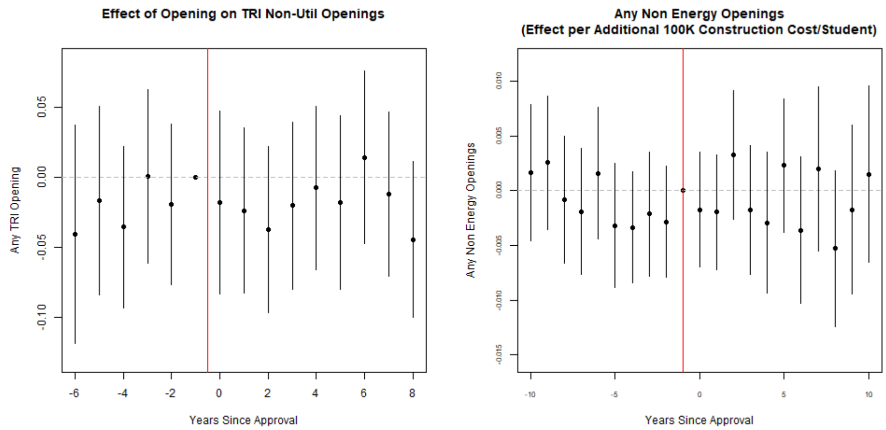
Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



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.

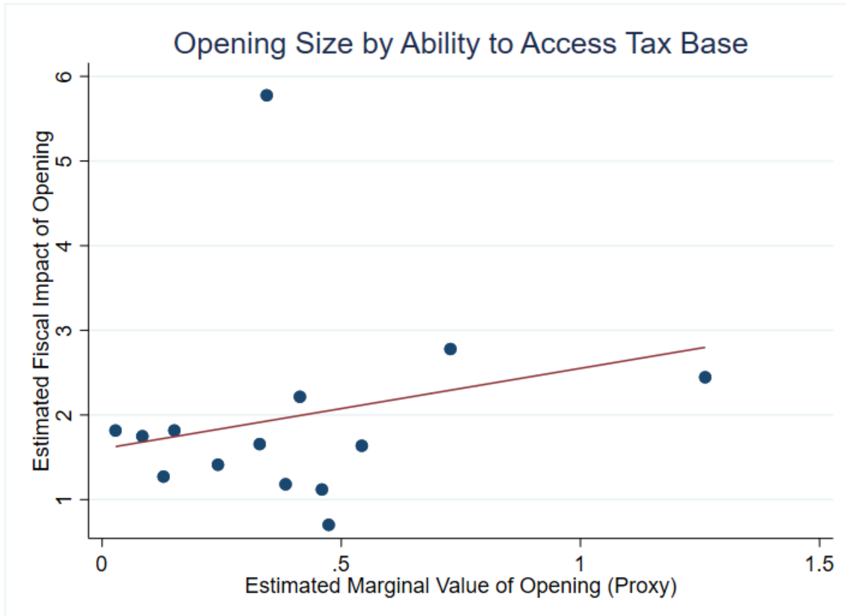
Figure 3.A.2: Data Coverage of Property Tax Rates and Taxable Value

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



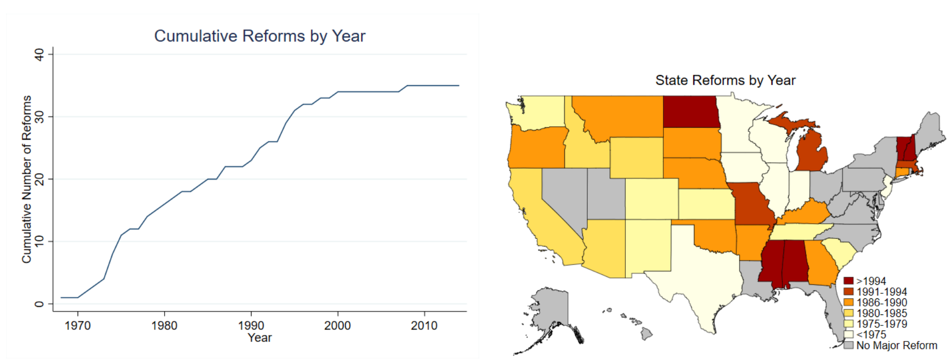
This figure shows the likelihood of a district having any facility opening in a year surrounding the start of construction on a power plant. Opening data comes from the Toxic Release Inventory (TRI) and is based on the first year that a facility appears in the data. Coefficients come from a regression of the relevant outcome variable on district fixed effects, year x border pair fixed effects and interactions between indicators for years since approval (-1 is the omitted category) and whether or not a district receives a plant. Estimated effect of the opening comes from dividing estimated construction costs (created using parameters provided in the EIA's Annual Energy Outlook) divided by the number of students in a district at the year of approval. Border pairs are any two districts that share a border within the same state and only one of the two districts experienced a plant opening between 1995 and 2015. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district level. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally. Plant data comes from the Toxic Release Inventory run by the EPA. Plant opening data come from the Energy Information Administration (EIA).

Figure 3.A.3: Opening of Non-Utility TRI Facilities



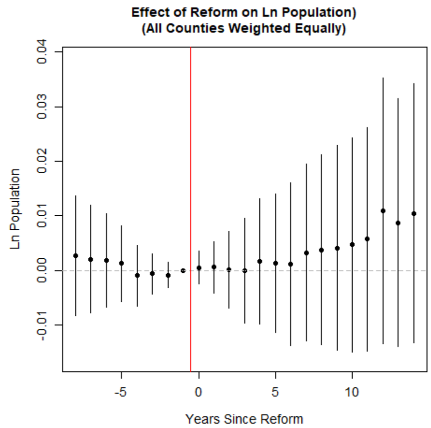
This figure shows the relationship between the expected marginal value of an additional dollar of tax base with respect to school spending and the estimated tax base impact of a plant. Estimated tax base impact of a plant is equal to the plant's estimated construction cost divided by the number of students 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.

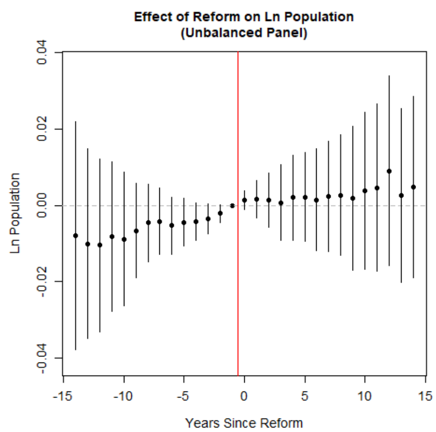
Figure 3.A.4: Estimated Tax Base Effect of Opening by Estimated Marginal Value of Tax Base in Opening State-Year



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.

Figure 3.A.5: School Finance Reforms Geographic and Temporal Distribution

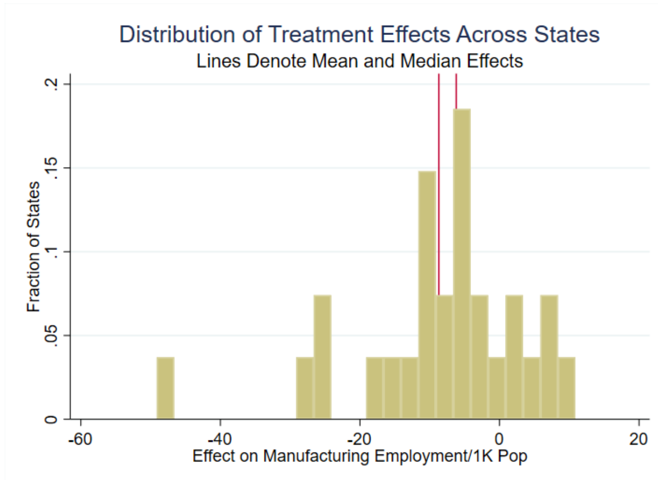




This figure shows the effect of a school finance reform on large local manufacturing establishments (>500 employees) and manufacturing employment per 1,000 population using a contiguous border county difference-in-differences design. Coefficients come from a regression of the outcome variable on indicators 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 state 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).

Figure 3.A.6: Effect of School Finance Reform on County Population

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.



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 state 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).

Figure 3.A.7: Distribution of Treatment Effects by Reform State

Table 3.A.1: Correlates of Plant Opening

VARIABLES	(1) Ln Stud	(2) URM	(3) Pct FRL	(4) Loc Rev/Stud ('000)	(5) Tot Rev/Stud ('000)	(6) Ln 1990 Home Val
Treated District	0.0389	0.0222**	0.00855	0.141	0.115	0.00241
	(0.0585)	(0.00943)	(0.00802)	(0.120)	(0.150)	(0.0168)
Observations	2,660	2,660	2,372	2,624	2,626	2,392
R^2	0.731	0.845	0.786	0.804	0.780	0.877

Clustered 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. All demographic characteristics come from the year a plant opens and are taken from the National Center of Education Statistics (NCES).

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.2: Effects of Plant Opening on School District Tax Base, Revenue and Expenditure Outcomes: Including Covariate by Year FE

VARIABLES	(1) Ln Tax Base /Stud	(2) Tax Rate (Mills)	(3) Loc Rev /Stud	(4) Tot Rev /Stud	(5) Tot Exp /Stud
Treat x Post Yrs 0-2	0.0210	0.136	0.196***	0.157*	0.144
	(0.0130)	(0.130)	(0.0706)	(0.0869)	(0.231)
Treat x Post Yrs 3-8	0.111***	-0.115	0.581***	0.450***	0.731*
	(0.0267)	(0.153)	(0.133)	(0.128)	(0.381)
Observations	19,508	19,290	34,824	34,824	35,704
R^2	0.976	0.977	0.967	0.955	0.885
Pair x Year FE	Y	Y	Y	Y	Y
Sample	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small
Dep. Var. Mean	580887	10.56	4.991	11.79	12.15

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 natural log of taxable value per student and other property tax and school funding outcomes. Coefficients come from a regression of the outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state (unless otherwise noted) and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). All specifications also include baseline student, baseline free lunch share and 1990 home value by year by state fixed effects. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally unless otherwise noted. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district 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 come from the Energy Information Administration (EIA).

Table 3.A.3: Effects of Plant Opening on School District Tax Base, Revenue and Expenditure Outcomes: Unbalanced Panel

VARIABLES	(1) Ln Tax Base /Stud	(2) Tax Rate (Mills)	(3) Loc Rev /Stud	(4) Tot Rev /Stud	(5) Tot Exp /Stud
Treat x Post Yrs 0-2	0.0189*	0.177**	0.150***	0.133**	0.109
	(0.0111)	(0.0737)	(0.0509)	(0.0585)	(0.137)
Treat x Post Yrs 3-8	0.104***	-0.0758	0.441***	0.345***	0.546**
	(0.0191)	(0.108)	(0.0918)	(0.0914)	(0.244)
Observations	47,798	47,292	83,150	83,000	84,752
R^2	0.970	0.973	0.949	0.927	0.846
Pair x Year FE	Y	Y	Y	Y	Y
Sample	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small
Dep. Var. Mean	580887	10.56	4.991	11.79	12.15

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 natural log of taxable value per student and other property tax and school funding outcomes. Coefficients come from a regression of the outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state (unless otherwise noted) and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with greater than $10,000 in expected tax base per student are included (≈ 10% of openings are dropped). All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally unless otherwise noted. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district 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 come from the Energy Information Administration (EIA).

Table 3.A.4: Effects of Plant Opening on School District Tax Base, Revenue and Expenditure Outcomes: All Openings Included

VARIABLES	(1) Ln Tax Base /Stud	(2) Tax Rate (Mills)	(3) Loc Rev /Stud	(4) Tot Rev /Stud	(5) Tot Exp /Stud
Treat x Post Yrs 0-2	0.0219*	0.0343	0.121**	0.0939	0.0222
	(0.0122)	(0.107)	(0.0543)	(0.0739)	(0.155)
Treat x Post Yrs 3-8	0.0911***	-0.244*	0.404***	0.329***	0.604**
	(0.0222)	(0.127)	(0.0998)	(0.104)	(0.303)
Observations	24,090	23,942	45,414	45,334	46,332
R^2	0.966	0.975	0.956	0.937	0.855
Pair x Year FE	Y	Y	Y	Y	Y
Sample	All	All	All	All	All
Dep. Var. Mean	584616	10.57	5.007	11.84	12.22

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 natural log of taxable value per student and other property tax and school funding outcomes. Coefficients come from a regression of the outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state (unless otherwise noted) and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally unless otherwise noted. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district 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 come from the Energy Information Administration (EIA).

Table 3.A.5: Effects of Plant Opening on School District Tax Base, Revenue and Expenditure Outcomes: By Plant Type

VARIABLES	(1) Tax Base /Stud	(2) Tax Base /Stud	(3) Rate /Stud	(4) Rate /Stud	(5) Loc Rev /Stud	(6) Loc Rev /Stud	(7) Tot Rev /Stud	(8) Tot Rev /Stud
Treat x Post Yrs 0-2	0.00818	0.0481**	-0.0107	0.245	0.0970	0.215*	0.0982	0.199
	(0.0168)	(0.0200)	(0.154)	(0.167)	(0.0651)	(0.119)	(0.0967)	(0.137)
Treat x Post Yrs 3-8	0.0701**	0.158***	-0.170	-0.166	0.358***	0.752***	0.226**	0.727***
	(0.0303)	(0.0356)	(0.190)	(0.189)	(0.116)	(0.226)	(0.113)	(0.240)
Observations	13,668	7,572	13,060	8,032	26,952	12,032	26,892	12,070
R^2	0.958	0.975	0.979	0.964	0.963	0.940	0.936	0.925
Pair x Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Sample	NG	WND	NG	WND	NG	WND	NG	WND
Dep. Var. Mean	496473	660553	10.69	10.48	4.608	5.469	11.33	12.86

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 natural log of taxable value per student and other property tax and school funding outcomes. Coefficients come from a regression of the outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant and whether or not the district receives a plant. Border pairs are any two districts that share a border within the same state (unless otherwise noted) and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Only openings with greater than \$10,000 in expected tax base per student are included ($\approx$ 10% of openings are dropped). Effects for natural gas plants are reported in the odd columns and wind turbines are reported in the even columns. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally unless otherwise noted. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district 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 come from the Energy Information Administration (EIA).

Table 3.A.6: Effects of Plant Opening on School District Tax Base, Revenue and Expenditure Outcomes: Triple Difference Design

VARIABLES	(1) Ln Tax Base /Stud	(2) Tax Rate (Mills)	(3) Loc Rev /Stud	(4) Tot Rev /Stud	(5) Tot Exp /Stud
Treat x Post Yrs 0-2 x Ln Exp Tax Base/Stud	0.0111	0.241***	0.0177**	0.00540	0.00454
	(0.00885)	(0.0693)	(0.00733)	(0.00390)	(0.00554)
Treat x Post Yrs 3-8 x Ln Exp Tax Base/Stud	0.0431***	0.239***	0.0571***	0.0135***	0.0212***
	(0.0163)	(0.0868)	(0.0110)	(0.00494)	(0.00741)
Observations	24,076	23,936	45,410	45,332	46,332
R^2	0.974	0.978	0.975	0.955	0.909
Pair x Year FE	Y	Y	Y	Y	Y
Sample	Non-Small	Non-Small	Non-Small	Non-Small	Non-Small
Dep. Var. Mean	584453	10.40	4.924	11.68	12.22

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 natural log of taxable value per student and other property tax and school funding outcomes. Coefficients come from a regression of the outcome variable on district fixed effects, year x border pair fixed effects and interactions between periods relative to the treatment district of the pair gaining approval for a plant, whether or not the district receives a plant and the log expected tax base impact per student. Expected tax base per student is calculated by Border pairs are any two districts that share a border within the same state (unless otherwise noted) and only one of the two districts experienced a plant opening between 1995 and 2015. Only openings with data 6 years prior to an opening and 8 years following an opening are included unless otherwise indicated. Estimated effect of the opening comes from dividing estimated construction costs (created using parameters provided in the EIA's Annual Energy Outlook) divided by the number of students in a district at the year of approval. All district-years are weighted by the inverse of the total number of districts attached to a given opening in a year in order to weight all plant openings equally unless otherwise noted. We implement two-way clustered standard errors at the plant (all border districts attached to a given opening) and district 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 come from the Energy Information Administration (EIA).

Table 3.A.7: Effects of Plant Opening on Hedonic Characteristics of Homes Sold

VARIABLES	(1) Lot Size	(2) Lot Size	(3) Bedrooms	(4) Bedrooms	(5) Age	(6) Age	(7) Sqft	(8) Sqft	(9) SFH	(10) SFH
Treat x Post Yrs 0-2	-0.00992	-0.0253	0.0370*	0.0291	-0.457	0.249	6.598	12.50	0.00379	3.85e-05
	(0.0122)	(0.0211)	(0.0194)	(0.0437)	(0.382)	(0.713)	(5.801)	(11.22)	(0.00527)	(0.00709)
Treat x Post Yrs 3-8	-0.00992	-0.0215	0.0137	-0.0509*	-0.759	-0.741	-0.0295	-0.000517	0.000648	-0.00517
	(0.00849)	(0.0166)	(0.0205)	(0.0297)	(0.494)	(0.482)	(4.915)	(10.17)	(0.00524)	(0.00777)
Observations	387,584	133,615	402,197	100,502	441,313	140,445	492,910	189,787	501,701	198,578
R^2	0.890	0.899	0.542	0.580	0.845	0.865	0.932	0.894	0.822	0.828
Bandwidth	2 km	2 km	2 km	2 km	2 km	2 km	2 km	2 km	2 km	2 km
Weighted	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sample	All	No CA	All	No CA	All	No CA	All	No CA	All	No CA
Dep Var Mean.	0.383	0.383	3.203	3.203	20.86	20.86	1736	1736	0.829	0.829

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on various hedonic characteristics using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and .004 degree latitude x .004 degree longitude x year fixed effects. Different observations for each outcome occur because different variables have different levels of missing values in the Zillow database. Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—values of the outcome variables above the 99th percentile are excluded as outliers. Only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than $10,000 are excluded. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.A.8: Effects of Plant Openings on Home Prices: Repeat Sales Only

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Ln Price	Ln Price	Ln Price	Ln Price	Ln Price	Ln Price
Treat x Post Yrs 0-2	0.0246	0.0246	0.0141	0.0224	0.0348	0.0141
	(0.0172)	(0.0172)	(0.0168)	(0.0169)	(0.0341)	(0.0164)
Treat x Post Yrs 3-8	0.0293	0.0293	0.0161	0.0310*	0.0500	0.0180
	(0.0180)	(0.0180)	(0.0148)	(0.0169)	(0.0342)	(0.0150)
Observations	308,590	308,590	192,032	324,121	125,988	198,133
R^2	0.875	0.875	0.942	0.881	0.827	0.941
Weighted	Y	Y	Y	Y	Y	Y
Spec	Repeat	Repeat	Repeat	Repeat	Repeat	Repeat
Distr Sample	>10K Pop	>10k Pop	>10k Pop	All	All	All
State Sample	All	No CA	CA Only	All	No CA	CA only

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and an individual parcel fixed effect. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below \$5,000 or greater than \$1,500,000 are excluded as outliers. Only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than \$10,000 are excluded. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.A.9: Effects of Plant Opening on Quantity of Homes Sold

VARIABLES	(1) Ttl Sales	(2) Any Sales	(3) New Sales	(4) Any New	(5) Old Sales	(6) Any Old Sales
Treat x Post Yrs 0-2	13.39	0.0301	5.925	0.0381	7.463	0.0329
	(12.81)	(0.0257)	(7.488)	(0.0344)	(6.574)	(0.0251)
Treat x Post Yrs 3-8	22.49*	0.000793	8.503	0.0851**	13.99*	0.00610
	(12.71)	(0.0268)	(5.695)	(0.0362)	(8.383)	(0.0255)
Observations	3,268	3,268	3,268	3,268	3,268	3,268
R^2	0.944	0.722	0.796	0.862	0.966	0.724
Weighting	All Plant =	All Plant =	All Plant =	All Plant =	All Plant =	All Plant =
Dep. Var. Mean	81.38	0.831	15.36	0.342	66.01	0.818

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-differences design. We only include sales within a bandwidth of 2 km 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. All housing data come from the Zillow ZTRAX database—sales below $5,000 and above $1,500,000 as likely outliers. Only openings with an expected tax base impact of more than $10,000/student are included.

Table 3.A.10: Effects of Plant Openings on Home Prices: Excluding New Construction

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price	(5) Ln Price	(6) Ln Price
Treat x Post Yrs 0-2	0.0310**	0.0543**	0.00605	0.0351**	0.0615**	0.00449
	(0.0137)	(0.0219)	(0.0115)	(0.0153)	(0.0252)	(0.0111)
Treat x Post Yrs 3-8	0.0399***	0.0700***	0.00979	0.0469***	0.0776***	0.0137
	(0.0132)	(0.0186)	(0.0149)	(0.0153)	(0.0238)	(0.0149)
Observations	409,654	164,804	244,850	436,812	184,493	252,319
R^2	0.751	0.692	0.827	0.766	0.728	0.830
Weighted	Y	Y	Y	Y	Y	Y
Distr Sample	>10K Pop	>10k Pop	>10k Pop	All	All	All
State Sample	All	No CA	CA Only	All	No CA	CA Only

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We include sales within a bandwidth of 2 km from the border that were not constructed within a year of the sale date. 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 by month fixed effects and border pair by district by .004 degree latitude and .004 degree longitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below \$5,000 or greater than \$1,500,000 are excluded as outliers. Only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than \$10,000 are excluded as are any homes sold in their construction year. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.A.11: Effects of Plant Openings on Home Prices: Different Sample Criteria and Fixed-Effect Models

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price	(5) Ln Price	(6) Ln Price	(7) Ln Price
Treat x Post Yrs 0-2	0.0217	0.0247	0.00775	0.0233	0.0208	0.0283	0.0467**
	(0.0327)	(0.0187)	(0.0203)	(0.0153)	(0.0150)	(0.0229)	(0.0192)
Treat x Post Yrs 3-8	0.0673**	0.0615***	0.0294	0.0468***	0.0304**	0.0373*	0.0550***
	(0.0286)	(0.0179)	(0.0231)	(0.0158)	(0.0139)	(0.0199)	(0.0188)
Observations	201,810	375,062	378,741	502,930	487,046	504,227	504,677
R^2	0.730	0.743	0.774	0.676	0.789	0.584	0.651
Weighted	Y	Y	Y	Y	Y	Y	Y
Spec	Brdr >5 km from Plnt	No Cnty Bndry	Balanced	.008 Dgr FE	.001 Deg FE	Distr FE	Border Pair x Yr FE

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and border pair x district x .004 degree x .004 degree latitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins).Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. "Brdr >5 km" means all transactions in the border pair are at least 5 km from the plant. "No Cnty Bndry" means there are no county boundaries within 2.5 km of the school district boundary. Balanced means that all openings have data for at least 6 years before approval and 8 years after approval. ".008 Dgr FE" (.001) means that a .008 (.001) degree latitude x .008 (.001) degree longitude x border pair x district fixed effect is included as the geographic fixed effect. "Distr FE" means that a school district x border pair x district fixed effect is included as the geographic fixed effect. "Border pair by year" means border pair by year (instead of border pair by year by month) is included as the time varying fixed effect, a .004 degr x .004 degree x border pair by district fixed effect is also included as in the main specification. All housing data come from the Zillow ZTRAX database—sales below $5,000 or greater than $1,500,000 are excluded as outliers. Only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than $10,000 are excluded. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.A.12: Effects of Plant Openings on Home Prices: By Plant Type

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 Yrs 0-2	0.0281*	-0.0219	0.0281*	-0.0219	0.0273**	0.0371	0.0273**	0.0371
	(0.0142)	(0.0615)	(0.0142)	(0.0615)	(0.0136)	(0.0691)	(0.0136)	(0.0691)
Treat x Post Yrs 3-8	0.0398**	0.123***	0.0398**	0.123***	0.0406**	0.122*	0.0406**	0.122*
	(0.0151)	(0.0377)	(0.0151)	(0.0377)	(0.0172)	(0.0667)	(0.0172)	(0.0667)
Observations	478,916	22,783	478,916	22,783	512,032	26,321	512,032	26,321
R^2	0.701	0.730	0.701	0.730	0.712	0.757	0.712	0.757
Weighted	Y	Y	Y	Y	Y	Y	Y	Y
Spec	.4km grid	.4km grid	.4km grid	.4km grid	.4km grid	.4km grid	.4km grid	.4km grid
Dist Sample	.>10K Pop	.>10K Pop	>10K Pop	.>10K Pop	All	All	All	All
Sample	NG	REN	NG-No CA	REN-No CA	NG	REN	NG-No CA	REN-No CA

Clustered standard errors in parentheses

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

This table shows the effect of a plant opening on log local housing prices using a border difference-in-differences design. We only include sales within a bandwidth of 2 km 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 by month fixed effects and border pair x district x .004 degree x .004 degree latitude fixed effects. The outcome variable is residualized for hedonic by state fixed effects, which include land use, home age by plant district (5 year bins with 1 year bins for ages <5), bedrooms, bathrooms, square footage (500 sq ft bins), heating type and lot size (1 acre bins).Each observation is weighted by the inverse of the number of sales in its treated unit each year (i.e. border pair x treat). Standard errors are clustered at the plant district level. All housing data come from the Zillow ZTRAX database—sales below $5,000 or greater than $1,500,000 are excluded as outliers. Only pairs of districts in which both districts have a 2000 population greater than 10,000 are included in the regression. Openings with an expected tax base impact per student of less than $10,000 are excluded. Data are from fourteen states in total: Arizona, California, Colorado, Georgia, Illinois, Indiana, Iowa, Michigan, Minnesota, New York, North Carolina, Ohio, Oklahoma and Pennsylvania. These make up roughly 60% of openings. Texas and Kansas both have large number of openings, but do not have publicly available home sales data.

Table 3.A.13: Effects of Plant Opening on Key School Finance Variables: Home Price Analysis

VARIABLES	(1) Loc Rev	(2) Loc Rev	(3) Tot Rev	(4) Tot Rev	(5) Ttl Exp	(6) Ttl Exp
Treat x Post Yrs 0-2	0.281***	0.338***	0.331	0.464***	0.273	0.0116
	(0.105)	(0.109)	(0.234)	(0.167)	(0.361)	(0.418)
Treat x Post Yrs 3-8	0.311	0.484***	0.247	0.579***	0.284	0.684
	(0.200)	(0.139)	(0.241)	(0.203)	(0.344)	(0.471)
Observations	498,086	197,591	497,589	197,365	498,086	197,591
R^2	0.978	0.991	0.958	0.989	0.923	0.934
Cut-off	>10K	>20K	>10K	>20K	>10K	>20K
Weighted	Y	Y	Y	Y	Y	Y
Sample	All	No CA	All	No CA	All	No CA

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-differences design. We only include sales within a bandwidth of 2 km 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 (.004 degree latitude) x distance to plant (.004 degree longitude) 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 $5,000 and above $1,500,000 as likely outliers. Only openings with an expected tax base impact of more than $10,000/student are included.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.14: Effects of Plant Opening on Nearby Home Prices: Robustness Check

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price	(5) Ln Price	(6) Ln Price
5–10 km x Post Yrs 0-2	0.00210		0.00344		-0.00702	
	(0.0140)		(0.0174)		(0.0167)	
5–10 km x Post Yrs 3-8	-0.0289		-0.0102		-0.0354	
	(0.0210)		(0.0194)		(0.0240)	
<5 km x Post Yrs 0-2	0.00192		-0.0106		-0.0309*	
	(0.0143)		(0.0150)		(0.0176)	
<5 km x Post Yrs 3-8	-0.0630**		-0.0388		-0.0679**	
	(0.0299)		(0.0246)		(0.0302)	
Ln Dist x Post Yrs 0-2		-0.00673		0.0375*		0.0148
		(0.0140)		(0.0205)		(0.0130)
Ln Dist x Post Yrs 3-8		0.0422*		0.0478**		0.0291
		(0.0226)		(0.0191)		(0.0194)
Observations	1,764,776	1,764,776	1,487,430	1,487,430	1,441,980	1,441,980
R^2	0.703	0.703	0.721	0.721	0.850	0.850
Size Cutoff	>100MW	>100MW	>100MW	>100MW	>100MW	>100MW
Hedonics	Y	Y	Y	Y	Y	Y
Max Dist	15 km	15 km	20 km	20 km	20 km	20 km
Weighted	Y	Y	Y	Y	Y	Y
Model	Base	Base	Balanced Panel	Balanced Panel	Repeat Sale	Repeat Sale

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, .004 degree longitude x .004 degree latitude bins, and district x year fixed effects unless otherwise 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), bedrooms, square footage (500 sq ft bins), and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Only homes within 20 km of the opening plant are included unless otherwise indicated. Only plants who are in districts with a population greater than 10,000 are included in weighted analysis. Standard errors are clustered at the plant district level. Only openings with at least two years of pre and two years of post data are included except for the balanced panel specification in which only plants with at least 6 years of pre data and 8 years of post data are included. All housing data come from the Zillow ZTRAX database—sales that are less than $5,000 or greater than $1,500,000 are excluded as outliers. Weighted specifications are weighted by the inverse of the number of sales within 20 km of the plants in a given year.

Table 3.A.15: Effects of Plant Opening on Nearby Home Prices: No District by Year FE

VARIABLES	(1) Ln Price	(2) Ln Price	(3) Ln Price	(4) Ln Price
5–10 km x Post Yrs 0-2	0.00423 (0.0107)			
5–10 km x Post Yrs 3-8	-0.00804 (0.0128)			
$<$5 km x Post Yrs 0-2	0.0163 (0.0126)			
$<$5 km x Post Yrs 3-8	-0.00596 (0.0213)			
$<$ 5 km x Post Yrs 0–2		-0.00872 (0.00946)		-0.00853 (0.0125)
$<$Km x Post Yrs 3–8		0.000391 (0.0126)		-0.0129 (0.0119)
5-10Km x Post Yrs 0-2 x Nameplate Capac ('00s MW)			0.000140 (0.00215)	
5-10Km x Post Yrs 3-8 x Nameplate Capac ('00s MW)			-0.00551** (0.00265)	
$<$5 km x Post Yrs 0-2 x Nameplate Capac ('00s MW)			-0.000511 (0.00282)	
$<$5 km x Post Yrs 3-8 x Nameplate Capac ('00s MW)			-0.00760** (0.00374)	
Ln Dist x Post Yrs 0-2 x Nameplate Capac ('00s MW)				0.000949 (0.00197)
Ln Dist x Post Yrs 3-8 x Nameplate Capac ('00s MW)				0.00673** (0.00277)
Observations	2,228,460	2,228,460	3,964,622	4,107,596
R^2	0.677	0.677	0.684	0.670
Size Cutoff	$>$100MW	$>$100MW	N	N
Hedonics	Y	Y	Y	Y
Max Dist	20 km	20 km	20 km	20 km
Weighted	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 and .004 degree longitude x .004 degree latitude bins. 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), bedrooms, square footage (500 sq ft bins), and lot size (1 acre bins). Missing hedonics are included as a separate indicator. Only homes within 20 km of the opening plant are included. Only plants who are in districts with a population greater than 10,000 are included in weighted analysis. Standard errors are clustered at the plant district level. Only openings with at least two years of pre and two years of post data are included. All housing data come from the Zillow ZTRAX database—sales that are less than \$5,000 or greater than \$1,500,000 are excluded as outliers. Weighted specifications are weighted by the inverse of the number of sales within 15 km of the plants in a given year.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.16: Census Tract Demographics by Distance to Plant and District Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	LnInc	LnInc	LnHomeVal	LnHomeVal	OwnShare	OwnShare	WhiteShare	WhiteShare
Type	Absolute	Pctile	Absolute	Pctile	Absolute	Pctile	Absolute	Pctile
In Distr, $\leq$5 km from Plant	-0.0557***	-0.0510***	-0.0552***	-0.0426**	-0.0534***	-0.134***	-0.0311***	-0.0938***
	(0.0164)	(0.0181)	(0.0191)	(0.0202)	(0.00853)	(0.0176)	(0.00940)	(0.0199)
In Distr, 5 km-15 km from Plant	-0.00516	-0.0118	0.00270	0.00792	-0.0390***	-0.0919***	-0.0237***	-0.0776***
	(0.0127)	(0.0143)	(0.0151)	(0.0162)	(0.00668)	(0.0146)	(0.00778)	(0.0166)
Outside Distr,$\leq$5 km from Plant	-0.0660***	-0.0542**	-0.0879***	-0.0578**	-0.0473***	-0.0787***	-0.0394***	-0.0707***
	(0.0225)	(0.0221)	(0.0248)	(0.0243)	(0.0114)	(0.0213)	(0.0129)	(0.0231)
Outside Distr, $>$15 km from Plant	-0.0203	-0.0345**	-0.0610***	-0.0332**	-0.0459***	-0.0869***	-0.0345***	-0.0622***
	(0.0147)	(0.0149)	(0.0189)	(0.0169)	(0.00694)	(0.0147)	(0.00894)	(0.0170)
Observations	1,894	1,879	1,893	1,879	1,894	1,879	1,894	1,879
R^2	0.714	0.203	0.831	0.202	0.470	0.222	0.832	0.221

Twoway clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the association between being within a given distance of a plant opening and inside or outside the plant's district and various demographic outcomes. Demographic outcomes categorized as absolute show the association with that demographic's value and outcomes categorized as percentiles shows the association with a tract's outcome's percentile within their school district. Tracts that are in the plant's school district and whose centroid is more than 15 km from the plant are the omitted category. All regressions control for plant district fixed effects and weight all plant districts equally. Demographic data come from the 2000 Census.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.17: School Finance Reform and Power Plants: Openings and Retirements

VARIABLES	(1) Open 50MW	(2) Open 50MW	(3) Open 250MW	(4) Open 250MW	(5) Retire 50MW	(6) Retire 50MW	(7) Retire 250MW	(8) Retire 250MW
Treat x Post Yr ≤5	-0.00348	-0.00510*	-0.00323	-0.00368	0.00217	0.000969	0.00174	0.001000
	(0.00226)	(0.00288)	(0.00212)	(0.00283)	(0.00157)	(0.00170)	(0.00141)	(0.00158)
Treat x Post Yr >5	-0.00174	-0.00159	-0.000473	-2.83e-07	0.000196	-0.000630	0.000454	-0.000504
	(0.00129)	(0.00148)	(0.000987)	(0.00120)	(0.00150)	(0.00154)	(0.00145)	(0.00154)
Observations	212,448	122,938	212,448	122,938	212,448	122,938	212,448	122,938
R^2	0.524	0.522	0.521	0.520	0.545	0.556	0.527	0.533
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	0.00395	0.00368	0.00245	0.00245	0.00165	0.00147	0.00102	0.00102

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

This table shows the effect of a school finance reform on power plant openings and retirements. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Table 3.A.18: Baseline Differences in Key Demographic and Economic Characteristics

VARIABLES	(1) Urban Share	(2) Urban Share	(3) Ln Pop	(4) Ln Pop	(5) White Share	(6) White Share	(7) Pov Share	(8) Pov Share
Treat	0.00918	0.000126	-0.0731	-0.140	0.00107	0.00200	-0.00799	-0.00413
	(0.0165)	(0.0208)	(0.0764)	(0.105)	(0.00584)	(0.00662)	(0.00482)	(0.00640)
Observations	9,000	5,350	9,000	5,350	9,000	5,350	9,000	5,350
R^2	0.608	0.594	0.730	0.709	0.856	0.862	0.805	0.810
Balanced Panel	N	Y	Y	N	Y	N	Y	N

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

This table shows the effect of school finance reforms on baseline covariates. Counties whose geographic centroids were less than 60 miles apart were included. Coefficients come from a regression of the outcome variable on the an indicator for whether a district is in a reform state. Controls include county pair fixed effects. Standard errors are clustered at the state and state border pair level. All data are from the US Census. Population data are annual estimates, while other outcomes are assigned the value of the most recent Decennial Census. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.19: Baseline Differences in Manufacturing and Power Plant Exposure

VARIABLES	(1) EmpManf	(2) EmpManf	(3) BigManfEst	(4) BigManfEst	(5) AnyPlant	(6) AnyPlant	(7) AnyLrgPlnt	(8) AnyLrgPlnt
Treat	11.73***	12.24**	0.0126***	0.0137**	0.0125	0.00747	0.0150	0.00663
	(3.596)	(5.031)	(0.00417)	(0.00544)	(0.0247)	(0.0368)	(0.0186)	(0.0283)
Observations	8,112	4,256	8,768	4,748	9,034	4,894	9,034	5,382
R^2	0.686	0.689	0.654	0.657	0.549	0.551	0.526	0.511
Balanced Panel	Y	N	Y	N	Y	N	Y	N
Sample	Base	Base	Base	Base				

Clustered standard errors in parentheses

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

This table shows the effect of a school finance reform on baseline covariates. Counties whose geographic centroids were less than 60 miles apart were included. Coefficients come from a regression of the outcome variable on the an indicator for whether a district is in a reform state. Controls include county pair fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Table 3.A.20: School Finance Reform and Manufacturing Employment: Robustness Check

VARIABLES	(1) LnEmpManf	(2) LnEmpManf	(3) EmpManf	(4) EmpManf	(5) EmpManf	(6) EmpManf	(7) EmpManf	(8) EmpManf	(9) EmpManf	(10) EmpManf
Treat x Post Yr ≤5	-0.0575**	-0.0606**	-3.287**	-2.569	-3.020**	-2.290	-1.533	-1.731	-3.316*	-2.880
	(0.0270)	(0.0287)	(1.571)	(1.672)	(1.447)	(1.398)	(2.347)	(1.826)	(1.817)	(2.182)
Treat x Post Yr >5	-0.113***	-0.0946**	-7.270***	-5.910**	-8.112***	-7.026***	-4.965**	-4.878**	-7.753**	-6.355*
	(0.0372)	(0.0409)	(2.517)	(2.727)	(1.930)	(2.116)	(2.308)	(2.145)	(3.064)	(3.324)
Observations	165,576	91,792	156,840	83,012	56,618	29,860	37,976	32,756	89,902	63,312
R^2	0.953	0.957	0.956	0.961	0.957	0.967	0.954	0.961	0.963	0.965
Robustness	Log DV	Log DV	Pre-1998	Pre-1998	w/i .5 ln(bl manuf)	w/i .5 ln(bl manuf)	w/i 25/1K	w/i 25/1K	No outlier states	No outlier states
Dep. Var Mean	70.29	70.29	72.01	72.01	85.46	85.46	56.23	56.23	61.96	61.96

Clustered standard errors in parentheses

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

This table shows the effect of a school finance reform on manufacturing outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Sample restrictions are as indicated. “Outlier states” refers to states whose baseline difference in manufacturing employment was greater than 20 workers per 1,000 population. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.21: School Finance Reform and Manufacturing Establishments: Robustness Check

VARIABLES	(1) LnLrgManfEst	(2) LnLrgManfEst	(3) LrgManfEst	(4) LrgManfEst	(5) LrgManfEst	(6) LrgManfEst	(7) LrgManfEst	(8) LrgManfEst	(9) LrgManfEst	(10) LrgManfEst
Treat x Post Yr ≤5	-0.117**	-0.109**	-0.00680**	-0.00495*	-0.00492**	-0.00423	-0.00324	-0.00407	-0.00306*	-0.00359**
	(0.0497)	(0.0480)	(0.00259)	(0.00250)	(0.00238)	(0.00260)	(0.00378)	(0.00281)	(0.00159)	(0.00176)
Treat x Post Yr >5	-0.169***	-0.147***	-0.0118***	-0.00959***	-0.0108***	-0.0104***	-0.00682*	-0.00929***	-0.00718**	-0.00630**
	(0.0533)	(0.0542)	(0.00354)	(0.00348)	(0.00272)	(0.00313)	(0.00367)	(0.00301)	(0.00284)	(0.00302)
Observations	201,752	117,162	190,660	106,154	59,498	31,992	45,546	94,870	116,132	85,332
R^2	0.902	0.912	0.900	0.915	0.900	0.916	0.889	0.910	0.910	0.916
Robustness	Log DV	Log DV	Pre-1998	Pre-1998	w/i .5 ln(bl manuf)	w/i .25 ln(bl manuf)	w/i .25 ln(bl manuf)	w/i .25 ln(bl manuf)	States w/ no dif	States w/ no dif
Dep. Var Mean	68.48	68.48	0.0608	0.0608	0.0743	0.0743	0.0461	0.0630	0.0484	0.0484

Twoway clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on manufacturing outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Sample restrictions are as indicated. "Outlier states" refers to states whose baseline difference in manufacturing employment was greater than 20 workers per 1,000 population. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.22: School Finance Reform and Employment and Establishments for Non-Manufacturing Industries

VARIABLES	(1) Non-Manf	(2) Non-Manf	(3) Non-Manf Est>250Emp	(4) Non-Manf Est>250Emp	(5) Non-Manf Est>500Emp	(6) Non-Manf Est>500Emp
Treat x Post Yrs ≤5	0.0329	0.0611	-6.72e-05	0.000609	-0.000407	-0.00141
	(1.317)	(1.562)	(0.00100)	(0.00114)	(0.000652)	(0.00128)
Treat x Post Yrs ¿5	0.115	0.364	0.000876	0.00243	0.000211	-0.000402
	(2.066)	(2.498)	(0.00177)	(0.00190)	(0.000750)	(0.00122)
Observations	167,412	92,432	208,062	120,052	119,938	119,938
R^2	0.970	0.973	0.873	0.885	0.879	0.892
Weight	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	186.3	160.7	0.0277	0.0324	0.0104	0.0134

Clustered 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 outcomes by industry using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed-effects. Standard errors are clustered at the state and state border pair level. Employment data come from County Business Patterns and are suppressed for counties with few establishments. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. We exclude outcome values greater than the 99th percentile as outliers.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.23: 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
Treat x Post Yr $\leq$5	0.0859	-1.667	0.108	-0.0799	-0.472	0.251	0.207
	(0.171)	(1.223)	(0.240)	(0.531)	(0.286)	(0.259)	(1.344)
Treat x Post Yr >5	0.184	-0.751	-0.238	-0.464	-0.562	0.439	0.0101
	(0.205)	(1.521)	(0.344)	(0.791)	(0.432)	(0.398)	(2.124)
Observations	24,686	20,780	102,058	118,298	87,842	103,678	120,498
R^2	0.859	0.931	0.913	0.954	0.911	0.935	0.922
Dep. Var Mean	1.612	11.27	11.65	47.08	11.17	13.60	85.82

Clustered 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 outcomes by industry using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing employment data come from County Business Patterns. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. We exclude outcome values greater than the 99th percentile as outliers.

Table 3.A.24: School Finance Reform and Power Plant Openings: School District Overlap

VARIABLES	(1) Open>25MW	(2) Open>25MW	(3) Open>25MW	(4) Open>25MW	(5) Open>100MW	(6) Open>100MW	(7) Open>100MW	(8) Open>100MW
Treat x Post Yr $\leq$5 x Overlap	-7.399*	-6.682	0.00492	0.00641	-0.0210	-0.0274	-0.0233	-0.0373
	(4.294)	(5.723)	(0.00847)	(0.00736)	(0.0272)	(0.0445)	(0.0300)	(0.0402)
Treat x Post Yr $\leq$5 x Overlap	-24.61***	-29.17***	-0.00733	-0.0115	-0.0400	-0.0498	-0.0556	-0.0716
	(8.596)	(10.45)	(0.00948)	(0.0117)	(0.0471)	(0.0650)	(0.0429)	(0.0535)
Observations	197,854	114,286	206,366	119,352	212,112	122,708	212,112	122,708
R^2	0.950	0.954	0.912	0.922	0.972	0.976	0.968	0.971
Dep. Var Mean	69.30	69.30	0.0609	0.0609	0.216	0.216	0.140	0.140

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

This table shows the effect of school finance reform on manufacturing and power generation outcomes and the degree of within-county overlap between school and zoning jurisdictions using a county border pair difference-in-differences design. Overlap is measured as the interaction between population-weighted HHI of school districts within each zoning jurisdiction in a county and the population-weighted HHI of zoning districts within each school district in a county. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.25: School Finance Reform and Power Plant Openings: SUTVA Check

VARIABLES	(1) EmpManf	(2) EmpManf	(3) LrgMnfEst	(4) LrgMnfEst	(5) AnyGen(50)	(6) AnyGen(50)	(7) AnyGen(250)	(8) AnyGen(250)
Treat x Post Yrs ≤5 x Dist to Bord.	0.0325		3.17e-05		0.000107		5.63e-05	
	(0.0720)		(0.000130)		(0.000611)		(0.000519)	
Treat x Post Yrs >5 x Dist to Bord.	-0.0304		-3.65e-05		0.000194		0.000229	
	(0.0870)		(0.000142)		(0.000705)		(0.000662)	
Treat x Post Yrs ≤5		-1.596		-0.00353		-0.0101	-0.0152	-0.0105
		(1.675)		(0.00231)		(0.0155)		(0.0151)
Treat x Post Yrs >5		-7.008**		-0.00778**		-0.0294	-0.0322	-0.0242
		(2.919)		(0.00345)		(0.0190)		(0.0196)
Observations	176,266	95,792	232,134	121,578	238,364	125,856	238,364	125,856
R^2	0.957	0.958	0.915	0.902	0.970	0.964	0.964	0.953
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Balanced	Balanced	Balanced	Balanced	Balanced	Balanced	Balanced	Balanced
Sample Type	All	>45 Mi border	All	>45 Mi border	All	>45 Mi border	All	>45 Mi border
Dep. Var Mean	70.51	67.24	0.0598	0.0529	0.227	0.224	0.133	0.133

Twoway clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 90 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers. Distance to border refers to the mininum distance of the control in a pair to its nearest treated neighbor.

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

Table 3.A.26: School Finance Reform and Power Plant Openings: Weighting by State

VARIABLES	(1) EmpManf	(2) EmpManf	(3) LrgMnfEst	(4) LrgMnfEst	(5) AnyPlnt50	(6) AnyPlnt50	(7) AnyPlnt500	(8) AnyPlnt500
Treat x Post Yr ≤ 5	-4.699***	-3.589**	-0.00618***	-0.00638**	0.0165	-0.00108	0.00632	-0.00957
	(1.659)	(1.726)	(0.00217)	(0.00237)	(0.0154)	(0.0158)	(0.0113)	(0.0104)
Treat x Post Yr >5	-10.33***	-9.210***	-0.0121***	-0.0128***	-0.0233	-0.0297	-0.0284	-0.0331
	(2.468)	(2.799)	(0.00325)	(0.00397)	(0.0236)	(0.0254)	(0.0208)	(0.0224)
Observations	165,876	94,038	210,828	121,876	212,448	122,938	212,448	122,938
R^2	0.959	0.965	0.911	0.925	0.960	0.964	0.956	0.960
Weight	All State=	All State=	All State=	All State=	All State=	All State=	All State=	All State=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	68.20	67.79	0.0539	0.0532	0.234	0.215	0.159	0.144

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

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. All observations are weighted such that each treated state-year counts equally, and each treated county receves equal weight within each treated state. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.27: School Finance Reform and Power Plant Openings: Weighting by Population

VARIABLES	(1) EmpManf	(2) EmpManf	(3) LrgMnfEst	(4) LrgMnfEst	(5) AnyPlnt50	(6) AnyPlnt50	(7) AnyPlnt500	(8) AnyPlnt500
Treat x Post Yr $\leq$5	-5.651***	-3.240*	-0.00734**	-0.00720**	-0.0678	-0.116	-0.0686	-0.115
	(1.559)	(1.851)	(0.00291)	(0.00318)	(0.0528)	(0.0862)	(0.0528)	(0.0862)
Treat x Post Yr >5	-10.41***	-9.654**	-0.00981***	-0.00973***	-0.222**	-0.298**	-0.222**	-0.293**
	(3.333)	(4.033)	(0.00349)	(0.00323)	(0.0957)	(0.120)	(0.0952)	(0.122)
Observations	165,876	91,940	210,828	121,876	212,448	122,938	212,448	122,938
R^2	0.964	0.968	0.927	0.938	0.947	0.933	0.943	0.928
Weight	Pop	Pop	Pop	Pop	Pop	Pop	Pop	Pop
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Dep. Var Mean	93.70	92.01	0.0760	0.0744	0.497	0.493	0.390	0.393

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

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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). All observations are weighted by county population times the inverse of the number of border pairs a county has in a given year. Controls include county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.28: School Finance Reform and Manufacturing Employment by Bandwidth Distance

VARIABLES	(1) EmpManf	(2) EmpManf	(3) EmpManf	(4) EmpManf	(5) EmpManf	(6) EmpManf	(7) EmpManf	(8) EmpManf	(9) EmpManf	(10) EmpManf
Treat x Post Yr ≤5	-5.068**	-4.357**	-3.021**	-2.008	-2.752*	-2.051	-1.876	-1.264	-1.529	-0.846
	(2.218)	(1.979)	(1.486)	(1.378)	(1.455)	(1.527)	(1.350)	(1.425)	(1.301)	(1.378)
Treat x Post Yr >5	-7.561***	-8.129***	-6.879***	-6.244***	-7.027***	-6.131***	-5.500***	-4.586**	-5.111**	-4.267**
	(2.714)	(2.710)	(2.073)	(2.233)	(2.056)	(2.226)	(1.877)	(2.093)	(1.921)	(2.110)
Observations	20,950	11,338	69,724	38,962	161,216	89,556	291,016	159,870	446,176	247,654
R^2	0.956	0.962	0.957	0.962	0.954	0.959	0.952	0.956	0.952	0.956
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Max BW	15	15	45	45	60	60	75	75	90	90
Dep. Var Mean	75.25	78.45	71.82	74.95	69.36	73.18	68.98	72.72	68.20	71.98

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

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were closer together than the max bandwidth specified were included in each analysis. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.29: School Finance Reform and Manufacturing Establishments by Bandwidth Distance

VARIABLES	(1) LrgMnfEst	(2) LrgMnfEst	(3) LrgMnfEst	(4) LrgMnfEst	(5) LrgMnfEst	(6) LrgMnfEst	(7) LrgMnfEst	(8) LrgMnfEst	(9) LrgMnfEst	(10) LrgMnfEst
Treat x Post Yr ≤ 5	-0.0116***	-0.00648*	-0.00692**	-0.00329	-0.00659**	-0.00482	-0.00599**	-0.00514*	-0.00574**	-0.00469
	(0.00416)	(0.00326)	(0.00310)	(0.00265)	(0.00275)	(0.00290)	(0.00288)	(0.00293)	(0.00272)	(0.00287)
Treat x Post Yr >5	-0.0143***	-0.00942*	-0.0116***	-0.00884**	-0.0115***	-0.00936***	-0.00974***	-0.00864***	-0.00860***	-0.00737**
	(0.00512)	(0.00466)	(0.00358)	(0.00347)	(0.00313)	(0.00327)	(0.00290)	(0.00301)	(0.00284)	(0.00297)
Observations	27,216	15,380	69,724	43,134	161,216	98,716	291,016	176,758	446,176	272,004
R^2	0.902	0.917	0.902	0.922	0.903	0.918	0.900	0.913	0.900	0.913
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Max BW	15	15	45	45	60	60	75	75	90	90
Dep. Var Mean	0.0698	0.0711	0.0665	0.0713	0.0649	0.0682	0.0636	0.0678	0.0627	0.0667

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

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were closer together than the max bandwidth specified were included in each analysis. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers.

Table 3.A.30: School Finance Reform and Power Plant Openings by Bandwidth Distance

VARIABLES	(1) AnyGen	(2) AnyGen	(3) AnyGen	(4) AnyGen	(5) AnyGen	(6) AnyGen	(7) AnyGen	(8) AnyGen	(9) AnyGen	(10) AnyGen
Treat x Post Yr ≤ 5	-0.0102	-0.00259	-0.00281	-0.00292	-0.00583	-0.0110	-0.00160	-0.00881	-0.00106	-0.00956
	(0.0134)	(0.0135)	(0.00909)	(0.00930)	(0.00892)	(0.00979)	(0.00813)	(0.00796)	(0.00712)	(0.00868)
Treat x Post Yr >5	-0.0152	-0.0130	-0.0150	-0.0141	-0.0281*	-0.0302*	-0.0200*	-0.0237*	-0.0173	-0.0230*
	(0.0151)	(0.0182)	(0.0135)	(0.0159)	(0.0141)	(0.0164)	(0.0119)	(0.0135)	(0.0106)	(0.0128)
Observations	27,438	15,838	91,978	54,032	212,448	122,938	384,308	220,086	588,130	338,396
R^2	0.970	0.973	0.968	0.972	0.965	0.969	0.966	0.971	0.965	0.970
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Max BW	30	30	45	45	60	60	75	75	90	90
Dep. Var Mean	0.246	0.233	0.222	0.217	0.214	0.215	0.211	0.212	0.217	0.220

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

This table shows the effect of a school finance reform on power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were closer than the indicated maximum bandwidth were included in each regression. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event.

Table 3.A.31: School Finance Reform and Manufacturing Employment: Alternate Reform Identification Strategy

VARIABLES	(1) EmpManf	(2) EmpManf	(3) LrgManf	(4) LrgManf	(5) AnyGen50	(6) AnyGen50	(7) AnyGen500	(8) AnyGen500
Treat x Post Yrs $\leq$5	-2.596*	-2.112	-0.00654**	-0.00586*	-0.0245**	-0.0466**	-0.0137	-0.0300
	(1.300)	(1.626)	(0.00244)	(0.00293)	(0.00954)	(0.0186)	(0.00848)	(0.0197)
Treat x Post Yrs >5	-4.972*	-3.841	-0.00928***	-0.00657*	-0.0365**	-0.0685**	-0.0140	-0.0393
	(2.570)	(3.310)	(0.00327)	(0.00365)	(0.0171)	(0.0259)	(0.0156)	(0.0260)
Observations	179,022	74,092	215,728	79,856	229,150	86,710	229,150	86,710
R^2	0.955	0.959	0.899	0.913	0.966	0.965	0.959	0.958
Weight	All County=	All County=	All County=	All County=	All County=	All County=	All County=	All County=
Controls	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year	Pair x Year
Sample	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced	Balanced
Dep. Var Mean	74.12	84.93	0.0671	0.0671	0.224	0.224	0.148	0.148

Twoway clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the effect of a school finance reform on manufacturing and power generation outcomes using a county border pair difference-in-differences design. Counties whose geographic centroids were less than 60 miles apart were included. 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 county pair by year fixed effects and county fixed effects. Standard errors are clustered at the state and state border pair level. Manufacturing data come from County Business Patterns. Power plant data come from EIA Form 860. Only plants that were owned by utilities still in operation in 1990 were included. Counties with fewer than 1,000 population in 1970 are excluded. Our unbalanced sample consists of 14 years before and after the reform year, while our balanced sample consists of 8 years prior and 14 years following the event. Outcomes with values greater than the 99th percentile are excluded as outliers. Reform events were the first event in each state identified by Jackson et al. (2014).

3.B ZTRAX Database

Home sales data come from the Zillow ZTRAX database and are merged to assessment records using parcel ID. We restrict sales to properties categorized by Zillow as single-family units. This includes single-family homes and condominiums, but excludes multi-family units although results are robust to their inclusion. Included sales are non-foreclosures 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 flagged by Zillow as 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.[38] 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.[39] 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 originate from county governments. Because data are provided at the county level, the years in which counties enter our sample differ even within a state. However, we cannot simply use the first year a county has a transaction in the data as the year in which data becomes available for a county because many counties include a small minority of transactions ($< .1\%$ of housing units) for many years in the past before reporting all transactions. Accordingly, we identify the starting year for each county in the following way.

[38] 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.

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

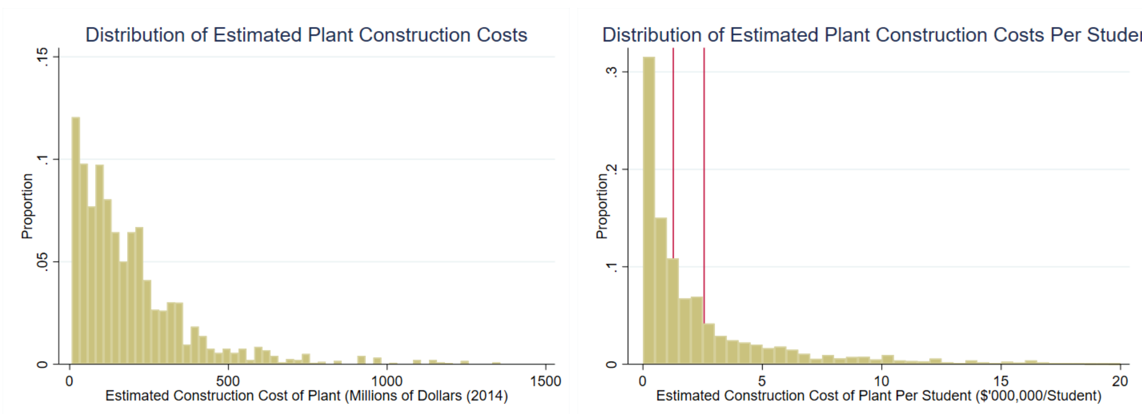
We first identify all years in which a county had a greater than 300% increase in sales (off a minimum of a base of 5 transactions). This threshold is chosen because it is greater than any increase we would expect to observe in the course of normal annual fluctuations and therefore is likely driven by changes in reporting. We then 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 transactions are recorded if $>$300% increase never occurred). We drop all transactions prior to our empirically-defined "start year" from our analysis. Results are robust to alternative specifications of start year. Finally, because our home price analysis uses a border difference-in-differences design that in some cases span counties we drop all transactions within the border-pair prior to the year in which the last county began full transaction reporting to ensure sample consistency.

3.C 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 of overnight construction costs per kWh 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 using either one. All estimates were adjusted for inflation and are presented in 2014 dollars.

An important note is that EIA estimates are presented for the construction of 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 3.C.1 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 allow for better interpretability, we drop all impacts above 2 million dollars of construction costs per student (<2% of plants) from the figure. Most districts have increases in expected tax base large enough to expect meaningful fiscal impact for local schools—the median opening has roughly $127,000 in estimated construction costs per student, while the mean opening has approximately $256,000 per student in estimated construction costs.[40]



This figure shows various summary statistics on estimated plant construction costs, which we use as a proxy of plant valuation. Both figures show the distribution of estimated plant construction costs and construction costs per student (we exclude the small number of plant openings with grater than $2 million in construction costs per student). Construction cost data and plant opening data come from the EIA, while enrollment data comes from the NCES.

Figure 3.C.1: Estimated Fiscal Impact

[40]Most school district property taxes are between .3%-1.5%.

3.D 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 to which increases in local revenue crowd out state and federal transfers 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 next 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 state had multiple reform 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 their foundation value 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,

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

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 3.D.1 shows the correlation between our qualitatively-identified reforms and the first major reform identified by Jackson et al. (2014) in their paper examining the long-run educational and labor market effects of these reforms. Note that Jackson et al. (2014) were searching for reforms that changed the distribution of school funding, not events that shocked the marginal value of tax base per se, and therefore we would not necessarily expect to identify all the same years. 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 et al. (2014). There were a further ten states that had reforms as identified by Jackson et al. (2014), but for which we did not find a major change in their marginal value of tax base or tax limitations. Nonetheless, this coincidence in reforms is reassuring.

This figure compares the year of our qualitatively-identified reform with the reform in a state identified by Jackson et al. (2014).

Figure 3.D.1: Correlation Between Qualitatively-Defined Reforms and Reforms Identified in Jackson et al. (2014)

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	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)*P	.01*P	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
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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post re-form tax limit	Shock Driver
CT	1990	Legislation	Guaranteed Tax Base in which each district was guaranteed $\tau^* P_8 5$, but b/c underfunded only received pro-rated share of available funding (which was very low)	τ^* $Inc_D/IncMax$ 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
ID	1979	Initiative	.022*P	.0036*P	.027 (no limit with vote)	..004	Limitation
IL	1973	Legislation	1.12*.011*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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post reform tax limit	Shock Driver
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
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	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 /AvgInc, but complicated so does not apply uniformly to all districts	.025 (all rates)	.025 (all rates)	Crowd-out
MI	1993	Initiative	τ*P if value/student <$40K (up to first 30 mills)	.018*P	.05 (all taxes)	.021	Crowd-out (Proportional) + Limitation

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post reform tax limit	Shock Driver
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	τ*P* Inc/AvgInc	No-limit w/ vote	No limit w/vote	Crowd-out
MT	1989	Litigation	0	(τ^*)*P where $\tau*$ is the tax rate necessary to get 40% of state funding on 175% x avg state val (so 0 If >175% Avg Wealth)	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 negligible)	.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		.				

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post reform tax limit	Shock Driver
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
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
OH	None						
OK	1990	.018*P (but many districts (≈50% held harmless)	Nearly full crowd-out for districts with <$55K/student				

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post reform tax limit	Shock Driver
OR	1991	Initiative	τ^{*}*P where τ^{*} 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						
SC	1977	Legislation	None	τ^{*}*P where τ^{*} is tax rate necessary to generate 30% of guaranteed funding	No	No	Crowd-out
SD	Legislation	1986	.018*P	τ^{*}*P where τ^{*} 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)	τ^{*}*P where τ^{*} 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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at `http://www.zillow.com/ztrax`. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

St	Yr	Type	Pre-Reform Crowd-out	Post-Reform Crowd-out	Pre-reform tax limit	Post reform tax limit	Shock Driver
UT	None						
VT	1997	Legislation	.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
WV	None						
WI	1973	Legislation	τ^{*}*P where τ^{*} is tax producing equalization amt in district with 45k val/stud	τ^{*}*P where τ^{*} 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

Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of the Zillow Group.

References

Abel, J. R., Deitz, R., and Su, Y. (2014). Are recent college graduates finding good jobs? *Current Issues in Economics and Finance*, 20(3).

Aladangady, A. (2017). Housing wealth and consumption: Evidence from geographically-linked microdata. *American Economic Review*, 107(11):3415–46.

Alesina, A., Baqir, R., and Easterly, W. (1999). Public goods and ethnic divisions. *The Quarterly Journal of Economics*, 114(4):1243–1284.

Altonji, J. G., Kahn, L. B., and Speer, J. D. (2016). Cashier or consultant? entry labor market conditions, field of study, and career success. *Journal of Labor Economics*, 34(S1):S361–S401.

Anderson, N. B. (2006). Beggar thy neighbor? property taxation of vacation homes. *National Tax Journal*, pages 757–780.

Angrist, J. and Guryan, J. (2005). Does Teacher Testing Raise Teacher Quality? Evidence from State Certification Requirements. IZA Discussion Papers 1500, Institute for the Study of Labor (IZA).

Antipov, E. A. and Pokryshevskaya, E. B. (2012). Mass appraisal of residential apartments: An application of random forest for valuation and a cart-based approach for model diagnostics. *Expert Systems with Applications*, 39(2):1772 – 1778.

Bacolod, M. P. (2007). Do Alternative Opportunities Matter? The Role of Female Labor Markets in the Decline of Teacher Quality. *The Review of Economics and Statistics*, 89(4):737–751.

Ballou, D. (1996). Do public schools hire the best applicants? *The Quarterly Journal of Economics*, 111(1):97–133.

Banzhaf, H. S. (2013). The market for local public good. *Case W. Res. L. Rev.*, 64:1441.

Banzhaf, S., Ma, L., and Timmins, C. (2019). Environmental justice: The economics of race, place, and pollution. *Journal of Economic Perspectives*, 33(1):185–208.

Barrows, G., Garg, T., and Jha, A. (2018). The economic benefits versus environmental costs of india's coal fired power plants. *Available at SSRN 3281904*.

Barseghyan, L. and Coate, S. (2016). Property taxation, zoning, and efficiency in a dynamic tiebout model. *American Economic Journal: Economic Policy*, 8(3):1–38.

Bartik, A., Currie, J., Greenstone, M., and Knittel, C. R. (2018). The local economic and welfare consequences of hydraulic fracturing. *Available at SSRN 2692197*.

Bayer, P., Ferreira, F., and McMillan, R. (2007). A unified framework for measuring preferences for schools and neighborhoods. *Journal of political economy*, 115(4):588–638.

Beaudry, P. and DiNardo, J. (1991). The effect of implicit contracts on the movement of wages over the business cycle: Evidence from micro data. *Journal of Political Economy*, 99(4):pp. 665–688.

Benabou, R. (1996). Heterogeneity, stratification, and growth: macroeconomic implications of community structure and school finance. *The American Economic Review*, pages 584–609.

Benabou, R. (2000). Unequal societies: Income distribution and the social contract. *American Economic Review*, 90(1):96–129.

Benítez-Silva, H., Eren, S., Heiland, F., and Jiménez-Martín, S. (2015). How well do individuals predict the selling prices of their homes? *Journal of Housing Economics*, 29:12–25.

Biasi, B. (2019). School finance equalization increases intergenerational mobility: Evidence from a simulated-instruments approach. Technical report, National Bureau of Economic Research.

Black, D. A. and Smith, J. A. (2004). How robust is the evidence on the effects of college quality? evidence from matching. *Journal of econometrics*, 121(1-2):99–124.

Black, D. A. and Smith, J. A. (2006). Estimating the returns to college quality with multiple proxies for quality. *Journal of Labor Economics*, 24(3):701–728.

Black, S. E. (1999a). Do better schools matter? parental valuation of elementary education. *The Quarterly Journal of Economics*, 114(2):577–599.

Black, S. E. (1999b). Do better schools matter? parental valuation of elementary education. *The Quarterly Journal of Economics*, 114(2):577–599.

Boer, J. T., Pastor, M., Sadd, J. L., and Snyder, L. D. (1997). Is there environmental racism? the demographics of hazardous waste in los angeles county. *Social Science Quarterly*, 78(4):793–810.

Bogin, A., Doerner, W., and Larson, W. (2019). Local house price dynamics: New indices and stylized facts. *Real Estate Economics*, 47(2):365–398.

Borge, L.-E. and Rattsø, J. (2004). Income distribution and tax structure: Empirical test of the meltzerrichard hypothesis. *European Economic Review*, 48(4):805 – 826.

Borge, L.-E. and Rattsø, J. (2014). Capitalization of property taxes in norway. *Public Finance Review*, 42(5):635–661.

Borjas, G. J. (2002). The wage structure and the sorting of workers into the public sector. Technical report, National Bureau of Economic Research.

Boustan, L., Ferreira, F., Winkler, H., and Zolt, E. M. (2013). The effect of rising income inequality on taxation and public expenditures: Evidence from us municipalities and school districts, 1970–2000. *Review of Economics and Statistics*, 95(4):1291–1302.

Bowlus, A. J. (1995). Matching workers and jobs: Cyclical fluctuations in match quality. *Journal of Labor Economics*, pages 335–350.

Boyd, D., Lankford, H., Loeb, S., Ronfeldt, M., and Wyckoff, J. (2011). The role of teacher quality in retention and hiring: Using applications to transfer to uncover preferences of teachers and schools. *Journal of Policy Analysis and Management*, 30(1):88–110.

Boyd, D., Lankford, H., Loeb, S., and Wyckoff, J. (2005). The draw of home: How teachers' preferences for proximity disadvantage urban schools. *Journal of Policy Analysis and Management*, 24(1):113–132.

Boyd, D., Lankford, H., Loeb, S., and Wyckoff, J. (2013). Analyzing the determinants of the matching of public school teachers to jobs: Disentangling the preferences of teachers and employers. *Journal of Labor Economics*, 31(1):83–117.

Bradley, S. (2017). Inattention to deferred increases in tax bases: How michigan homebuyers are paying for assessment limits. *Review of Economics and Statistics (forthcoming)*, 99.

Brewer, D. J., Eide, E. R., and Ehrenberg, R. G. (1999). Does it pay to attend an elite private college? cross-cohort evidence on the effects of college type on earnings. *The Journal of Human Resources*, 34(1):104–123.

Britton, J. and Propper, C. (2016). Teacher pay and school productivity: Exploiting wage regulation. *Journal of Public Economics*, 133:75–89.

Brueckner, J. K. (2000). A tiebout/tax-competition model. *Journal of Public Economics*, 77(2):285–306.

Brunner, E. J. and Ross, S. L. (2010). Is the median voter decisive? evidence from referenda voting patterns. *Journal of Public Economics*, 94(11-12):898–910.

Burnes, D., Neumark, D., and White, M. J. (2011). Fiscal zoning and sales taxes: do higher sales taxes lead to more retailing and less manufacturing? Technical report, National Bureau of Economic Research.

Cabral, M. and Hoxby, C. (2012). The hated property tax: salience, tax rates, and tax revolts. Technical report, National Bureau of Economic Research.

Calabrese, S. M., Epple, D. N., and Romano, R. E. (2011). Inefficiencies from metropolitan political and fiscal decentralization: Failures of tiebout competition. *The Review of Economic Studies*, 79(3):1081–1111.

Caplan, B. (2001). Standing tiebout on his head: Tax capitalization and the monopoly power of local governments. *Public Choice*, 108(1-2):101–122.

Card, D. and Payne, A. A. (2002). School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of public economics*, 83(1):49–82.

Careaga, M. and Weingast, B. (2003). Fiscal federalism, good governance, and economic growth in mexico. *In search of prosperity: analytical narratives on economic growth*, pages 399–435.

Cellini, S. R., Ferreira, F., and Rothstein, J. (2010). The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125(1):215–261.

Chamberlain, A. (2007). What does america think about taxes. *The 2007 annual survey of US attitudes on taxes and wealth. Tax Foundation Special Report*, (157).

Chan, S. (2001). Spatial lock-in: Do falling house prices constrain residential mobility? *Journal of urban Economics*, 49(3):567–586.

Chatterjee, S. and Eyigungor, B. (2015). A quantitative analysis of the us housing and mortgage markets and the foreclosure crisis. *Review of Economic Dynamics*, 18(2):165–184.

Cirillo, R., Wolsko, T., Mueller, R., Dauzvardis, P., Senew, M., Gamauf, K., and Seymour, D. (1977). Evaluation of regional trends in power plant siting and energy transport. Technical report, Argonne National Lab., IL (USA).

Clotfelter, C., Ladd, H. F., Vigdor, J., and Wheeler, J. (2006). High-poverty schools and the distribution of teachers and principals. *NCL Rev.*, 85:1345.

Cloyne, J., Huber, K., Ilzetzki, E., and Kleven, H. (2019). The effect of house prices on household borrowing: A new approach. *American Economic Review*, 109(6):2104–36.

Corcoran, S. and Evans, W. N. (2010). Income inequality, the median voter, and the support for public education. Technical report, National Bureau of Economic Research.

Corcoran, S. P., Evans, W. N., and Schwab, R. M. (2004a). Changing labor-market opportunities for women and the quality of teachers, 1957-2000. *American Economic Review*, 94(2):230–235.

Corcoran, S. P., Evans, W. N., and Schwab, R. M. (2004b). Women, the labor market, and the declining relative quality of teachers. *Journal of Policy Analysis and Management*, 23(3):449–470.

Cowan, J., Goldhaber, D., Hayes, K., and Theobald, R. (2016). Missing elements in the discussion of teacher shortages. *Educational Researcher*, 45(8):460–462.

Currie, J., Davis, L., Greenstone, M., and Walker, R. (2015). Environmental health risks and housing values: evidence from 1,600 toxic plant openings and closings. *American Economic Review*, 105(2):678–709.

Davis, L. W. (2011). The effect of power plants on local housing values and rents. *Review of Economics and Statistics*, 93(4):1391–1402.

Davis, M. and Ferreira, F. V. (2017). Housing disease and public school finances. Technical report, National Bureau of Economic Research.

DiPasquale, D. and Glaeser, E. (1999). Incentives and social capital: Are homeowners better citizens? *Journal of Urban Economics*, 45(2):354–384.

Dolton, P. J. (2006). Chapter 19 teacher supply. volume 2 of *Handbook of the Economics of Education*, pages 1079 – 1161. Elsevier.

Dube, A., Lester, T. W., and Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The review of economics and statistics*, 92(4):945–964.

Epple, D. and Romano, R. E. (1996). Ends against the middle: Determining public service provision when there are private alternatives. *Journal of Public Economics*, 62(3):297–325.

Evans, W. N., Schwab, R. M., and Wagner, K. L. (2014). The great recession and public education.

Falch, T., Johansen, K., and Strm, B. (2009). Teacher shortages and the business cycle. *Labour Economics*, 16(6):648 – 658. European Association of Labour Economists 20th annual conference University of Amsterdam, Amsterdam, The Netherlands 1820 September 2008.

Farnham, M. and Sevak, P. (2006). State fiscal institutions and empty-nest migration: Are tiebout voters hobbled? *Journal of Public Economics*, 90(3):407–427.

Ferreira, F., Gyourko, J., and Tracy, J. (2010). Housing busts and household mobility. *Journal of urban Economics*, 68(1):34–45.

Ferrey, S. (2016). Siting technology, land-use energized. *Cath. UL Rev.*, 66:1.

Figlio, D. N. (2002). Can public schools buy better-qualified teachers? *Industrial and Labor Relations Review*, 55(4):pp. 686–699.

Fischel, W. A. (1998). School finance litigation and property tax revolts: How undermining local control turns voters away from public education. *Lincoln Institute of Land Policy, Cambridge, MA*.

Fischel, W. A. (2010). The congruence of american school districts with other local government boundaries: A google-earth exploration. *Available at SSRN 967399*.

Fischel, W. A. (2013). Fiscal zoning and economists' views of the property tax. *Lincoln Institute of Land Policy, Cambridge, MA*.

Fligstein, N., Hastings, O. P., and Goldstein, A. (2017). Keeping up with the joneses: How households fared in the era of high income inequality and the housing price bubble, 1999–2007. *Socius*, 3:2378023117722330.

Gadenne, L. and Singhal, M. (2014). Decentralization in developing economies. *Annu. Rev. Econ.*, 6(1):581–604.

Genda, Y., Kondo, A., and Ohta, S. (2010). Long-term effects of a recession at labor market entry in japan and the united states. *The Journal of Human Resources*, 45(1):pp. 157–196.

Georgiadis, A. and Manning, A. (2012). Spend it like beckham? inequality and redistribution in the uk, 1983–2004. *Public Choice*, 151(3):537–563.

Gibbons, S. (2015). Gone with the wind: Valuing the visual impacts of wind turbines through house prices. *Journal of Environmental Economics and Management*, 72:177–196.

Goldhaber, D., Strunk, K. O., Brown, N., and Knight, D. S. (2016). Lessons learned from the great recession: Layoffs and the rif-induced teacher shuffle. *Educational Evaluation and Policy Analysis*, 38(3):517–548.

Goldhaber, D. and Theobald, R. (2013). Managing the teacher workforce in austere times: The determinants and implications of teacher layoffs. *Education Finance and Policy*, 8(4):494–527.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.

Greenstone, M., Hornbeck, R., and Moretti, E. (2010). Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings. *Journal of Political Economy*, 118(3):536–598.

Gyourko, J., Saiz, A., and Summers, A. (2008). A new measure of the local regulatory environment for housing markets: The wharton residential land use regulatory index. *Urban Studies*, 45(3):693–729.

Hamilton, B. W. (1975). Zoning and property taxation in a system of local governments. *Urban Studies*, 12(2):205–211.

Hamilton, B. W. (1976). Capitalization of intrajurisdictional differences in local tax prices. *The American Economic Review*, 66(5):743–753.

Han, L. and Kung, J. K.-S. (2015). Fiscal incentives and policy choices of local governments: Evidence from china. *Journal of Development Economics*, 116:89–104.

Hanushek, E. A., Kain, J. F., O'Brien, D. M., and Rivkin, S. G. (2005). The market for teacher quality. Technical report, National Bureau of Economic Research.

Hanushek, E. A. and Rivkin, S. G. (2006). Teacher quality. *Handbook of the Economics of Education*, 2:1051–1078.

Hardman, A. M. and Ioannides, Y. M. (1995). Moving behavior and the housing market. *Regional Science and Urban Economics*, 25(1):21–39.

Harris, D. N. and Sass, T. R. (2011). Teacher training, teacher quality and student achievement. *Journal of public economics*, 95(7):798–812.

Hastie, T., Tibshirani, R., Friedman, J., and Franklin, J. (2005). The elements of statistical learning: data mining, inference and prediction. *The Mathematical Intelligencer*, 27(2):83–85.

Hinrichs, P. (2014). What kind of teachers are schools looking for? evidence from a randomized field experiment. Working Paper 14-36, Federal Reserve Bank of Cleveland.

Hoekstra, M. (2009). The effect of attending the flagship state university on earnings: A discontinuity-based approach. *The Review of Economics and Statistics*, 91(4):717–724.

Hoxby, C. M. (2001). All school finance equalizations are not created equal. *The Quarterly Journal of Economics*, 116(4):1189–1231.

Hoxby, C. M. and Kuziemko, I. (2004). Robin hood and his not-so-merry plan: Capitalization and the self-destruction of texas' school finance equalization plan. Technical report, National Bureau of Economic Research.

Hoxby, C. M. and Leigh, A. (2004). Pulled away or pushed out? explaining the decline of teacher aptitude in the united states. *American Economic Review*, 94(2):236–240.

Ihlanfeldt, K. R. (2011). Do caps on increases in assessed values create a lock-in effect? evidence from florida's amendment one. *National Tax Journal*, 64(1):7.

Jackson, C. K. (2013). Match quality, worker productivity, and worker mobility: Direct evidence from teachers. *Review of Economics and Statistics*, 95(4):1096–1116.

Jackson, C. K., Johnson, R., and Persico, C. (2014). The effect of school finance reforms on the distribution of spending, academic achievement, and adult outcomes. Technical report, National Bureau of Economic Research.

Jackson, C. K., Johnson, R. C., and Persico, C. (2015). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1):157–218.

Jackson, C. K., Wigger, C., and Xiong, H. (2018). Do school spending cuts matter? evidence from the great recession. Technical report, National Bureau of Economic Research.

Jacob, B., Rockoff, J. E., Taylor, E. S., Lindy, B., and Rosen, R. (2016). Teacher applicant hiring and teacher performance: Evidence from dc public schools. Working Paper 22054, National Bureau of Economic Research.

Jepsen, C. and Rivkin, S. (2009). Class size reduction and student achievement the potential tradeoff between teacher quality and class size. *Journal of human resources*, 44(1):223–250.

Johnson, E. B. and Walsh, R. (2009). The effect of property taxes on location decisions: Evidence from the market for vacation homes. Technical report, National Bureau of Economic Research.

Kahn, L. B. (2008). Job durations, match quality and the business cycle: what we can learn from firm fixed effects. *Unpublished manuscript, Harvard University, Cambridge*.

Kahn, L. B. (2010). The long-term labor market consequences of graduating from college in a bad economy. *Labour Economics*, 17(2):303 – 316.

Kahn, L. B. and McEntarfer, E. (2014). Employment cyclicality and firm quality. Working Paper 20698, National Bureau of Economic Research.

Kenworthy, L. and McCall, L. (2007). Inequality, public opinion and redistribution. *Socio-Economic Review*, 6(1):35–68.

Kirk, D. S. and Laub, J. H. (2010). Neighborhood change and crime in the modern metropolis. *Crime and justice*, 39(1):441–502.

Knight, D. S. (2017). Are high-poverty school districts disproportionately impacted by state funding cuts?: School finance equity following the great recession. *Journal of Education Finance*, 43(2):169–194.

Krueger, A. B. (1988). The determinants of queues for federal jobs. *Industrial and Labor Relations Review*, 41(4):567–581.

Lafortune, J., Rothstein, J., and Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2):1–26.

Lafortune, J. and Schonholzer, D. (2019). School finance reform and the distribution of student achievement. *Working Paper*.

Lang, K. and Palacios, M. D. (2018). The determinants of teachers occupational choice.

Leachman, M., Albares, N., Masterson, K., and Wallace, M. (2016). Most states have cut school funding, and some continue cutting. *Center on Budget and Policy Priorities*, page 4.

Livy, M. R. (2018). Intra-school district capitalization of property tax rates. *Journal of Housing Economics*, 41:227 – 236.

Loeb, S. and Béteille, T. (2008). Teacher labor markets and teacher labor market research. *Teacher quality: Broadening and deepening the debate*, pages 27–58.

Loeb, S. and Miller, L. C. (2006). *A review of state teacher policies: What are they, what are their effects, and what are their implications for school finance?* Governor's Committee on Education Excellence.

Loeb, S. and Page, M. E. (2000). Examining the link between teacher wages and student outcomes: The importance of alternative labor market opportunities and non-pecuniary variation. *The Review of Economics and Statistics*, 82(3):pp. 393–408.

Long, M. C. (2008). College quality and early adult outcomes. *Economics of Education Review*, 27(5):588–602.

Luechinger, S. (2014). Air pollution and infant mortality: A natural experiment from power plant desulfurization. *Journal of health economics*, 37:219–231.

Lutz, B. (2015). Quasi-experimental evidence on the connection between property taxes and residential capital investment. *American Economic Journal: Economic Policy*, 7(1):300–330.

Mallach, A. (2009). *Addressing Ohio's foreclosure crisis: Taking the next steps*. Metropolitan Policy Program at Brookings.

Marchand, J., Weber, J., et al. (2015). The labor market and school finance effects of the texas shale boom on teacher quality and student achievement. Technical report.

Martin, I. W. (2008). *The permanent tax revolt: How the property tax transformed American politics*. Stanford University Press.

Martin, I. W. (2009). Proposition 13 fever: How california's tax limitation spread. *California Journal of Politics and Policy*, 1(1).

Martin, I. W. and Beck, K. (2017). Property tax limitation and racial inequality in effective tax rates. *Critical Sociology*, 43(2):221–236.

Martin, I. W. and Beck, K. (2018). Gentrification, property tax limitation, and displacement. *Urban Affairs Review*, 54(1):33–73.

Martinez, L. R. (2016). Sources of revenue and government performance: evidence from colombia. *Available at SSRN 3273001*.

Meltzer, A. H. and Richard, S. F. (1981). A rational theory of the size of government. *Journal of Political Economy*, 89(5):914–927.

Mian, A., Rao, K., and Sufi, A. (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics*, 128(4):1687–1726.

Mian, A. and Sufi, A. (2011). House prices, home equity-based borrowing, and the us household leverage crisis. *American Economic Review*, 101(5):2132–56.

Mieszkowski, P. and Zodrow, G. R. (1989). Taxation and the tiebout model: the differential effects of head taxes, taxes on land rents, and property taxes. *Journal of Economic Literature*, 27(3):1098–1146.

Miller, C. L. (2018). The effect of education spending on student achievement: Evidence from property values and school finance rules.

Munnell, A. H., Fraenkel, R. C., et al. (2013). Public sector workers and job security. *State and Local Pension Plans Issue in Brief*, 31.

Murnane, R. and Phillips, B. R. (1981). Learning by doing, vintage, and selection: Three pieces of the puzzle relating teaching experience and teaching performance. *Economics of Education Review*, 1(4):453–465.

Mustre-del Rio, J. (2016). Job duration and match quality over the cycle. Technical report, Federal Reserve Bank of Kansas City.

Nagler, M., Piopiunik, M., and West, M. R. (2020). Weak markets, strong teachers: Recession at career start and teacher effectiveness. *Journal of Labor Economics*, 38(2):453–500.

Nguyen-Hoang, P. and Yinger, J. (2011). The capitalization of school quality into house values: A review. *Journal of Housing Economics*, 20(1):30–48.

Norregaard, M. J. (2013). *Taxing immovable property revenue potential and implementation challenges*. Number 13-129. International Monetary Fund.

Oates, W. E. (1969). The effects of property taxes and local public spending on property values: An empirical study of tax capitalization and the tiebout hypothesis. *Journal of Political Economy*, 77(6):957–971.

Oates, W. E. and Fischel, W. A. (2016). Are local property taxes regressive, progressive, or what? *National Tax Journal*, 69(2):415.

Oreopoulos, P., von Wachter, T., and Heisz, A. (2012). The short- and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, 4(1):1–29.

OSullivan, A., Sexton, T. A., and Sheffrin, S. M. (1995). Property taxes, mobility, and home ownership. *Journal of Urban Economics*, 37(1):107–129.

Pack, H. and Pack, J. R. (1977). Metropolitan fragmentation and suburban homogeneity. *Urban Studies*, 14(2):191–201.

Palmon, O. and Smith, B. A. (1998). New evidence on property tax capitalization. *Journal of Political Economy*, 106(5):1099–1111.

Persico, C. and Venator, J. (2018). The effects of local industrial pollution on students and schools.

Peske, H. G. and Haycock, K. (2006). Teaching inequality: How poor and minority students are shortchanged on teacher quality: A report and recommendations by the education trust. *Education Trust*.

Podgursky, M. (2011). Teacher compensation and collective bargaining. volume 3, chapter 05, pages 279–313. Elsevier, 1 edition.

Provasnik, S. and Dorfman, S. (2005). Mobility in the teacher workforce: Findings from the condition of education 2005. *National Center for Education Statistics*, 114.

Reback, R. (2006). Entry costs and the supply of public school teachers. *Education Finance and Policy*, 1:247–265.

Reininger, M. (2012). Hometown disadvantage? it depends on where you're from teachers' location preferences and the implications for staffing schools. *Educational Evaluation and Policy Analysis*, 34(2):127–145.

Rhode, P. W. and Strumpf, K. S. (2003). Assessing the importance of tiebout sorting: Local heterogeneity from 1850 to 1990. *American Economic Review*, 93(5):1648–1677.

Ries, J. and Somerville, T. (2010). School quality and residential property values: evidence from vancouver rezoning. *The Review of Economics and Statistics*, 92(4):928–944.

Rink, R. P. (1981). After house bill 920: An analysis of needed real property tax reform. *Clev. St. L. Rev.*, 30:137.

Ross, J. M. (2013). Are community-nuisance fiscal zoning arrangements undermined by state property tax reforms? evidence from nuclear power plants and school finance equalization. *Land Economics*, 89(3):449–465.

Ross, S. and Yinger, J. (1999). Sorting and voting: A review of the literature on urban public finance. *Handbook of regional and urban economics*, 3:2001–2060.

Samilton, T. (2018). Monroe county says dte energy blindsided it in property tax negotiation. Technical report, Michigan Radio.

Sances, M. W. and You, H. Y. (2017). Economic shocks, targeted transfers, and local public goods: Evidence from us shale gas boom.

Shan, H. (2010). Property taxes and elderly mobility. *Journal of Urban Economics*, 67(2):194–205.

Shimer, R. (2008). The probability of finding a job. *American Economic Review*, 98(2):268–73.

Sirmans, S., Gatzlaff, D., and Macpherson, D. (2008). The history of property tax capitalization in real estate. *Journal of Real Estate Literature*, 16(3):327–344.

Testa, J. W. (2018). Annual report: Fiscal year 2018. Technical report, Ohio Department of Taxation.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of Political Economy*, 64(5):416–424.

Urban Institute-Brookings Institution Tax Policy Center (2017). State & local government finance data query system. Technical report.

U.S. Census Bureau (2017). Public education finances: 2015. Technical report.

Wasi, N. and White, M. J. (2005). Property tax limitations and mobility: The lock-in effect of california's proposition 13. Working Paper 11108, National Bureau of Economic Research.

Weber, M., Srikanth, A., and Baker, B. (2016). School funding fairness data system codebook. *Newark, NJ: Rutgers Graduate School of Education-Education Law Center*.

Weingast, B. R. (2009). Second generation fiscal federalism: The implications of fiscal incentives. *Journal of Urban Economics*, 65(3):279–293.

Williams, N. (2018). Rural school dependent on power plant. Technical report, Illinois Farmer Today.

Zhang, C. and de Figueiredo, J. M. (2018). Are recessions good for government hires? the effect of unemployment on public sector human capital. Working Paper 24538, National Bureau of Economic Research.

Zhuravskaya, E. V. (2000). Incentives to provide local public goods: fiscal federalism, russian style. *Journal of Public Economics*, 76(3):337–368.