Three Essays on the Economic Well-Being of Communities Near Energy Development

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Recent United States energy policies have sought to reduce greenhouse gas emissions while keeping fuel and electricity prices affordable. For example, continued promotion of the burgeoning natural gas and renewable energy industries, along with the installation of new utility infrastructure, could provide extensive economic benefits. However, in each of these industries, local communities have faced disamenities, often without sharing in the associated benefits. Citizens have responded by opposing development, resulting in outcomes that range from delays caused by prolonged zoning hearings, to statewide bans on unconventional natural gas development. This suggests that policymakers must understand and address disamenities, or else risk the creation of inequities or the prevention of otherwise welfare-improving investments. This dissertation is comprised of three essays, each of which aims to develop our understanding of the distribution of costs and benefits near energy developments, and the extent to which public policy can modify them to promote the well-being of both local communities and society.

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Preface

In the first essay, I compare real estate investment patterns near plugged and unplugged oil and gas wells in Washington County, Pennsylvania. Many studies estimate the effect of environmental hazards on property values over short time periods, during which the supply of real estate remains constant. But over many years, hazards can affect local investment by influencing who lives nearby, what types of industries are present, or how many buildings are constructed. I find that the typical unplugged well deterred 70 square meters of building over a half-century, and estimate that the forgone building leads properties near an unplugged well to have market values that are on average 9 to 17 percent less than properties near a plugged well.

In the second essay, I exploit discontinuity in compensation policies at the Pennsylvania-Ohio border to understand how restrictions affect local investment of revenues from the shale gas industry. Ohio delivers unrestricted revenues to school districts and municipalities with drilling. Pennsylvania leaves out schools and restricts municipal expenditures to several categories related to the industry's impacts. While Pennsylvania municipalities save the money and use it to pay down preexisting debt, Ohio school districts leverage it to increase borrowing, suggesting more local demand for school investments than for municipal investments. My findings suggest that policies that provide revenues to local governments but restrict their spending to address disamenities from the revenue-generating industry can preclude investment in highly-valued public goods.

In the third essay, I estimate uncompensated losses that are borne by households near high-voltage transmission lines. Transmission construction projects can achieve legislative goals for expanding renewable electricity generation, but households near the lines often bear a disproportionate share of the projects' costs, broadly understood. Using data on real estate transactions, I estimate that households near the Competitive Renewable Energy Zone (CREZ) transmissions lines in Texas bear $253.9 million in costs associated with marred views, buzzing lines, and fears for their health and safety. The vast majority of these costs are uncompensated, because they accrue to owners of nearby properties that are not crossed by the lines, and therefore receive no money for the use of their land. These uncompensated losses are an extremely small share (less than 4 percent) of the project's costs. The findings suggest that compensating all affected residents, either with cash or in kind, would entail a small increase in costs that could have a large impact on public and political support for these otherwise welfare-enhancing infrastructure projects.

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1.0 Environmental Hazards, Remediation, and Local Investment: Evidence from Over a Half-Century of Abandoned Oil and Gas Wells

1.1 Introduction

How do environmental hazards affect local investment over several decades? Numerous hedonic studies estimate the impact of hazards on local welfare in the short term by examining their effects on property values just after they become present or are remediated. Most of the studies treat local investment—such as the supply of houses and other infrastructure—as fixed, and improvements or deteriorations in environmental quality clear the real estate market entirely through price responses. In the case of remediation, the hedonic framework assumes that prices respond because individuals that value environmental quality highly move towards the remediated hazard, representing an outward shift in local demand for property. But remediation decisions can affect more than just prices. They can affect who lives nearby, what types of industries and amenities are present, or how many buildings are constructed. For instance, in the long term it is probable that real estate supply is elastic and variable, meaning that changes in environmental quality may result in both price and quantity responses.

In this essay, I study whether environmental hazards have deterred one type of investment, real estate investment, over several decades. A direct study of real estate investment complements the hedonic literature by shedding light on one mechanism through which hazards affect property values. A better understanding of the mechanism may help explain heterogenous findings about the relationship between hazard remediation and property values across settings (Kiel and Williams, 2007; Mastromonaco and Maniloff, 2018). I show that decisions to remediate hazards can affect long term investment patterns in rural and suburban areas with elastic land and housing supplies.

Specifically, I study how abandoned oil and gas wells have influenced real estate investment over half a century. During that dawn of the modern oil and gas industry in the late 19th century, the US was the global leader in oil and gas production (Meko and Karklis, 2017). Even then, states had laws requiring operators to plug wells and remediate the surrounding environment once they stopped producing oil or gas. But weak enforcement and a series of busts that forced operators out of business have left hundreds of thousands of unplugged wells scattered throughout fields, forests, and backyards in states with historic production, like Pennsylvania, California, and Texas.

Pennsylvania has the longest history of oil and gas development, but has limited data on old wells. Estimates suggest that between 300,000 and 750,000 have been drilled there, and as many as 225,000 are unplugged (Dilmore et al., 2015; Kang et al., 2016). I focus on Washington County because the state's Bureau of Topographic and Geologic Survey has digitized its historical records, which provide the locations and characteristics of thousands of wells drilled in the late 19th and early 20th centuries. Given its proximity to Pittsburgh, parts of the county have experienced significant real estate development since the mid-20th century, and the county government maintains digital data on the outline of every building within its borders, enabling me to estimate spatially-precise relationships between investment and wells

Landowners and developers may be hesitant to build near unplugged wells because they come with deteriorating equipment and unrestored land that are visually unappealing, and because they can endanger human health and safety. Unplugged wells can leak harmful gases and liquids that contaminate soil and water at the surface and underground (Boothroyd et al., 2016; Kang et al., 2015). If abandoned wells allow gas to move from underground to near-surface environments, such as water wells or basements of nearby homes, they can create explosive risks. In two cases in Firestone, Colorado and Greene County, Pennsylvania, methane leaks from abandoned wells into homes resulted in fatal explosions (Finley, 2017; Kusnetz, 2011). Modern plugging techniques largely mitigate the risks, but reclaiming a single well can cost between \$10,000 and \$80,000 (Ho et al., 2018). Instead of incurring the costs, individuals and developers may avoid purchasing or building on land with unplugged wells.

The main challenge in identifying the long term effect of hazards on real estate investment is to find suitable control units of unaffected land. Even with spatial controls that capture topographic and soil characteristics, there can be unobserved differences in the attractiveness of land for building, especially if characteristics that firms look for when siting hazardous infrastructure are the same ones that are better for other structures. I overcome this challenge by estimating a difference-in-difference model that compares building near plugged and unplugged wells. The primary identifying assumption is that plugging was not determined by unobserved factors that also determined where investment occurred. To show that the assumption likely holds, I present historical evidence that early operators plugged wells to prevent depletion of oil and gas reservoirs in areas with further production potential, and to protect coal miners. Because geologic conditions determined which wells were plugged rather than surface use considerations, areas near plugged and unplugged wells are not systematically different across variables observed to affect building, such as soil quality, topography, proximity to urban areas, and the amount of building present before the wells stopped producing oil or gas.

I find that areas near plugged and unplugged wells have more favorable soil and topographic conditions for building than areas further away, and are especially suitable for "out buildings," including barns, sheds, or stand-alone garages. But structures are not built if a well is left unplugged. Over the period 1970 to 2017, areas within 50 meters of the typical unplugged well received about 52 fewer square meters of out building and 18.5 fewer square meters of commercial and residential building relative to areas near plugged wells. The results are nearly identical when I exclude wells plugged within ten years of the start of the study period, suggesting that they are not driven by building decisions that predated plugging decisions. I find no evidence that unplugged wells cause building to shift across space, and conclude that they have caused landowners and developers to forgo some construction projects.

I estimate that forgone building caused by the typical unplugged well leads nearby properties to have market values that are 9 to 17 percent less than properties near plugged wells. More broadly, because real estate is typically assessed at its market value, I estimate that the 2,276 known unplugged wells have led to \$24.9 million to \$37 million in forgone tax revenues for the county government, municipalities, and school districts over the period 1970 to 2020. For the school district with the most unplugged wells, they deterred between \$70 and \$100 per student in annual revenues. My estimates of forgone revenues could be two to four times larger if I was able to account for all unplugged wells in the county, based on the ratio of wells in state records to recent estimates of the total number of wells drilled in Pennsylvania.

My findings suggest that operators' failure to plug wells can impose costs on landowners and taxpayers over several decades. The findings motivate two modifications to remediation policies. First, forgone investment presents an additional rationale for raising the dollar amount in bonds that operators set aside with states prior to drilling, to be used for reclamation if they dissolve. In most states, bond amounts are much lower than estimated plugging costs, especially for new shale wells (Ho et al., 2018; McClure et al., 2020). Taxpayers bear reclamation costs in excess of bonds. Second, the findings suggest that targeting already committed public dollars to locate and reclaim wells in areas with significant investment potential may increase the aggregate benefits that state plugging programs create. Wells without identifiable owners become the responsibility of the state where they are located. Because funds to investigate and plug wells are limited, most states prioritize plugging wells that are posing the clearest environmental threats, rather than considering impacts on investment or property values.

1.2 Literature and Conceptual Framework

Many studies rely on the hedonic framework to infer how much the public values environmental quality. In recent years, several have estimated the value of cleaning up abandoned or contaminated sites, including industrial and commercial facilities, underground storage containers, landfills, and hazardous waste dumps (Zabel and Guignet, 2012; Alberini, 2007; Sousa et al., 2009; Haninger et al., 2017; Ho and Hite, 2008; Kinnaman, 2009; Linn, 2013). The strength of the hedonic approach is that it estimates the total dollar amount that a hazard capitalizes into nearby property values, which under certain assumptions can be interpreted as the public's willingness to pay for environmental remediation. This information can be included in cost-benefit analyses to inform immediate policy decisions, such as whether to fund remediation programs. For instance, Gamper-Rabindran and Timmins (2013) estimate that within ten years of the US government's remediation of hazardous waste sites, benefits as measured by increased property values outweighed cleanup costs by over four times.

Rosen (1974) formalized the concept of hedonic or implicit prices—prices attributable to an individual characteristic of a good that is a bundle of several characteristics. His key insight is that a regression of observed prices of goods on their differentiated characteristics estimates the hedonic price function, which contains slope coefficients for each characteristic that under certain assumptions describe buyers' marginal willingness to pay to accept or avoid that characteristic. Early studies estimated willingness to pay for environmental quality using data on housing transactions observed at a single point in time. But researchers quickly realized that cross-sectional estimates may be biased due to endogeneity between environmental quality and other unobserved attributes of communities, because households can sort across communities and vote for desired levels of public goods (Goldstein and Pauly, 1981).

More recent hedonic studies address endogeneity by leveraging temporal variation in public goods that is exogenous to unobserved community characteristics. But Kuminoff and Pope (2014) show that shocks to environmental quality can shift the hedonic price function, leading to capitalization estimates that are biased representations of local welfare changes. For instance, if remediation of an environmental hazard coincides with the migration of higher income individuals towards a hazard, capitalization estimates may conflate willingness to pay for environmental quality with changes in how residents value other public goods and changes in the stock of real estate. Most studies respond to this concern by making a "time-constant hedonic gradient assumption," which they adhere to by examining small shocks to environmental quality, shortening study periods, and selecting cases where demographics and housing stock remain constant.

My conceptual contribution is to consider whether hazards have important effects on local communities that, by the nature of the time-constant gradient assumption, are outside of the scope of most hedonic studies. By examining non-price responses, I complement the hedonic literature in two ways. First, I explore the effects of hazards over several decades. Because preferences, demographic composition, and land use are more likely change when study periods grow longer, hedonic studies typically consider changes in property values over much shorter periods. Second, I shed light on one mechanism, real estate investment, through which hazards can affect property values.

To illustrate how direct study of real estate investment complements the hedonic approach, Figure 1.1 maps the relationships between hazards, changes in property values, and changes in local welfare. It shows that when a hazard is placed in a community, it can affect welfare through three types of disamenities. First, it can damage the aesthetics of a neighborhood by emitting unpleasant odors or by hosting unsightly materials, structures, and equipment. Second, it can pose real or perceived risks to the health and safety of nearby residents if it contains substances that may contaminate air, soil, or water. And third, it can tie up land that would otherwise be put to higher valued uses, such as agricultural, residential, or commercial development. In urban areas where land is scarce and returns to investment are sufficiently high, individuals and firms may be incentivized to remediate hazards. But in suburban or rural areas, such as those I consider here, developers may avoid land with hazards if many other clean properties are available, if the projected return on development is low relative to cleanup costs, or if complying with remediation laws is burdensome.

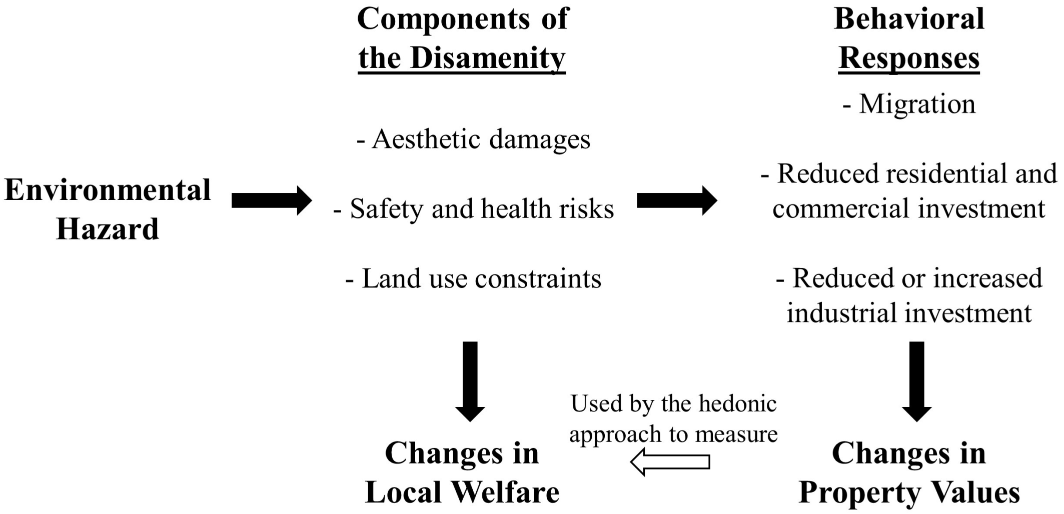

Figure 1.1: Conceptual Model

It is outside of the scope of hedonic studies to consider which and how much each of the three disamenities factor into housing buyers' assessment of environmental quality, with some exceptions. For instance, Muehlenbachs et al. (2015) observe housing transactions within and outside of public water service areas to isolate residents' willingness to pay to avoid groundwater contamination risks posed by shale gas wells. I take a more direct approach to isolate the effect of the third type of disamenity by comparing several measures of real estate investment across remediated and unremediated hazards.

Figure 1.1 also shows that hazards may induce several behavioral responses from individuals and firms that in turn affect property values. Most hedonic models assume that hazards capitalize in property values because individuals that value environmental quality highly move away and are replaced by individuals that value it less and are willing to buy homes and land at the hazard-induced discount. If only a few individuals sort towards the hazard or those that do are demographically similar to preexisting residents, the hedonic gradient may be stable. If so, the price reduction aggregated over all affected properties is an unbiased estimate of the welfare losses borne by preexisting landowners. But migratory responses to changes in environmental quality can induce large changes in local demographics and income (Banzhaf and Walsh, 2008). For this reason, hedonic studies use cases and study periods over which these outcomes are constant. A separate environmental justice literature directly addresses whether minority and low-income individuals disproportionately move towards hazards.

Much like the environmental justice literature, my empirical approach aims to understand just one behavioral response to hazards, rather than to estimate aggregate welfare effects. Most hedonic studies hold the supply of buildings constant by comparing properties that are identical except for the presence of hazards. Local supply may be fairly constant and inelastic over short study periods, or in communities with fully-developed properties or strict land use regulations.[1] But over many years and in areas with investment potential, individuals and firms may invest in buildings and improvements on properties unaffected by hazards and

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[1] In a non-environmental context, Lutz (2015) finds that exogenous reductions in property taxes increase residential construction only in municipalities with more elastic housing supply, which are typically less densely populated areas and further from major urban centers. He also shows that municipalities that experienced increased building responded by making their land use policies more restrictive to slow construction.

avoid affected properties. Under certain assumptions, some of the forgone investment caused by hazards represents lost wealth for owners of affected properties.[2] Hazards may also affect communities more broadly, because forgone investment ultimately reduces the property tax bases of local governments. If forgone investment is not tightly tied to demand for public services, affected governments would decrease public good provision or increase tax rates, relative to a counterfactual where hazards are remediated.

I am aware of only two studies that examine whether shocks to environmental quality affect the supply of residential and commercial real estate.[3] Greenstone and Gallagher (2008) show that Census tracts where Superfund hazardous waste sites are remediated do not experience significant increases in new home construction. Noonan et al. (2007) study the same program at the Census block group level and find mixed evidence that remediation leads to newer and higher quality housing stock. I complement them in two ways. First, I leverage spatially-precise data on the characteristics of each building near a remediated or unremediated hazard, rather than aggregating building characteristics across a tract or block group. Because many studies show that hazards only affect properties within a few miles or less (Boslett et al., 2019; Currie et al., 2015; Davis, 2011; Hite et al., 2001), my approach eliminates the concern that aggregated data masks investment changes within blocks or tracts. It also eliminates the need for imperfect aggregation techniques to account for hazards affecting real estate in adjacent blocks or tracts, and the concern that community characteristics are endogenous to environmental goods if the Census intentionally creates homogenous geographic units (Banzhaf and Walsh, 2008). Second, I study thousands of small hazards that in most cases are located directly on private individuals' properties. It is reasonable expect greater investment effects in this context relative to contexts where affected properties are adjacent to a smaller sample of large hazards.

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[2] Greenstone and Gallagher (2008) note that if a hazard affects housing stock in the long run, a short term hedonic study that holds supply constant may understate the welfare effects of environmental hazards. In Appendix A.1, I consider the circumstances under which a short run hedonic study would fail to capture the forgone value of real estate investment that a hazard induces in the long run.

[3] In contrast to residential and commercial investment, firms may specifically target areas with existing hazards for industrial investment. They may more easily gain regulatory approval to site facilities near existing hazards, or benefit from backward and forward supply chain linkages in areas with other industrial firms that create pollution. For example, "Cancer Alley" in Louisiana hosts a dense concentration of chemical plants and petroleum refineries.

1.3 Abandoned Wells in Pennsylvania

1.3.1 Booms and Busts

In 1859, Edwin Drake drilled the world's first commercial oil well in in Titusville, Pennsylvania. The success of the "Drake Well" precipitated an oil rush in the northwestern counties, which made Pennsylvania the leading oil producer in the world until supply dwindled and it was surpassed in the 1880s by Midwestern states, and later Texas and California. Of Pennsylvania's early oil industry, Yergin (1991) notes, "Never again would any single region have such a grasp on supply of the raw material." Still, oil production has continued to the present day, as has natural gas production which became profitable by the 1880s and flourished in Pennsylvania's southwestern counties.

Throughout Pennsylvania's history of oil and gas development, it was common for operators to become bankrupt and dissolve, leaving their unplugged wells without a responsible party. Development was defined by a series of boom periods, when high prices and discoveries of new oil and gas fields triggered the creation of many small firms that drilled thousands of wells. But as prices fell or oil and gas resources became depleted, the booms were followed by busts that forced operators out of business or otherwise encouraged them to stop maintaining wells. James Hay Reed, former president of the Consolidated Gas Company of Pittsburgh, captures how the boom and bust cycle unfolded in western Pennsylvania's McKeesport gas field between 1919 and 1920:

> The tragic story of the McKeesport field is still fresh in my memories. The drilling of the Foster well, with its enormous flow and tremendous pressure, brought on an old-fashioned boom. Companies were hastily formed and stock sold by the thousands of shares to persons who were not familiar with the business and who ultimately lost their entire investment. A great number of wells were drilled in the neighborhood, some of which were productive for a short time, but it was noticed that each new productive well took a little off the pressure and flow of the original well until, as was expected by practical gas men, the flow practically ceased, the wells were abandoned and their fittings and machinery sold for junk (Reed, 1928, p.132).

In 1934, The Pittsburgh Press reported that many of the nearly 22,000 derricks in the McKeesport field that were once "standing like gravestones" had been removed, but that "pipes still reach into the ground in several of the yards" (Sample, 1934). A 2007 study by the National Energy Technology Laboratory revealed that unplugged wells in part of the field under the borough of Versailles serve as conduits for stray gas to accumulate in buildings, and a century after the boom residents still use monitoring and venting systems to mitigate explosive risks (Litvak, 2014). Although the risks in Versailles may be extraordinary, exacerbated by many wells drilled in close proximity, the boom and bust cycle of the McKeesport field played out in many oil and gas fields across the state, leaving hundreds of thousands of unplugged wells.

1.3.2 The Evolution of Plugging Decisions

The profusion of unplugged wells is attributable to a history of weak or nonexistent regulations to ensure reclamation. During the first two decades of oil and gas development, the state had no reclamation requirements, meaning that firms could simply walk away from wells when they chose to stop maintaining them. In 1878, the state passed the first legislation requiring operators to plug abandoned oil wells, and passed similar legislation in 1885 for gas wells, but no agency was tasked with enforcing them (Tarr and Clay, 2015). Reclamation regulations were revised several times in the late 19th and early 20th century to reflect improved plugging technology (e.g., Pennsylvania General Assembly (1921)). But until the 1984 Oil and Gas Act, there was poor documentation of well locations and weak regulatory infrastructure to enforce reclamation rules. Even today, state records indicate that there are thousands of known unplugged wells that have not reported production since 1984 (Hegde et al., 2019).[4]

But if regulatory enforcement has been weak and many firms became insolvent before plugging their wells, why were some wells plugged? Historical evidence suggests that for wells drilled before the 1970s, incentives for firms to plug evolved in three waves, each of which was driven by geological considerations. The first wave began in the late 19th century, when operators and regulators learned that drilling many wells in close proximity would reduce the amount of oil or gas that could be extracted. In their examination of Pennsylvania's

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[4] Wells are "known" or accounted for by the state if they were drilled after the enactment of the 1955 Act's permitting requirements, if they were retroactively registered as required by the 1984 Act, or if their paper records were digitized by the state's Bureau of Topographic and Geologic Survey. State records indicate that nearly 48,000 wells have been plugged, and 38,000, have been abandoned and left unplugged.

first oil boom, Cone and Johns (1870) describe an early understanding of this phenomenon:

> The greatest obstacle to the oil operator is the large quantity of water met with. This water comes from the surface and from the water veins beneath... and is in such quantity as to often literally drown him out. Hundreds of wells have been rendered worthless by "flooding," ... The sinking of one well in close proximity to another... generally flooded, or drew from the well its oil supply, by diverting the current of oil and gas in its own direction. As leases consist of from one to half an acre each, the operator in previous as in present years has no protection against this serious evil. When a good well was struck, parties commenced immediately to sink wells in as close proximity as possible to it, for the express purpose of flooding or obtaining its supply of oil (p. 166).

A more modern understanding of this phenomenon, which also occurred in early gas fields, is twofold. First, too many wells were sunk in close proximity for efficient production of the resource, which depleted the pressure of reservoirs and slowed the flow of oil and gas to the surface (Norvell, 2015). Second, water from the surface, and from subsurface aquifers for wells that were not properly cased with steel, descended into the oil or gas reservoir. A water-flooded oil reservoir can result in "water coning," in which water displaces oil at the bottom of the well, which subsequently produces mostly water. Water also reduces the permeability of gas producing rock formations, slowing or cutting off the flow of gas to the surface (Yuster, 1946).

In the earliest remedy to these problems, operators learned to plug some wells to increase the production of others nearby. Cone and Johns discuss early plugging efforts by landowners in what is now the Allegheny National Forest after a bust led to the exodus of operating firms: "By plugging up a portion of the wells, effectively shutting off the surface water, they can obtain a number of excellent wells at moderate cost. This plan is being fully carried out by the land owners in all former large producing localities" (p. 454). In other words, landowners (and later, operators) chose to plug some wells in order to improve production from the remainder.

The second wave occurred in early 20th century, when diminishing productivity of natural gas wells led to supply shortages and increased prices for a growing number of industrial and residential consumers (Wyer, 1918b). The shortage led experts to scrutinize the wasteful practices of the industry, which publicized the fact that unplugged wells compromise production of new wells by allowing gas to migrate between subsurface formations or leak into the atmosphere (Wyer, 1918a; Ashley, 1920). As higher prices led operators to explore for new reserves in existing gas fields, they responded to the increased salience of conservation practices by plugging nearby, uneconomical wells.

The first two waves of plugging incentives suggest that as understanding of petroleum geology improved, operators and engineers began recognizing the importance of plugging to protect oil and gas resources (Tarr and Clay, 2015, p. 340). There is no evidence that they prioritized plugging in more heavily inhabited areas, or areas with more attractive environmental qualities for building or recreation. Even if there were cases in which the state attempted to enforce regulations, it would likely have been in areas with higher projected production potential, as the earliest reclamation regulations in Pennsylvania and elsewhere appear to have been motivated not by a desire to protect the environment, but to prevent flooding, depressurization, and waste that would decrease ultimate recovery of reserves (National Petroleum Council (2011, p. 6), Pennsylvania Department of Environmental Protection (2000, p. 4)).

The third wave also began in the first two decades of the 20th century, when firms began plugging wells drilled through coal seams in order to protect the safety of miners. In February 1913, the federal Bureau of Mines convened state geologists, mine inspectors, and representatives from the oil, gas, and coal industries in Pittsburgh, with the aim of procuring comments on model legislation for state regulation of oil and gas operations in coal fields. The conference report sheds light on the issue of drilling in coal fields, and the factors that determined plugging in these areas. In an introductory statement, George Rice, the Chief Mining Engineer with the Bureau makes clear that unplugged wells were a primary safety concern:

> The preliminary inquiries of the engineers of the bureau had disclosed that there was no uniformity in the methods of protecting mines against the leakage of gas from wells, that thousands of wells in coal fields are abandoned yearly without adequate plugging...It is such unplugged, uncharted wells that are the greatest menace in mining. The quantity of gas that the wells produce, although not sufficient to be commercially available, is enough, if it were to leak into a mine and be ignited, to cause explosions or fires, with possible loss of life (Bureau of Mines, 1913, p. 5).

Participants' discussion of proposed plugging regulations to prevent fires and explosions that killed several coal miners reveal the rationale for early plugging in coal fields.[5] Frank DeWolf, Director of the Illinois Geological Survey, notes that "it was a matter of agreement, with certain of the large gas companies and certain large coal operators, that they should fill the holes…" (p. 90). E.A. Watters, an engineer with Pennsylvania's Hicks Coal Company reinforces this point, and in discussing plugging notes: "I do not believe in putting a burdensome load on the oil and gas operator (p. 89)," and that "gas companies showed the greatest willingness to help us out, and I never had any trouble with them in that respect" (p. 91). Two inferences can be made from the discussion: oil and gas operators bargained with coal companies to determine which wells to plug, and large firms were more likely bargain than smaller ones.

Representatives of both industries recognized that plugging, especially to standards that allowed for nearby mining, was expensive. E.E. Crocker of South Penn Oil Company notes: "Complete plugging is a pretty expensive point with us. There are places where it is almost impossible to get anything to fill in with, even ordinary dirt" (p. 88). Given the expense, large oil and gas operators could defer costs by conferring with coal companies and plugging only in areas where they had prospective mines. Rice notes that plugging should be prioritized in coal beds with immediate economic potential and in deeper beds that might become profitable in the distant future (p. 88, 90). Archival research by (Tarr and Clay, 2015) reveals that coal companies hired surveyors to map the locations of abandoned wells in these areas (p. 338). By partnering with coal companies to plug this subset of wells, oil and gas operators could avoid the weight of the coal lobby that might otherwise elicit stricter enforcement of regulations that would require them to plug all abandoned wells.

But it was likely only larger oil and gas firms that had contact with coal firms and available capital to plug wells. L.F. Barger, General Superintendent of Peoples Natural Gas Co. makes this distinction:

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[5] It appears that Pennsylvania's Act 322 of 1921, which mandated improved plugging technologies, was in part informed by this and related policy discussions. When plugging wells in coal seams, the Act required operators to cement in place a vent pipe to prevent the accumulation of pressure against the plug and leaking gas into coal mines. This technology was discussed at the 1913 conference (p. 89). The 1921 Act did not, however, adopt the report's recommendations to register the locations of wells or establish an agency to enforce plugging rules, nor did it create a clear threshold for abandonment, meaning that it likely did little to modify incentives to plug.

> . . . the cost of plugging, if it were borne by large corporations, possibly would not work grave harm; but if borne by the individual operator it would work serious harm. A large number of wells are drilled by people who have perhaps just about money enough to drill a well. When you come to saddling them with an added sum of $5,000 (for plugging), from which they receive no benefit whatever, it is going to do them harm, and it will possibly check the corporations in a great many cases (Bureau of Mines, 1913, p. 95).

Larger firms were more likely to plug non-producing wells in coal fields, but this logic may not extend to wells that remained productive. Wells produce the most oil and gas immediately after they are drilled, and production declines over time. How steeply it declines and in turn how long a well remains productive are determined by engineering and geologic factors. Bishop (2013), who interviews oil and gas experts, highlights that it is rare for large companies to plug wells, because if production diminishes to an unprofitable level, they can sell them to smaller firms with lower operating costs. This chain may continue over 30 or more years with sales to even smaller operators, who may plug the well at the end of its life, or may dissolve before that time comes. If firms did dissolve or were unable to afford plugging, or if a highly profitable coal field contained many marginally producing wells, coal companies sometimes acquired wells and paid for or executed plugging themselves.[6]

In sum, the third wave of plugging depended on whether wells were in profitable coal seams, the duration that wells produced and in turn the size of the final operator. Because some wells continued producing longer than others explains why there are many plugged wells and unplugged wells in heavily-mined areas, like most of Washington County. As in the first two waves, there is no evidence that plugging occurred in more populated or environmentally attractive areas.

The Gas Operations, Well Drilling, Petroleum and Coal Mining Act of 1955 required that operators receive permits and register the locations of new wells before drilling, with the express purpose of protecting personnel employed in coal mining. But it probably did little to modify the incentives of firms to plug wells in the short term. It is possible that

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[6] The Bureau's report discusses Hicks Coal Company's consideration of plugging a well without an identifiable owner (p. 91). Today, coal companies are required by the Federal Mine Safety and Health Act (1977) to take reasonable measures to locate abandoned wells, and either avoid mining near them, or plug them to mine-through standards. Cramer (1993) documents several cases in which coal companies acquired abandoned wells with the intention of plugging, or were granted authority by courts to plug wells without owners. My personal conversations with landowners in Greene County, Pennsylvania also revealed that present-day mining companies hired laborers to walk leased properties to locate unregistered wells and paid for plugging.

the 1955 Act's creation of the Oil and Gas Division of the Department of Mines, the first oil and gas regulator in Pennsylvania's history, did present an arena for coal companies to pursue plugging in profitable coal fields. But outside of this, systematic enforcement would have been impeded by the lack of ownership records for wells that predated the act and the inability of small operators to afford plugging.

After the 1970s, incentives to plug wells changed substantially, and may be correlated with surface characteristics. There are four drivers of this change. First, according to the National Petroleum Council (2011), from the 1970s and on environmental protection became a major driver of state-level oil and gas regulation. In Pennsylvania, the 1984 Oil and Gas Act required major technological improvements in well construction and plugging standards. It also provided specific requirements to protect surface and groundwater, and, in absence of an approved defense, presumed operators to be liable for the pollution of nearby water supplies (Act 223, 1984, Sec. 601.207-8). It is likely that these changes incentivized some operators to properly plug and reclaim wells, especially in areas where residents rely on groundwater.

Second, the 1984 Act provided additional incentives for operators to plug wells by substantially raising penalties and enacting bonding requirements. For all wells drilled after April 17, 1985, bonding has required operators to set aside funds with the state prior to drilling a well, which are forfeit if operators do not comply with reclamation laws. Operators weigh the forfeiture of their bond in determining whether to plug wells. They also weigh financial penalties for violating reclamation requirements, and administrative penalties they may face, such as being denied permits to drill new wells. Third, the 1984 Act provided a definition of an abandoned well to include one that has not produced in the last year, required annual production reporting, and required extant operators to plug any abandoned well from which they received economic benefit after April 1979. For wells drilled before 1984 that remained productive, operators were required to retroactively register them with the state, and transfer the registration to a new owner if it changed hands. These requirements provided information to the state, allowing them link wells that were verifiably abandoned with their owners, enabling more systematic enforcement.

Fourth and finally, the 1984 Act made wells without surviving operators, as well as those for which operators received no economic benefit after April 1979, the responsibility of the Department of Environmental Protection (DEP). To pay for reclamation of these wells, a 1992 amendment created the Orphan Well Plugging Fund, which is funded by a small fee attached to new drilling permits. Their longstanding policy has been to prioritize plugging wells that are clearly posing threats to public health and water sources, such as those near buildings or drinking water supplies (Pennsylvania Department of Environmental Protection, 2000, 2019). But public funds to investigate and plug wells have been limited, and DEP has plugged only 3,418 wells since its well plugging program began in 1989 (Pennsylvania Department of Environmental Protection, 2020a). An additional 8,536 known unplugged abandoned wells are the responsibility of DEP, along with possibly thousands more that are unaccounted for. Moreover, thousands of wells have owners but have never reported production, and may become the responsibility of the DEP if their owners dissolve.

1.4 Empirical Approach

The ideal experiment to understand the causal effect of abandoned wells on real estate investment would start by randomly selecting a sample of land units of some discrete size (i.e., 250 square meters) from a population of land units situated above oil and gas resources. Half of the sample would be randomly assigned to receive an oil or gas well, which would be drilled, produced, and maintained by an operator until maintenance was no longer economical, at which point the well would be abandoned and left unplugged. The land units in the other half of the sample would be assigned no wells, but alternative uses, such as building structures or using the land for agriculture, would be left to the decisions of landowners. Because of random selection and random assignment, the samples of land units with and without abandoned wells would be statistically identical, and the presence of unplugged wells would be uncorrelated with unobserved land characteristics that may affect real estate investment. At several intervals after abandonment (i.e., in 20, 30, or 50 years), the researcher could compare the mean number of structures built on land units containing abandoned wells to the mean number of structures on undrilled land units. The difference would represent forgone real estate investment caused by the presence of abandoned wells. Similar differences would reveal whether buildings near abandoned wells were of lower quality, such as if they were smaller or had fewer bedrooms or bathrooms.

Unlike in the ideal experiment, I have no way of identifying a control sample of undrilled land units that are identical to those where historic drilling occurred. Throughout the history of oil and gas drilling—and still today—operators drill wells in places with geology they believe will produce profitable quantities of oil and gas, and in places with attributes that aid in drilling, production, and maintenance, such as proximity to roads or markets for raw materials (Muehlenbachs et al., 2015). In the absence of wells, deep geologic conditions may not affect real estate development. But the same cannot be said of property and neighborhood attributes, many of which I do not observe.

One approach that can overcome this challenge is comparing investment in a circle "near" unplugged wells to investment in a "far" ring, just outside of the circle. Researchers have applied the approach in the urban economics literature to study the effect of housing foreclosures on crime (Cui and Walsh, 2015; Spader et al., 2016). Its theoretical advantage is that near and far areas are in the same vicinity, reducing concerns that differences in outcomes (e.g., crime, real estate development) are driven by differences in land and neighborhood attributes. The approach requires that near and far areas are similar on unobserved characteristics, which has held in urban studies because both areas are heavily developed. But as I show in Section 1.6, in the rural areas I consider, land near wells has more favorable slope and soil quality for building than areas further away. Because observed differences are likely correlated with unobserved characteristics (e.g., forest cover, distance to roads, drainage features), comparing building near unplugged wells to areas further away would likely underestimate the negative effect of unplugged wells on investment.

To overcome the bias inherent in a simple near-far comparison, I adopt an alternative identification strategy—one that leverages variation across both space and plugging status. My approach is most similar to that of Currie et al. (2015), who compare property values and birth weights in concentric areas surrounding open and closed industrial plants. Figure 1.2 helps illustrate. Circle A represents an area near a well that was plugged prior the start of my study period, while Ring B represents an area in the vicinity, but far from the plugged well. I define the near area as a circle with a radius representing the distance within which an unplugged well is hypothesized to negatively influence landowners' investment decisions. Similarly, Circle C represents an area near a well that was left unplugged for the entire study period, and Ring D represents a far area. The theoretical advantage of this approach is that it compares outcomes between the near areas (Circles A and C), which deals with the concern that areas where well operators choose to drill are better for building than the areas they do not. In this setup, rings serve as controls for minor differences in the general vicinity of plugged and unplugged wells.

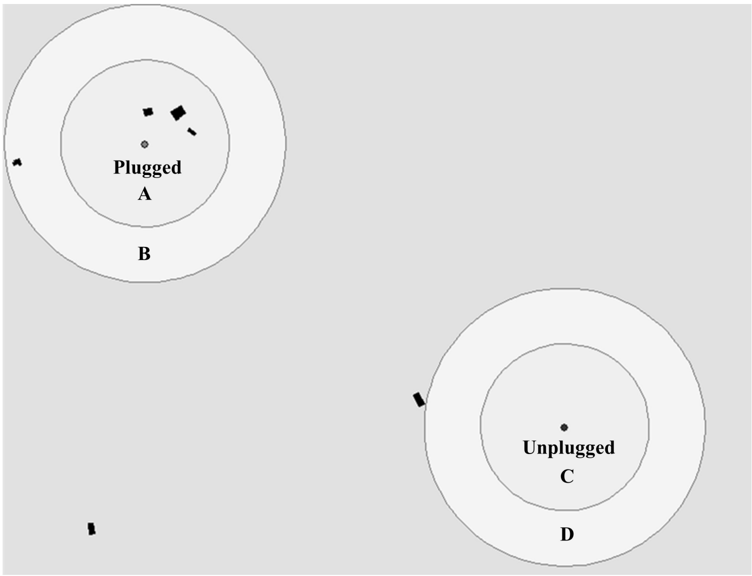

Figure 1.2: Areas Near and Far from Plugged and Unplugged Wells

The dark polygons in Figure 1.2 represent building footprints built during the study period. The polygons and their characteristics define the values of my outcome variables. My baseline differences-in-differences model of the effect of unplugged wells on real estate development, estimated using ordinary least squares is:

$$Y_{wd}^{Post} = \beta_0 + \beta_1 Near_{wd} + \beta_2 UP_w + \beta_3 \left(UP_w \times Near_{wd}\right) + \beta_4 Y_{wd}^{Pre} + \epsilon_i, \tag{1}$$

Where Y_{wd}^{Post} represents one of three measures of real estate development—the number of buildings, number of rooms, and total square footage—within distance d (either near or far) of well w. The binary variables $Near_{wd}$ equals one if the observation represents an area near well w (Circles A and C in Figure 1.2), and UP_w equals one if well w is left unplugged over the entire study period.

The coefficient of interest is β_3, which is an estimate of the effect of leaving a well unplugged on real estate investment. The coefficient represents the change in building outcomes occurring over the study period 1970 to 2017 near an unplugged well relative to the change near a plugged well. If it is negative and statistically significant, then unplugged wells negatively influence real estate investment. In estimating the coefficient, the model accounts for the possibility that different levels of building have occurred in the general vicinity of unplugged relative to plugged wells by differencing away β_2, the difference in post-1970 building in the far rings. The model also accounts for the possibility that different levels of building occurred in near areas relative to far areas, by differencing away β_1, the difference in building between near and far areas that is not attributable to plugging status. In addition, I control for Y_{wd}^{Pre}, the level of the outcome variable associated with buildings built before 1970, as well as fixed effects for each oil and gas pool.[7] I also control for a vector of variables that characterize the physical environment at the well level, which includes measures of slope, soil quality, and the distance to the nearest point in a public park, coal mine, water feature, and Interstate 79.

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[7] An oil or gas pool is a single subsurface oil or gas accumulation. There are over 500 conventional oil and gas pools in Washington County. I dissolved overlapping pools together, and include fixed effects for 241 non-overlapping pools throughout my analysis. In a separate specification, I drop the unplugged well binary and instead use well fixed effects. The inclusion of well fixed effects provides an alternative method of addressing the concern that unobserved factors that affect building at the well level (over the entire 0 to 250 meter range) are correlated with plugging status, such as tree cover or proximity to roads.

1.5 Data and Descriptive Statistics

1.5.1 Well Data

To measure whether unplugged wells constrain real estate development, I use data on the location, type, status (plugged or unplugged), and various dates associated with oil and gas wells that come from three datasets maintained by the Commonwealth of Pennsylvania.[8] The first is the Department of Environmental Protection (DEP) well database made available by the Pennsylvania Geospatial Data Clearinghouse. Second, I use the DEP's records of production reports prepared by well operators between 1980 and May 2018. Third, I use the Department of Conservation and Natural Resources' (DCNR) Exploration and Development Well Information Network (EDWIN), which contains information on permitted wells, as well as thousands of wells that were never officially permitted but have adequate records and location information due to the Bureau of Topographic and Geologic Survey's efforts to digitize this information, especially for wells located in Washington County. Washington County is an appropriate location to study the effect of abandoned wells on real estate investment primarily because it contains thousands of abandoned wells.

1.5.2 Building Data and Controls

Washington County was also selected because it has reliable digital geospatial data on buildings, and, given its proximity to Pittsburgh, parts of it have experienced real estate development over the study period. Figure 1.3 depicts structural footprints built there, before and after 1970. Other counties with many abandoned wells, such as Venango and Mercer, have experienced relatively slow real estate development. Leveraging variation in building within these areas enables identification of the effect of abandoned wells on development.

Data on buildings come from two sources. The first is geospatial polygon data on the location, shape, and size of parcels and structures made available by the Washington County GIS Department. The second is tax assessment data from the Washington County Revenue Department that includes the construction date of each structure, and data on various char-

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[8] The steps that I took to combine the three datasets can be found in Appendix A.2

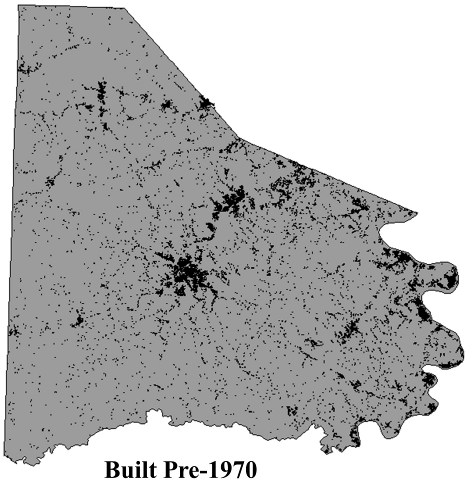

Figure 1.3: Building Construction in Washington County, PA

acteristics of each building. I matched the building polygons in the GIS data with an entry in the assessment data.[9] This allowed me to attach a construction year and building characteristics to the polygons, and subsequently aggregate these outcomes at both the well level, and the "well area" level (i.e., near and far).[10]

I utilize control variables from several sources to account for characteristics of the physical environment that affect real estate development. They include the locations of water features (streams, rivers, pond and lakes), the slope of land, the quality of soil for supporting buildings with and without basements, the location of public parks and state gamelands, and the location of Interstate 79 between the City of Washington and the Allegheny County border (around which there has been substantial real estate development).

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[9] The steps that I took to match buildings in the GIS data and assessment data and the method that I utilized to aggregate building outcomes for each well can be found in Appendix A.2.

[10] The data sources for control variables, and the steps I took to aggregate control variables at the well level and "well area" level (i.e., near and far) using geospatial software can be found in Appendix A.2.

1.5.3 Study Period and Sample

The temporal component of my data allows me to examine a long difference in real estate investment that occurred between 1970 and 2017. I examine a long difference, rather than leveraging year to year changes in building, because I do not know when each well came into existence. Instead, I assign each well a minimum year, which represents the earliest year that it was recorded in state records (as permitted, completed, produced, stimulated, or plugged). I use 1970 as the start date of my study period because I know that wells missing all date data were in existence before 1970, and plugged wells without plugging dates were plugged before 1970.[11] Building and well data were collected up to 2017, which I use as the end of my study period.

There are 4,561 conventional oil and gas wells drilled in Washington County prior to 1970 in my sample. I drop from my sample 433 wells that are "switchers"—wells that were plugged between 1970 and 2017. This allows me to compare only wells that were unplugged for the entire study period to wells that were plugged for the entire study period. I also drop 111 wells that are within 300 meters of the cities of Washington and Canonsburg. Within these cities, the relatively high concentration buildings makes incentives to plug wells and build structures greater than in more rural areas, where there are more vacant parcels serving as available substitutes for investment.

My final sample contains 4,017 wells that were drilled prior to 1970 (Figure 1.4). Of those 2,510 were plugged for the entire study period and 1,507 were unplugged for the entire period. Of the unplugged wells, 1,028 never reported production after 1980, and I can be confident that they were abandoned and not active for at least three quarters of the study period.

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[11] I know that wells without dates were drilled sometime before the Gas Operations, Well Drilling, Petroleum and Coal Mining Act of 1955 began requiring permitting of new wells. Plugged wells without dates were retroactively entered by DCNR, with their plugging status determined by a 1964 mineral resource report. Using 1970 as a start date also serves to exclude wells that were plugged after the 1984 Oil and Gas Act, which, for reasons described in Section 1.3, significantly modified incentives for operators to plug wells.

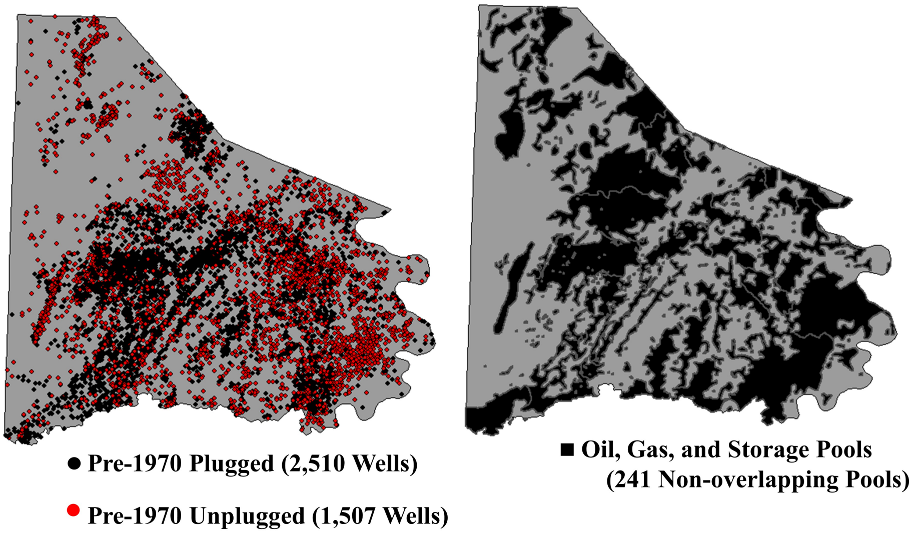

Figure 1.4: Historic Oil and Gas Wells in Washington County, PA

1.5.4 Defining Near and Far Areas

My empirical approach relies on selecting a "near" circle with a radius within which an unplugged well negatively influences landowners' investment decisions. The rows in Table 1.1 represent five concentric circles at 50 meter intervals around wells (depicted in Figure 1.5), and compares the mean footprint built post-1970 in each circle across plugged and unplugged wells.[12] It shows that most of the differential in post-1970 building occurs within the first 50 meters, where there is a difference of 50 percent between plugged and unplugged wells. I use this information to define the 0 to 50 meter circle as the near area, and the ring from 50 meters to 250 meters as the far area.

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[12] To determine a suitable radius for the near area, Tables 1.1 and 1.2 exclude oil and gas wells in four fields that intersect the metropolitan boundaries of Washington and Canonsburg. An oil or gas field is an accumulation, pool or group of pools in the subsurface. The entire surface of Washington County is covered by 43 fields. Excluding these four fields provides a better representation of the unconditional investment effects of wells across space, because they contain a high concentration of plugged wells that fall in overlapping coal fields, and because they are suburban and contain relatively more building. This leads to spurious correlations between plugging and building that obscures the unconditional spatial relationship between wells and building. These correlations are differenced away in equation 1 with a control for the distance to a coal mine and either pool or well fixed effects.

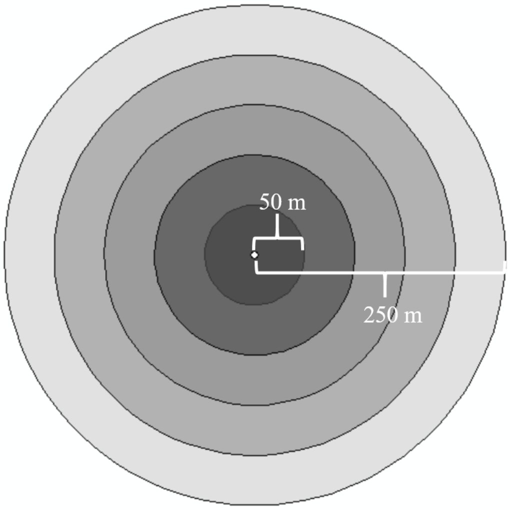

Figure 1.5: Distance Intervals

1.5.5 Descriptive Statistics

I present descriptive statistics to explore land characteristics and building across near and far areas, and across plugging status. Table 1.3 presents average values of control variables that measure the ability of the physical environment to support building. Panel A takes averages across all wells (plugged and unplugged), and shows significant differences between near and far areas at the greater than 99 percent level across all variables except slope, which is significant at the 10 percent level. Significant differences on five of the six variables hold across sub-samples that include only unplugged wells (Panel B) and only plugged wells (Panel C). The most plausible explanation for the observed differences is that land characteristics that well operators look for when drilling are similar to those that are better for building. The building data supports this explanation, as near areas received more building per square meter both after 1970 (Table 1.1) and before 1970 (Table 1.2).

Table 1.1: Mean Post-1970 Building Footprint by Location and Plugging Status

	Unplugged (1970 ~ 2017)	**Plugged (Before 1970)**			
Sample Size	**1,090**	**1,388**			
	Mean	**Mean**	**Difference**	**P-Value**	**Percent Difference**
0 to 50 m	.63	1.05	-.42	**0.05***	**-50%**
50 to 100 m	.48	.54	-.06	0.48	-12%
100 to 150 m	.40	.52	-.12	0.04*	-26%
150 to 200 m	.46	.45	+.01	0.76	+2%
200 to 250 m	.47	.52	-.05	0.46	-10%

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Two-Sided P-Values are from a T-test for the difference in means, unpaired data assuming unequal variances. To facilitate comparisons across locations, the dependent variable is expressed as square meters of building per 100 square meters of land.

Table 1.2: Mean Pre-1970 Building Footprint by Location and Plugging Status

	Unplugged (1970 ~ 2017)	**Plugged (Before 1970)**			
Sample Size	**1,090**	**1,388**			
	Mean	**Mean**	**Difference**	**P-Value**	**Percent Difference**
0 to 50 m	.50	.42	+.08	0.45	+17%
50 to 100 m	.46	.37	+.09	0.14	+20%
100 to 150 m	.43	.36	+.07	0.16	+18%
150 to 200 m	.48	.36	+.12	0.04*	+29%
200 to 250 m	.44	.35	+.09	0.09	+22%

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Two-Sided P-Values are from a T-test for the difference in means, unpaired data assuming unequal variances. To facilitate comparisons across locations, the dependent variable is expressed as square meters of building per 100 square meters of land.

Table 1.3: Comparison of Near and Far Areas

A. All Wells (4,017)

	Far (>50 to 250 m) **Mean**	**Near (<50m)** **Mean**	**Std. Mean Diff**	**P-Value**
Slope	17.209	16.935	0.043	0.054
Share of Area Covered in Water	0.076	0.029	0.358	0.000***
Soil Support of Dwelling With Basement	1.123	1.221	-0.258	0.000***
Soil Support of Dwelling Without Basement	1.291	1.371	-0.168	0.000***
Soil Very Limited (Basement)	0.878	0.779	0.264	0.000***
Soil Very Limited (No Basement)	0.709	0.630	0.168	0.000***

B. Unplugged Wells (1,507)

	Far (>50 to 250 m) **Mean**	**Near (<50m)** **Mean**	**Std. Mean Diff**	**P-Value**
Slope	17.562	17.099	0.069	0.058
Share of Area Covered in Water	0.070	0.027	0.366	0.000***
Soil Support of Dwelling With Basement	1.112	1.220	-0.287	0.000***
Soil Support of Dwelling Without Basement	1.281	1.377	-0.202	0.000***
Soil Very Limited (Basement)	0.887	0.779	0.293	0.000***
Soil Very Limited (No Basement)	0.719	0.624	0.203	0.000***

C. Plugged Wells (2,510)

	Far (>50 to 250 m) **Mean**	**Near (<50m)** **Mean**	**Std. Mean Diff**	**P-Value**
Slope	16.997	16.836	0.026	0.355
Share of Area Covered in Water	0.080	0.029	0.356	0.000***
Soil Support of Dwelling With Basement	1.129	1.221	-0.241	0.000***
Soil Support of Dwelling Without Basement	1.298	1.368	-0.148	0.000***
Soil Very Limited (Basement)	0.872	0.779	0.246	0.000***
Soil Very Limited (No Basement)	0.703	0.634	0.148	0.000***

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Two-Sided P-Values are from a T-test for the difference in means, unpaired data assuming unequal variances. Soil support variables are coded as 1= Very Limited, 2=Somewhat Limited, 3= Not Limited, and are simple averages across geologic contours that intersect 0 to 50 meter circles and 50 to 250 meter rings.

My empirical approach assumes that areas near plugged and unplugged wells are similar across unobserved characteristics that affect post-1970 building. To consider whether this assumption holds, I compare observed characteristics across the two types of wells. Table 1.2 shows that prior to the study period there were similar levels of building near plugged and unplugged wells. It suggests that prior to abandonment, the land near plugged wells was not systematically more attractive for building. To probe the assumption further, I compare observable characteristics that may affect building across plugged and unplugged wells within the same subsurface pools. I compare unplugged and plugged wells within pools (Figure 1.4), because raw comparisons would pick up differences in distances to other features (i.e., Interstate 79, parks) that are artifacts of the pools that experienced the most plugging. For instance, if to protect the pressure of an oil reservoir operators plugged more wells in a large, productive pool near Interstate 79, raw comparisons would show that plugged wells are much closer to the Interstate, even if plugging was distributed randomly within that pool.

To make within-pool comparisons, Table 1.4 compares residuals of a regression of each characteristic on a set of binary variables that represent the pool in which the well is located. It shows that, within pools, plugged and unplugged wells are statistically similar on all but two covariates. Plugged and unplugged wells are located on similar soil quality and slopes, and have similar measures of pre-1970 building. Although plugged wells are around 300 meters closer to the interstate on average, the magnitude of this difference is small relative to the geographic scope of my study, and I do not believe it is suggestive of systematic differences on unobservable attributes. Table 1.4 also shows that plugged wells are on average 160 meters closer to coal mines, which supports historical evidence in Section 1.3 that the third wave of plugging was driven by a desire to protect miners. I control for the distance to the nearest coal mine in equation 1, which takes on a value of zero if the well is within a mined area. But β_2, and β_3, would contain positive bias if mines are associated with unobserved land attributes that make building less attractive, in which case my estimates represent a lower bound of the true of the effect of unplugged wells on real estate investment.

Table 1.4: Comparison of Plugged and Unplugged Wells

	Unplugged (1970 ~ 2017)	**Plugged (Before 1970)**		
Sample Size	**1,507**	**2,510**		
	Mean Residual	**Mean Residual[1]**	**Standardized Mean Difference**	**P-Value**
Distance to I-79	205.78	-123.55	0.12	0.00***
Distance to Nearest Water Feature	-7.39	4.44	-0.02	0.48
Distance to Nearest Park or Gameland	5.65	-3.39	0.01	0.77
Distance to Nearest Coal Mine	104.67	-62.85	0.16	0.00***
Soil Very Limited (Basement)	0.01	0.00	0.03	0.35
Soil Very Limited (No Basement)	0.01	0.00	0.03	0.39
Slope	0.01	-0.01	0.00	0.96
Number of Pre-1970 Buildings				
Within 50	-0.02	0.01	-0.03	0.38
Within 100	-0.09	0.05	-0.04	0.24
Within 150	-0.18	0.11	-0.04	0.21
Within 200	-0.25	0.15	-0.03	0.31
Within 250	-0.34	0.20	-0.03	0.36
Pre-1970 Square M Footprint				
Within 50	-16.12	9.68	-0.03	0.32
Within 100	-33.12	19.88	-0.04	0.22
Within 150	-51.02	30.63	-0.05	0.11
Within 200	-67.25	40.38	-0.05	0.14
Within 250	-71.31	42.81	-0.04	0.23

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Residuals from a regression of each covariate on a set of binary variables that represents the pool in which the well is located. Two-Sided P-Values are from a T-test for the difference in means, unpaired data assuming unequal variances. In creating the slope and "soil ability to support" variables I took simple averages across all geologic contours that intersect 250 meter circles around wells.

1.6 Results

1.6.1 The Effect of Unplugged Wells on Post-1970 Building

Table 1.5 presents the results of a regression in the form of equation 1 for my three outcome variables—the number of buildings, number of rooms, and square footage—all built post-1970 and expressed per 100 square meters of land. In all of my results tables, model 1 includes no controls, model 2 adds pool fixed effects, and model 3 additionally adds controls for the slope, a binary variable indicating the soil is very limited to support dwellings with basements, and the first four "distance to" covariates listed in Table 1.4. Model 4 accounts for spatial bunching of wells by adding four controls for the number of wells within 250 meters across the dimensions of unplugged/plugged and pre/post-1970. Finally, model 5 drops the unplugged well binary and replaces pool fixed effects with well fixed effects.

Across all three outcome variables, significant and positive coefficients on "Near (0 to 50m)" suggest that near areas are better for building. Conversely, the "Unplugged Well" coefficients are insignificant in all but the most weakly controlled model. This suggests that my controls account for differences in post-1970 building that have occurred in the general vicinity (i.e., in the far rings) of unplugged wells relative to plugged wells.

Estimates of the effect of leaving a well unplugged on nearby investment, the "Unplugged Well X Near (0 to 50 m)" coefficients, are mixed across the three outcome variables. In Panel A and B, significant coefficients on "Near (0 to 50m)" combined with small, insignificant coefficients on "Unplugged Well X Near (0 to 50 m)" indicate that there are slightly more buildings and rooms within 50 meters of both plugged and unplugged wells, but the magnitude of the differences are practically unimportant. In Panel C, the significant coefficient on "Near (0 to 50m)" implies that plugged wells experience on average 74 to 84 additional square meters of footprint within 50 meters, relative to areas further away.[13] But almost all of the additional footprint is not built if the well is left unplugged: the coefficient on

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[13] In Table 1.5, as in all of my results tables, I present coefficients per 100 square meters of land. The 0 to 50 meter circle contains about 7,854 square meters. Multiplying 78.54 (7,854 square meters divided by 100 square meters of land) by the coefficients on "Near (0 to 50m)" and "Unplugged Well X Near (0 to 50 m)" allows for interpretation of the coefficients in their standard units (i.e., number of buildings, number of rooms, and square meters of footprint).

"Unplugged Well X Near (0 to 50 m)" indicates that there is an average of 70 to 73 fewer square meters of footprint near unplugged wells relative to plugged wells.

Table 1.5: Effect of Unplugged Wells on Post-1970 Building

	A. Dependent Variable: Post-1970 Number of Buildings				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unplugged Well	-0.001	-0.001	-0.000	-0.000	-
	(0.00)	(0.00)	(0.00)	(0.00)	-
Intercept	0.003***	0.003***	0.011***	0.010***	0.005***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

	B. Dependent Variable: Post-1970 Number of Residential Rooms				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.002	-0.002	-0.003	-0.003	-0.003
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.005**	0.005**	0.005**	0.005**	0.005***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unplugged Well	-0.006	-0.004	-0.002	0.000	-
	(0.00)	(0.00)	(0.00)	(0.00)	-
Intercept	0.015***	0.014***	0.060***	0.054***	0.016***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)

	C. Dependent Variable: Post-1970 Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.895*	-0.895*	-0.898*	-0.899*	-0.934***
	(0.35)	(0.35)	(0.36)	(0.36)	(0.27)
Near (0 to 50 m)	1.063**	1.063**	1.066**	1.067**	0.945***
	(0.34)	(0.34)	(0.34)	(0.34)	(0.23)
Unplugged Well	-0.433*	-0.154	0.045	0.325	-
	(0.20)	(0.15)	(0.08)	(0.21)	-
Intercept	0.903***	0.798***	6.351***	5.266***	2.013**
	(0.21)	(0.08)	(1.27)	(1.03)	(0.74)
N	4,017	4,017	4,017	4,017	4,017
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Unplugged Wells	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variables are expressed per 100 square meters of land area.

The results in Table 1.5 imply that unplugged wells do not affect the number of buildings or rooms constructed nearby, but do cause landowners to build less footprint near them. The most plausible explanation for these mixed results is that near areas are ill-suited for structures that are typically built close together (i.e., dense residential developments comprised of small homes, apartments, condominiums). If this were the case, systematic variation in the number of buildings and rooms near wells would be small, resulting in small coefficients on "Near (0 to 50m)" and "Unplugged Well X Near (0 to 50 m)." Instead, near areas may be suited for larger, stand-alone buildings and out buildings such as barns, sheds, or large garages.

Table 1.6 explores the effect of unplugged wells on the number of different types of structures built within 50 meters. Positive and statistically significant coefficients on "Near (0 to 50m)" in panels A and C suggest that there are slightly more residential and out buildings near wells. The coefficient is strongest for residential buildings (Panel A), and suggests that on average only .08 more residential buildings are built in near areas. The effect is less than half of this magnitude for out buildings (Panel C), and is insignificant in statistical and practical terms for commercial buildings (Panel B). Across the three panels, there is practically no difference between the number of buildings of any type between plugged and unplugged wells.

Table 1.7 explores the effect of unplugged wells on the amount of building footprint of different types built within 50 meters of wells. Nearly 75 percent of the positive relationship between building footprint and near areas is driven by out buildings, which can be seen when comparing the coefficients on "Near (0 to 50m)" across the three panels. Similarly, comparing the coefficients on "Unplugged Well X Near (0 to 50m)" across the three panels reveals that around 70 percent of the effect of unplugged wells on footprints is attributable to smaller out buildings, with the remaining 30 percent attributable to residential and commercial buildings.

Table 1.6: Effect of Unplugged Wells on the Number of Post-1970 Buildings

	A. Dependent Variable: Post-1970 Number of **Residential Buildings**				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.001***	0.001***	0.001**	0.001**	0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unplugged Well	-0.001	-0.000	-0.000	0.000	-
	(0.00)	(0.00)	(0.00)	(0.00)	-
Intercept	0.002***	0.002***	0.008***	0.008***	0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	B. Dependent Variable: Post-1970 Number of **Commercial Buildings**				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unplugged Well	-0.000*	-0.000*	-0.000	-0.000	-
	(0.00)	(0.00)	(0.00)	(0.00)	-
Intercept	0.000***	0.000***	0.001***	0.000***	0.000***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	C. Dependent Variable: Post-1970 Number of **Out Buildings**				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.000**	0.000**	0.000**	0.000**	0.000*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unplugged Well	-0.000	-0.000*	-0.000	-0.000	-
	(0.00)	(0.00)	(0.00)	(0.00)	-
Intercept	0.001***	0.001***	0.003***	0.002***	0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	4,017	4,017	4,017	4,017	4,017
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Unplugged Wells	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** p<0.001, ** p<0.01, and * p<0.05. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variables are expressed per 100 square meters of land area.

Table 1.7: Effect of Unplugged Wells on Post-1970 Building Footprint

	A. Dependent Variable: Post-1970 **Residential** Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.086	-0.086	-0.085	-0.085	-0.086*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Near (0 to 50 m)	0.138***	0.138***	0.137***	0.138***	0.131***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Unplugged Well	-0.139	-0.084	-0.044	0.002	-
	(0.07)	(0.07)	(0.04)	(0.03)	-
Intercept	0.367***	0.346***	1.345***	1.237***	0.458***
	(0.06)	(0.02)	(0.20)	(0.18)	(0.07)
	B. Dependent Variable: Post-1970 **Commercial** Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.152	-0.152	-0.150	-0.150	-0.166
	(0.10)	(0.10)	(0.10)	(0.10)	(0.09)
Near (0 to 50 m)	0.137	0.137	0.136	0.136	0.119
	(0.11)	(0.11)	(0.11)	(0.11)	(0.07)
Unplugged Well	-0.049*	-0.060	-0.033	-0.018	-
	(0.02)	(0.03)	(0.03)	(0.03)	-
Intercept	0.115**	0.119**	1.165**	1.126**	0.404**
	(0.03)	(0.04)	(0.39)	(0.42)	(0.14)
	C. Dependent Variable: Post-1970 **Out Building** Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.658	-0.658	-0.659	-0.659	-0.689**
	(0.35)	(0.35)	(0.36)	(0.36)	(0.26)
Near (0 to 50 m)	0.788*	0.788*	0.789*	0.789*	0.701**
	(0.36)	(0.36)	(0.36)	(0.36)	(0.22)
Unplugged Well	-0.245*	-0.011	0.120	0.340	-
	(0.11)	(0.07)	(0.08)	(0.20)	-
Intercept	0.421***	0.333***	3.788***	2.853**	1.194
	(0.12)	(0.10)	(1.10)	(0.87)	(0.74)
N	4,017	4,017	4,017	4,017	4,017
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Unplugged Wells	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variables are expressed as square meters of building per 100 square meters of land.

Together, Tables 1.5, 1.6 and 1.7 provide clarity about the relationship between wells and real estate development, which can be summarized as follows: Areas within 50 meters are especially suitable for large stand-alone out buildings, such as barns, garages, and sheds. But if wells are not plugged, landowners are discouraged from building near them, either because they tie up land or are perceived as hazards. For these reasons, unplugged wells are associated with an average of 70 to 73 fewer square meters, or 46 to 48 percent less building within 50 meters relative to plugged wells. Forgone building near a single unplugged well represents roughly half of the median building in the Washington County tax assessment data, which has a footprint of 131 square meters.

1.7 Robustness

A concern with the results presented in the preceding section is that some of the wells I code as "unplugged" may actually be active wells that are being properly maintained by their operators. I do not observe when an operator abandons a well, only whether it was plugged prior to 1970. Including active wells could bias the estimates in either direction, depending on whether active wells are more or less attractive to build near than abandoned wells. With this in mind, Table 1.8 includes a narrower sample of unplugged wells that have never reported production to the state since reporting began in 1980. This means that they are likely to have been completely abandoned for over two-thirds of the study period. The results for this trimmed sample are nearly identical in magnitude and significance as the main results in Table 1.5, Panel C.

Table 1.8: Effect of Likely Abandoned Wells on Post-1970 Building Footprint

Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.926*	-0.926*	-0.929*	-0.930*	-0.962***
	(0.38)	(0.38)	(0.38)	(0.38)	(0.27)
Near (0 to 50 m)	1.063**	1.063**	1.067**	1.068**	0.926***
	(0.34)	(0.34)	(0.34)	(0.34)	(0.23)
Unplugged Well	-0.395*	-0.154	0.018	0.271	-
	(0.18)	(0.13)	(0.10)	(0.21)	-
Intercept	0.903***	0.833***	6.640***	5.460***	2.155**
	(0.21)	(0.09)	(1.22)	(0.95)	(0.84)
	3,583				
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Unplugged Wells	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variable is expressed as square meters of building per 100 square meters of land.

Another plausible concern is that levels of post-1970 investment are endogenous to plugging decisions, which would occur if landowners arrange to plug wells immediately in advance of building structures. This could occur if landowners seek to make land more attractive or safe before construction projects begin, and spend their own money or pressure well operators or the state to plug wells. This would create negative bias in the estimated effects of unplugged wells on investment, because plugging would be correlated with landowner decisions that lead to real estate investment.

To explore whether this concern has biased the estimates, I test the sensitivity of the results in Table 1.5 by trimming the sample to exclude wells that were plugged in the 1960s. It is possible that landowners plugged or took steps to initiate plugging in the 1960s to prepare land for building in the early 1970s. It is less likely that landowners plugged wells prior to the 1960s with the intent of building ten or more years in the future. Table 1.9 presents results using a control sample that excludes wells plugged in the 1960s. The results are nearly identical in magnitude and significance to those in Table 1.5, Panel C. This suggests that reverse causality, in which landowners plug wells to prepare for imminent building, has not biased the estimates.

Table 1.9: Robustness: Effect Unplugged Wells on Post-1970 Building Footprint

Independent Variables:	(1)	(2)	(3)	(4)	(5)
Unplugged Well X Near (0 to 50 m)	-0.916**	-0.916**	-0.919**	-0.920**	-0.950***
	(0.34)	(0.34)	(0.35)	(0.35)	(0.28)
Near (0 to 50 m)	1.084***	1.084***	1.087**	1.088**	0.948***
	(0.32)	(0.32)	(0.33)	(0.33)	(0.24)
Unplugged Well	-0.446*	-0.160	0.055	0.347	-
	(0.19)	(0.15)	(0.08)	(0.21)	-
Intercept	0.916***	0.806***	6.285***	5.148***	2.080**
	(0.21)	(0.07)	(1.22)	(0.97)	(0.75)
N	3,917	3,917	3,917	3,917	3,917
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Unplugged Wells	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variable is expressed as square meters of building per 100 square meters of land. The sample excludes wells plugged in the 1960s.

1.8 The Forgone Value of Real Estate Investment

Before interpreting the estimates as evidence that unplugged wells caused landowners and developers to forgo building in Washington County, I must first rule out that the wells caused building to shift across space. Table la provides some initial evidence that localized shifting has not taken place, because unplugged wells have similar or fewer square meters of post-1970 buildings in the rings beyond 50 meters relative to plugged wells. But because far areas are less attractive for building, I need a more rigorous test.

To test for shifting, I leverage knowledge that areas near wells are typically good for building, and that plugged and unplugged wells tend to cluster together in space. Table 1.10 re-estimates the models using only the 2,510 plugged wells, and replacing the "unplugged" binary with a continuous variable indicating the number of unplugged wells within 50 to 250 meters.[14] The interaction terms in Table 1.10 all have negative signs and are statistically insignificant, which suggests that having more unplugged wells nearby does not lead to more building within 50 meters of plugged wells. In other words, builders do not appear to have shifted building away from unplugged wells and towards other nearby areas that are good for building. Finding no evidence of shifting, I interpret the estimated effect of 70 to 73 fewer square meters near unplugged wells as forgone building.

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[14] Significance of the results remain unchanged when I instead consider the number of unplugged wells that are within 50 to 100 meters.

Table 1.10: Shifting Test: Effect of a Plugged Well Near an Unplugged Well

	A. Dependent Variable: Post-1970 Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Number of UP Wells (50 to 250) X Near (0 to 50 m)	-0.317	-0.317	-0.320	-0.320	-0.325
	(0.23)	(0.23)	(0.23)	(0.23)	(0.19)
Near (0 to 50 m)	1.202**	1.202**	1.209**	1.210**	1.206***
	(0.44)	(0.44)	(0.45)	(0.45)	(0.30)
Number of Unplugged Wells (50 to 250m)	-0.238	-0.370**	-0.154**	0.090	0.000
	(0.12)	(0.13)	(0.05)	(0.30)	(.)
Intercept	1.007***	1.065***	7.549***	5.978***	0.923***
	(0.27)	(0.12)	(1.05)	(0.72)	(0.12)
	B. Dependent Variable: Post-1970 **Out Building** Square Meters				
Independent Variables:	(1)	(2)	(3)	(4)	(5)
Number of UP Wells (50 to 250) X Near (0 to 50 m)	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Near (0 to 50 m)	0.000	0.000	0.000	0.000	0.000*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of Unplugged Wells (50 to 250m)	0.000	-0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(.)
Intercept	0.001***	0.001***	0.003***	0.003***	0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	2,510	2,510	2,510	2,510	2,510
Pool Fixed Effects	No	Yes	Yes	Yes	No
Controls for Pre-1970 Level of Dependent Variable	No	No	Yes	Yes	Yes
Controls (Slope, Soil, and Nearby Water, I-79, Park, Mine)	No	No	Yes	Yes	Yes
Controls for Nearby Plugged and Post-'70 Unplugged	No	No	No	Yes	Via Fixed Effects
Well Fixed Effects	No	No	No	No	Yes

Note: Statistical significance represented by *** p<0.001, ** p<0.01, and * p<0.05. Robust standard errors clustered by pool in models 1-4, by well in model 5. To facilitate comparisons across locations, the dependent variable is expressed as square meters of building per 100 square meters of land. The sample includes the 2,510 pre-1970 plugged oil and gas wells .

To understand the dollar value of forgone building caused by unplugged wells, I first estimate the value that an additional square meter contributes to a property's total value. To do so, I create parcel-level datasets by combining the tax assessment data with separate data from Washington County on parcel transactions. My sample of assessments includes 18,237 parcels and the sample of transactions includes 7,853 parcels transacted between 1978 and 2012. In both datasets, I convert sale prices and assessed values to 2017 dollars. I remove parcels smaller than half an acre, which are mostly in urban areas where there are few wells, and parcels larger than the 95th percentile of approximately 20 acres. I also drop parcels with assessed values and sale prices that are greater than the 95th percentile and less than the 5th percentile, and with main or out building square meters greater than the 95th percentile.

Table 1.11 displays the results of two regressions, one with assessed value as the dependent variable, and the other with sale price. They show that conditional on acreage, access to public utilities, and municipal fixed effects, an additional square meter of residential building contributes on average \$454 to \$893 to the value of a parcel, and an additional square meter of out building contributes on average \$71 to \$99. I do not estimate similar figures for commercial buildings because the tax roll does not include data on their assessed values, and because they account for only 251 transactions in my sample.

One way to understand the relevance of the estimates is to compare the value of forgone building to the value of a typical property. Table 1.12, column A recognizes that 70 percent of the estimated effect, or about 52 square meters, is attributable to out buildings, while the remaining 30 percent (18.5 square meters) is attributable to residential or commercial buildings. I multiply the estimates of forgone square meters by the estimates of the value of square meters. The product indicates that each well prevents between \$12.1 K and \$21.6 K of building, or between 9 and 17 percent of the value of an approximately 1 acre property, which is roughly the median property size in the sample. Not all of the forgone value is likely to accrue directly on the property that contains the well. Each unplugged well has an average of one neighboring parcel within a 50 meter radius. To the extent that neighboring landowners avoid building near the wells, they may also bear the constraint on building.

Table 1.11: The Value of Square Meters of Building in Washington County

Dependent Variable:	**Assessed Value (2017 Dollars)**	**Sale Price (2017 Dollars)**
Independent Variables:	(1)	(2)
Square Meters of Residential Building	893.19***	453.84***
	(34.32)	(19.14)
Square Meters of Out Building	70.86***	99.10**
	(15.65)	(33.10)
Total Acres	457.87*	1,344.13***
	(219.05)	(378.49)
Public Water Area	763.47	3,814.38
	(2079.12)	(4,234.12)
Public Sewer Area	11,976.95**	10,343.45
	(3,721.98)	(10,050.72)
Public Gas Area	12,653.91***	19,896.72*
	(3,172.36)	(9,211.52)
Intercept	33,631.96***	85,192.91***
	(4,129.12)	(3,578.67)

Note: Statistical significance represented by *** $p<0.001$, ** $p<0.01$, and * $p<0.05$. Model 1 is run on a sample of 18,237 assessed parcels and includes municipal fixed effects. Model 2 is run on a sample of 7,853 parcel transactions made between 1978 and 2012 and includes municipality by year fixed effects. Robust standard errors are clustered at the level of the fixed effects.

Another reference point comes from comparing the total value of forgone building to the total tax base in Washington County. In Table 1.13, I multiply the forgone value estimates by 2,276—the number of pre-1970 unplugged wells in state records in Washington County. Doing so indicates that the known wells account for between \$27.5 million and \$49.3 million in forgone tax base. In 2019 Washington County levied a tax rate of .243 percent of assessed value, meaning that its known wells cause between \$75 K and \$112 K in forgone revenues annually, or \$3.8 million to \$5.6 million over a 50 year period (Table 1.14).

To estimate total forgone revenues for school districts and municipal governments, I create a municipal-level dataset that includes the number of pre-1970 unplugged wells, and the municipal and school district tax rates effective in 2019. For each municipality and school district, I multiply the number of unplugged wells by the estimated value of forgone square meters per well to calculate the estimated foregone tax base, and multiply the resulting product by the effective tax rate to estimate forgone tax revenues. Table 1.14 shows that over a 50 year period, unplugged wells led to \$1.8 million to \$2.7 million in forgone revenues

Table 1.12: Estimated Forgone Value of Building on a Typical Property

	A	**B**	**C**
	Estimated Square Meters of Building Forgone Per Well	**Estimated Value Per Square Meter**	**Estimated Value Per Forgone Square Meter (A×B)**
Residential/Commercial	18.5	$454 - $893	$8.4 K – 16.5 K
Out Building	52	$71 - $99	$3.7 K – 5.1 K
D. Estimated Total Forgone Value:			**$12.1 K - $21.6 K**
E. Mean Value of a .8 Acre to 1.2 Acre Lot:			$130 K
Forgone Value as Share of the Value of a Typical Parcel (D ÷ E):			9% - 17%

for municipalities, and $19.3 million and $28.8 million for school districts. McGuffey School District contains 700 unplugged wells, more than any other district in the county. For this district alone, I estimate that the known unplugged wells deter annual revenues of $117 K to $175 K, or about $70 to $100 per enrolled student, based on enrollment data from the 2016 Census of Local Governments.

Altogether, I estimate that the known unplugged wells have led to $24.9 million to $37 million in forgone tax revenues for the county government, municipalities, and school districts over the period 1970 to 2020. The dollar estimates in Tables 1.13 and 1.14 are only for known unplugged wells in state records. The estimates could be two to four times larger if I were to account for all unplugged wells in the county, based on the ratio of wells in state records to recent estimates of the total number of wells drilled in Pennsylvania.

Table 1.13: Forgone Tax Base from Known Unplugged Wells in Washington County

	A	B	C	D
	Estimated Square Meters of Building Forgone Per Well	**Estimated Value Per Square Meter**	**Number of Known Wells**	**Estimated Total Tax Base Forgone (A×B×C)**
Residential/Commercial	18.5	$454 - $893	2,276	$19.1 M - $37.6 M
Out Building	52	$71 - $99		$8.4 M - $11.7 M
			E. Estimated Total Forgone Tax Base:	**$27.5 M - $49.3 M**
			F. Total Tax Base:	$17.3 B
			Share of Tax Base Forgone (E ÷ F):	.16% - .28%

Table 1.14: Forgone Tax Revenues from Known Unplugged Wells in Washington County

	Estimated Forgone Annual Tax Revenues Due to Unplugged Wells	**Estimated Forgone Tax Revenues for the Period 1970 to 2020**
County	$75 K - $112 K	$3.8 M - 5.6 M
Municipalities	$35 K - $53 K	$1.8 M - $2.7 M
School Districts	$386 K - $575 K	$19.3 M - 28.8 M
Total	**$496 K - $740 K**	**$24.9 M - $37 M**

1.9 Conclusion

I have applied a spatially-precise empirical approach to understand whether environmental hazards can deter real estate investment over several decades. I find that hazards can deter investment in rural and suburban areas by modifying landowners' decisions about how big to build their homes and whether to build additional structures on their properties. Unlike in urban areas where returns to investment are often large and greatly outweigh reclamation costs, in more rural areas adding reclamation costs to a project's outlay may make marginal investments unattractive. If environmental hazards deter investment, they may reduce the market value of nearby properties and reduce revenues for local governments. In the long term, remediation may increase the investment potential of nearby properties.

Using the case of abandoned oil and gas wells, I find that the typical unplugged well deterred around 70 to 73 square meters of building over a half-century, and provide several estimates of the value of forgone building in terms of reduced property values, local government revenues and tax bases. I do not provide evidence that the benefits of plugging a typical well outweigh projected plugging costs, because my approach does not directly estimate the public's willingness to pay for plugging. Doing so would require theoretical models an empirical approaches that can estimate the effects of hazards on local welfare over several decades, which is an area ripe for further inquiry. Instead, my findings imply that, all else equal, regulators should target already committed public dollars to locate and reclaim unplugged wells in areas with significant investment potential. They also provide an additional rationale for selecting policies to avert improper abandonment of currently active wells, such as increasing the dollar amount of well bonds to match reasonable estimates of plugging and reclamation costs.

A clear understanding of the costs of currently abandoned wells may have its greatest value in helping stakeholders select policies to shield against similar costs from the current surge in shale oil and gas development. In Pennsylvania alone, operators have drilled over 11,000 shale wells over the last 15 years, and most projections indicate that drilling will continue over the next several decades. Although operators have abandoned very few shale wells, some observers note that they often sell older wells to smaller, less capitalized firms

(Bishop, 2013; Mitchell and Casman, 2011). If sustained periods of low oil and gas prices force smaller firms into bankruptcy, they could saddle the state with hundreds of additional unplugged wells. If shale gas wells are left unplugged, reclaiming them may cost taxpayers much more than older wells, because they are drilled deeper, use more land, and require more equipment on the surface. The empirical estimate of the effect of unplugged wells on nearby real estate investment presents one benefit of selecting more stringent policies to avert the improper abandonment of wells that could harm public welfare for many decades into the future.

2.0 Compensating Communities for Industrial Disamenities: The Case of Shale Gas Development

2.1 Introduction

Many industries create benefits that are experienced broadly, but disamenities for those who live near production sites such as factories, mines, or wells. For instance, over the last decade domestic natural gas production has boomed, and US consumers have saved more than $74 billion annually because of lower natural gas prices (The Council of Economic Advisers, 2020; Hausman and Kellogg, 2015). But some residents near oil and gas wells experience disamenities such as noise, traffic, and air pollution.

Central governments have policies to ensure that rising industrial activity contributes new revenues to local governments, which can compensate affected communities. All sixteen of the major oil and gas producing states provide revenues to local governments or school districts (Raimi and Newell, 2016). The policies are not unique to the oil and gas industry, and vary on the types of local jurisdictions that receive revenues and what they can be spent on. Some require that governments use revenues to repair damages attributable to the industry. For instance, under the Surface Mining Control and Reclamation Act, the US government taxes coal extraction, and states use the revenues to clean up land and water and respond to emergencies at mines. Other policies have fewer restrictions, and governments can spend the money on unrelated public goods or return it to residents.[1] States authorize local jurisdictions to collect property taxes from landfills, mines, and factories, and place few restrictions on how they are spent. But which approach benefits communities more? Does restricting spending to address industrial disamenities lead to investments with higher returns, or should revenues be used more broadly?

Legislators enact spending restrictions for political reasons, including cultivating public support for environmental taxes and locking in political agendas over time (Kallbekken

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[1] Throughout the essay I use the term "public goods" to refer to goods provided by government entities, as opposed to the alternate definition of goods that are non-excludable and non-rivalrous in consumption.

and Aasen, 2010; Sælen and Kallbekken, 2011; Brett and Keen, 2000; Jackson et al., 2013). Despite their political advantages, it is unclear whether restrictions improve well-being in communities facing industrial development relative to unrestricted revenues. Ideally, local officials maximize residents' well-being by balancing the marginal return to public revenues across all uses, including returning them to taxpayers. Some approved projects can present relatively high returns, and are easy for restriction-enacting legislators to identify and local officials to execute. Examples include repairing roads and infrastructure, or increasing funding for overburdened emergency services. But restrictions can force local officials to spend too much on approved uses and not enough on others. If some disamenities are unrepairable, such as noise or health risks, restrictions may prevent officials from compensating residents for them by funding unrelated, yet welfare-enhancing investments.

I study shale gas development in Pennsylvania and Ohio to understand how the restrictiveness of compensation policies affects local public investment. The neighboring states cover the Marcellus and Utica shale formations, two of the most prolific sources of natural gas in the world. Local jurisdictions in the two states experienced similar drilling trajectories, and are similar in size and the public goods they provide. The similarities enable a natural experiment to understand how their main difference—restrictions on the types of jurisdictions that receive revenues—affects how local governments spend, save, or return revenues to taxpayers. In Ohio, school districts and municipalities levy property taxes on the expected profits from an oil or gas well, and use the revenues in any way they see fit. Pennsylvania leaves out schools, and transfers revenues from a fee paid by well operators to municipalities and restricts spending to several broad categories related to drilling.

Using data on the locations of shale wells and financial data from jurisdictions within 75 miles of the Pennsylvania-Ohio border, I compare fiscal responses to drilling from 2011 to 2016. Specifically, I look at the fiscal responses of school districts and municipalities, which are jurisdictions that overlap geographically but are entirely separate political entities (e.g., they have separate elected officials and budgets). I find that restrictions tying compensation to industrial disamenities can limit money from reaching jurisdictions with the capacity and authority to invest in highly-valued public goods. The typical Pennsylvania municipality uses three-quarters of the revenues generated by a one-year-old well to repair roads, and saves or eliminates debt with the vast majority of revenues received in subsequent years. In Ohio, where policies shield municipalities from paying for road damage, the typical municipality with wells also increases its savings. Growing savings in both states indicate that there is weak demand for municipal goods outside of road repair, or that municipalities have limited capacity to repair non-road damages and increase the quantity or quality of unrelated goods. Several other studies document cases where new revenues failed to improve local well-being because recipient jurisdictions lacked the capacity to spend them in ways that increase local productivity (Arellano-Yanguas, 2011; Caselli and Michaels, 2013; Cust and Poelhekke, 2015).

If the goal of compensation policies is to improve local well-being, it may be best achieved by a policy that enables broad local discretion. The relative strength of the property tax system is that decisions about how much money comes in and how it is allocated across overlapping jurisdictions are decentralized, which appears to provide money to jurisdictions with the capacity and authority to make investments that residents value. The typical shale well in Ohio creates 20 times more property tax revenues for the school district in which it is located relative to the municipality in which it is located. The typical drilled school district saves much of these revenues over the period. But they simultaneously take on debt, with outstanding debt increasing by an average of $53,000 per well. Although I am unable to estimate a relationship between Ohio school districts' rising property tax revenues and their expenditures, their rapidly increasing debt suggests that they are leveraging shale revenues to fund capital investments, which take several years to finance. Marchand and Weber (2020) also find that districts in Texas respond to rising shale property tax revenues by issuing new debt, and in their setting the property tax revenues are used to pay the principal and interest on new bonds to finance capital projects. I also find that Ohio districts return a small amount of their shale revenues to taxpayers by lowering residential property tax rates—an estimated 8 percent reduction for the district with the mean number of wells. Altogether, my findings are consistent with studies showing that decentralized compensation schemes, including the local property tax system (Weber et al., 2016) and unrestricted transfers (Cust et al., 2014), can boost public goods provision and improve economic outcomes in communities hosting industrial activities.

2.2 Development and Disamenities in the Marcellus and Utica

In the early 2000s, technological innovations and high natural gas prices made it profitable for firms to drill wells in gas-rich shale formations (Wang and Krupnick, 2015). The innovations include hydraulic fracturing, or fracking, which involves the injection of vast quantities of water, sand, and chemicals at high pressure to release oil and gas from otherwise impermeable shale rock. They also include improvements in drilling long horizontal sections through thin layers of shale, which increased productivity by allowing each well to contact more oil or gas.

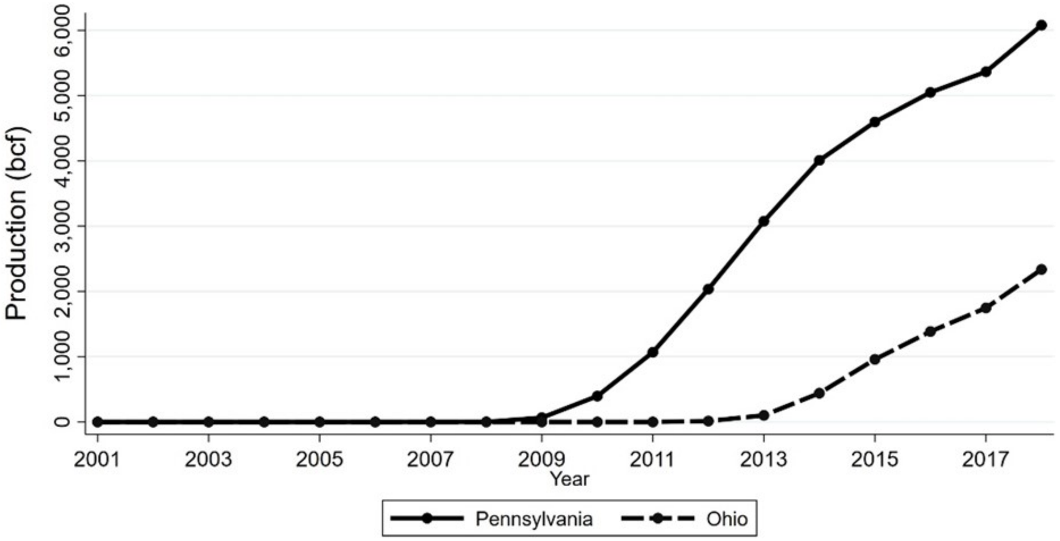
In 2000, natural gas from Pennsylvania amounted to less than one percent of US production (US Energy Information Administration, 2020a). But by 2009, shale production took off there (Figure 2.1). Today natural gas production in Pennsylvania is second only to Texas, and accounts for around 17 percent of US production. Like Pennsylvania, Ohio produced small amounts of natural gas prior to the shale boom. Production in Ohio lagged behind Pennsylvania, primarily because its most prolific formation, the Utica, is deeper and can be more costly to drill. The lack of facilities to prepare natural gas for pipeline distribution also constrained production there (US Energy Information Administration, 2014). But as drilling refinements improved profitability in the Marcellus, operators began exploratory drilling in the Utica, which also underlies Pennsylvania and West Virginia (US Energy Information Administration, 2014). Both drilling and production saw large increases in 2014, when several processing facilities were brought into service and reduced distribution constraints. Today Ohio is a top-ten natural gas producer, accounting for around 7 percent of domestic production.

Many studies explore the economic and environmental impacts of Marcellus and Utica development, and reveal that it has both benefited and disrupted residents in communities with wells. They have benefited from land and mineral lease payments and growth in jobs and wages (Brown et al., 2016; Komarek, 2016; Jacobsen, 2019). But they have faced or feared impairments to their health and safety, especially those related to deteriorating air and water quality. Research in Pennsylvania has documented declines in surface water quality from improper fracking wastewater treatment (Warner et al., 2013; Olmstead et al., 2013), and increased methane in groundwater due to failure of well casings (Brantley et al., 2014;

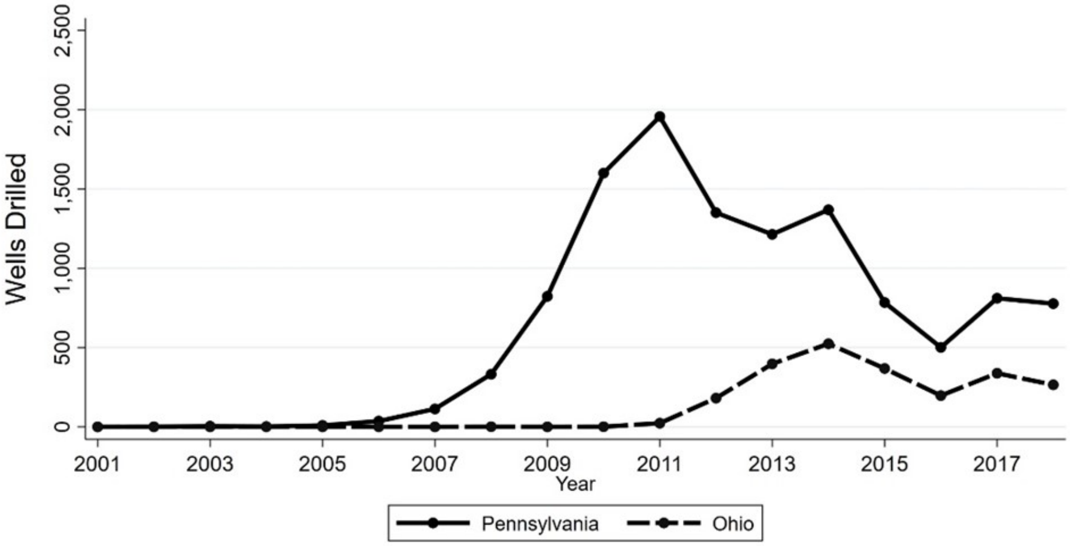
Jackson et al., 2013). Muehlenbachs et al. (2015) find that the perceived risk of groundwater contamination decreases the value of the typical Pennsylvania home within one kilometer of a well by around 16 percent. Research from across the US has found elevated levels of volatile organic compounds in air near wells, processing stations, and wastewater impoundments, some of which are known to cause cancer and other health conditions (Macey et al., 2014; McKenzie et al., 2012; Colborn et al., 2014). Loomis and Haefele (2017) estimate that air pollution presents the greatest public health costs from shale development.

Outside of environmental and health risks, shale development can strain local public infrastructure, especially roads. Abramzon et al. (2014) estimate that the hundreds of truck trips required to deliver equipment, water, and sand to just one well can cause up to $10,000 in damages to state roads. Deteriorating roads and the presence of trucks may make driving more dangerous. Muehlenbachs et al. (2017) estimate that for each shale well in Pennsylvania, there are an additional .25 truck accidents and .84 non-truck accidents in the quarter and county where it is drilled. Outside of roads, the arrival of the industry and an influx of workers can increase local public expenditure on water infrastructure, law enforcement, emergency services, housing, and land use planning (Newell and Raimi, 2018; Kelsey, 2014).

More broadly, shale development can transform the character of the small, rural communities where it occurs. When drilling rigs arrive, they can be accompanied by noise, traffic, crime, construction of pipelines and processing facilities, population growth, and rising prices of non-tradeable goods. Bartik et al. (2019) study nine major shale plays across the US and estimate the total value of local disruptions to be $1,400 annually for the typical household in a heavily drilled county. To compensate communities that bear the costs, Pennsylvania and Ohio have policies that collect revenues from oil and gas firms and allocate them to local jurisdictions.



(a) Production (bcf)



(b) Wells Drilled

Figure 2.1: Shale Gas Development in Two States

Note: Production data are from the US Energy Information Administration (2020c). Data for Pennsylvania are from the Pennsylvania Department of Environmental Protection (2020b). For Ohio, data are from the Ohio Department of Natural Resources (2020).

2.3 Fiscal Institutions in Two States

In this section, I review the policies that provide oil and gas revenues to local governments across the two states. While Pennsylvania requires that recipients spend the revenues in specified expenditure categories, Ohio has no such requirements. But the relative restrictiveness of Pennsylvania's policy primarily comes from limits on the jurisdictions that receive revenues. Pennsylvania provides no revenues to schools, because education is unrelated to drilling. The exclusion is notable: across the major oil and gas producing states, 67 percent of oil and gas revenues received by local entities go to schools (Raimi and Newell, 2016).

2.3.1 Ohio's Oil and Gas Property Taxation

Local governments in the US have collected residential and commercial property taxes since colonial times, and today they remain the primary revenue source for most local governments. Property taxes on oil and gas reserves, production, and equipment represent the leading source of local government revenues from the oil and gas industry (Raimi and Newell, 2016).

Ohio allows municipal and county governments, school districts, and special districts to collect oil and gas property taxes. The Department of Taxation determines the taxable value of oil and gas reserves for each producing well using a formula that factors in oil and gas prices and the well's average daily production over the tax year (Ohio Department of Taxation, 2020). County treasurers calculate annual tax bills for each operator by adding up, for all of the wells the operator owns, the product of each well's taxable value and the aggregate commercial tax rate where the well is located. The aggregate rate is the sum of the rates set by all jurisdictions that overlap in space (e.g., the county, municipality, school district, and fire district). A given jurisdiction sets one commercial rate that applies uniformly to oil and gas property and other commercial, mineral, and industrial property. It sets a separate rate for residential and agricultural property.

Rising Utica development has resulted in rapid growth in oil and gas property tax revenues. The Ohio Oil and Gas Association and Energy In Depth (2019) report that in 2016, when the first payments became due for the many wells drilled in 2014, local jurisdictions in the eight heaviest drilled counties received $48 million in oil and gas property taxes. In 2011, prior to the sharp uptick in Utica drilling, this number was less than $800,000.

Local communities in Ohio are shielded from paying for damage to municipal and county roads caused by the oil and gas industry. State law conditions the approval of shale drilling permits on operators demonstrating a good-faith effort to enter into road user maintenance agreements with municipal or county governments. The agreements commit the operator to a specific traffic route and to either post a bond or maintain the route themselves. It can also require the operator to improve roads prior to drilling a well to ensure that the route can safely handle increased truck traffic. Use of the agreements has been widespread: as of 2013, local governments in the five counties with significant drilling had signed over 300 agreements that together required around $89 million in road improvements (Ohio Department of Transportation, 2014). Unlike Ohio, Pennsylvania does not require operators to negotiate agreements. Any government in Pennsylvania can set weight limits on roads and bridges and require bonds from haulers that exceed them. But its policies place the burden on local governments to anticipate damages from oil and gas development and take several pre-emptory actions before posting the limits, such as conducting a traffic study and adopting a local traffic ordinance (Pennsylvania Department of Transportation, 2018).

2.3.2 Pennsylvania's Impact Fee

In 2002, the Pennsylvania Supreme Court decided that state law does not authorize local governments to tax oil and gas reserves (Supreme Court of Pennsylvania, 2002). Rather than modifying state law to authorize it, state legislators took a different approach to provide revenues to affected communities. Under Act 13 of 2012, the state began collecting "Impact Fees" from well operators and disbursing them annually to county and municipal governments. Over its first four years, the fee provided an annual average of $203 million to state agencies, special districts, and county and municipal governments (Pennsylvania Public Utility Commission, 2020).

Operators pay the Impact Fee once every year for each of their shale wells. The fee amount is based on a schedule that factors in the age of the well and the annual price of natural gas. When prices are lower and wells are older, the operator pays lower fees. Each year, about 20 percent of the fee goes to state agencies and county conservation districts, and the remaining 80 percent goes to county and municipal governments. Of the money that goes to municipalities, about 80 percent goes to municipalities that host shale wells, while the remaining 20 percent goes to municipalities without wells but in counties with wells. Two complex disbursement formulas determine how much each municipality receives, and factor in its population, highway mileage, and the number of wells in its borders, all relative other municipalities in the state or in the same county.

Act 13 enumerates thirteen approved expenditure categories that it states are "associated with natural gas production," and are listed in Appendix B.4. Although Pennsylvania ostensibly restricts spending to activities associated with natural gas production, the approved expenditure categories are broad and encompass many of the activities traditionally performed by municipal governments. Instead, an implicit restriction on the types of jurisdictions that can receive fee revenues is likely to have the greatest influence on whether the money offsets industrial disruptions or funds unrelated public goods. Barring unlikely transfers from county or municipal governments to school districts, the Impact Fee program rules out using oil and gas revenues to fund education. More broadly, because each type of jurisdiction has the capacity to deliver some public goods but not others, policies that exclude a jurisdiction effectively prohibit spending on the goods that it provides.

2.4 Motivations for Compensation and Restrictions

Central governments can use revenues from taxes on industrial activities in many ways. For instance, they can eliminate other distortionary taxes, offset the burden of the new tax on consumers, or invest in less damaging production technologies (Marron and Morris, 2016). Regardless of which use creates the largest returns, they often use them to compensate communities that host the industry, presumably to stem discontent that might arise if residents experience the costs of industrial activity without opportunities to share in its benefits (Harleman and Weber, 2017). Central governments typically provide local governments with industrial tax revenues rather than compensating communities in kind with centrally-planned projects, because local officials are presumed to have more complete information on residents' demand for public goods (Tiebout, 1956; Oates et al., 1972).

The rationale spending restrictions is less intuitive, but a vast literature suggests that legislators enact them to leverage at least four types of political advantages.[2] First, legislators use them to build support for new taxes that are unpopular despite being efficient, in the sense that they force industrial firms to internalize their environmental impacts and consumption of public goods. Recent evidence reveals that dedicating revenues to environmental purposes can elevate public approval for environmental taxes, even among those not directly affected by the disamenities (Sælen and Kallbekken, 2011; Thalmann, 2004; Steg et al., 2006; Kallbekken and Aasen, 2010; Amdur et al., 2014). Legislators may also use restrictions as bargaining chips within the legislative body, by agreeing to vote for unrelated budgetary restrictions in exchange for votes to siphon money towards public goods demanded by their supporters (Jackson, 2011; Goetz, 1968).

Second, legislators with a strong preference for mitigating industrial damage may enact restrictions to make it harder for future legislative bodies to use the money for other purposes (Jackson et al., 2013). Brett and Keen (2000) use a game theoretical framework to show that "green" legislators, those with the same environmental preferences as the electorate, will restrict revenues for environmental purposes if the efficiency loss caused by the restrictions is less than the loss from their successor fully wasting the revenues.

Third, restrictions may enable legislators to take credit for local investments and signal their commitment to supporting affected communities (Brett and Keen, 2000). Without restrictions, voters may see little connection between legislators and local investments, even if legislators authorize unrestricted transfers or enable local jurisdictions to levy taxes.

Fourth and finally, legislators may enact restrictions to exert control over spending, especially if they think that recipients would spend unrestricted money wastefully. Even if local

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[2] Much of this literature is on "earmarking," which traditionally refers to dedicating revenues from a single tax to single public service. I use the term "restrictions" rather than "earmarking" to encompass a wider range of limitations, including limiting spending to numerous, broad categories or limiting the types of local jurisdictions that receive revenues. In a seminal paper on earmarking, Buchanan (1963, p. 458) notes that granting taxing powers to local governments that overlap in space but have discrete responsibilities has similar theoretical implications to earmarking as a budgetary activity.

officials are responsible for wasteful spending, residents may blame legislators if underlying inequities are unresolved because central governments are responsible for regulating industrial activities. Restrictions and related expenditures create a formal line of accountability between local officials and legislators that may be particularly attractive when an industry causes highly salient disruptions but provides few benefits to residents in the form of jobs and other income.

2.5 Restrictions and Local Government Spending

Despite their political advantages, it is unclear whether restrictions are efficient, in terms of their ability to improve residents' well-being relative to unrestricted compensation. McCleary (1991) notes that although restrictions can assure stable financing for public goods that residents value, they can force local officials to spend too much on approved projects and not enough on projects that residents value even more. Efficient local spending of industrial revenues may depend on two factors: the type of restrictions and the uses they authorize, and the capacity and authority of recipients to carry out approved projects.

2.5.1 An Illustration of the Effects of Restrictions

A simple illustration sheds light on how the two factors intervene in the relationship between restrictions and residents' well-being. Imagine that an industrial activity creates D dollars in damages, and local jurisdictions receive R dollars in industrial revenues through prevailing fiscal policies. Together, recipient jurisdictions are capable of repairing d dollars in damages attributable to the industrial activity (e.g., damaged roads, polluted land, etc.).

Consider a scenario where fiscal policies generate revenues exactly equal to the damages, and where recipient jurisdictions are capable of executing projects that perfectly rectify the damages, or in our notation $R = D = d$. In an ideal world, local officials are benevolent and maximize residents' well-being by spending R so that the last dollar would create equivalent improvements in well-being regardless of what it was spent on. More practically, because most investments require non-marginal changes in spending and their impacts on well-being are not perfectly foreseeable, benevolent local officials pursue a constrained optimum by conceiving of discrete projects, ranking all possible projects based on some intuition or projection of their impacts on well-being, and successively selecting the project that most improves well-being until R is exhausted. In this setup, officials could consider one spending category, such as road paving, as several discrete and tiered projects (i.e., paving 10, 20, or 30 miles of road).

Without restrictions, benevolent local officials spend all of R to offset D if repairing industrial damages benefits residents more than every other available project. Otherwise, officials repair $R - q^* = d^*$ in damages and spend the remaining q^* on unrelated projects. Here d^* and q^* is the combination of repairs and unrelated projects that most improves residents' well-being. The unrelated projects may provide additional public goods, and may also include tax cuts.

There are two types of restrictions, and both may create sub-optimal spending outcomes. In the first type, central governments require that recipients spend all of R to offset D.[3] With the restrictions, officials achieve optimal spending outcomes only if repairing industrial damages benefits residents more than all unapproved projects (i.e., $q^* = 0$). Otherwise, officials spend all of R to repair D, which does less to improve well-being than the unrestricted spending combination of d^* and q^*. In this scenario, restricted revenues are preferable to unrestricted revenues only if we relax the assumption that local officials are benevolent, and instead assume that spending all of R on D is preferable to the mix of projects selected by non-benevolent officials.

Under the second type of restriction, central governments limit local discretion on how much of R each local jurisdiction receives. Officials achieve optimal spending outcomes only if all of the most welfare-enhancing projects fall under their jurisdiction, or if they transfer some of R to other jurisdictions. In practice, transaction costs probably prevent the full

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[3] In this discussion, I assume that restrictions are binding. Some studies find that recipients use restricted revenues to supplement spending on approved projects (Evans and Owens, 2007; Evans and Zhang, 2007; Bartle, 1995), while others find that they shift preexisting spending on approved projects to unapproved projects (Baicker and Staiger, 2005; Cascio et al., 2013). Restrictions can be made binding with "maintenance of effort" provisions, in which central governments withhold funds if spending on approved projects falls below some share of prior levels.

set of transfers that achieves the optimal spending outcome. If legislators lack information about the distribution of D, the restrictions may prevent jurisdictions capable of offsetting damages from receiving revenues, so that the total damage recipients can repair is d^o, where $D = d > d^o$. In this scenario, officials spend $d^1 = R - q^1 \leq d^o$ to repair damages and q^1 on only those unrelated projects that fall within their jurisdiction. This outcome does less to improve local well-being than d^* and q^*, which is achieved with less restrictive rules that allocate R across jurisdictions in proportion to their share of the most welfare-enhancing repairs and unrelated projects.

Restrictions of the second type may also determine how much of R recipients convert to private income. With or without restrictions, local officials may be reluctant to lower tax rates, because they may face political backlash if the industry contracts and they must increase rates to maintain baseline levels of public goods (Stein, 1984; Fossett, 1990). But when central governments decide how much each jurisdiction receives, officials may be even less inclined to return revenues relative to a case where officials set tax rates upfront. This is because of the behavioral economic concept of loss aversion, in which individuals value resources that they have on-hand more highly than resources they have not yet acquired (Kahneman and Tversky, 1984; Kahneman et al., 1991).[4]

With or without restrictions, it is likely that local officials are incapable of fully rectifying industrial damages, so that $D > d$. Some damages may be easy for local officials to repair and for legislators to identify, such as damage to roads or infrastructure. But others may be unavoidable industrial byproducts, such as noise and traffic, or damages that recipient jurisdictions lack the capacity or authority to repair, such as air or water pollution. With $R = D > d$ and without restrictions, local officials spend $R - q^* = d^*$ on damages that they are capable of repairing and will benefit residents more than other available projects, and spend the remaining q^* on the unrelated projects with the next highest returns. But with both types of restrictions in place, officials are bound to spending all of R to offset D, and the remaining $R - d^o$ either increases the savings of recipients or is returned to the grantor government, depending on specific rules. This outcome is inefficient because d^o may exclude

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[4] An alternate but not mutually-exclusive explanation is that providing R to more local jurisdictions in each tax district results in larger aggregate tax cuts if officials resist the efficient cut commensurate with their revenues, but respond to any new revenues with small, arbitrary cuts.

some of the most welfare-enhancing repairs, and because it excludes unrelated but preferred projects in both recipient and non-recipient jurisdictions.

In reality, the most probable scenarios are $R < D > d$ or $R > D > d$, because both central and local officials cannot accurately measure D or set tax rates and allocation formulas so that $R \approx D$. But inequality of R and D does not substantively alter our conclusions. Under all of the former assumptions but with $R > D$, communities are better off but inefficiencies created by the restrictions are greater because more money cannot flow to the most welfare-enhancing projects. Similarly, restrictions create smaller inefficiencies if $R < D$, but less money is available to address underlying inequities created by the industry.

2.5.2 Hypothesized Relationships Guiding the Empirical Analysis

The illustration reveals three hypotheses about the relationship between restrictions and local public investment in Pennsylvania and Ohio. First, local officials in Ohio will make more investments unrelated to industrial impacts relative to Pennsylvania. Ohio's less restricted officials can set tax rates upfront to allocate money across jurisdictions in approximate proportion to their share of the most welfare-enhancing repairs and unrelated projects. In Pennsylvania, local officials select a bundle of unrelated projects that excludes education, a highly salient public good.

Second, because Pennsylvania municipalities may lack the capacity or authority to fully rectify damages or spend them on unrelated projects, revenues there will increase municipal fund balances or be paid down on debt. The Impact Fee can represent a large share of municipalities' annual revenues, but they only have expertise in a few core functions, such as road repair and solid waste removal. If they are unable to execute approved projects related to drilling impacts, they will save more than Ohio municipalities that can raise revenues commensurate with spending capacity.

Third, local officials in Pennsylvania will enact fewer and smaller property tax cuts. Although tax cuts are an approved use of the Impact Fee, loss aversion may prevent municipalities with transferred cash on-hand from returning them to residents relative to Ohio jurisdictions that face the choice upfront when setting rates.

2.6 Data and Sample Selection

I compile a dataset of public finance variables and shale well counts for school districts and municipalities in the most heavily drilled parts of western Pennsylvania and eastern Ohio. I focus on school districts and municipalities because they represent the most local level of government, and in both states oil and gas revenues represent a larger share of municipal revenues than county revenues. Data on municipal and school district finances come from the US Census Bureau's Census of Governments, which collects data on revenues, expenditures, debts, and assets from every local jurisdiction every five years. The most recent census was conducted in 2017 and provides financial data for the 2016 fiscal year. I acquired data for the years 2001, 2006, 2011, and 2016 from the Government Finance Database, which aggregates data across census years (Pierson et al., 2015).

I begin with 2,185 municipalities (towns, boroughs, cities, villages, and townships) that fall within in the US Energy Information Administration's Appalachia drilling region.[5] To yield a comparable sample across states, I retain the 1,275 municipalities within 75 miles of Pennsylvania's western and Ohio's eastern border. I limit the sample within 75 miles, rather than a greater distance like 100 miles, to exclude heavily drilled jurisdictions near the Pennsylvania-New York border, which are not contiguous with my final sample and experience different financial trends. Urban jurisdictions also experience different financial trends than the rural areas that are drilled. I remove 185 urban municipalities—those in NHCS-designated central counties of metropolitan statistical areas and those that share borders with Census-designated principal cities. I also remove 10 municipalities with fiscal years that do not end on December 31st like the rest of the sample, and 65 with missing or inaccurate data.

Of the remaining 1,025 municipalities, 319 contain wells drilled by 2016. Data on the drilling dates and locations of shale wells are from Pennsylvania Department of Environmental Protection (2020b) and Ohio Department of Natural Resources (2020). I match well and financial records using municipal and county names, and count the number of wells in a

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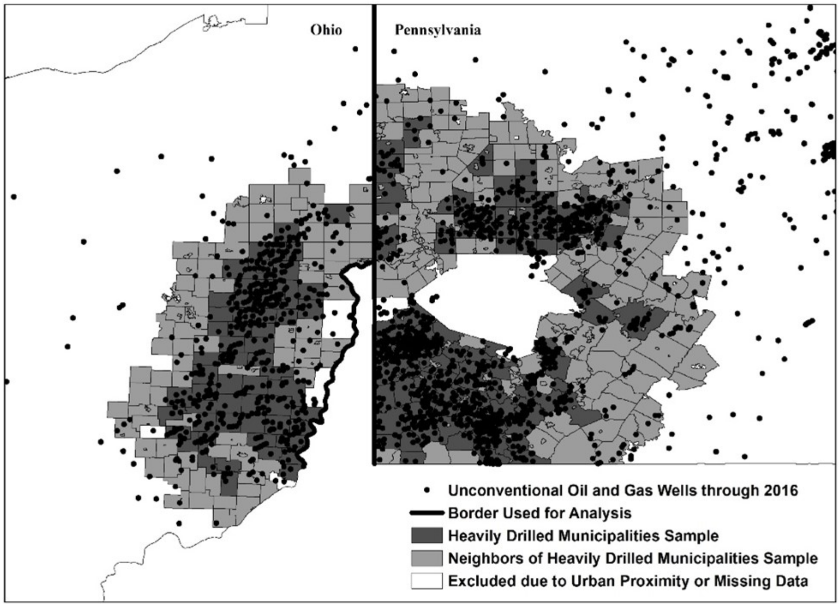
[5] Appendix B.2 details the geospatial data used to select and map the samples, and the steps I took to create geospatial control variables.

municipality in each of the four census years. I classify 165 municipalities with greater than the median drilling density of .25 wells per square mile as “heavily drilled.” Well density captures where widespread drilling and production has occurred, rather than where operators drilled a few scattered exploratory wells. I use first and second order neighbors of the heavily drilled municipalities as controls. My sample of local jurisdictions contains 618 municipalities which are displayed in Figure 2.2.

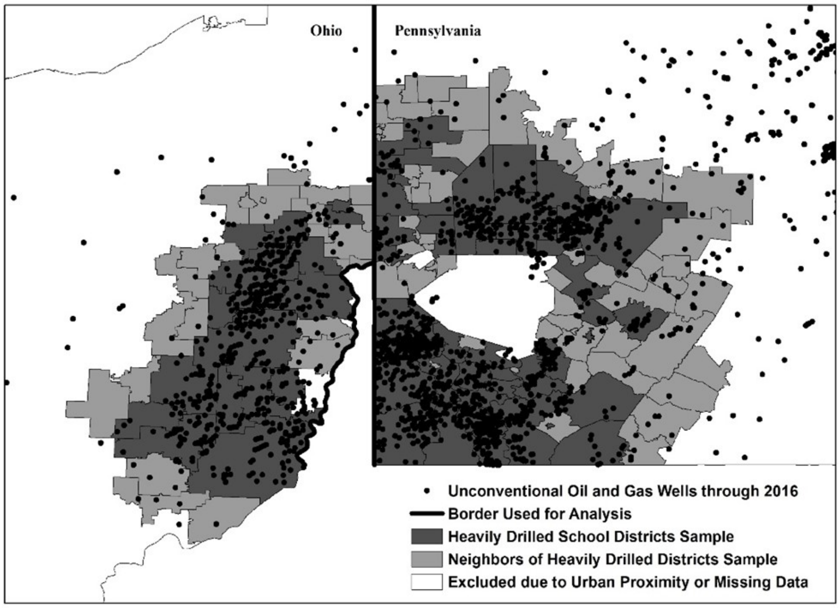
To maintain a consistent geographic scope, I capture a sample of school districts that overlap the municipalities. I begin with data on 1,717 districts in Pennsylvania and Ohio, and drop community colleges, vocational schools, non-operating schools, and educational services agencies to retain 1,426 elementary and combined elementary and secondary districts. I select only the districts that contain sample municipalities, but exclude six school districts associated with principal cities and one district with missing census data. The final sample includes 141 school districts, with 63 that contain heavily drilled municipalities. To obtain wells counts for each census year, I use geospatial software to match each well record to a school district based on their geographic coordinates. Districts included in the sample are displayed in Figure 2.2.

Table 2.1 and 2.2 display means in 2011 for financial variables and well counts for heavily drilled and control jurisdictions. Comparing means across Pennsylvania and Ohio, the tables show that Pennsylvania jurisdictions have higher average levels of all financial variables. But in 2011, prior to widespread drilling in Ohio and before Pennsylvania introduced the Impact Fee, the means are relatively similar within state and across heavily drilled and neighboring jurisdictions. The tables show that Pennsylvania experienced drilling prior to 2011, but that the number of wells drilled between 2011 and 2016 in the typical heavily drilled jurisdiction is similar across states.

Tables 2.1 and 2.2 also include mean millage rates, which are the amount in property tax dollars due for every $1,000 in assessed value. Millage data come from the Ohio Department of Taxation (2020) and the Pennsylvania Department of Community and Economic Development (2020). Because the millage records are incomplete, my data contains millage rates for 120 of the 141 school districts and 572 of the 618 municipalities. Pennsylvania requires jurisdictions to levy a uniform millage rate on commercial and residential property. Because residential property makes up a larger proportion of the tax base in rural areas, I consider Pennsylvania's millage rates alongside Ohio's residential rate throughout the analysis.



(a) Municipalities



(b) School Districts

Figure 2.2: Study Area and Sample

Table 2.1: Pennsylvania and Ohio School Districts by Drilling Status in 2016

	PA Heavily Drilled		PA Neighbors		OH Heavily Drilled		OH Neighbors	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Total Revenue	22,958	15,915	19,714	11,216	12,008	7,219	12,295	5,552
Property Tax Revenue	8,319	8,044	7,393	6,705	3,479	2,069	3,450	1,722
Total Expenditures	23,309	15,635	19,545	11,456	12,420	8,627	12,610	5,621
Current Expenditures	21,559	15,186	18,618	10,861	10,561	4,363	11,785	5,319
Fund Balance	9,336	11,256	8,110	6,849	4,275	7,847	3,594	2,214
Total Outstanding Debt	24,939	22,895	18,911	17,313	1,876	3,368	2,700	3,769
Residential Millage	66	39	59	31	16	6	20	7
Commercial Millage	.	.	.	.	18	8	22	8
Unconv Wells Drilled Prior to 2011	23	30	1	2	0	1	0	0
Unconv Wells Drilled 2011 to 2016	57	71	3	4	62	99	1	2
Share Urban/Suburban	14	27	34	96	2	4	4	16
Distance to Principal City	12	7	15	8	12	9	10	9
Enrollment 2011 Census	2,284	1,614	1,926	1,249	1,424	563	1,552	704
N	46		52		17		26	

Note: All financial variables are for the year 2011 and are in thousands of real (2000) dollars.

Table 2.2: Pennsylvania and Ohio Municipalities by Drilling Status in 2016

	PA Heavily Drilled		PA Neighbors		OH Heavily Drilled		OH Neighbors	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Total Revenue	992	1,108	1,398	2,968	249	293	726	2,233
Property Tax Revenue	208	262	293	617	91	112	141	235
State Transfer Revenue	265	476	184	315	121	158	143	308
Total Expenditures	1,378	1,627	1,686	3,516	351	377	798	2,213
Road Expenditures	375	307	324	644	140	171	131	158
Fund Balance	805	1,474	1,375	3,621	215	459	728	4,699
Total Outstanding Debt	480	1,859	918	3,317	3	9	601	4,342
Residential Millage	7	6	11	10	3	2	3	2
Commercial Millage	.	.	.	.	3	2	3	3
Unconv Wells Drilled Prior to 2011	9	17	0	1	0	0	0	0
Unconv Wells Drilled 2011 to 2016	23	23	0	1	18	18	1	1
Share Urban/Suburban	8	59	11	37	0	2	0	2
Distance to Principal City	16	8	20	9	17	8	18	11
Population 2010 Census	3,521	2,941	3,029	4,766	1,833	2,478	2,167	2,905
N	110		319		55		134	

Note: All financial variables are for the year 2011 and are in thousands of real (2000) dollars.

2.7 Empirical Approach

My empirical approach is in two parts. First, I document the magnitude of revenues that a typical well contributes to the municipality and school district where it is located in each of the two states.[6] Second, I consider how school districts and municipalities use the revenues by estimating the effect of a shale well on expenditures, fund balances, outstanding debt, and local millage rates.

To estimate the effect of an additional well on public finance outcomes (Y_i), I utilize a first difference model that exploits within-state variation in the number of wells that contribute revenues across drilled and undrilled jurisdictions:

$$\triangle Y_i = \delta_0 + \beta_1 \triangle Wells_i + \beta_2(\triangle Wells_i \times PA_i) + \beta_3 PA_i + \triangle \epsilon_i, \tag{2}$$

where i indexes municipalities or school districts, $\triangle$ is the change from 2011 to 2016, and $\triangle Wells_i$ is the number of shale wells drilled between 2011 to 2016 and contribute revenues in 2016. Interacting $\triangle Wells_i$ with a binary variable equal to one if jurisdiction i is in Pennsylvania allows the marginal effect of a contributing well to differ across the two states. Differencing over 2011 to 2016 captures changes in financial outcomes from the introduction of the Impact Fee in Pennsylvania and the start of widespread drilling in Ohio, which both occurred in 2012.[7] I also control for the number of wells drilled prior to 2011 and three other variables: population in 2010 (for municipalities) or enrollment in 2011 (for schools), the

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[6] The magnitude of shale revenues is an empirical question because there is no obvious way to use the tax assessment formula in Ohio or the fee distribution formula in Pennsylvania to project how much a well contributes to the jurisdictions where it is located. In Ohio, revenues depend on the assessed value of production, which varies with gas prices and the productivity of each well. They also depend on local tax rates, which jurisdictions can adjust in response to assessments. In Pennsylvania, fees are distributed to municipalities with wells and without wells, based on relative levels of drilling, population, and highway mileage, which vary over time.

[7] I consider changes in levels of the outcomes, rather than the natural log of the changes. A well causes a linear change in revenues, not a proportional change, because the revenues it generates do not depend on preexisting revenue levels.

share of the jurisdiction's land area in a Census-designated urban area, and distance to a principal city.[8]

The definition of a contributing well in 2016 varies by state. In Pennsylvania, $\triangle Wells_i$ is the number of wells drilled between 2011 and 2015, because wells first contribute revenues the year after they are drilled. In Ohio, $\triangle Wells_i$ is the number of wells drilled between 2011 and 2014, because jurisdictions receive property taxes two years after the end of a production year.

I first estimate equation 2 for property tax and state transfer revenues. With the revenue outcomes, I adjust $\triangle Wells_i$ based on the age of each well in municipality i in 2016. Because the amount of natural gas a well produces is highest in its first year and declines over time, using unadjusted well counts would underestimate revenues contributed by a new well. The age adjustment relies on data from the US Energy Information Administration (2020b) on the amount of natural gas the typical well in Appalachia produces in each year of its life. To create the age-adjusted variable, I count the number of wells in municipality i that are in their first year of contributing revenues in 2016, giving each a value of one. I add to the count of first year wells the number of second year wells, giving each a value of .45, the ratio of production of the typical well in its second year relative to its first, and so on for older wells. More details on the age adjustment are in Appendix B.3. With the adjustment, β_1 is the revenues generated by a typical one-year-old well in Ohio, and $\beta_1 + \beta_2$ has the same interpretation for a well in Pennsylvania.

Next, I estimate equation 2 for municipal road expenditures, the primary expenditure category affected by drilling. With the age-adjusted treatment measure, β_1 gives road expenditures induced by the typical one-year-old well in Ohio, and $\beta_1 + \beta_2$ gives the same for Pennsylvania.

I also consider three stock variables as outcomes: fund balance (the total amount of cash and securities at year end), outstanding debt at year end, and millage rates. With these

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[8] Descriptive statistics for the controls are in Tables 2.1 and 2.2. I control for population and enrollment levels, rather than considering the outcomes in per capita or per student terms because after the great recession many drilled and undrilled jurisdictions experienced declining population but relatively stable financial outcomes. Controlling for pre-treatment population accounts for larger expected changes in more populous jurisdictions, but avoids bias that spurious correlation between drilling intensity and population decline would introduce in a per capita model.

outcomes, $\triangle Wells_i$ is a simple, unadjusted count of wells. An older well could have the same effect on the stock variables as newer wells, or even larger if its effect accumulates year-over-year. For instance, a well drilled in 2013 in Pennsylvania could generate revenues that accumulate for three years in municipal fund balances by 2016. With unadjusted well counts, β_1 is the cumulative effect of a typical well (with the typical age) on savings, debt, and millage over the 2011 to 2016 period in Ohio, and $\beta_1 + \beta_2$ has the same interpretation for a well in Pennsylvania.

The relationship between wells and revenues is direct because compensation policies provide revenues through prescribed channels. Statistical power is lower for the spending, savings, debt, and tax rate outcomes because their relationship with wells is mediated by preexisting fiscal conditions and local demand for public goods. With the non-revenue outcomes, the coefficients in equation 2 reflect how jurisdictions expend the revenues, and also reflect fiscal responses to wells straining public goods or generating revenues by inducing economic activity.

2.7.1 Identification of the Fiscal Effects of Wells

Differencing the outcomes in equation 2 removes unobserved and time-constant characteristics of jurisdictions that may be correlated with financial outcomes and drilling, and the model is unbiased provided that $\triangle Wells_i$ is uncorrelated with $\triangle \epsilon_i$. This is the case if two assumptions hold. First, pre-2011 trends in the outcomes must not be correlated with 2011 to 2016 drilling intensity. Second, 2011 to 2016 drilling intensity must not be correlated with contemporaneous and unrelated shocks to the outcomes. To test whether the first identifying assumption holds, I visually examine the coefficients in a regression of the form

$$Y_{it} = \beta_0 + \beta_1 \triangle Wells_i + \beta_2 (\triangle Wells_i \times PA_i) + \beta_3 PA_i + \epsilon_{it}. \tag{3}$$

The model is identical to equation 2, but outcomes are levels in year t. For each outcome, I estimate the model once for each census year and graphically plot the estimates of β_1 and $\beta_1 + \beta_2$. Note the lack of a t subscript in $\triangle Wells_i$, because the variable is drilling intensity from 2011 to 2016 regardless of the outcome year. The first identifying assumption holds if the relationship between $\triangle Wells_i$ and the financial outcomes is constant over the 2001 to 2011 period, and if trends in the relationship are parallel between Pennsylvania and Ohio.[9]

There is no way to be certain that the second identifying assumption holds. But jurisdictions that are closer geographically should be similar on observed and unobserved characteristics. For instance, relative to the flat and agricultural jurisdictions in western Ohio, eastern Ohio and western Pennsylvania are mountainous and traditionally dependent on coal mining and manufacturing. Jurisdictions with similar geography and industrial composition should experience similar sub-regional shocks. I therefore present the results of equation 2 for samples that are 75, 50, and 25 miles away from the Pennsylvania-Ohio border. As the sample declines with distance we are comparing jurisdictions that are more similar, and differential responses to drilling should be driven only by discontinuity in compensation policies at the border.

2.8 Results

In this section, I show that the parallel trends assumption holds for several financial outcomes of interest. Next, I estimate the magnitude of local compensation from the typical well and explore how it is distributed across jurisdictions in the two states. I also estimate the effect of a well in each state on several financial outcomes, which for school districts include fund balance, outstanding debt, and millage rates, and for municipalities include road expenditures, fund balances, and outstanding debt.

2.8.1 Parallel Trends Analysis

Figure 2.3 plots coefficients from equation 3 for school districts. Panel a shows the relationship between drilling and property tax revenues is relatively constant and parallel between Pennsylvania and Ohio from 2001 to 2011. In 2016, the first census year that Ohio districts

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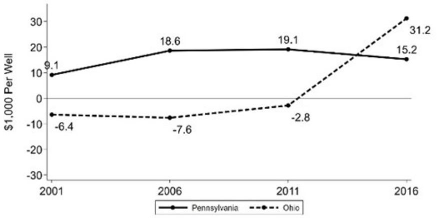
[9] I use a model in the form of equation 3 as my main test of the first assumption, rather than a model with the change $\triangle Y_i$ as the outcome, because it is easier to examine visually. In Appendix B.5 I apply an event study-style model as an additional test for divergent trends in the relationship between $\triangle Wells_i$ and $\triangle Y_i$ within each state.

have wells contributing property tax revenues, we see a spike in the relationship between an additional well and property taxes. The relationship increases from -\$2,800 per well in 2011, to over \$31,000 per well in 2016, suggesting a large effect that the first difference model will estimate more precisely.

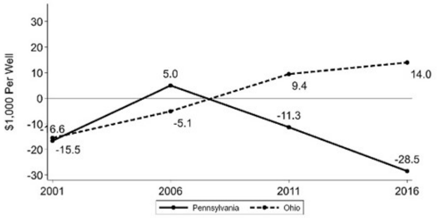
Panel b shows non-parallel trends in revenues other than property taxes. This is driven by governmental or private grants that districts receive intermittently over the study period. To see this, panel c displays constant and parallel trends in other tax revenues, which primarily consist of income taxes. If districts save or borrow in response to grants, the non-parallel trends could bias the relationship between wells and savings, debt, and millage. I probe this concern in panel d, by removing three districts in Pennsylvania and five in Ohio that experience the most extreme changes in non-tax revenues per student due to grants. Qualitative evidence on the specific districts that were removed reveals that the grants are associated with immediate capital expenditures. The erratic pretends are largely resolved when excluding these observations, and without them trends in the remaining panels are not substantively altered.

Panel e shows that school district spending is highly erratic. This is likely because intermittent grants and intermittent projects (i.e., capital upgrades) drive large changes in spending from year to year. Moreover, the samples of heavily drilled districts are small, meaning that large intermittent expenditures by just a few districts can have large effects on average expenditures. To see this, Figures 2.4 and 2.5 look at Ohio districts as an example and place 95 percent confidence intervals around the coefficients. Figure 2.4 uses property tax revenues as the outcome. It shows that the confidence intervals are relatively narrow, and the relationship between wells and drilling intensity is insignificant in all but the treatment year. By comparison, Figure 2.5 uses total expenditures as the outcome and the confidence intervals are much wider (plus or minus \$45,000 in 2011), and the relationship between drilling intensity and expenditures is insignificant in every year. Because of the variability in expenditures, and because there is no direct link between drilling and the need to spend more on any specific expenditure category, I do not estimate the relationship between wells and the school expenditure variable (although I do estimate the relationship for the school district revenue, fund balance, debt, and tax rate variables, which have more stable and parallel trends over the study period).

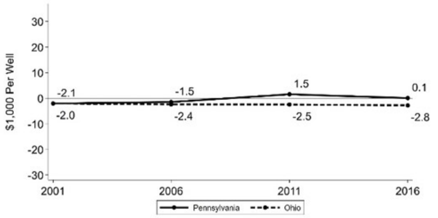
Panel f of Figure 2.3 shows that the relationship between drilling and fund balance is relatively flat for both states until 2016, when we see a spike for Ohio districts that mirrors the spike in property tax revenues. Similarly, the relationship with outstanding debt in panel g is flat and spikes in 2016 for Ohio districts. The relationships remain flat for Pennsylvania. Panels h and i show that the relationships with residential and commercial millage are constant, but in 2016 residential rates increase in Pennsylvania and both rates slightly decrease in Ohio.



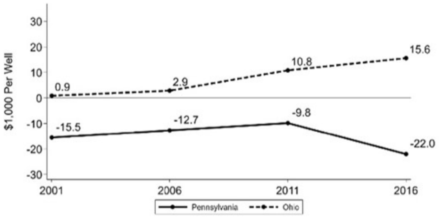
(a) Property Tax Revenue Per Well



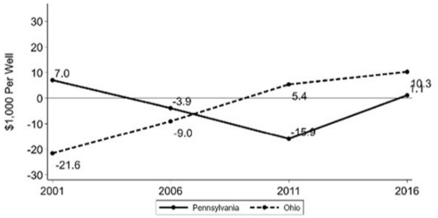
(b) Other Revenue Per Well



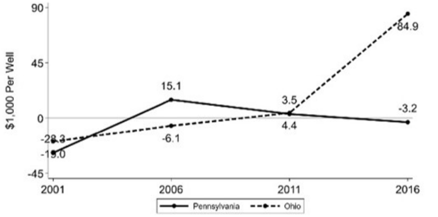
(c) Other Tax Revenue Per Well



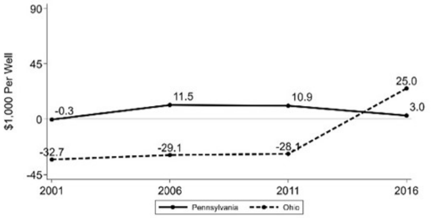
(d) Other Revenue Per Well (Without Outliers)



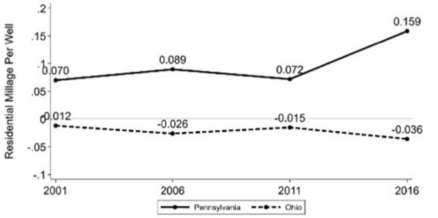
(e) Total Expenditures Per Well



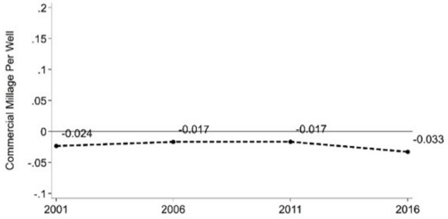
(f) Fund Balance Per Well



(g) Outstanding Debt Per Well



(h) Residential Millage Per Well



(i) Commercial Millage Per Well (Ohio)

Figure 2.3: Drilling Intensity from 2011 to 2016 and School District Finances

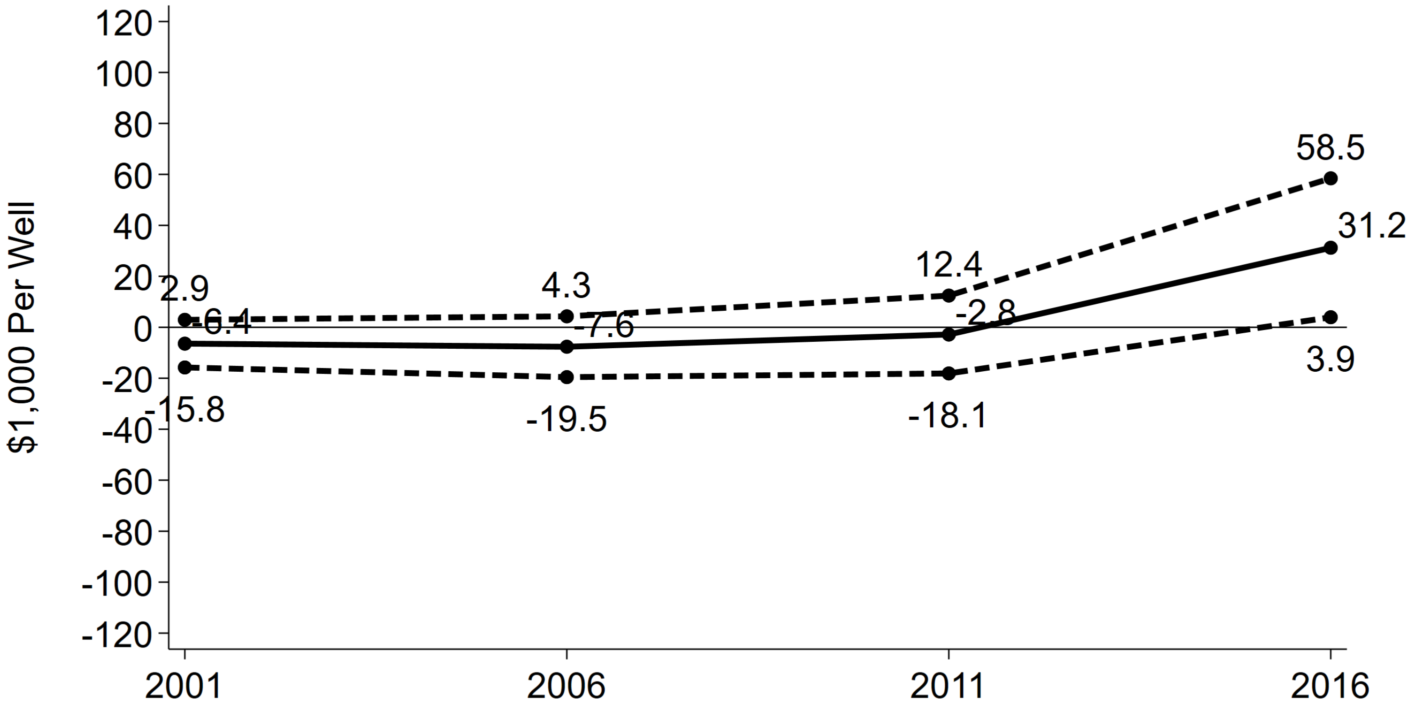


Figure 2.4: Drilling Intensity from 2011 to 2016 and Ohio School Property Tax Revenues

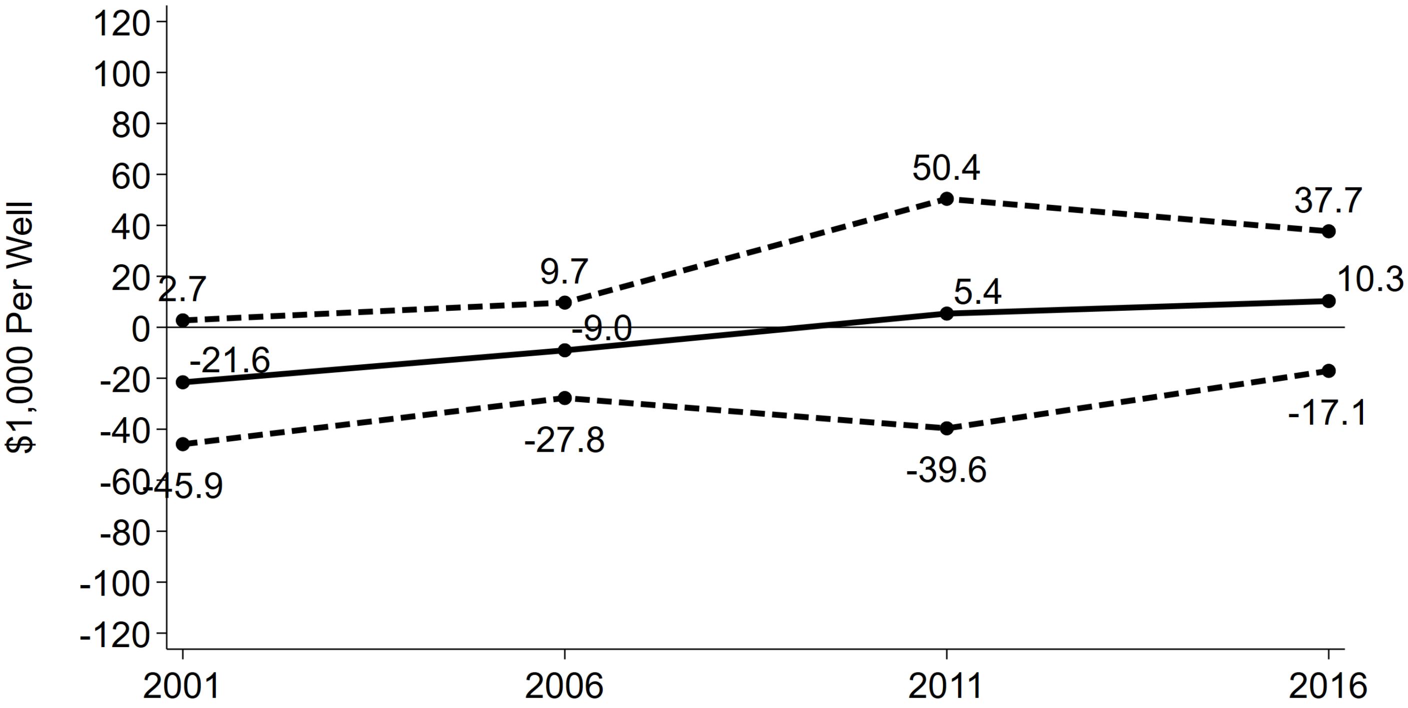
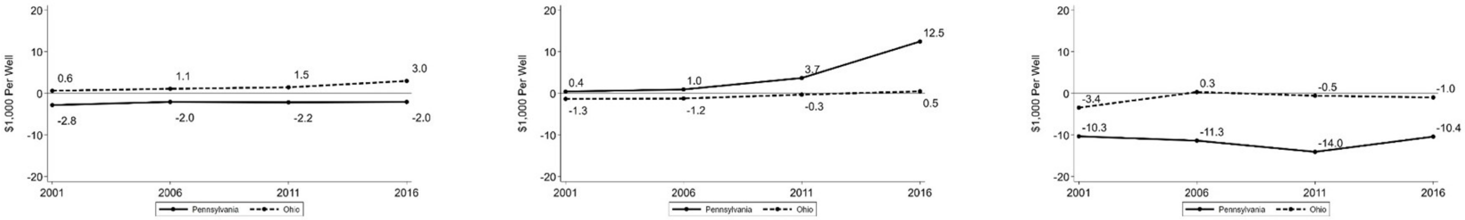


Figure 2.5: Drilling Intensity from 2011 to 2016 and Ohio School Expenditures

Moving to municipalities, panels a and b in Figure 2.6 plot the coefficients for the two channels associated with shale revenues. As with school districts, the relationships are relatively stable until 2016, when we see an increase in property taxes received by Ohio municipalities and a spike in state transfers received by Pennsylvania municipalities (2016 is the first census year that Pennsylvania municipalities receive Impact Fees). In panel c, the relationships with other revenue sources are flat over the entire period, suggesting that shale wells are affecting revenues only through the compensation policies.

Panel d shows that in both states total expenditures per well are growing over the entire period. But growth in expenditures per well between 2006 and 2011 is similar to the growth between 2011 and 2016, suggesting that drilling does little to alter preexisting expenditure trends in aggregate. For just road expenditures, panel e shows that the relationship between wells and expenditures is flat through 2011, but spikes for Pennsylvania in 2016.

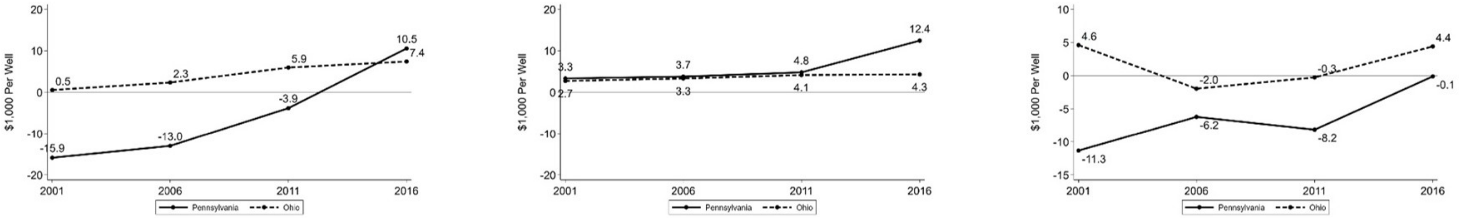
In the remaining panels of Figure 2.6, the relationship between wells and financial outcomes are relatively constant from 2001 to 2011. In panel f, both states experience increases in fund balances per well in 2016, suggesting that municipalities may be saving shale revenues. In panel g, the relationship with outstanding debt remains stable in Ohio, but decreases slightly in Pennsylvania. Both residential and commercial millage rates remain constant over the entire period, which reveals that municipalities in both states are not converting the revenues to private income. That Ohio municipalities do not, on average, respond to the growing oil and gas tax base by raising the commercial property tax rate suggests that they do not require additional money to repair damages from the industry.



(a) Property Tax Revenue Per Well

(b) State Transfer Revenue Per Well

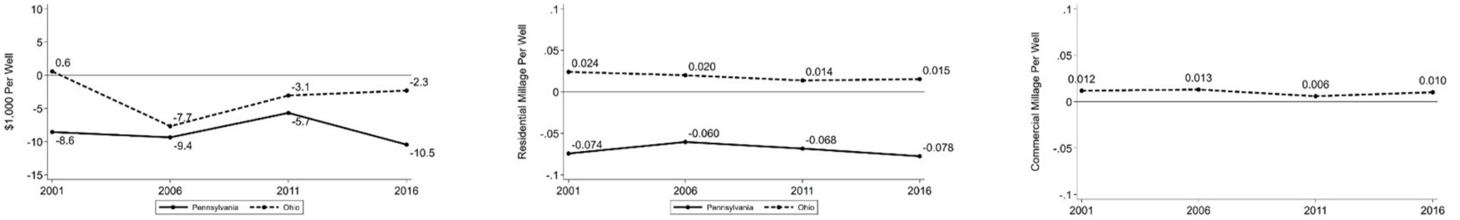
(c) Other Revenue Per Well



(d) Total Expenditures Per Well

(e) Road Expenditures Per Well

(f) Fund Balance Per Well



(g) Outstanding Debt Per Well

(h) Residential Millage Per Well

(i) Commercial Millage Per Well (Ohio)

Figure 2.6: Drilling Intensity from 2011 to 2016 and Municipal Finances

2.8.2 The Magnitude and Distribution of Local Revenues

With parallel trends in property tax revenues and state transfers, it is reasonable to assume that the first difference approach offers unbiased estimates of revenues generated by a one-year-old well. Table 2.3 presents the estimated coefficients from equation 2. In panel a, the coefficient on "Contributing Wells Drilled 2011 to 2016" reveals that the typical school district in Ohio receives around $34,000 from a well in its first year. The coefficient is nearly identical across the three distance samples, suggesting that there is little variation in Ohio's property tax rates within 75 miles of the border. The coefficients on "Contributing Wells X PA" are negative and indicate that shale wells contribute roughly zero dollars in property tax revenues to Pennsylvania districts.

Table 2.3 panel b shows that the typical Ohio municipality receives around $1,500 in property taxes from a one-year-old well. State policy prevents Pennsylvania municipalities from levying property taxes on wells, but panel c shows that the state transfers between $8,000 and $9,300 in Impact Fees to the typical municipality hosting a one-year-old well. The small coefficients on "Contributing Wells Drilled 2011 to 2016" suggest that outside of authorizing property taxes, the state of Ohio provides no additional monetary compensation to municipalities.

Table 2.4 summarizes the local revenues generated by the typical well in its first year. It relies on the coefficient estimates from the sample of municipalities within 25 miles of the border, because these jurisdictions are presumably most similar across states. In Pennsylvania and at the most local level of government, 100 percent of the compensation accrues to municipalities. In Ohio, 20 percent of compensation accrues to municipalities, and 80 percent accrues to school districts. The value for Ohio municipalities includes $1,600 in property taxes from Table 2.3, plus $7,100 in avoided road repairs from Table 2.7 as explained in the following section.

Table 2.4 shows that local compensation is nearly five times larger in Ohio than in Pennsylvania. The differential is partially driven by the property tax system front-loading compensation in a well's early years. Table 2.5 estimates the compensation generated by the typical shale well in its first five years. In Ohio, where property tax revenues depend on annual production, I multiply the first year revenue estimate by typical production decline ratios in years 2 through 5 and sum across the five years. The decline ratios are the same ratios used in the age adjustment and in Appendix Table B4. In Pennsylvania, fees paid by operators to the state decline with a well's age. But the amount the state transfers to a municipality factors in the number of wells in its borders, not the age of its wells. I therefore assume that a well generates $9,300 for its host municipality in each of its first five years. Table 2.5 shows that over five years, estimated local compensation is $46,500 per well in Pennsylvania and $65,400 per well in Ohio.

Table 2.3: Unconventional Wells and Local Public Revenues

(a) Change in School District Property Tax Revenues, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	34.0***	34.0***	33.3***
	(6.7)	(6.7)	(7.1)
Contributing Wells X PA	-37.9***	-39.4***	-38.9***
	(9.3)	(9.6)	(10.4)
PA	-127.1	-59.7	-262.2
	(156.5)	(166.7)	(283.9)
Constant	-240.1	-255.2	-349.3
	(266.7)	(282.8)	(385.8)
R-squared	0.59	0.59	0.68
N	141	126	84

(b) Change in Municipal Property Tax Revenues, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	1.5*	1.5*	1.6*
	(0.7)	(0.7)	(0.8)
Contributing Wells X PA	-1.4	-1.4	-1.8*
	(0.8)	(0.8)	(0.9)
PA	-11.2	-11.4	-13.3
	(6.2)	(6.5)	(9.4)
Constant	16.3	16.3	3.4
	(10.2)	(11.3)	(21.7)
R-squared	0.03	0.04	0.11
N	618	546	317

(c) Change in Municipal State Transfers, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	0.8	0.7	0.0
	(1.0)	(1.0)	(1.2)
Contributing Wells X PA	8.0***	8.2***	9.3***
	(1.9)	(2.0)	(1.9)
PA	58.2*	56.6	55.4
	(27.9)	(30.2)	(30.8)
Constant	-41.8	-28.8	-45.8
	(27.0)	(28.4)	(48.2)
R-squared	0.05	0.05	0.21
N	618	546	317

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. Outcomes are in thousands of dollars.

Table 2.4: Estimated Compensation for Hosting a Shale Well in its First Year

	Pennsylvania		Ohio	
	Dollar Estimate	Share	Dollar Estimate	Share
Municipal Government	$ 9,300	100%	$ 8,700	20%
School District	$ -	0%	$ 33,300	80%
Total Local Revenues	**$ 9,300**		**$ 42,000**	

Table 2.5: Estimated Compensation for Hosting a Shale Well in its First Five Years

	Pennsylvania		Ohio	
	Dollar Estimate	Share	Dollar Estimate	Share
Municipal Government	$ 46,500	100%	$ 9,800	15%
School District	$ -	0%	$ 55,600	85%
Total Local Revenues	**$ 46,500**		**$ 65,400**	

2.8.3 Financial Outcomes

Table 2.6 presents the relationship between wells and the school district financial outcomes that showed parallel trends in Section 2.8.1. Panel a shows that Ohio districts are saving shale revenues. A well drilled over the 2011 to 2016 period is associated with an average of $80,000 in savings in 2016. This figure is larger than the five-year revenue estimate in Table 2.5, which suggests that districts may be using shale revenues to leverage additional financing. This appears to be the case in panel b, which shows that outstanding debt increases by an average of $53,000 per well.

Panel c shows that the typical Ohio district reduces residential millage by .02 per well. For a heavily drilled district with the mean of 62 wells, this corresponds to an 8 percent reduction from the mean millage rate of 16 in 2011. Panel d compares drilled and undrilled districts in Ohio and shows that the effect on commercial millage is also about -.02 per well.

Table 2.7 shows that municipal road expenditures remain flat in Ohio. This is evidence that Ohio's road use agreements have shielded municipalities from paying for industry-related road damages. In Pennsylvania, a one-year-old well is associated with around $7,000 in road expenditures for the typical municipality. In other words, Pennsylvania municipalities are spending three-quarters of the revenues from a one-year-old well to repair the road damage it creates.

Beyond the first year, panels b and c of Table 2.7 provide imprecise evidence that both Ohio and Pennsylvania municipalities are saving shale revenues, and that Pennsylvania municipalities are also using them to pay down preexisting debt. Although the coefficients on the interaction terms are not significant in panel b, the linear combinations of "Contributing Wells" and "Contributing Wells X PA" are significant at the 95 percent level and indicate that the typical well in Pennsylvania accumulates between $6,000 and $8,000 in savings over the 2011 to 2016 period. The linear combinations are noisier for the debt outcome. Despite the relatively large standard errors in panel b, the positive relationship between wells and savings is consistent with Weber and Harleman (2015), who show that municipal fund balances in areas with substantial drilling more than doubled two years after introduction of the Impact Fee.

Table 2.6: Unconventional Wells and School District Finances

(a) Change in Fund Balance, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	80.5**	80.9**	79.7**
	(24.4)	(24.4)	(26.5)
Contributing Wells X PA	-87.2**	-85.4**	-93.1**
	(27.4)	(27.8)	(32.4)
PA	-671.8	-925.9	-1522.2
	(1249.4)	(1367.7)	(1968.7)
Constant	1293.4	1610.3	-2520.8
	(2476.5)	(2651.6)	(2474.5)
R-squared	0.19	0.19	0.33
N	141	126	84

(b) Change in Outstanding Debt, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	53.1***	53.3***	53.7***
	(10.6)	(10.7)	(12.6)
Contributing Wells X PA	-61.1**	-62.7***	-73.2**
	(18.2)	(18.4)	(22.1)
PA	-2319.7*	-1960.0	-1273.7
	(984.0)	(1061.0)	(1521.6)
Constant	1679.9	1871.4	1661.1
	(1328.7)	(1381.7)	(1839.5)
R-squared	0.18	0.19	0.21
N	141	126	84

(c) Change in Residential Millage, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.02***	-0.02***	-0.02
	(0.01)	(0.01)	(0.01)
Contributing Wells X PA	0.07*	0.05	0.04
	(0.03)	(0.02)	(0.02)
PA	-4.45	1.70	0.86
	(4.55)	(3.68)	(4.76)
Constant	2.38	0.80	0.76
	(2.57)	(1.62)	(2.33)
R-squared	0.04	0.03	0.08
N	113	101	64

(d) Change in Commercial Millage, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.02***	-0.02***	-0.01
	(0.00)	(0.00)	(0.01)
Constant	1.22	1.22	3.04
	(1.93)	(1.93)	(3.30)
R-squared	0.39	0.39	0.49
N	42	42	30

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust SEs in parentheses. Outcomes in $1,000.

Table 2.7: Unconventional Wells and Municipal Finances

(a) Change in Road Expenditures, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	0.2	0.2	-0.8
	(0.6)	(0.6)	(0.7)
Contributing Wells X PA	7.5***	7.5***	7.1**
	(2.1)	(2.1)	(2.3)
PA	5.6	3.7	19.0
	(20.3)	(21.1)	(21.7)
Constant	-69.5**	-61.8**	-79.7*
	(22.5)	(22.7)	(38.5)
R-squared	0.14	0.17	0.40
N	618	546	317

(b) Change in Fund Balance, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	4.7	4.7	5.6
	(3.3)	(3.4)	(3.1)
Contributing Wells X PA	3.4	2.8	0.6
	(5.5)	(5.6)	(4.2)
PA	-6.5	45.4	354.4**
	(391.0)	(376.0)	(125.2)
Constant	-336.8	-465.8	-977.1*
	(191.2)	(243.6)	(413.2)
R-squared	0.03	0.03	0.29
N	618	546	317

(c) Change in Outstanding Debt, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	0.7	0.8	-0.2
	(2.3)	(2.3)	(2.2)
Contributing Wells X PA	-5.5	-5.3	0.9
	(4.1)	(4.1)	(2.4)
PA	66.9	57.4	-44.7
	(121.2)	(130.7)	(91.5)
Constant	-20.4	-35.6	-263.3
	(132.2)	(146.8)	(319.2)
R-squared	0.01	0.01	0.07
N	618	546	317

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust SEs in parentheses. Outcomes in $1,000.

2.9 Robustness

One concern with my results is that prior drilling in a jurisdiction is likely correlated with contemporaneous drilling, and contemporaneous drilling can induce economic activity that immediately generates public revenues or can cause jurisdictions to immediately repair public infrastructure. In Appendix B.1, I additionally control for the difference in wells drilled in 2016 and in 2011. All of the results are similar in magnitude and significance. Similarly, all results remain relatively unchanged when controlling for the number of contributing wells in 2016 in the county where municipality i is located.

Another concern is that the outcomes in equation 2 are changes in levels, which makes the estimated coefficients more sensitive to outliers than if the outcomes were log-transformed. I probe this concern with the following model:

$$\triangle asinh(Y_i) = \delta_0 + \beta_1 Heavy_i + \beta_2 (Heavy_i \times PA_i) + \beta_3 PA_i + \triangle\epsilon_i, \tag{4}$$

where $asinh(Y_i)$ is the inverse hyperbolic sine transformation of outcomes, which is preferable to the natural logarithm because road expenditures and debt are zero for some observations. Here $Heavy_i$ is a binary variable that equals one if the municipality or district was defined as "heavily drilled" during sample selection. Municipalities are heavily drilled if they have greater than or equal to the median drilling density of .25 wells per square mile, and school districts are heavily drilled if they contain heavily drilled municipalities. The model includes the same controls as models 1 and 2, and β_1 represents the average difference in $\triangle asinh(Y_i)$ between heavily drilled and neighboring jurisdictions in Ohio. I interact $Heavy_i$ with the Pennsylvania binary to allow the average difference between heavily drilled and neighboring jurisdictions to differ across states.

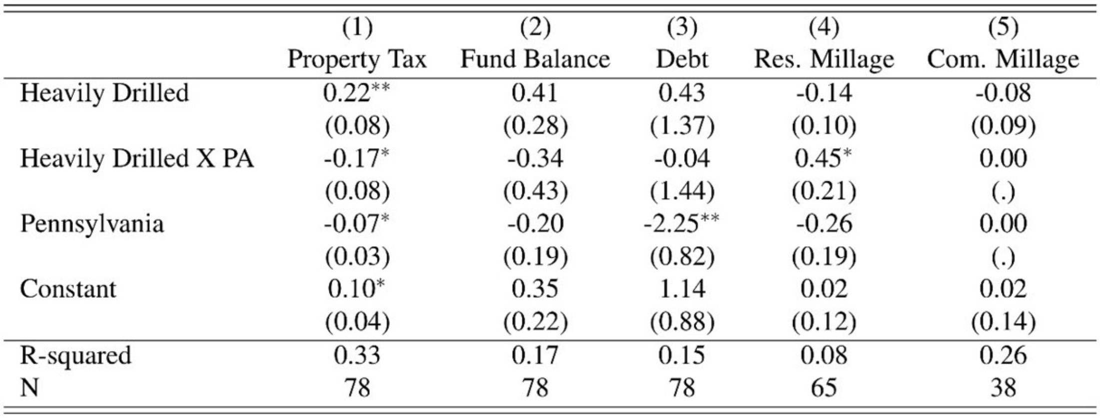
To capture the impact of compensation policies on financial outcomes, rather than differences in drilling intensity, I estimate equation 4 using a matched sample of jurisdictions. To create the matched sample for school districts, I begin with the 43 districts in Ohio. I match 30 of them with a randomly selected district in Pennsylvania with the same number of contributing wells in 2016, and 9 within two contributing wells. I drop the remaining four Ohio districts for which there are no Pennsylvania districts within two wells. I do the same for municipalities, starting with the 189 in Ohio and matching 176 to a municipality in Pennsylvania with the same number of contributing wells and 11 within two wells. The matched samples contain neighboring and heavily drilled jurisdictions that have a nearly identical average number of contributing wells across states. The heavily drilled districts in both states contain on average 30.2 wells, and the heavily drilled municipalities contain on average 18.5 wells.

The results of equation 4 for both municipalities and school districts are in Table 2.8. With the revenue variables as outcomes, the direction and significance of the results are similar to the results from equation 2 presented in Table 2.3. For instance, panel a of Table 2.8 reveals that a heavily drilled school district in Ohio experiences a 22 percent increase in property tax revenues between 2011 and 2016, while a district in Pennsylvania sees virtually no increase.

Results are less precise with road expenditures, savings, debt, and millage rates as outcomes, in part due to the smaller size of the matched sample. But the ratio of "Heavily Drilled" to "Heavily Drilled X PA" is similar to the ratio of "Contributing Wells" to "Contributing Wells X PA" in the preceding results. An exception is with fund balances, where the coefficients still show that both Pennsylvania and Ohio municipalities are saving shale revenues, but the coefficient on "Heavily Drilled X PA" presents imprecise evidence that Pennsylvania municipalities are saving less than in Ohio.

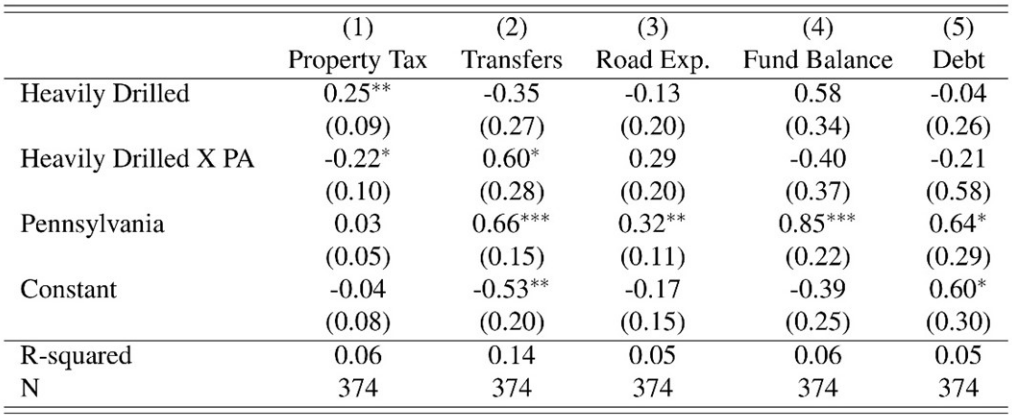
Table 2.8: Robustness: Difference in the Inverse Sine of Financial Outcomes

(a) School Districts



	(1) Property Tax	(2) Fund Balance	(3) Debt	(4) Res. Millage	(5) Com. Millage
Heavily Drilled	0.22^{**}	0.41	0.43	-0.14	-0.08
	(0.08)	(0.28)	(1.37)	(0.10)	(0.09)
Heavily Drilled X PA	-0.17^{*}	-0.34	-0.04	0.45^{*}	0.00
	(0.08)	(0.43)	(1.44)	(0.21)	(.)
Pennsylvania	-0.07^{*}	-0.20	-2.25^{**}	-0.26	0.00
	(0.03)	(0.19)	(0.82)	(0.19)	(.)
Constant	0.10^{*}	0.35	1.14	0.02	0.02
	(0.04)	(0.22)	(0.88)	(0.12)	(0.14)
R-squared	0.33	0.17	0.15	0.08	0.26
N	78	78	78	65	38

(b) Municipalities



	(1) Property Tax	(2) Transfers	(3) Road Exp.	(4) Fund Balance	(5) Debt
Heavily Drilled	0.25^{**}	-0.35	-0.13	0.58	-0.04
	(0.09)	(0.27)	(0.20)	(0.34)	(0.26)
Heavily Drilled X PA	-0.22^{*}	0.60^{*}	0.29	-0.40	-0.21
	(0.10)	(0.28)	(0.20)	(0.37)	(0.58)
Pennsylvania	0.03	0.66^{***}	0.32^{**}	0.85^{***}	0.64^{*}
	(0.05)	(0.15)	(0.11)	(0.22)	(0.29)
Constant	-0.04	-0.53^{**}	-0.17	-0.39	0.60^{*}
	(0.08)	(0.20)	(0.15)	(0.25)	(0.30)
R-squared	0.06	0.14	0.05	0.06	0.05
N	374	374	374	374	374

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. Outcomes are in thousands of dollars.

2.10 Efficient Allocation of Industrial Compensation

The findings highlight that broad use of industrial compensation benefits communities more than allocating them to a narrow set of jurisdictions and uses. This is particularly relevant when many of the disamenities are unrepairable or fall outside of the purview of recipients. This appears to be the case in Pennsylvania, where the typical well generates $9,300 annually for municipalities. With 23 wells and 3,521 residents in the typical heavily drilled municipality (Table 2.2), this means that Impact Fee generates about $240 per household (assuming 4 people per household). This number is smaller than Bartik et al.'s (2019) estimate of $1,400 per household in annual disruptions, suggesting some disamenities would be unaddressed even if municipalities spent all of their shale revenues. But growing fund balances and declining debt suggest that after repairing roads, municipalities see few opportunities to repair more diffuse disamenities or deliver benefits to residents "in kind" through unrelated projects.

Municipal officials in Pennsylvania may be saving to address longer term impacts of drilling. But plugging a well and restoring surrounding land and water is the legal responsibility of the well operator. If the operator dissolves, remediation falls to the state, rather than to local governments. Moreover, Impact Fee savings far outweigh what public finance experts typically recommend to ensure stable provision of public goods. Among heavily drilled municipalities in Pennsylvania, the average fund balance as a share of annual expenditures in 2016 was 86 percent, much greater than the recommended share of 8 to 25 percent (Shelton et al., 1998). Too much savings guarantees residents a below market return on their tax dollars, because municipalities are legally required to invest in low-interest vehicles like US Treasury Bills, certificates of deposit, and savings accounts.

In Ohio, heavily drilled school districts are also saving their shale windfalls. But simultaneous growth in outstanding debt suggests that officials are leveraging a dollar in shale revenues to generate greater than one dollar in capital investments. The school districts' response aligns with the theoretical view that tax bills are more salient to residents than service quality. In normal times, this would lead district officials to keep property taxes low and fund public goods at less than their optimal levels (Krane et al., 2004; Schneider, 1989).

But during a boom, officials can renovate and expand educational infrastructure that was previously under-funded, and also return some of the growing base with tax cuts. Lower average tax rates reduce the deadweight loss of taxation and create an economic benefit that is broadly distributed within affected communities, which highlights the advantage of incorporating new industries into existing tax structures. Many localities do the opposite by granting tax breaks to specific firms (e.g., Stonesifer (2016)). While the tax breaks could convince firms to bring jobs and energize a local economy, such benefits should be weighed against the distributional concern raised by preexisting residents and businesses subsidizing public goods consumed by the newcomer.

2.11 Conclusion

This essay presents evidence that allocating public revenues broadly—across more jurisdictions and uses—may be the best way to compensate residents dealing with noise, traffic, and other industrial disruptions. At first glance, central legislators and citizens might find it appealing to allocate them to jurisdictions that traditionally manage the industry's impacts. But local officials may have more complete information on local spending capacity and residents' demand for public goods (Tiebout, 1956; Oates et al., 1972). In the case of Ohio, the property tax system provides local officials with more discretion over how much money comes in to each overlapping jurisdiction. This provides the most money to school districts, and their use of the revenues reveals local demand for educational goods that are entirely unrelated to drilling.

The findings contribute to the literature on local governance of natural resources, which includes several studies that consider one compensation policy and institutional setting. The studies present mixed evidence on whether compensation improves local economic outcomes (Cust and Poelhekke, 2015). By comparing two policies side-by-side, I show that a more decentralized policy can lead to higher-valued investments. This finding may generalize to other states and nations with strong fiscal accountability. It may generalize less well to settings with rampant corruption, because fewer restrictions may enable officials to use revenues for their own ends.

The findings also contribute to policy debates about the best way to manage local impacts of shale oil and gas development. The shale gas boom has led several states to enact or revise policies to compensate affected communities. In Pennsylvania, there is a debate as to whether approved spending categories should be more closely tied to the impacts of drilling (Pennsylvania Department of the Auditor General, 2016). My findings suggest that this may be misguided if the goal is to offset a loss in well-being from the disamenities of shale development.

Outside of tax revenues, there are other channels that deliver benefits to communities hosting industrial activities. Local governments in several states receive royalties for leasing the rights to develop mineral resources on public lands. Some companies sign agreements with local jurisdictions that directly provide revenues, in kind benefits, and employment guarantees. Comparing the ability of these alternative mechanisms to improve local outcomes with property taxation and intergovernmental transfers represents a promising area for future research.

3.0 Who Bears the Cost of Renewable Power Transmission Lines? Evidence from Housing Values

3.1 Introduction

Many US citizens and government officials support policies to expand renewable electricity generation, for reasons that include decreasing emissions of CO_2 and other harmful pollutants and creating jobs in emerging industries. But a major constraint for private renewable generators is the dearth of high voltage transmission lines (HVTLs) connecting population centers with areas abundant in wind or solar resources. A recent National Renewable Energy Laboratory (2018) study shows that connecting the eastern and western US power grids with cross-national HVTLs will enable the US to generate over 70 percent of its electricity from CO_2-free sources, and that doing so would create net benefits by decreasing electricity prices and improving grid reliability.[1]

Despite benefits that would be shared broadly across electricity consumers and environmentalists, for decades residents and landowners along proposed routes have opposed HVTL construction. Opposition stems from landowners' preferences for views unmarred by steel towers and wires, perceptions that the lines are noisy, fears that electromagnetic fields will harm their health, and concerns that their property values will fall (Cain and Nelson, 2013; Furby et al., 1988). Public opposition has led to long project delays, cancellations, and increased costs for utilities and ultimately electricity consumers. For instance, in 2013 residents of one Southern California town successfully lobbied state regulators to order a utility to remove nineteen already constructed towers and place the lines underground, even though the regulator had previously approved the project and the changes added millions in construction costs (Nelson et al., 2018; Thomas and Welke, 2017). In 2018, New Hampshire regulators blocked lines that would have transported hydroelectricity from Quebec to populated areas

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[1] Even more modest renewable energy goals would require significant investment in new transmission infrastructure. The North American Electricity Reliability Corporation (2015) estimated that approximately 7,000 miles of new transmission lines would be required to cut domestic CO_2 emissions from electricity generation by 32 percent by 2030.

of Massachusetts, finding that the utility had not, among other things, meet its burden "in demonstrating that the Project's impact on property values will not unduly interfere with the orderly development of the region" (New Hampshire Site Evaluation Committee, 2018; Supreme Court of New Hampshire, 2019).

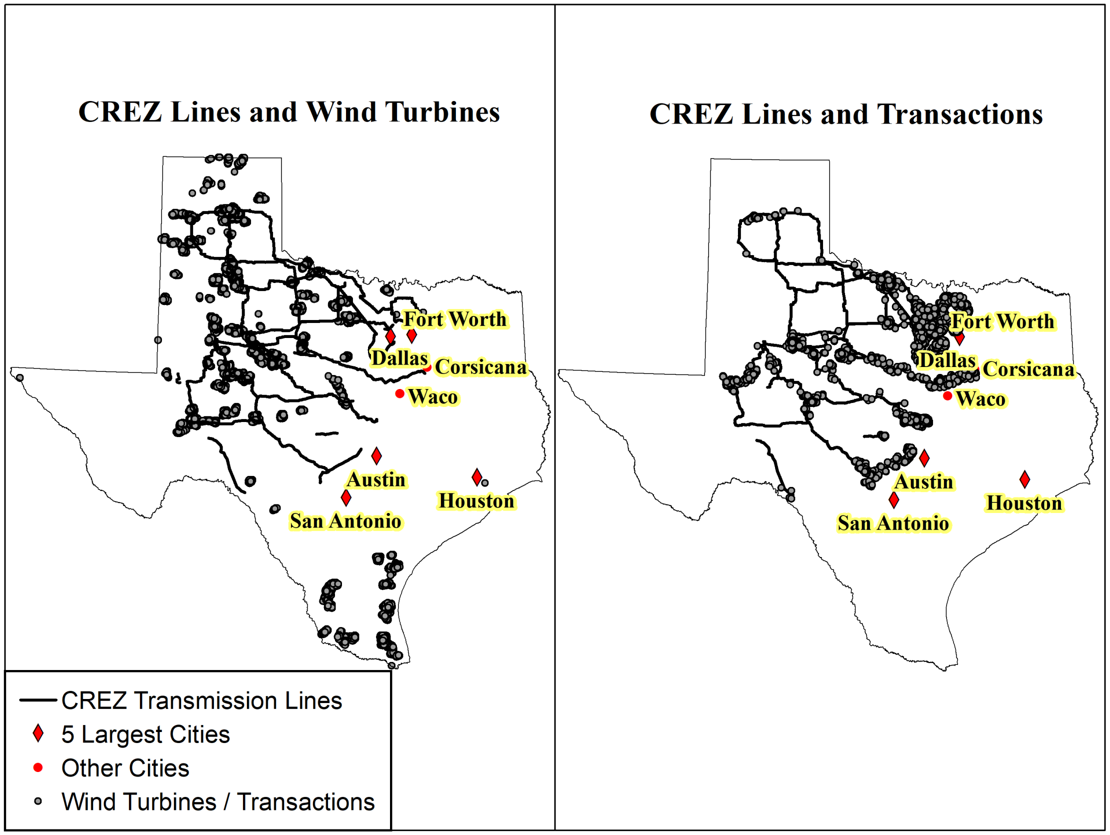
In both the Southern California and New England cases, the proposed lines were driven by state policies to expand renewable electricity production. And in both cases, opponents argued that regulators should halt or require costly modifications in part because the lines would reduce nearby property values. Impacts on property values receive so much attention because they represent a monetary value of the real and perceived damages of new lines—or households' "willingness to pay" to avoid them. Presumably when a prospective buyer makes an offer for a nearby home, she factors in a discount attributable to tainted views, buzzing lines, and constrained use of land near the lines. But the change in a property's value alone may not capture the overall loss or gain for the landowner, because utilities provide owners of land crossed by the lines with one-time payments intended to compensate them at a level equal to the difference in the market price of the land with and without the lines (Furby et al., 1988, p. 70). If policymakers understand the *uncompensated loss* created by HVTL construction—aggregate losses after accounting for compensation—and the types of landowners and communities that bear them, they can utilize this information to develop siting processes and compensation schemes that avoid costly delays, create equitable outcomes, and achieve their goal of expanding renewable electricity generation.

In this paper, I estimate the uncompensated loss created by the Competitive Renewable Energy Zone (CREZ) project in Texas, which was one of the largest HVTL investment projects in US history and was driven by a state policy to expand renewable electricity generation. In 2005, the Texas Legislature directed the Public Utility Commission of Texas (PUCT) to designate "competitive renewable energy zones" in windy West Texas and develop a transmission plan to connect them with heavily populated areas in East Texas (Figure 3.1). PUCT's plan, which has been cited as a national model for expanding renewable generation (Lee and Hurlbut, 2017), led to the construction of over 3,600 circuit miles of HVTLs between 2010 and 2014. The lines allowed Texas to more than double its wind generation capacity and become the largest producer of wind electricity in the US (Behr, 2019; Malewitz, 2013).

I study the impact of CREZ on property values with a difference-in-difference design, which accounts for preexisting differences between the values of properties near the lines and those further away. In doing so, I improve upon many previous studies of HVTLs and property values that use only post construction sales.

Using data on real estate transactions, I find that CREZ reduced the value of the typical residential property crossed by the lines by 22 percent within three years of construction, or by $28,600 for the property with the median value. But properties that receive compensation for hosting the lines make up only 37 percent of affected properties. The remaining 63 percent are nearby, "unencumbered" residential properties within .5 km of the lines, and their values fall by 10 percent on average, or around $13,000 for the property with the median value. I estimate that reductions on unencumbered properties amount to $234 million in uncompensated losses, less than 4 percent of the $6.9 billion it cost to construct CREZ.

The findings are particularly relevant in light of national efforts to modernize the electricity grid, and because several state and federal entities are designing transmission and siting plans to integrate renewables (e.g., US Bureau of Land Management (2012); National Renewable Energy Laboratory (2021); California Energy Commission (2016); Oregon Department of Energy (2021)). Fierce local opposition to transmission projects can be explained in part by the large share of affected property owners that do not receive compensation. But losses for uncompensated households may be small relative to the total cost of a transmission project or the benefits that it creates, meaning that fully compensating them would entail a small cost increase that could have a large impact on local acceptance. Utilities could compensate nearby properties that are in view of the lines based on some share of their taxed assessed value (e.g., 10 percent), or with a fixed amount (e.g., 10 percent of the median property value in the affected municipality). Alternatively, where it is necessary to build broader support—which could be the case where residents value the scenic or historic character of their community—utilities could compensate affected neighborhoods "in kind" by offering long-term electric rate reductions and providing associated goods, such as broadband upgrades. This type of "in kind" compensation package was recently applied in Maine to cultivate public approval for the new route of the Quebec to Massachusetts line (Maine Office of the Governor, 2020; Chesto, 2021).



CREZ Lines and Wind Turbines

CREZ Lines and Transactions

Figure 3.1: CREZ Transmission Lines, Wind Turbines, and Real Estate Transactions

Note: Wind turbine locations are from Hoen and Hunt (2021) and cities are from Texas Deparment of Transportation (2016). The other data sources are discussed in Section 3.5.

3.2 Transmission Lines, Renewable Energy, and Opposition

Areas where the wind blows hardest and the sun shines brightest are often far away from population centers and existing transmission infrastructure. This puts renewable generation facilities at a disadvantage relative to traditional power generation facilities, like coal and natural gas-fired power plants, because developers can factor in transmission access when deciding where to build them. This creates both a financial and legal dilemma for wind and solar developers, which some commentators have referred to as a "chicken and egg problem."

The financial dilemma is that while wind and solar generation projects take only one to three years to build, constructing transmission systems that connect them with population centers or existing lines can take five to ten years (Lee and Hurlbut, 2017). Investors are hesitant to tie up the millions of dollars that it takes to build a wind or solar farm until a line is built (Hurlbut, 2008, 136).[2] The legal dilemma is that in most states, utilities cannot build transmission lines and pass the costs on to electricity consumers without demonstrating a proven need for new service. In Texas, for example, the need for new transmission is not legally justified until the generating facility is built (Smith and Diffen, 2009). In other words, wind and solar developers are unable or unwilling to build generating facilities without transmission lines, and utilities are unable to build transmission lines to remote areas without generating facilities.

While the vast majority of US residents support further development of wind and solar power generation (Pew Research Center, 2020, 2016), for decades residents and landowners have opposed the construction of new transmission lines. Furby et al. (1988) note that individuals oppose the construction of new lines or pay less for properties near existing lines because they dislike the look of them, are concerned about interference with electrical equipment, or worry about accidents involving falling wires or towers. Residents also fear that radiation from transmission lines can cause cancer. Some studies find that exposure to very high levels of magnetic field radiation from transmission lines are associated with

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[2] In the case of Texas, generators can post a bond that covers the cost of a new line to a prospective site. Hurlbut (2008) and Smith and Diffen (2009) note that, although the bond would be returned after the transmission lines came into service, prior to CREZ wind developers were reluctant to tie up millions of dollars over the many years that it takes to acquire land, secure permits, and construct the lines.

increased risk of leukemia in children (Kheifets et al., 2010).

Transmission lines also create a conflict between public and private land use. Landowners may oppose new lines because they can be forced to surrender rights to their property if states grant utilities eminent domain authority. Regulators grant eminent domain to utilities for infrastructure projects that they deem are in service of the public good. If a utility is granted eminent domain, landowners that are unable or unwilling to negotiate agreements with utilities would still be forced to host lines, and would receive a level of compensation deemed "fair" by courts. Utilities gain legal access to private land through an "encumbrance" or "easement" agreement, which carries the right to use land under and very close to lines and towers (this area is known as a "right-of-way"). An easement grants the utility the legal right to use the land for constructing and maintaining HVTLs and comes with restrictions about how the landowner can use it. In particular, HVTL easements generally restrict landowners from building structures, altering ground elevation, and planting tall trees beneath lines or very close to lines.

3.3 The Case of CREZ

In 1999, Texas had 183 megawatts (MW) of installed wind capacity, which if run at the typical 33 percent capacity would have powered roughly 50,000 homes per month (US Deparment of Energy, 2021; US Energy Information Administration, 2019). In 1999, the state legislature enacted a renewable portfolio standard, which required PUCT to implement policies aimed at increasing renewable generating capacity, with biannual targets that scaled up to 2,880 MW in 2009. Wind generators appetite for investing in Texas exceeded the legislature's expectations—the state exceeded the 2003 target in 2001, and in early 2005 was on tract to surpass the 2009 target (Hurlbut, 2008, p. 132).

Almost all of the new wind capacity, around 755 MW, was added in sparsely populated west Texas. But the new wind capacity exceeded transmission capacity of around 400 MW, forcing the grid operator, ERCOT, to frequently curtail wind generation and leading investors to cancel at least one major wind development project (Hurlbut, 2008, p. 136). The Texas

Legislature responded in 2005 with Senate Bill 20, which increased the renewable capacity targets, and directed PUCT to designate “competitive renewable energy zones” in windy West Texas and to develop a transmission plan to connect them with heavily populated areas in east Texas (Texas State Legislature, 2005). The legislature effectively solved the chicken and egg problem by mandating the construction of transmission to areas where wind developers indicated that they would invest, as demonstrated by cash collateral, leases, and prior development expenditures (Smith and Diffen, 2009, p. 210).

PUCT’s initial transmission plan in 2008 was for 2,400 miles of HVTLs at $4.9 billion (Lasher, 2014). Over the next three years, PUCT embraced extensive stakeholder engagement by holding open hearings, which led some lines to be rerouted (Billo, 2017). Ultimately, utilities constructed over 3,600 miles of 345 kilovolt (kV) transmission lines between 2009 and 2014, for a total cost of $6.9 billion which is currently being borne by all electricity consumers within ERCOT. CREZ played a significant role in expanding wind capacity: today it has over 23,000 MW of wind generating capacity (ERCOT, 2020), enough to power about 6.3 million homes per month and roughly three times more than the next leading state (US Deparment of Energy, 2021).

Like other HVTL projects, CREZ faced considerable opposition from local residents during planning. In fact, opposition was so fierce that a key advocate of CREZ stated after its completion: “There were so many times this thing could have crashed and burned that it is almost a miracle it actually happened” and credits its success to the commitment of the Governor of Texas and the efforts of contracted utilities to appease opposing landowners (Trabish, 2015). This was especially true in central and southern Texas, where landowners, expressing concerns about the adulteration of their scenic Hill Country views, successfully lobbied PUCT to modify routes and even forgo some lines in favor of upgrading existing infrastructure (Galbraith, 2010a,b). But routes were not modified in all communities, and opposition during planning suggests that residents’ concerns may have capitalized in the values of nearby properties upon HVTL construction.

There have been dozens of studies estimating the effects of HVTLs on property values by comparing properties near the lines to properties further away, while controlling for property and vicinity characteristics (i.e., lot size, structure type, neighborhood characteristics).

They have produced inconsistent findings. Some find null effects on nearby but unencumbered properties (Chalmers and Voorvaart, 2009; Rigdon, 1991; Wolverton and Bottemiller, 2003). Others find small negative effects of 2 to 10 percent of a property's value, with proximity effects largely diminishing beyond 100 or 200 meters, and easements having a greater negative effect than proximity alone (Bottemiller and Wolverton, 2013; Colwell and Foley, 1979; Hamilton and Schwann, 1995; Tatos and Glick, 2016). Others find larger negative effects of over 20 percent, with the greatest effects for residential properties with clear views of lines or towers from the front or rear of the home (Wyman and Mothorpe, 2018; Sims and Dent, 2013). Still others find positive effects on some properties, presumably from increased transportation access and privacy created by the right-of-way (Des Rosiers, 2002; Jackson et al., 2012).

I build on these studies in two important ways. First, all but one study uses only post-construction sales, leaving open the possibility that unobserved differences between nearby properties and those further away are driving the estimated effects of HVTLs on property values. Thomas and Welke (2017) leverage transactions that occurred before and after construction of HVTL towers on a four-mile right-of-way in California, and utilize a difference-in-difference design to account for preexisting differences in nearby properties and control properties. They find that an easement caused an 8.3 percent decrease in the value of the typical property, and being within 100 meters of the right-of-way diminished values by around 3 percent on average. Importantly, Thomas and Welke study a case where towers were constructed, but successful opposition meant that the towers never received cables, and they were removed after two years. I build upon their study by using a difference-in-difference design to examine the effect of HVTLs that were completed and electrified.

Second, I aggregate diminished property values over one of the largest transmission projects in history to estimate total uncompensated losses. Because of its size, and because the vast majority of properties in Texas are privately owned, CREZ runs through and near thousands of unique properties. The size of CREZ and the coverage of my real estate data allows me to leverage over 4,000 transactions in my preferred specification, and over 30,000 in my most expansive specification. CREZ traverses 3,600 miles of diverse terrain and property types, meaning that it runs through communities that are high and low income, rural and urban, hilly and flat. This means that my estimate of the impact of CREZ on the typical property accounts for a variety of local conditions, making it more generalizable to other transmission projects.[3] In fact, the large west to east transmission project proposed by NREL has been referred to as "a nationwide version of CREZ" (Osborne, 2019).

There are three limitations that may inhibit the generalizability of the losses created by CREZ to other cases. First, over 90 percent of the CREZ lines are 345 kV, which PUCT selected because they integrated well with Texas' preexisting grid. The 345 kV lines typically have 150 foot right-of-ways, and have towers that are around 120 feet tall. I expect my results to generalize best to other 345 kV or similar sized projects.[4] Second, most of the CREZ lines run through areas with sparse vegetation. Bottemiller and Wolverton (2013) study a HVTL project in Oregon and Washington and note that large trees conceal the lines from many nearby properties, mitigating negative property value effects. Third, PUCT solicited citizen input during the planning process, meaning that some residents that would have experienced the largest reductions may have successfully lobbied to have the lines rerouted. To the extent that this occurred, my estimates represent a lower bound of the negative effect that would occur in a less accommodating siting regime.

3.4 Uncompensated Loss

Many of the benefits from building transmission lines to support renewable electricity generation, such as reduced CO_2 emissions or reduced electricity prices, would be shared broadly

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[3] Past studies show that lines may have greater negative effects on very expensive properties (Bottemiller and Wolverton, 2013), may have different effects on vacant and agricultural parcels relative to residential parcels (Wyman and Mothorpe, 2018; Colwell and Sanders, 2017), and may have smaller effects where they are less visible, such as in hilly or wooded areas (Bottemiller and Wolverton, 2013; Sims and Dent, 2013; Wyman and Mothorpe, 2018).

[4] Whether higher voltage or lower voltage lines would have a greater negative effect is somewhat ambiguous. Lower voltage lines have smaller right-of-ways, and therefore can be built closer to homes. Higher voltage lines, like 500 kV lines or 400 kV HVDC lines have wider right-of-ways (160 to 200 feet), but are taller (up to 200 feet), which means they may be more of an eyesore.

by state residents.[5] But many of the costs are heavily concentrated on households near the lines. One way to measure the costs is to use property values to infer the typical household's willingness to pay to avoid the lines, and then add up this value for all affected households. Aggregate willingness to pay minus compensation received by landowners is the uncompensated loss created by the project. Uncompensated loss can be thought of as falling into four categories, three of which I consider in this study.

First, nearby properties that are not crossed by the lines may be affected by aesthetic disamenities and risk perceptions that cause their market values to decrease. Any decrease in value for nearby but unencumbered properties represents uncompensated loss, because their owners do not receive compensation from utilities.

Second, there may be insufficient compensation for encumbered landowners—those with easements, towers, or lines directly on their properties. If an encumbered landowner receives an amount at least equal to the reduction of her property's value, then she is at least as well off as before the lines were constructed.[6] But compensation may be less than the reduction if the means of determining fair compensation is flawed. Courts typically offer compensation using a "comparable sales approach," which relies on market transactions deemed comparable by professional appraisers. Utilities begin negotiations with numbers derived from a similar approach. But whether comparable sales provide a valid measure of a targeted property's value is uncertain. Land could be targeted precisely because it is suitable to support lines (e.g., if it is flat or has suitable soil) which could also make it more attractive for higher-value uses than the comparables. If this were the case, the landowner may not receive compensation that fully offsets the reduction in her property's value that is

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[5] Whether increasing renewable generation lowers electricity prices is up for debate. While Tsai and Eryilmaz (2018) find that additional wind generation suppressed ERCOT *wholesale* electricity prices in the three years after CREZ was completed, Greenstone and Nath (2019) find that states' renewable portfolio standards lead to increased *retail* electricity prices because consumers pay for new transmission infrastructure and new dispatchable generating units.

[6] This assumes that preferences are homogenous across society, so that the current owner values the encumbered land at the same amount as its market value, which can be defined as "the most probable price which a property would bring in a competitive and open market under the conditions of a fair sale, without the price being affected by undue stimulus" (Sanders, 2018). But when utilities possess eminent domain authority, the current owner is forced to accept the easement either through a negotiated settlement or court proceedings. If preferences are not homogenous, and the owner experiences above-market benefits from the land that are not valued by a hypothetical marginal buyer, as with inherited or ancestral lands, then even compensation that is greater than the property value loss caused by the lines may ultimately leave them worse off.

attributable to the obsolescence of future uses.

Third, there may be additional losses for residents of the surrounding jurisdiction if tax assessors reduce the assessed value of encumbered or nearby properties. Reduced assessed values would translate into reduced property tax bases, meaning that counties, municipalities, and school districts would either have to reduce public goods or increase tax rates.[7] Beyond losses experienced by nearby landowners, residents of the relevant jurisdiction may experience losses from diminished recreational or non-use value of public land. In the aforementioned case of transporting power from Quebec to Massachusetts, an alternative line is now being considered in Maine. To appease citizens and environmental groups (who are in part concerned because the line will cross the Kennebec River Gorge, a pristine public asset used for outdoor recreation), the utility offered a compensation package valued at over $170 million, mostly consisting of electricity rate reductions for Maine residents (Chesto, 2019; Maine Office of the Governor, 2020).

Fourth, outside of short-run property value impacts, both encumbered and unencumbered landowners may experience additional loses in the medium and long-run if presence of lines changes the highest-valued use of nearby properties. For instance, a line may discourage residential development, and instead encourage the siting of a landfill or industrial storage site. If new land use patterns further depresses property values in the general vicinity, these spillover effects represent uncompensated losses that may not be captured in short-run estimates of HVTLs' effects on property values. Moreover, even with compensation that fully captures the true opportunity cost of building HVTLs in the short-run, in the long-run preferences and opportunities may change to reveal even more highly-valuable uses for the encumbered land. To the extent that HVTLs limit these long-run best uses, compensation in the short-run may not fully cover property value losses that are measured in the long-run. Given the relatively short study period in this paper, I do not directly estimate uncompensated loss that HVTLs cause by modifying long-run investment trends, which represents an

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[7] Whether construction of the lines results in a net increase or decrease in the local property tax base depends on the assessed value and tax rate applied to transmission infrastructure and wind turbines. In Texas, most transmission infrastructure is taxable as property (Association of Electric Companies of Texas, 2014). Wind generation facilities are also taxable as property, but Texas law allows local taxing authorities to reduce the assessed value or tax rate on wind farms and other capital-intensive properties (Griffiths and Baldick, 2021, p. 51).

opportunity for future inquiry.

3.5 Data and Sample

I compile a dataset of real estate transactions and classify them based on their distance to transmission lines that were constructed as part of the CREZ initiative. Data on the locations of CREZ lines come from three sources. The Texas Parks and Wildlife Department (2014) provides GIS shapefiles of PUCT approved routes. The US Department of Homeland Security (2014) Homeland Infrastructure Foundation-Level Data (HIFLD) provides shapefiles of all transmission line locations (not just the CREZ lines), with line locations in Texas validated through 2017. Some of the planned routes in the Wildlife Department data do not reflect where the lines were eventually built, so I remove the unbuilt segments and replace them with the actual segments as identified in the HIFLD data.[8] The Wildlife Department data also does not capture all of the CREZ segments, so I use maps in the final oversight report that the engineering consultancy RS&H (2014) compiled for PUCT to select the remaining segments from the HIFLD data. The PDF report contains the most detailed maps of CREZ that are publicly available, and also provides the exact construction start and end dates for each segment.[9]

Data on real estate transactions come from the Zillow Transaction and Assessment Dataset (ZTRAX), which contains property locations, transaction prices and dates, and various property and building characteristics.[10] There are 50,308 transactions of residential or agricultural properties that occurred within 5 years of construction of the nearest CREZ segment, and are within 10 km of that segment (the furthest distance that I use in the pro-

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[8] In my data, the CREZ lines are represented as 135 line segments with endpoints that connect with electrical substations or other segments. The average segment length is 54 miles, and the maximum length is 197 miles. There are 87 segments with at least one transaction over my study period.

[9] I corresponded with PUCT, which indicated that they do not provide coordinate information for utility infrastructure for security reasons. I also corresponded with ERCOT, which also indicated that they do not share transmission line maps publicly.

[10] I obtained access to the Zillow Transaction and Assessment Dataset (ZTRAX) as an authorized user listed in a data use agreement signed by my department. ZTRAX is a compilation of real estate transaction and property tax assessment data collected and disclosed by local governments. More information on the data can be found at http://www.zillow.com/ztrax

ceeding analysis to define the real estate market). I drop 641 properties that sold more than four times over the period, and drop 3,795 transactions with a price of less than \$10,000 to include only arms-length transactions.[11] I also drop 16 transactions with a price of more than \$10 million, to limit the effect of outliers.

My full sample contains 45,856 transactions within 10 km of the lines: 43,985 residential properties with at least one building, and 1,871 agricultural or vacant properties. I focus the vast majority of my analysis on the residential transactions because the sample of agricultural transactions is small, and because I only observe fourteen transactions of agricultural properties that the lines intersect (and only 8 in the post-period), which are the agricultural properties most likely affected because of constricted land use. The CREZ routes and my sample of residential transactions are mapped in the right side of Figure 3.1. Notice that some segments have few or no transactions around them. This is because Texas is a "non-disclosure state," which means some counties are not legally required to place real estate transaction data in the public records that Zillow uses to compile ZTRAX.

I use transactions that occurred within a maximum of 5 years (before or after) the conclusion of construction of the nearest CREZ segment, which cover the years 2004 to 2018. Table 3.1 displays the number of observations by year in my treatment group (transactions 0 to .5 km from the lines) and control group (.5 to 2 km), with these distances selected based on the analysis in the next section. I select the period 5 years before and after, rather than a greater number, because sample sizes grow progressively smaller for the later years in the post-period. This is because construction of the segments did not occur at the same time, as depicted in Figure 3.2, and because my data ends in 2019, meaning that some segments do not have transactions beyond 5 years. In several specifications, I further limit the period to 8 years (5 years before and 3 years after) and 6 years (3 years before and after). Studying shorter time windows of 10 years or less minimizes the concern that the hedonic price function may shift over longer time horizons as populations and preferences change, leading to biased estimates of the marginal willingness to pay for avoiding transmissions lines (Kuminoff and Pope, 2014).

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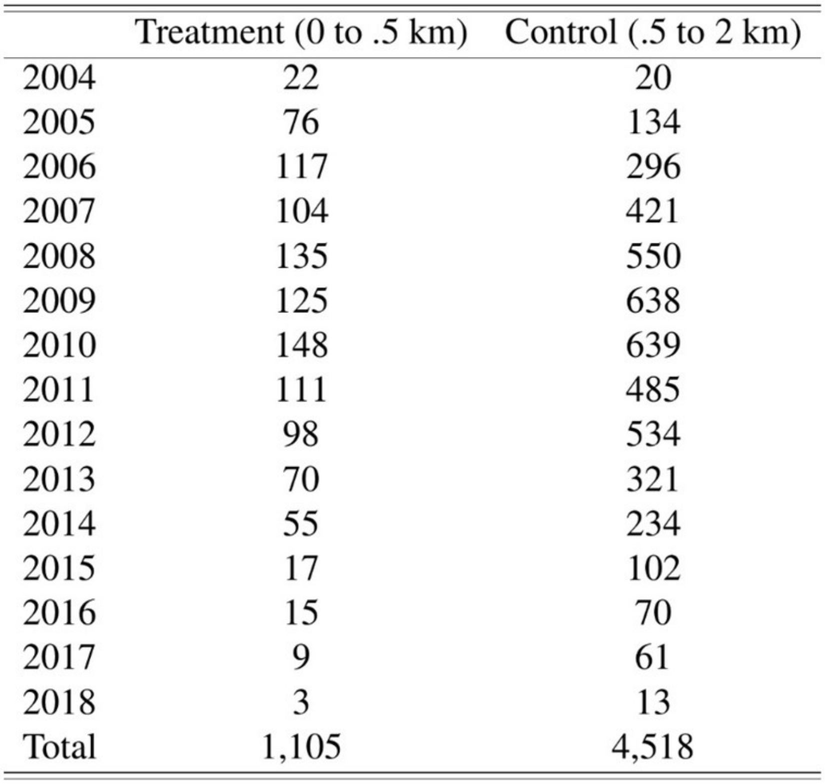
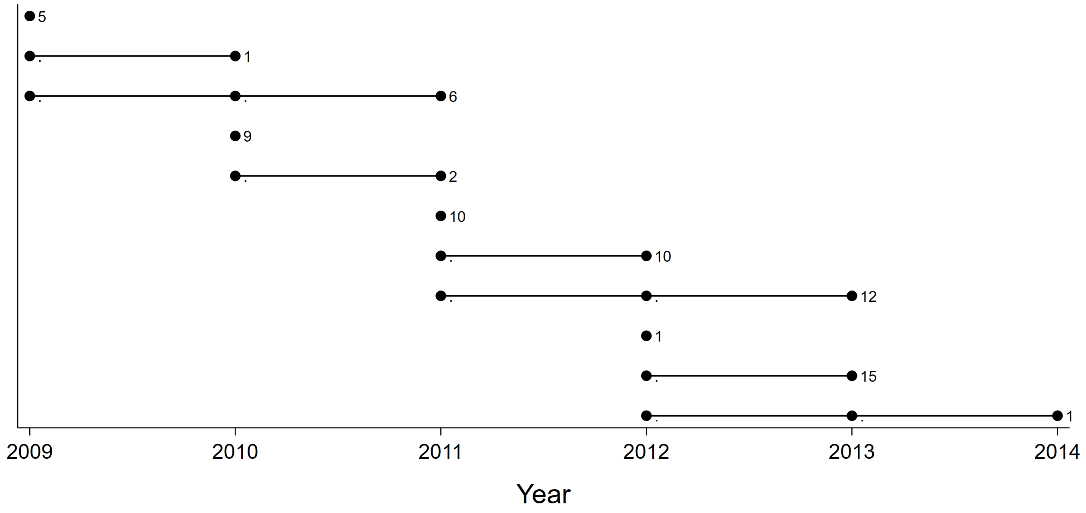
[11] ZTRAX also contains an indicator for inter-family transfers. When compiling the transactions, I exclude verified inter-family transfers, but also drop those below \$10,000 because thousands of records have missing data for this indicator.

Table 3.1: Number of Transactions by Year and Treatment Group



	Treatment (0 to .5 km)	Control (.5 to 2 km)
2004	22	20
2005	76	134
2006	117	296
2007	104	421
2008	135	550
2009	125	638
2010	148	639
2011	111	485
2012	98	534
2013	70	321
2014	55	234
2015	17	102
2016	15	70
2017	9	61
2018	3	13
Total	1,105	4,518

Figure 3.2: Construction Start and End Years for CREZ Segments



Note: The figure displays the construction periods of the 72 CREZ segments that have at least one transaction within 2km. The numbers to the right of each line indicate the number of segments built over each period.

Table 3.2 displays descriptive statistics for the residential transactions across the treatment and control groups.[12] Following Imbens and Wooldridge (2009), the table uses standardized mean differences to assess similarity on six observed control variables. Imbens and Wooldridge note that, as a rule of thumb, regression methods can produce valid estimates of causal effects by adjusting for differences in covariates when their standardized mean differences are below one-quarter, as they are for all of my observed covariates. The first two variables, which I refer to as property characteristics throughout, are lot size in acres and distance to the nearest national highway in meters.[13] Properties closer to the lines are larger and further from highways on average, but the standardized differences are less than one-tenth of a standard deviation. The standardized (and practical) differences are also small

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[12] Much of the analysis below relies on a slightly smaller samples than the total number of observations listed on the final line of Table 3.2, because some of the building characteristics are missing in the ZTRAX records. There are 4,949 transactions (933 in the treatment group and 4,016 in the control group) with complete data for all four building characteristics.

[13] Data on the locations of national highways (major roadways deemed important to the nation's economy, defense, and mobility) are available from Texas Deparment of Transportation (2020).

across four building characteristics—building area in square feet, building age, number of bathrooms, and number of bedrooms. Table 3.2 also shows that for transactions prior to the conclusion of construction of the nearest segment, properties in both the treatment and control groups sold for \$155k on average.

I use one additional data source to identify encumbered properties—those made up of one or more parcels crossed by the lines—which I use to count the number of properties affected by CREZ lines in Section 3.8. Shapefiles on land parcels come from Texas Natural Resources Information System (2019) (TNRIS), which collects the data from appraisal districts and their third-party vendors.

Table 3.2: Summary Statistics

	Mean (0 to .5 km)	SD	Mean (.5 to 2 km)	SD	Std Mean Diff
Acres	1.01	7.16	0.73	3.83	-0.05
Distance to Highway Meters	2,559.17	3,415.94	2,315.45	3,528.13	-0.07
Building Area Square Feet	1,979.28	847.25	2,014.63	902.35	0.04
Number of Bathrooms	1.46	1.01	1.59	1.01	0.13
Number of Bedrooms	2.45	1.65	2.58	1.61	0.08
Building Age	22.47	12.41	22.10	15.19	-0.03
Transaction Price (Pre-Period)	155,018.36	91,401.98	155,625.08	123,513.27	0.01
Number of Observations	1,105.00	.	4,518.00	.	.

3.6 Empirical Approach

My empirical approach for estimating the uncompensated loss created by CREZ is in two stages. First, I estimate a hedonic price function by regressing transaction prices on building and property characteristics, including indicators for the property's distance to the transmission lines. This allows me to infer how the lines affect residents based on how far away they live, and estimate the typical households' willingness to pay to avoid living near them. Because willingness to pay provides a discrete, monetized measure of the adverse visual, audible, and land use impacts of the lines, it is a suitable proxy for the losses that nearby households bear. Second, I estimate a different willingness to pay to avoid the lines across encumbered versus nearby properties, and use parcel data to estimate the number of affected properties of each type. These numbers allow me to produce an aggregate estimate of uncompensated loss.

3.6.1 Willingness to Pay

With Rosen (1974) and Freeman (1979), I estimate a hedonic price function to infer residents' willingness to pay to avoid living near the lines. My baseline model is:

$$ln(Price)_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Near_i + \beta_3(Near_i \times Post_t) + \epsilon_{it}, \tag{5}$$

where $ln(Price)_{it}$ is the natural logarithm of the transaction price of property i at time t in real 2018 dollars (the last year in the study period), $Post_t$ is a binary variable that equals one if the property is transacted after the nearest CREZ segment is constructed, and $Near_i$ is a binary variable that equals one if property i is within some distance of where the lines are eventually built (e.g., within 0 to .5 km), with this "treatment distance" selected based on the greatest extent at which I observe the lines affecting property values. In my preferred specification, I include county fixed effects to control for time-invariant and unobserved factors at the county level. I also include year-quarter fixed effects and county-by-year fixed effects to account for trends in transaction prices over time that are unrelated to CREZ. In most models, I control for the building and property characteristics discussed in the preceding section, with lot size and building area as their natural logarithms to allow for a more flexible specification. Because some of the building characteristics are missing for approximately 15 percent of my sample, I vary the controls that I include across models to test the sensitivity of the results.

Because I estimate equation 5 on samples that include control properties within the same housing market but outside of the treatment distance, it is a difference-in-difference model. To provide an intuition for how the difference-in-difference design works, Figure 3.3 displays mean residential transaction prices in each of the years prior to construction of the nearest segment, across properties very close to the lines (within .5 km) and those further away (.5 to 10 km). Because the transmission line locations are not randomly assigned, the figure shows that prior to construction properties in the treatment group sell for more than those further away. The model accounts for these time-constant and unobserved differences (e.g., the greater open space near treated properties that causes them to fetch higher prices), which are differenced away as β_2. It also differences away β_1, which represents changes in transaction prices over time that are not attributable to construction of the lines.[14] The coefficient of interest is β_3, which is the percent difference in the price of nearby properties caused by construction of the lines.

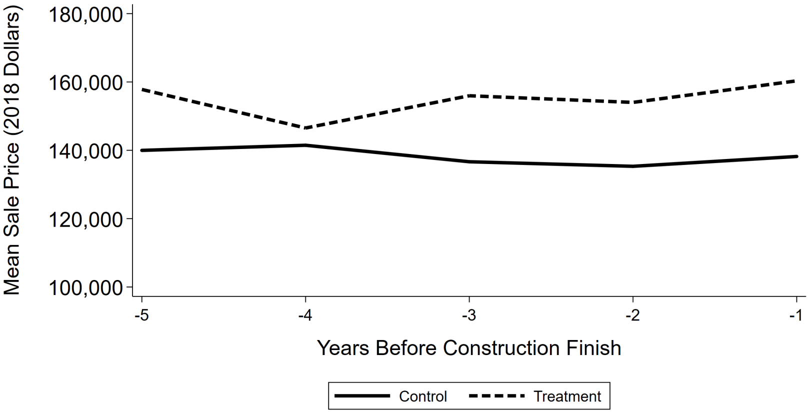

Figure 3.3: Real Estate Price Trends Prior to CREZ Construction

Note: The figure displays mean transaction prices in real 2018 dollars on the vertical axis. The horizontal axis groups transactions based on the time in years that they took place relative to conclusion of construction of the nearest CREZ segment (e.g., -1 means that transaction took place within 365 days before the construction completion date).

The coefficient β_3 is the marginal willingness to pay to avoid living near the lines, under the assumption that households' preferences for environmental quality are homogenous across the housing market, and that treated and control properties would have experienced parallel trends in transaction prices in absence of line construction. Because properties that are closer together are more homogenous, I present results using control samples that are progressively closer to the lines (.5 to 10 km, .5 to 5 km, .5 to 3 km, and .5 to 2 km). Nearer and more

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[14] An even more rigorous way to control for unobserved differences across the treatment and control groups and across time would be to estimate equation 5 using properties that sold at least once before construction and once after construction, and replace the county fixed effects with property fixed effects. Unfortunately, my sample does not permit this approach because there are only 47 of these "repeat sales" within .5 km of the lines and only 123 between .5 and 2 km.

similar properties are also likely to experience similar trends. Figure 3.3 shows a similar (and relatively flat) evolution of average transaction prices across the two groups, which provides initial evidence that the parallel trends assumption holds.

I test for parallel trends more rigorously by estimating an event study-style specification that replaces $Post$ with a set of binary variables τ_{it} that equal one if the transaction occurred in a given year-long period, normalized relative to conclusion of construction of the nearest segment (i.e., between three and two years before construction completion, within one year before, etc.):

$$ln(Price)_{it} = \beta_0 + \sum_{\tau=-5}^{5} \beta_\tau \tau_{it} + \beta_2 Near_i + \sum_{\tau=-5}^{5} \beta_{n\tau}(Near_i \times \tau_{it}) + \epsilon_{it}, \tag{6}$$

with the period between two and one year before as the omitted period. I omit this period, rather than between one and zero before construction completion, because construction of a segment took on average 276 days (median of 198 days) from start to finish. The control observations (those beyond the treatment distance) identify the coefficients β_τ, which give the average difference in $ln(Price)_{it}$ between period τ and the omitted period. In other words, the coefficients give the trend in property values in the vicinity of the lines, but outside the distance that the lines have a negative effect. The coefficients $\beta_{n\tau}$ represent the average difference between near (treated) and far (untreated but in the vicinity) property values in the year-long period τ, relative to the same difference in the omitted period. As with equation 5, I include county fixed effects, year-quarter fixed effects, county-by-year fixed effects, and the building and property characteristic controls.

3.6.2 Affected Properties

As discussed in Section 3.4, transmission lines are likely to have different effects on properties crossed by the lines versus properties where the lines cross through neighboring properties. In one specification, I allow for a different effect on encumbered properties by adding a binary variable equal to one if the property is encumbered, and the interaction of the encumbered binary and $Post_t$. When estimating this model, I replace $Near_i$ with zero for all encumbered properties. The coefficient on $Near_i \times Post_t$ gives the effect on nearby unencumbered properties, while the coefficient on $Encumbered_i \times Post_t$ gives the effect on properties crossed by the lines (due both to their immediate proximity, but also due to lost use and access to the land where lines are placed).

Multiplying the estimated coefficients by the number and value of affected residential properties provides an estimate of the aggregate property value loss. I will use the TNRIS parcel data to estimate the number of properties that are encumbered or within the distance at which I observe the lines to affect property values. Because nearby but unencumbered households do not receive compensation, their willingness to pay to avoid the lines represents entirely uncompensated loss. For encumbered properties, uncompensated loss is their willingness to pay minus the compensation they receive from utilities. Unfortunately, data on compensation is kept confidential, but my estimate of the value reduction on an encumbered property allows me to estimate the "full compensation" that would offset the typical household's willingness to pay to avoid the lines.

3.7 Results

I begin my empirical analysis by determining the maximum distance at which CREZ lines affect property values and further probing the parallel trends assumption of the difference-in-difference design. The solid line in Figure 3.4 depicts the coefficients on $Near_i * Post_t$, from a variation of equation 5 that replaces $Near$ with a set of binary variables that indicate the distance of the transacted property to the nearest segment (0 to .5 km, .5 to 1 km, and so on), and with 8 to 10 km as the omitted group. The figure suggests that properties within .5 km experience a 5 percent decline, an effect that the baseline difference-in-difference model will estimate with more statistical precision. Beyond .5 km, the lines appear to have no detrimental effect on property values. The figure also presents evidence that properties between .5 and 2 km are growing in value relative to properties further away.

To probe why properties between .5 and 2 km are growing, Figure 3.5 replaces the outcome with lot size and shows that properties within 2 km are larger than those further away. This is likely because neighborhoods near the lines are less heavily developed and contain more open space, which utility companies would look for when choosing where to place the lines.[15] Together, Figures 3.4 and 3.5 suggest that prices of properties in more sparsely developed neighborhoods (within 2 km of the lines) are growing faster than those further away. This finding supports using properties from .5 to 2 km as the control group, because they are most likely to reflect trends in property values that would have occurred for the treated properties had the lines not been built.

Using transactions within .5 km as those “near” the lines and properties from .5 to 2 km as the control group, panel a of Figure 3.6 further probes the parallel trends assumption with the event study specification in the form of equation 6. The figure shows a relatively flat trend of coefficients prior to construction, indicating that properties within .5 km did not experience different price trends than properties from .5 to 2 km. After construction, the lines appear to reduce property values by up to 13 percent for at least four years, but the results are statistically imprecise. The imprecision is because I split the treatment observations into small year-long cells, which grow progressively smaller over time (see Appendix Table C1).

Because of the large unexplained variance beyond year 4, Figure 3.4 and the proceeding tables are run on a sample of transactions that occurred up to three years after construction of the nearest segment ended. The main results are reproduced in Appendix C.1 using the full five year post-period, as well as a 3 year pre-period and 3 year post-period, and the estimates of willingness to pay are similar, albeit less statistically precise.

Panel b of Figure 3.6 uses transactions within .5 km as the treatment observations and transactions from .5 to 10 km as the control group. It shows that the values of properties within .5 km were growing faster in the pre-period than properties further away. This reinforces the selection of properties from .5 to 2 km as the control group: they are more similar in size to the treated properties, and prices for these larger properties experience different price trends than those further away.

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[15] Tatos and Glick (2016) also find that HVTLs are built near greater open space. They conducted field research and examined aerial photographs in Salt Lake County, Utah and found that higher voltage lines are often placed near greenways or walking paths.

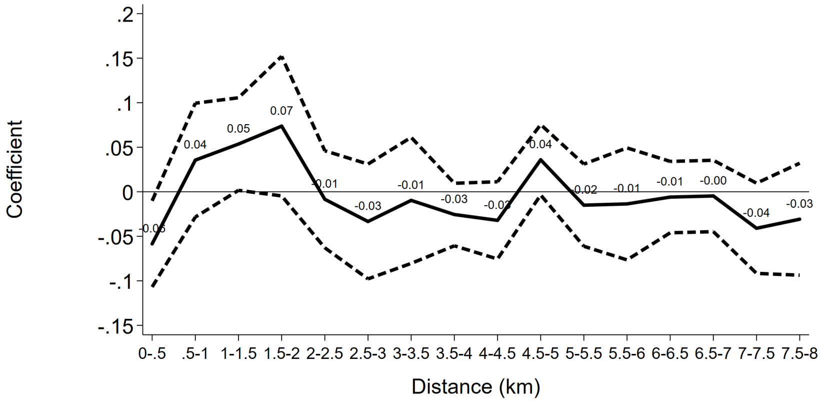


Figure 3.4: The Effect of CREZ Using Alternative Treatment Distances

Note: The solid line depicts the coefficients on the interaction terms $Near_i \times Post_t$, from a regression with the natural log of housing prices as the outcome, where $Near$ is a set of binary variables that indicate the distance of the transacted property to the nearest segment. The distance 8 to 10 km is the omitted group. The dashed lines represent 95 percent confidence intervals, calculated with robust standard errors clustered by county.

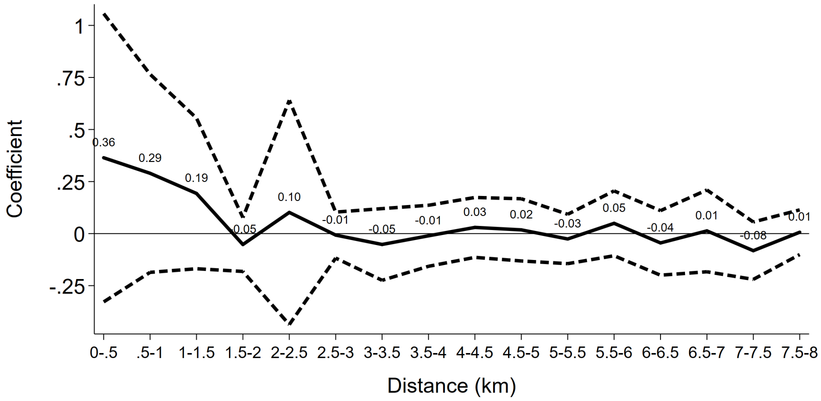
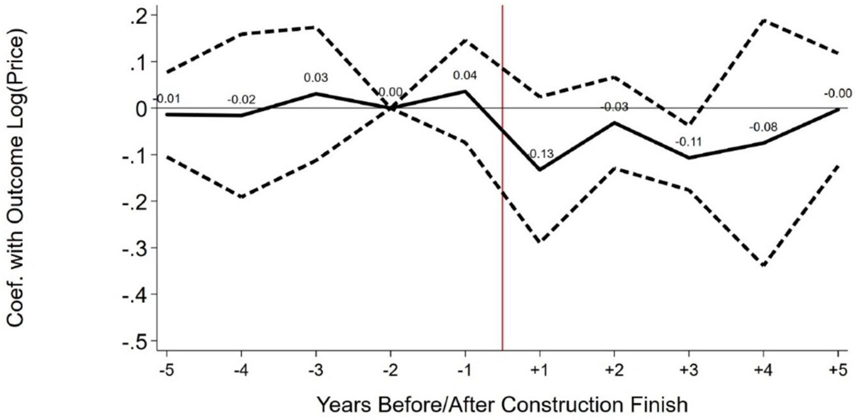
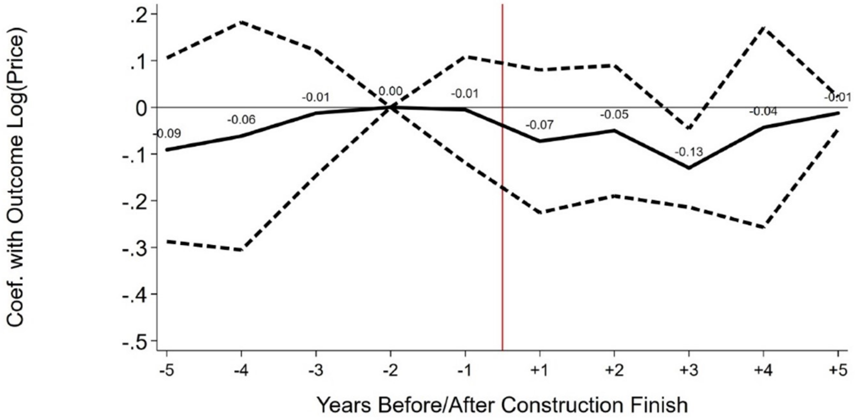


Figure 3.5: Lot Size at Alternative Treatment Distances

Note: The solid line depicts the coefficients on the terms $Near_i$, from a regression with the lot size in acres as the outcome, where $Near$ is a set of binary variables that indicate the distance of the transacted property to the nearest segment. The distance 8 to 10 km is the omitted group. The dashed lines represent 95 percent confidence intervals, calculated with robust standard errors clustered by county.



(a) Control Group .5 to 2 km



(b) Control Group 2 to 10 km

Figure 3.6: Event Study: The Effect of CREZ Within .5 km

Note: The solid line depicts the coefficients on the interaction terms $Near_i * T_{it}$, from a regression with the natural log of housing prices as the outcome. Here T is the time in years when the transaction took place relative to conclusion of construction of the nearest CREZ segment (e.g., -1 means that transaction took place within 365 days before the construction completion date), with two years before (-2) as the omitted period. The dashed lines represent 90 percent confidence intervals, calculated with robust standard errors clustered by county.

3.7.1 Distance to the Lines and Property Values

Table 3.3 presents estimates from the fully-controlled specification of equation 5, using alternate treatment distances (across panels a, b, c, and d), and with alternate control samples (across columns). In panel a, with treated properties as those within .25 km, the lines have a negative effect on property values but the coefficients are statistically insignificant regardless of the control sample. In panel b, with treatment defined as properties within .5 km, the coefficients indicate that the lines decrease the value of the typical property by between 6 and 10 percent. As in Figure 3.4, panels c and d show that beyond .5 km the effect of the lines becomes statistically indistinguishable from zero.

Tables 3.4 and 3.5 present my main results. They follow other papers in the hedonic literature (e.g., Boslett and Hill (2019); Muehlenbachs et al. (2015); Currie et al. (2015)) by defining the treatment threshold as the furthest distance at which a statistically significant effect is found, in this case .5 km.[16] Table 3.4 displays the results of the difference-in-difference model, progressively adding controls across columns. It shows that the estimated effect of the lines is sensitive to the inclusion of the building controls (building area, building age, and number of bathrooms and bedrooms). With the building and property controls, I estimate that the typical household's willingness to pay to avoid the lines is between 9 and 10 percent of a property's value, a result that is robust to the inclusion of year-quarter, county, and county-by-year fixed effects. Notably, the coefficient on "Within .5 km," which represents the difference in price between treated and control properties prior to construction, is positive but smaller in absolute value than the negative coefficient on "Post × Within .5 km". This suggests that the typical treated property had some unobserved amenity prior to construction (e.g., greater open space, which caused the utility company to identify the land as suitable for lines), but that this amenity is removed upon construction of the lines (e.g., the lines now subsume the open space, creating a visual disamenity).

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[16] This threshold is on the higher end of those used in other studies of HVTLs and property values. Most of the studies find that the effects of lines diminish beyond 100 or 200 meters. The larger threshold may be due to the relatively flat and sparsely wooded nature of the Texas landscape. Some studies find that lines may have smaller effects where they are less visible, such as in hilly or wooded areas (Bottemiller and Wolverton, 2013; Sims and Dent, 2013; Wyman and Mothorpe, 2018).

Table 3.3: The Effect CREZ Lines Using Alternative Treatment and Control Distances

(a) Treatment within .25 km

	Sample Restriction			
	(1) Within 10km	(2) Within 5km	(3) Within 3km	(4) Within 2km
Post X Within .25 km	-0.06	-0.09	-0.10	-0.10
	(0.05)	(0.05)	(0.05)	(0.05)
R-squared	0.43	0.34	0.32	0.34
N	33,363	13,684	6,772	4,285

(b) Treatment within .5 km

	Sample Restriction			
	(1) Within 10km	(2) Within 5km	(3) Within 3km	(4) Within 2km
Post X Within .5 km	-0.06*	-0.08*	-0.09*	-0.10*
	(0.02)	(0.03)	(0.04)	(0.04)
R-squared	0.43	0.34	0.32	0.34
N	33,363	13,684	6,772	4,285

(c) Treatment within 1 km

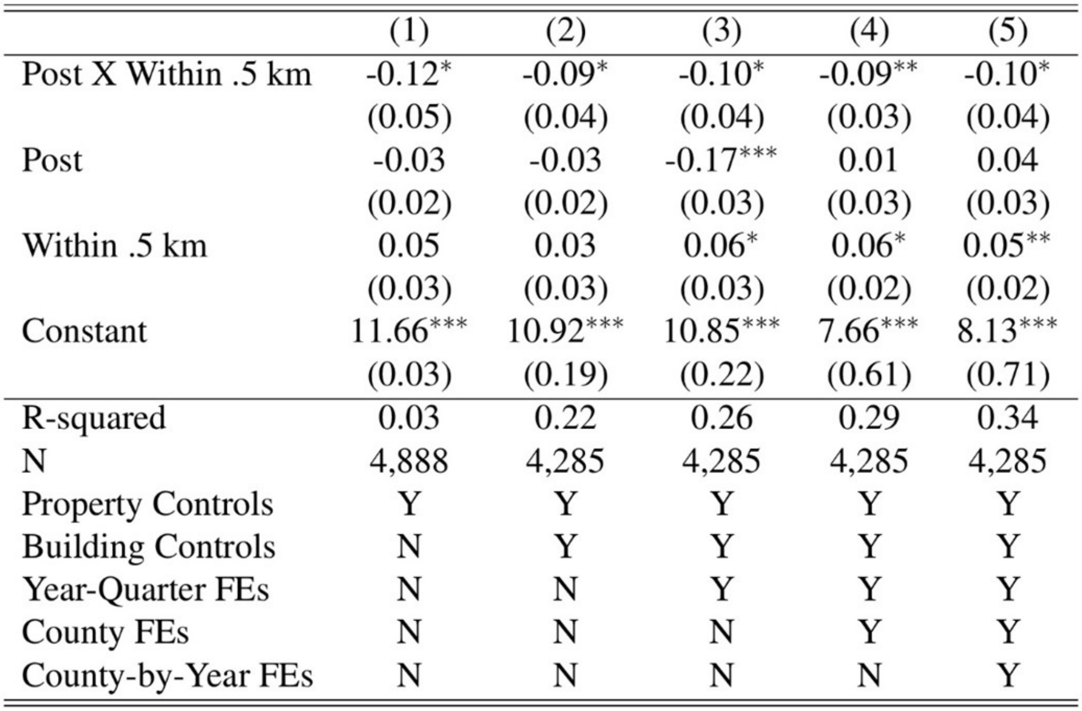
	Sample Restriction			
	(1) Within 10km	(2) Within 5km	(3) Within 3km	(4) Within 2km
Post X Within 1 km	-0.00	-0.02	-0.04	-0.06
	(0.02)	(0.02)	(0.03)	(0.03)
R-squared	0.43	0.34	0.32	0.34
N	33,363	13,684	6,772	4,285

(d) Treatment within 1.5 km

	Sample Restriction			
	(1) Within 10km	(2) Within 5km	(3) Within 3km	(4) Within 2km
Post X Within 1.5 km	0.02	0.01	-0.02	-0.06
	(0.02)	(0.02)	(0.03)	(0.04)
R-squared	0.43	0.34	0.32	0.34
N	33,363	13,684	6,772	4,285

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors clustered by county are in parentheses. All models include county, year-quarter, and county-by-year fixed effects, as well the controls for property and building characteristics. All models are run on a sample of transactions that occurred up to three years after construction of the nearest segment ended.

Table 3.4: The Effect of CREZ Within .5 km



	(1)	(2)	(3)	(4)	(5)
Post X Within .5 km	-0.12*	-0.09*	-0.10*	-0.09**	-0.10*
	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)
Post	-0.03	-0.03	-0.17***	0.01	0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Within .5 km	0.05	0.03	0.06*	0.06*	0.05**
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Constant	11.66***	10.92***	10.85***	7.66***	8.13***
	(0.03)	(0.19)	(0.22)	(0.61)	(0.71)
R-squared	0.03	0.22	0.26	0.29	0.34
N	4,888	4,285	4,285	4,285	4,285
Property Controls	Y	Y	Y	Y	Y
Building Controls	N	Y	Y	Y	Y
Year-Quarter FEs	N	N	Y	Y	Y
County FEs	N	N	N	Y	Y
County-by-Year FEs	N	N	N	N	Y

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors are presented, and are clustered by county in columns 4 and 5. All models are run on a sample of transactions that occurred up to three years after construction of the nearest segment ended.

3.7.2 Effect on Encumbered Properties

In Table 3.5, I add a binary variable equal to one if the property is encumbered, and interact it with $Post_t$, which allows for different effects on encumbered properties and those that are simply nearby. In the first column, I classify encumbered properties as those with a "verified intersection," meaning that the properties had a common identifier across the ZTRAX and TNRIS data that allowed me verify that the transacted property was intersected by the lines. The coefficient on "Post × Encumbered" suggests that construction of the lines decreased the value of an encumbered property by 22 percent. This is larger than the 10 percent reduction on a nearby property, suggesting that households have an additional willingness to pay to avoid an encumbrance because they lose the ability to use and access land where lines are placed.

Table 3.5: The Effect of CREZ on Encumbered Properties

	Verified Intersection (1)	Radius l.t. Distance (2)	Radius l.t. Distance - 50m (3)
Post X Encumbered	-0.22*	-0.12	-0.11*
	(0.08)	(0.07)	(0.04)
Post X Within .5 km	-0.10*	-0.09*	-0.09*
	(0.04)	(0.04)	(0.04)
Post	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Within .5 km	0.05**	0.05**	0.05**
	(0.02)	(0.02)	(0.02)
Encumbered	0.03	-0.00	-0.01
	(0.09)	(0.10)	(0.06)
Constant	8.13***	8.13***	8.12***
	(0.71)	(0.71)	(0.71)
R-squared	0.34	0.34	0.34
N	4,285	4,285	4,285
Number Encumbered	62	67	105
Number Encumbered Post	20	22	38

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. The model specifications and standard errors are identical to column 5 in Table 3.4. In this table, the columns differ on how encumbrance is operationalized. In column 1, the encumbered binary equals 1 only if I could verify that a CREZ line crossed the transacted property by mapping the lines and TNRIS parcel polygons. I additionally classify properties as encumbered if their inferred radius is less than their distance to the CREZ lines (column 2), or less than their distance to the CREZ lines minus 50 meters (column 3).

I could not match many transactions with the TNRIS data because they did not contain a common identifier. In the remaining columns of Table 3.5, I expand my definition of an encumbered property to include properties with an inferred radius that is less than their distance to the CREZ lines (column 2), or with an inferred radius that is less than their distance to the CREZ lines minus 50 meters (column 3).[17] In these specifications the coefficient on "Post × Encumbered" remains negative but attenuates towards zero, suggesting that using a radius to define encumbrance is capturing too many properties that the lines do not intersect.

Table 3.6 explores agricultural and vacant properties. In column 1, the positive coefficient and large standard error on "Post × Within .5 km" suggests that lines have no effect on neighboring agricultural properties. In column 2, I also fail to find an effect when defining treatment as properties within .25 km. Together, columns 1 and 2 suggest that the lines have no effect on nearby, unencumbered agricultural properties. In column 3, I estimate an imprecise coefficient on "Post × Encumbered." That I fail to find effects on agricultural properties is likely due to my very small sample of transacted encumbered properties (14 total and only 8 in the post-period). In reality, an encumbrance on an agricultural property would almost certainly decrease its value due to constrained use of land near the lines.

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[17] I infer a radius by assuming the properties are circular, and taking the square root of the lot size divided by pi.

Table 3.6: The Effect of CREZ on Vacant and Agricultural Properties

	Within .5km (1)	Within .25km (2)	Within .5km (3)
Post X Within	0.31	1.21	0.33
	(0.39)	(0.76)	(0.37)
Post	0.10	0.09	0.10
	(0.09)	(0.09)	(0.09)
Within	0.09	-0.52	0.02
	(0.37)	(0.42)	(0.42)
Post X Encumbered			0.04
			(0.85)
Encumbered			0.33
			(0.60)
Constant	9.89^{***}	9.89^{***}	9.89^{***}
	(0.05)	(0.05)	(0.05)
R-squared	0.07	0.07	0.07
N	2,301	2,301	2,301
Number Encumbered			14
Number Encumbered Post			8

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors clustered by county are in parentheses. All models include county and year-quarter fixed effects, as well the controls for property characteristics.

3.7.3 Robustness

In Table 3.7, I test the robustness of my results to four alternative specifications. In column 1, I drop transactions that occurred between the start and end of construction of the nearest segment. The previously estimated effects may be attenuated because they rely on construction finish to define the post-period, and the start of construction (or even project announcement) may mark when the lines begin to affect property values. The estimate in column 1 (an 9 percent reduction) is similar to the preceding results. Columns 2 and 3 further restrict the sample to between \$10K and \$1M, and \$25K to \$750K, to see if the effects are driven by outlying observations. I continue to find a negative relationship, but it lowers sightly to around 7 percent in column 3, suggesting that lines have a greater proportional effect on higher value properties. Column 4 replaces county fixed-effects with segment fixed effects and the results remain unchanged.

One concern is that properties transacted in the pre and post construction periods may differ on unobserved characteristics. Columns 5 and 6 probe this concern by estimating equation 5 with the natural logarithms of lot size (in acres) and building area (in square feet) as outcomes. They show that there is no statistical difference between the pre and post-period in either the treatment or control group, indicating that unobserved differences over time are not driving the main results (if lot and building size are a good proxies for unobserved building and property characteristics).

Table 3.7: Robustness Specifications

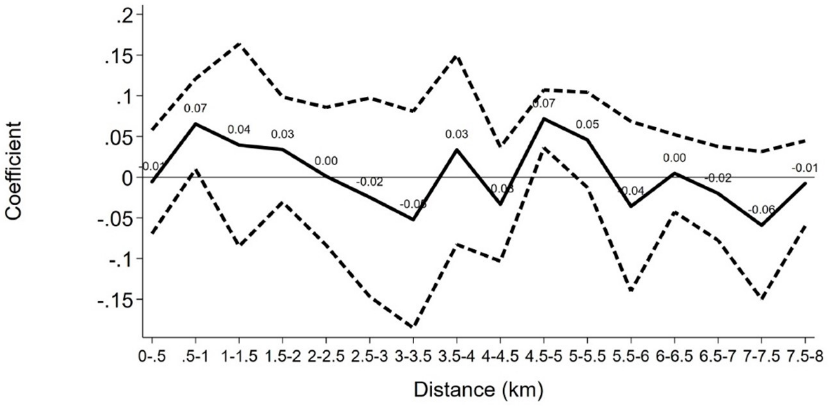
	Drop Constr. Period (1)	10K to 1M (2)	25K to 750K (3)	Segment FEs (4)	Log(Square Feet) (5)	Log(Acres) (6)
Post X Within .5 km	-0.09*	-0.09*	-0.07**	-0.10**	0.04	-0.05
	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.06)
Post	0.01	0.04	0.02	0.04	-0.01	0.06
	(0.05)	(0.03)	(0.02)	(0.03)	(0.01)	(0.08)
Within .5 km	0.04*	0.05*	0.04**	0.04	-0.06***	0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.09)
Constant	8.22***	8.14***	7.84***	8.73***	7.49***	-1.50***
	(0.71)	(0.71)	(0.64)	(0.76)	(0.00)	(0.03)
R-squared	0.34	0.34	0.39	0.34	0.00	0.00
N	3,840	4,279	4,152	4,285	4,285	4,285

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors are clustered by the level of the fixed effects. Columns 1 through 3 include year-quarter, county, and county-by-year fixed effects. Column 4 replaces the county fixed effects with segment fixed effects. All models are run on a sample of transactions that occurred before and up to three years after construction of the nearest segment ended, except for column 1 which excludes transactions that occurred between the construction start date and the construction end date.

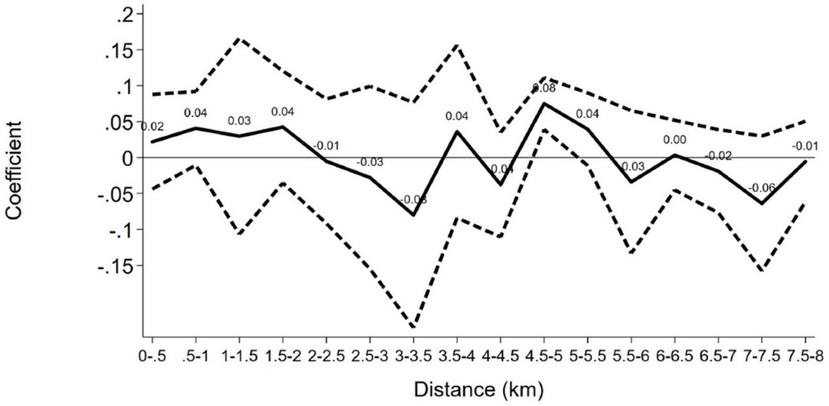
3.7.4 Effect Along Alternate Routes

If the previously estimated effects were spurious, we might expect to find similar effects along alternate routes where lines could have been built but were not. I create a set of "placebo routes," starting with routes that utilities proposed but PUCT modified or canceled. Because only 19 transactions occurred within .5 km of modified or canceled routes that are not also within .5 km of the actual lines, I create additional placebo routes near 16 segments that account for 93 percent of my treatment observations. I do this by connecting their endpoints (which are substation locations) with perfectly straight lines, and use those straight lines as the placebo routes. Appendix C.2 outlines the steps that I take to digitize the placebo routes.

Panel a of Figure 3.7 uses 32,536 transactions within 10 km of the placebo routes that are not also within .5 km of the actual lines. Unlike Figure 3.4, it does not show a statistically significant effect of the lines on post-construction property values (assuming that construction of the alternate routes would have been completed on the same date as the nearest actual route). But it does appear that properties within .5 km of the placebo routes sell for less than properties between .5 and 2 km. This is likely because of the spatial correlation between the placebo routes and the actual routes, which could have negative effects across the .5 km threshold. This appears to be the case in panel b of Figure 3.7, which only uses the 31,335 transactions that are more than 1 km away from the actual routes. It shows a flat relationship between property values and the placebo routes from 0 to 2 km. Similarly, Table 3.8 replicates the main results in column 5 of Table 3.4 and shows that being within .5 km of the placebo routes in the post-period has no effect on property values.



(a) Transactions Greater than .5km from Real Lines



(b) Transactions Greater than 1km from Real Lines

Figure 3.7: Placebo Test: The Effect of Alternate Routes

Note: The solid line depicts the coefficients on the interaction terms $Near_i \times Post_t$, from a regression with the natural log of housing prices as the outcome, where $Near$ is a set of binary variables that indicate the distance of the transacted property to the nearest segment. The dashed lines represent 95 percent confidence intervals, calculated with robust standard errors clustered by county.

Table 3.8: Placebo Test: The Effect of Alternate CREZ Routes

	$>$.5km from Real Lines (1)	$>$1km from Real Lines (2)
Post X Within .5 km	-0.02	-0.01
	(0.01)	(0.02)
Post	-0.10^{**}	-0.19^{***}
	(0.03)	(0.04)
Within .5 km	0.04^{*}	-0.01
	(0.01)	(0.03)
Constant	8.18^{***}	8.17^{***}
	(0.80)	(0.92)
R-squared	0.34	0.36
N	3,414	2,687
Number of Transactions Within .5km	1022	690
Number of Post Transactions Within .5km	365	238

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors clustered by county are in parentheses. All models include county, year-quarter, and county-by-year fixed effects, as well the controls for property and building characteristics. All models are run on a sample of transactions that occurred up to three years after construction of the nearest segment ended.

3.8 Uncompensated Loss from Transmission Lines

In this section, I use average transaction values by county, the TNRIS parcel data, and my preceding estimates to estimate the total uncompensated loss created by CREZ for residential households. I superimpose the TNRIS data over the CREZ lines in ArcGIS, and following the steps outlined in Appendix C.3 to estimate that there are 22,631 residential and agricultural properties across 92 counties that could have been affected by CREZ. Of them, 7,710 are encumbered agricultural properties. Table 3.9 presents the number of affected residential properties by type: 14,265 are unencumbered but nearby, and 656 are encumbered. As a share of the potentially affected properties, 63 percent (14,265 divided by 22,631) are not directly crossed by the lines, and their owners do not receive any compensation from the utilities.

Table 3.9: Affected Properties and Loss Estimates

Property Type	Number within .5km	Value (Millions)	Effect	Loss (Millions)
Residential, Encumbered	656	$ 88.8	-22%	$19.5
Residential, Unencumbered	14,265	$ 2,344	-10%	$234.4
Total	**14,921**	**$2,432.8**	-	**$253.9**

To put a value on the affected residential properties, I take the average transaction price in the post-period control group (.5 km to 2 km). For many counties, I have less than 20 transactions in the post-period control group, so I collapse them into in four regions (North East, West, East Central, and South) and take the average value by region. In the third column of Table 3.9, I sum (across all counties) the product of the number of properties and the median transaction price applied to each county. It shows that 14,921 residential properties worth around $2.4 billion are potentially affected by CREZ. The fourth column displays the typical household's willingness to pay to avoid the lines from Table 3.5, 10 percent of a property's value for nearby properties and 22 percent for encumbered properties. Multiplying the third and fourth columns and summing across suggests that the CREZ lines created $253.9 million in lost residential property value, with over 92 percent ($234.4 million) on unencumbered residential properties.

Because owners of nearby, unencumbered properties do not receive payments from the utilities, the \$234.4 million can be considered uncompensated loss. Whether owners of land crossed by the lines are fully compensated for the \$19.5 million loss on their properties depends on the size of the payments they receive. No data exists on how much landowners receive for hosting lines. Utilities file easement contracts with county clerks, but in every contract that I reviewed across four counties and five utilities the stated compensation was either \$1 or \$10, along with "other valuable considerations." This vague language is intended to keep confidential the actual compensation amount. In residential areas, full compensation depends on the value of the property. Based on the median property value of \$130,000, full compensation would have to exceed \$28,600 (22% × \$130,000).

There are also 7,710 encumbered agricultural properties worth an estimated \$9.6 million that are affected by the lines. The median transaction price for one agricultural acre without a building is around \$4,200. Assuming that the utility takes one acre of land, full compensation would be \$4,200 for a complete taking of the encumbered acre, plus some share of \$4,200 for each additional acre that is not taken but declines due to proximity of the lines or constricted land use. Unfortunately, my sample of encumbered agricultural properties is too small to precisely estimate this share.

There may be additional losses for residents of affected jurisdictions of up to \$5 million per year in the form of reduced tax revenues (assuming that assessors fully adjust assessments to capture the \$253.9 million in reduced value, and using the statewide average local property tax rate of 2 percent) (Association of Electric Companies of Texas, 2014). This is a small amount, considering that it is shared across 92 counties, and hundreds of school districts and cities. Moreover, new transmission and wind generation infrastructure is taxable as property, and the new revenues they provide will partially or fully offset the reduced taxes on residential property.

3.9 Policy Implications

By legally mandating CREZ and authorizing utilities to recover the costs from Texas electricity consumers, the Texas legislature signaled their belief that expanding transmission infrastructure to support renewable energy production was in the public interest. Compensating nearby, unencumbered property owners was not necessary to achieve this objective. But researchers have noted the unique factors that enabled the successful completion of CREZ, even in the face of community opposition. In particular, because ERCOT is within state lines, regulatory oversight of the siting process was vested in a single entity, the Public Utility Commission of Texas (Cohn and Jankovska, 2020, p. 4).

Interstate transmission projects require permits from multiple state and federal agencies, which means less flexibility to respond to residents' concerns and reroute lines, and multiple entry points for landowners to lobby public officials to cancel projects. This might be particularly relevant if residents feel that their land is being used as a "super-highway" to carry electricity to households far away, as was the case with the Quebec to Massachusetts line planned through New Hampshire. Environmental groups and residents along the proposed route opposed the line in written and in-person testimony that influenced state regulators' decision to reject it (New Hampshire Site Evaluation Committee, 2018). The Plains and Eastern Clean Line, a transmission project that would have carried enough wind energy from the Oklahoma and Texas panhandles to power 1.5 million homes in the Southeastern US, suffered a similar fate (Federal Permitting Improvement Steering Council, 2017). Despite a formal agreement between the project's developers and federal regulators that granted the power of eminent domain, the agreement was eventually terminated and the project delayed indefinitely after several parties—which notably included landowners and politicians in Arkansas—opposed the project (Gold, 2019; US Department of Energy, 2018; Lillian, 2017).

Imagine that CREZ was a contentious interstate project and that fully compensating all affected landowners would engender local acceptance. It seems unlikely that public officials would be willing to spend \$6.9 billion to construct the lines, but unwilling to spend the additional \$.23 billion to compensate landowners and ensure that the project went ahead.

One option to reduce uncompensated loss would be to select routes that avoid densely developed residential areas, because the lines appear to have no effect on unencumbered agricultural and vacant properties. But because transmission lines must reach population centers, utilities often cannot avoid routing them through some residential areas. In these areas, what is the best way to compensate affected landowners?

One option would be to compensate owners of any property with a view of the lines. Utilities and regulators could identify residential properties that are in view through on-the-ground site visits and with geospatial software that determines whether there is a clear viewshed between two points. Compensation could be based on some share of a property's taxed assessed value (e.g., 10 percent), which would be particularly suitable when most affected properties are relatively small (i.e., less than one or two acres). When some properties are large and much of their value is in land, it is reasonable to expect that only a portion of the land will be negatively affected by the lines, and providing a fixed amount (e.g., 10 percent of the median property value in the municipality) might better balance aggregate losses and aggregate compensation.

Perfectly balancing aggregate losses and compensation would be an impossible task, and some landowners may receive too much while others receive too little. Others that oppose the lines may be "out of view," and receive nothing. A less politically contentious solution might be to compensate affected neighborhoods "in kind" by offering long-term electricity rate reductions and improving associated goods, such as providing broadband and home heating upgrades. Such a compensation package was recently applied in Maine to cultivate public approval for the new route of the Quebec to Massachusetts line (Maine Office of the Governor, 2020; Chesto, 2021). Utilities and regulators could work together with local officials to design in kind compensation packages that are roughly equal to aggregate uncompensated loss projections and select a locally-acceptable definition of the affected community (e.g., all properties within the municipality, neighborhood, census block, etc.).

In defining the affected community and deciding between cash and in kind compensation, policymakers should consider the trade-off between fully compensating those closest to the lines and building broad community support. Cash compensation to those in view of the lines provides the most money to those most affected, which could build support among residents with the greatest standing in a formal siting process. But providing in kind compensation more broadly may be important when opposition stems from residents' perceptions that the lines would damage the scenic or historic nature of their community.

3.10 Conclusion

Transmission construction projects can achieve legislative goals for expanding renewable electricity generation, but households near the lines often bear a disproportionate share of the projects' costs, broadly understood. Using data on real estate transactions, I estimate that households near the CREZ transmissions lines in Texas bear \$253.9 million in costs associated with marred views, buzzing lines, and fears for their health and safety. The vast majority of these costs are uncompensated, because they accrue to owners of nearby properties that are not crossed by the lines, and therefore receive no money for the use of their land. These uncompensated losses are an extremely small share (less than 4 percent) of the project's costs. The findings suggest that compensating all affected residents, either with cash or in kind, would entail a small increase in costs that could have a large impact on public and political support for these otherwise welfare-enhancing infrastructure projects.

Appendix A Essay 1

A.1 Comparing Forgone Investment and Property Value Effects

In this appendix, I consider how the value of forgone investment estimated in the long term compares to property value changes estimated shortly after hazards become present. Greenstone and Gallagher (2008) use housing market data at the Census tract level to estimate the public's willingness to pay for remediating hazardous waste sites. They provide a careful treatment of the hedonic model that allows for taste-based sorting of households across communities in response to changes in environmental quality, which I adapt to illustrate how it compares to my focus on investment.

Consider Figure A1 panel a, which depicts the market for land near a hazard and immediately available for residential uses, such as those with soil and topography suitable for building. In the short term, the supply of the land is inelastic, because investment is required to convert land to residential uses. Upon the placement of hazards, such as the drilling of wells, households with a high valuation of environmental quality emigrate, and are replaced by households with weaker preferences for environmental quality. The net result is an inward shift in demand, where the quantity is unchanged and the price falls from P* to P1. In an empirical estimation of a function with transaction prices as an outcome, the coefficient on an exogenous measure of environmental quality estimates the price reduction. Under assumptions that equate the hedonic price function with marginal willingness to pay for environmental quality, multiplying the price reduction by the number of affected properties represents a reduction in the welfare of landowners near the hazards, represented by area C.

In the long term, Greenstone and Gallagher allow supply to become elastic (because nonresidential land can be converted to residential uses, and vice versa). In this case, placement of hazards can result in both price and quantity reductions (Figure A1 panel b). Their case considers large waste sites in relatively populous areas. But in rural communities that contain unplugged wells, those interested in building a home or other structure may have several alternatives for relatively low-priced and unaffected properties. If the cost of plugging

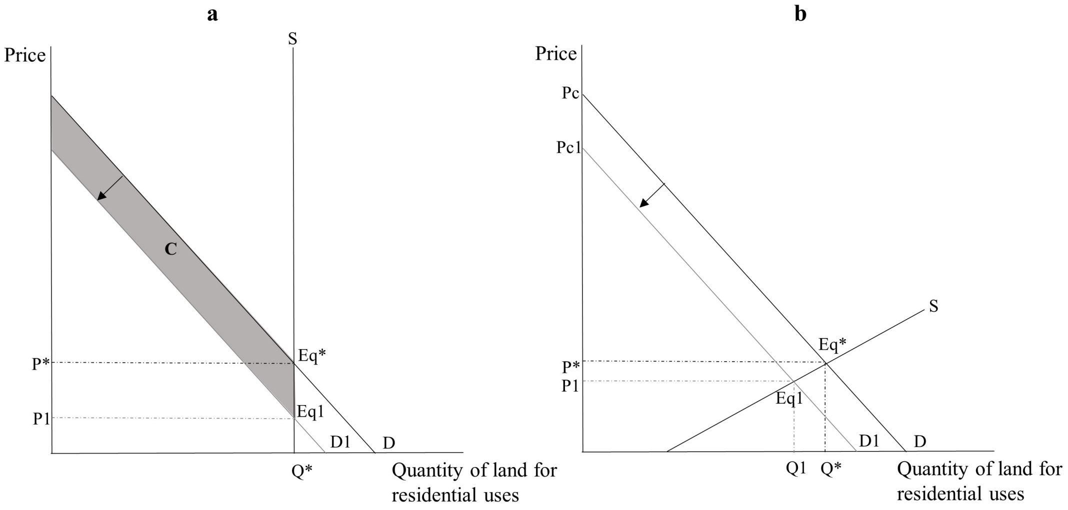


Figure A1: Market for Land in the Short and Long Term

a well is high relative to the value of the property where it is located, developers may build on unaffected properties. In an extreme case, there is no change in demand, but over time as affected properties are avoided, the supply of clean "no remediation necessary" properties shifts inward (Figure A2 panel a). If this were the case, prices would increase from P* to P2, quantity would fall from Q* to Q2, and area E would represent a reduction in the welfare of landowners near the wells.

A more likely case is that both demand and supply respond to the hazards in the long term. Demand may fall if landowners that desire additional improvements on their properties emigrate, and are replaced by households with weaker preferences for building or lower incomes. With both an inward shift in supply and demand, the quantity of "no remediation necessary" properties falls in equilibrium, and the price either rises or falls, depending on the relative size of the two shifts. Figure A2 panel b illustrates the case where an inward shift in supply is relatively small, and price falls from P* to P3. Superimposed on Figure A2b is the price reduction from Figure A1a caused entirely by a sorting-induced demand shift, the standard mechanism assumed to drive capitalization in short term hedonic studies. If supply

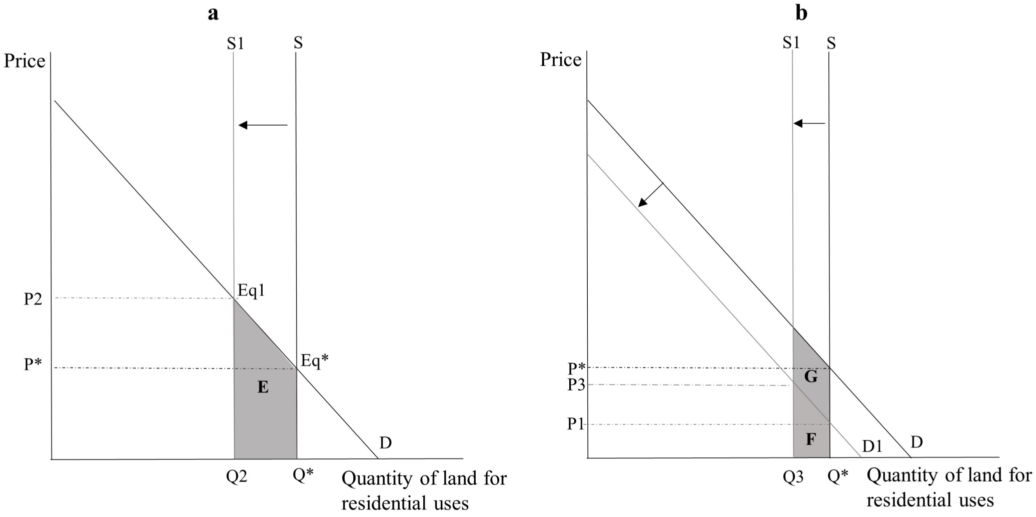


Figure A2: Market Response to Hazards

is variable in the long term, area F represents the forgone welfare caused by hazards due to a reduction in quantity that is not captured in the standard short term hedonic approach. Area G is captured both in the hedonic approach, and in a long term approach that considers variable supply. The two approaches would yield different estimates of producer surplus, which with inelastic supply equals the market value of all properties in the jurisdiction under consideration. If real estate is tax assessed at its market value, this means that the two approaches would yield divergent estimates of forgone tax bases and revenues.

In empirical applications, what explains the difference between welfare effects estimated with the short term hedonic approach and a long term approach that considers both demand and supply shifts? Identifying variation in hedonic program evaluation models comes from comparing properties that receive exogenous exposure to a hazard to properties that are unaffected. Because they typically hold constant all observable property characteristics, such as acreage or number of bedrooms, their capitalization estimates can be thought of as representing an average of the average differences in value between groups of transacted properties that are identical outside of the presence of the hazard.

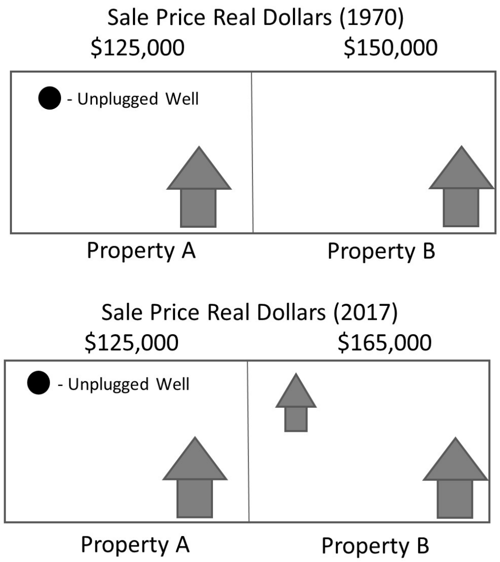


Figure A3: Time and Estimated Welfare Effects

For simplification, consider the case in Figure A3 which compare only two properties, A and B. The two properties are sold in 1970, and are identical in all ways except that property A contains an unplugged well. A short term hedonic study effectively attributes the difference in sale price, $25,000, entirely to the well. For illustration, assume that $10,000 of the difference is attributable to the buyer's perception that the well is aesthetically displeasing, while the other $15,000 is because the well constrains investment on a portion of the property. For this one property, $25,000 also represents forgone tax base caused by the well.

If property B presents no opportunity for future building, the hedonic approach may provide an unbiased estimate of the welfare effect of the unplugged well that persists indefinitely. But if property B receives an additional structure in 2017, the observed difference in sale price grows to $40,000, in real terms. Again, for illustration, assume that $10,000 is still attributable to aesthetic disamenities, and $15,000 was captured by the hedonic approach as a constraint on future investment. But what accounts for the remaining $15,000 that the hedonic approach has missed? Perhaps $10,000 represents the cost of constructing the new structure, which can be ignored from the perspective of the landowner's welfare. But if preferences for residential construction changed between 1970 and 2017, which could occur if the market experienced suburbanization, the constraint on building measured by the short term hedonic approach would miss $5,000 in surplus value of the structure—the landowner's enjoyment of the structure that she must forgo because the well prevents construction. It is reasonable to assume that preferences evolve naturally over time, or because changes in environmental quality induce changes in local demographics and income (Banzhaf and Walsh, 2008).

In addition to affecting landowners, hazards can affect communities more broadly. For instance, although production costs can be ignored from the perspective of landowner welfare, they still add to a property's observed market value. Because real estate is typically assessed at its market value, forgone investment ultimately reduces the tax bases for county and municipal governments and school districts. In the illustration, the full $15,000 value of the structure on property B represents forgone tax base caused by the unplugged well. To the extent that forgone investment is not tightly tied to demand for public services, which could be true for forgone improvements such as garages or additions, affected jurisdictions would decrease public good provision or increase tax rates, relative to a counterfactual where hazards are remediated.

The illustrations presented in this appendix show how in theory and empirical practice the short term hedonic approach may understate the long term effects of hazards on local communities. Although my empirical approach does not present a model for estimating welfare effects in long run equilibrium, it does pursue the more modest goal of considering whether environmental hazards can shift the supply of investment-ready properties. More broadly, my approach considers whether unbounding an empirical approach from the time-constant gradient assumption can reveal important effects on local communities that, by nature of the assumption itself, are outside the scope of most hedonic analyses.

A.2 Data and Geospatial Analysis

A.2.1 Well Data and Sample Construction

I combined data from the following three sources to arrive at the total number of unique wells contained in state records. The first dataset is the Department of Environmental Protection (DEP) well database made available by the Pennsylvania Geospatial Data Clearinghouse. Second, I use the DEP's records of production reports prepared by well operators between 1980 and May 2018. Third, I use the Department of Conservation and Natural Resources' (DCNR) Exploration and Development Well Information Network (EDWIN), which contains information on permitted wells, as well as thousands of wells that were never officially permitted but have adequate records and location information due to the Bureau of Topographic and Geologic Survey's efforts to digitize this information, especially for wells located in Washington County. The records associated with each source are:

1. The Pennsylvania Department of Conservation and Natural Resources' (DCNR) Exploration and Development Well Information Network (EDWIN). During November 2017, with permissions from DCNR's Bureau of Topographic and Geologic Survey, I remotely downloaded all 214,984 records of well permit numbers contained in EDWIN.
2. The DEP's records of 1,372,803 `well production reports` prepared and submitted by well operators between 1980 and May 2018.
3. The Pennsylvania Department of Environmental Protection's (DEP) well database (Summer 2017), made available by the `Pennsylvania Geospatial Data Clearinghouse`. The dataset contains 177,426 records of well permit numbers for which the DEP has geographic coordinate, which consists primarily of post-1955 wells with permits, as well as older wells that continued producing and required registration under the Oil and Gas Act of 1984.

I conducted the following steps in Stata to identify the total number of unique wells contained in the three datasets in Washington County, as well as information on each well's minimum year, plugging status (plugged and unplugged) and whether it had reported production in a DEP report at any point since 1980.

1. I dropped duplicate observations of the same permit number (30,446) from the EDWIN dataset. In the case of duplicate permit numbers, I retained the record with the well status that I assumed was recorded most recently. I prioritized retention of records with the well status of "plugged," as once a well is plugged the status is unlikely to change. Next, I prioritized the retention of "abandoned" wells, followed by "active," and "inactive" (I observed that it is more common in duplicate records for a well listed as inactive to later be switched to active by the operator, rather than the other way around). After dropping duplicates, I retained 184,538 well permit records from EDWIN.
2. In removing duplicates from the DEP production reports, I first removed 13,296 duplicate records associated with multiple production reports submitted for a given permit number in the same year. Duplicate production reports are usually the result of the well changing ownership in that year, causing more than one operator to submit a report, and in these cases, I retained the record with the well status that was assumed to be recorded most recently (as in step 1). With the remaining production reports, I retained the well status that was recorded in the most recent reporting year, and identified for each permit number the number of years it has been since 2017 that production was reported. Removing duplicate observations associated with reports being submitted for a permit number in multiple years, I retain records of 110,235 unique permit numbers that were listed in the production reports.
3. I merged the unique permit number records from EDWIN and the production reports with the DEP well database, which contained no duplicates. There were 113,627 permit numbers that were in either EDWIN or the DEP database but were not found in a DEP production report. These permit numbers were classified as never reporting production in a DEP production report. In total, merging the three datasets yielded 223,862 unique permit numbers.
4. Each of the three datasets contained its own well status variable. Since EDWIN contained the largest number of permit numbers, the well status in EDWIN was retained if it was not missing. If the well status in EDWIN was missing, I first replaced it with the status contained in the DEP well database. If the status was also missing in the DEP well database, I replaced it with the status contained in the most recent DEP production

report. Out of the 223,862 permit numbers, 14,298 did not have a well status in any of the three datasets, and I labeled the well status as "not recorded." For the rest of the wells, I standardized the well status variable for consistency. For example, I coded wells with a "regulatory inactive status" in the DEP well database as having an "inactive" well status, in order to match the other two datasets.

5. I classified the permit number as "plugged" if the well status indicated that the well was plugged by either the DEP or the operator. Conversely, I classified the permit number as "unplugged" if the well status indicated that the well was abandoned, orphaned, active, inactive, or if the well status was not recorded.
6. I assigned each well a "minimum year" based on the earliest year that it was recorded in any of the three data sources. The date fields that were considered when creating a minimum year include a) the date the DCNR received a well completion report, b) the date the DCNR received the well record, c) the date the well was permitted, d) the date the well was spud, e) the date the well completed stimulation, f) the date the well was plugged, and g) the date the well first reported production. I classify wells as being pre-1970 if they have a minimum year before 1970. There were 3,640 wells with a minimum year after 1957 (mostly dates associated with receipt of the well record from another state agency or the operator) but with "ninety-thousand series" permit numbers (of the form XXX-9XXXX), which indicate that they were retroactively assigned permit numbers by the Bureau of Topographic and Geologic Survey. Additionally, 687 have an "unknown" operator and a missing minimum year. I classify these 4,327 wells as having an "unknown" minimum year but consider them to be pre-1970 wells because permitting was required by a 1955 state law.
7. I assigned each plugged well a plugging date. Since the DEP database contained the most complete plugging date information, I retained these plug dates if they were not missing. I replace missing plugging dates from the DEP database with plugging dates from EDWIN.
8. Out of the 223,862 permit numbers across the state, I drop 212,987 to retain only the 10,875 permit numbers in Washington County.
9. Of the 10,875 permit numbers in Washington County, I drop the following:

   a. 1,454 permit numbers with a well status of "proposed but never materialized" or "operator reported not drilled."
   b. 70 permit numbers listed as having an "incomplete" or "junked" well type (a variable that I constructed from the three datasets in an identical manner as well status, which classifies the wells as oil, gas, injection, etc.).
   c. 1,905 permit numbers that were listed as unconventional or horizontal wells in the DEP datasets, which fall outside of our scope.
   d. 41 wells that do not have a date in any of the date fields across the three datasets.
   e. 117 wells that do not have latitude and longitude information.
   f. 1,752 wells that were drilled after 1970, the start of the study period.
   g. 433 wells that were plugged during the study period 1970 to 2017.
   h. 111 wells that are within 300 meters of the cities of Washington or Canonsburg, in order to focus on wells outside of urban areas.
   i. 975 wells that are not oil or gas wells (list as having a wells type of coalbed methane, injection, storage, test, water intake and disposal, dry hole wells, or unrecorded)

   This creates a sample of 4,017 wells (2,510 plugged and 1507 unplugged) that I utilize throughout our analysis.

A.2.2 Building Data

1. I acquired building and parcel data directly from the following sources:
   a. Geospatial data from the Washington County GIS Department (2017) that includes the location, shape, and size of parcels and structures.
   b. Tax assessment data for the year 2017 (the "Tax Roll" from the Washington County Revenue Department) that includes the construction date of each taxable structure in the county. The data on school district tax rates are from the county's 2019 millage listing, which I downloaded from the Washington County Tax Department's website.
2. I matched dates in the Tax Roll to building polygons in the GIS data based on a unique id for each parcel and the footprint size of each building. Duplicate and unmatched records

were dropped, including buildings smaller than 400 square feet (37 square meters) which were not included in the original GIS data.

A.2.3 Geospatial Control Variables and their Sources

1. Geospatial data on the boundaries of US counties were downloaded from the `US Census Bureau (2016)`. The outline of Washington County was retained as a layer.
2. Line and Polygon data on the location of streams, rivers, pond and lakes in Washington County were downloaded from the `US Census Bureau (2017)`. Line data were converted to polygon using the “Buffer” tool in ArcGIS. Stream and rivers were assumed to have a width of 8 meters when creating the buffers (4 meters distance in each direction). The buffer polygons were then merged with the lake and pond polygons using the “Merge” tool in ArcGIS.
3. Data on the slope of soil and its capacity for supporting dwellings with and without basements were made available by the U.S. Department of Agriculture (USDA), Natural Resources Conservation Service’s Soil Survey Geographic (SSURGO). The data were downloaded from `PASDA (2014)` for Green and Washington counties. The data were clipped to the Washington County outline in ArcGIS. To view and manipulate the data, the USDA’s Soil Data Viewer 6.2 was downloaded from the SSURGO website.
4. Data on the location of `I-79 (2018)` (the section running from the Allegheny County border to the city of Washington) and the cities of `Washington and Canonsburg (2017)` were made available by Pennsylvania Department of Transportation and were downloaded from PASDA. The features of interest (I-79 in Washington County and the two cities) were selected from the larger statewide road and municipal shapefiles and saved as new layers.
5. Data on the location of state parks (2017) and `local parks (2015)` were made available by the Pennsylvania Department of Conservation and Natural Resources (PA DCNR) and were downloaded from PASDA. In addition, data on the location of additional local parks were made available by the Washington County GIS Department (2017). The three data sets were merged, dissolved to deal with overlaps, and clipped to the Washington

County outline.

6. Data on the location of `state game lands (2017)` were made available by the Pennsylvania Game Commission and were downloaded from PASDA. The data were clipped to the Washington County outline in ArcGIS.
7. Data on the location of `underground coal resources (2018)` that have been extracted historically and more recently were made available by the DEP and were downloaded from PASDA. The data were clipped to the Washington County outline and were dissolved to ignore overlapping mines.
8. Data on the location of oil and gas pools come from the `Pennsylvania Department of Conservation and Natural Resources (DCNR)`, and was downloaded using our access to EDWIN in February 2018.

A.2.4 Geospatial Analysis

A.2.4.1 Aggregating Outcomes at the Well Level

For each well, I calculated the geodesic distance of all buildings within 250 meters using the Generate Near Table option in ArcGIS. Each building and well retained a unique ID which connects it with pertinent information in the original datasets (i.e., minimum observed well year, type of well (oil, gas, etc.), well status (plugged, unplugged), build year of the structure, type of building (main, out, commercial)). I then aggregate the outcome variables for each well location by summing up building within each location over the various study periods I consider in the essay (i.e., pre-1970, 1920-1970, 1970-2017).

A.2.4.2 Connecting Control Variables to Wells

1. I conducted a Spatial Join (Analysis) in ArcGIS between the well data (target features) and the USDA slope and soil quality data (join features). Three separate spatial joins classified the soil around each well based on the slope, capacity to support dwellings with basements, and capacity to support dwellings without basements of the geologic contour that the wells was within (using the WITHIN match option).

2. For each well, I calculated the distance to the nearest 1) water feature (stream, river, pond, lake), 2) state or local park, 3) state game land, 4) portion of I-79, 5) city (Washington or Canonsburg), 6) coal-mined area. This was done with the Near (Analysis) tool in ArcGIS and used geodesic distances between wells and the six feature categories.

A.2.4.3 Pool Fixed Effects

I created the pool fixed effects used in Table 1.4 by the following steps:

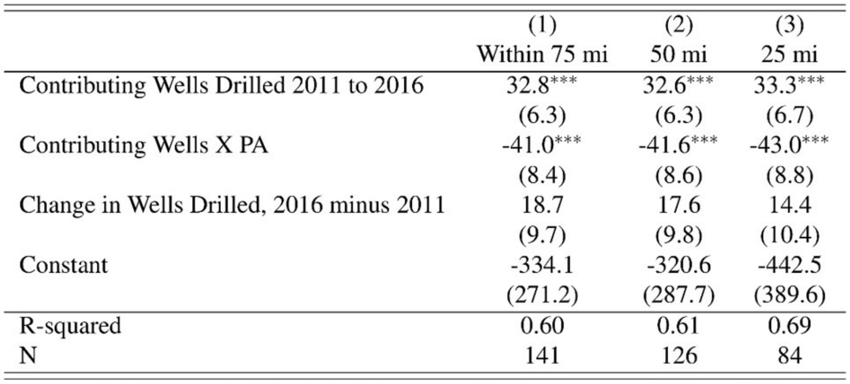
1. Downloaded oil and gas pools from the `Pennsylvania Department of Conservation and Natural Resources (DCNR)` using our access to EDWIN.
2. Dropped Shale Gas ("SH") pools because none of the pre-1980 wells should have targeted a shale gas pool, and I have removed shale gas wells from the analysis (the study period ends in 2005 and shale gas wells were not exported to GIS at any point).
3. Dissolved the 508 non-shale pools by the 42 fields using the Dissolve too in ArcGIS, to yield 241 non-overlapping pools.
4. Conducted a Spatial Join (Analysis) in ArcGIS between the 3,974 wells (target features) and the 241 pools (join features). The spatial join assigned each well to a pool, or as identified it as not in a pool.

Appendix B Essay 2

B.1 Additional Figures and Tables

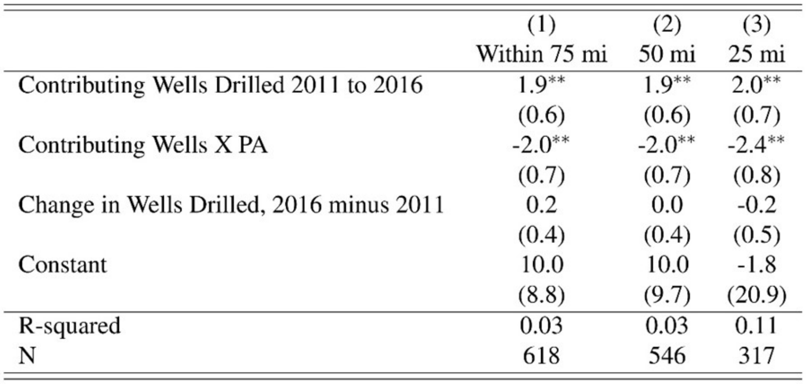
Table B1: Robustness: Wells and Revenues with Drilling Control

(a) Change in School District Property Tax Revenues, 2011 to 2016



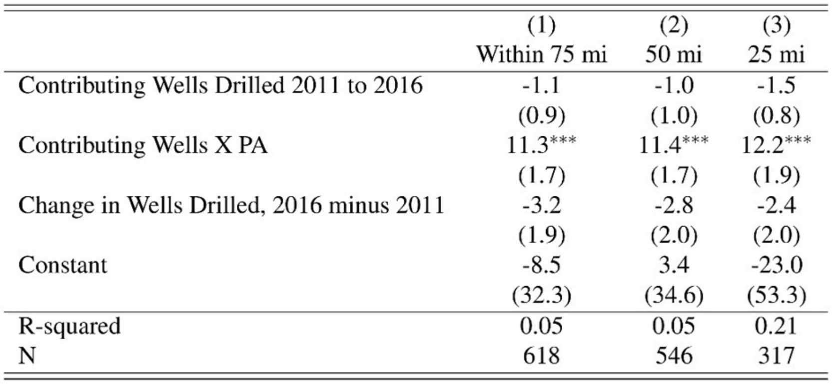
	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	32.8***	32.6***	33.3***
	(6.3)	(6.3)	(6.7)
Contributing Wells X PA	-41.0***	-41.6***	-43.0***
	(8.4)	(8.6)	(8.8)
Change in Wells Drilled, 2016 minus 2011	18.7	17.6	14.4
	(9.7)	(9.8)	(10.4)
Constant	-334.1	-320.6	-442.5
	(271.2)	(287.7)	(389.6)
R-squared	0.60	0.61	0.69
N	141	126	84

(b) Change in Municipal Property Tax Revenues, 2011 to 2016



	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	1.9**	1.9**	2.0**
	(0.6)	(0.6)	(0.7)
Contributing Wells X PA	-2.0**	-2.0**	-2.4**
	(0.7)	(0.7)	(0.8)
Change in Wells Drilled, 2016 minus 2011	0.2	0.0	-0.2
	(0.4)	(0.4)	(0.5)
Constant	10.0	10.0	-1.8
	(8.8)	(9.7)	(20.9)
R-squared	0.03	0.03	0.11
N	618	546	317

(c) Change in Municipal State Transfers, 2011 to 2016



	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-1.1	-1.0	-1.5
	(0.9)	(1.0)	(0.8)
Contributing Wells X PA	11.3***	11.4***	12.2***
	(1.7)	(1.7)	(1.9)
Change in Wells Drilled, 2016 minus 2011	-3.2	-2.8	-2.4
	(1.9)	(2.0)	(2.0)
Constant	-8.5	3.4	-23.0
	(32.3)	(34.6)	(53.3)
R-squared	0.05	0.05	0.21
N	618	546	317

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. Outcomes are in thousands of dollars.

Table B2: Robustness: Wells and School Finances with Drilling Control

(a) Change in Fund Balance, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	86.2***	87.2***	90.9**
	(24.1)	(24.3)	(28.5)
Contributing Wells X PA	-93.7***	-93.3***	-109.5***
	(25.4)	(25.6)	(31.1)
Change in Wells Drilled, 2016 minus 2011	-80.2	-86.4	-199.2
	(139.8)	(142.7)	(108.0)
Constant	1283.6	1584.0	-2410.3
	(2657.3)	(2806.3)	(2538.5)
R-squared	0.19	0.20	0.39
N	141	126	84

(b) Change in Outstanding Debt, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	57.0***	56.4***	55.6***
	(11.5)	(11.5)	(12.4)
Contributing Wells X PA	-68.9***	-69.4***	-77.1***
	(18.9)	(18.7)	(20.1)
Change in Wells Drilled, 2016 minus 2011	76.7	61.7	52.1
	(64.3)	(64.6)	(64.7)
Constant	529.7	978.7	1205.3
	(1296.7)	(1335.9)	(1739.7)
R-squared	0.18	0.18	0.21
N	141	126	84

(c) Change in Residential Millage, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.01	-0.02*	-0.02
	(0.01)	(0.01)	(0.02)
Contributing Wells X PA	0.05	0.05*	0.04
	(0.03)	(0.02)	(0.02)
Change in Wells Drilled, 2016 minus 2011	-0.01	-0.08	-0.06
	(0.11)	(0.07)	(0.10)
Constant	1.10	1.45	1.05
	(2.85)	(1.77)	(1.68)
R-squared	0.03	0.03	0.08
N	113	101	64

(d) Change in Commercial Millage, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.02**	-0.02**	-0.01
	(0.01)	(0.01)	(0.01)
Change in Wells Drilled, 2016 minus 2011	-0.23	-0.23	-0.17
	(0.15)	(0.15)	(0.15)
Constant	1.42	1.42	3.06
	(1.93)	(1.93)	(3.33)
R-squared	0.43	0.43	0.52
N	42	42	30

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust SEs in parentheses. Outcomes in \$1,000.

Table B3: Robustness: Wells and Municipal Finances with Drilling Control

(a) Change in Road Expenditures, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.0	0.1	-1.3
	(0.7)	(0.7)	(0.7)
Contributing Wells X PA	7.8***	7.7***	8.2***
	(1.9)	(1.9)	(2.1)
Change in Wells Drilled, 2016 minus 2011	0.0	-0.1	-1.4
	(2.1)	(2.2)	(2.3)
Constant	-66.4**	-59.8*	-71.7
	(23.9)	(24.1)	(36.8)
R-squared	0.14	0.17	0.40
N	618	546	317

(b) Change in Fund Balance, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	5.0	4.0	-0.4
	(7.3)	(6.5)	(3.3)
Contributing Wells X PA	2.9	3.8	9.5**
	(8.5)	(7.8)	(3.5)
Change in Wells Drilled, 2016 minus 2011	-8.3	-8.1	5.6
	(18.5)	(20.6)	(8.4)
Constant	-334.6	-435.6	-841.6*
	(295.9)	(364.7)	(412.4)
R-squared	0.03	0.03	0.28
N	618	546	317

(c) Change in Outstanding Debt, 2011 to 2016

	(1) Within 75 mi	(2) 50 mi	(3) 25 mi
Contributing Wells Drilled 2011 to 2016	-0.7	-0.4	0.6
	(1.3)	(1.4)	(2.1)
Contributing Wells X PA	-3.6	-3.8	-0.2
	(3.9)	(3.9)	(1.6)
Change in Wells Drilled, 2016 minus 2011	0.7	1.8	0.4
	(4.2)	(4.3)	(4.5)
Constant	15.8	-5.4	-281.3
	(136.7)	(150.4)	(321.3)
R-squared	0.01	0.01	0.07
N	618	546	317

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. Outcomes are in thousands of dollars.

B.2 Data

B.2.1 Geospatial Data Used for Sample Selection and Mapping

I downloaded 2019 `TIGER/LINE shapefiles` for county subdivisions, places, school districts, and states for Pennsylvania and Ohio. I also downloaded the West Virginia state shapefile. I used the Intersect tool in ArcGIS to create a feature that is shared boundary of three states. By hand, I used the edit tool in ArGIS to clip the border at a straight, horizontal line running through Pennsylvania's southwestern most point. This resulted in a feature that represents Pennsylvania's western and Ohio's eastern border, which is displayed in Figures 2.2 and 2.2. For each county subdivision, place, and school district I used the Near Analysis tool in ArcGIS to calculate the geodesic distance in meters to the border. I converted the variable to miles and used it to select the full sample of municipalities within 75 miles of the border. I also use it to narrow the sample to 50 and 25 miles from the border in both the municipal and district analysis.

Three other data sources were used to select the final samples of municipalities and districts. First, I used the `National Center for Health Statistics` Urban-Rural Classification Scheme for Counties identify and exclude municipalities in central counties of metropolitan statistical areas, which effectively excludes only Allegheny County, Pennsylvania. Second, I used the US Energy Information Administration's `Drilling Productivity Report` to identify municipalities and districts that fall in counties that belong to the Appalachia drilling region. Third, I used data from the US Census Bureau to identify principal cities and municipalities that share borders with principal cities. I describe how I created the distance to the nearest principal city variable below.

B.2.2 Control Variables and their Sources

1. **Population and Enrollment Data:** To control for effect of the size of districts and municipalities on financial outcomes, I use population and enrollment variables. In the municipal analysis the population variable is from the 2010 decennial census, downloaded from table B01003 of the `US Census Bureau` American Community Survey for cities and

places. For school districts, I control for enrollment recorded in the 2011 Census of Local Governments.

2. **Share of Area that is Urban/Suburban:** To control for the effect of being an urban area on financial outcomes, I used the field calculator tool in ArcGIS to create a variable that is the share of the district or municipality's land area that falls in a US Census-designated Urban Area. I downloaded data on the locations of Urban Areas for the year 2019 as TIGER/Line Shapefiles from the `US Census Bureau`.
3. **Distance to nearest principal city:** To control for the effect of being near a principal city of a metropolitan statistical area, I used the Near Analysis tool in ArcGIS to create a variable that is the geodesic distance in meters to the nearest principal city. I downloaded a list of principal cities from the `US Census Bureau`. I used the list to select principal cities in eastern Ohio, western Pennsylvania, and West Virginia by hand to create a shapefile of principal cities which include:
    a. Akron, OH
    b. Altoona, PA
    c. Canton, OH
    d. Massillon, OH
    e. Cleveland, OH
    f. Columbus, OH
    g. Erie, PA
    h. Johnstown, PA
    i. Mansfield, OH
    j. Morgantown, WV
    k. Vienna, WV
    l. Pittsburgh, PA
    m. State College, PA
    n. Steubenville, OH
    o. Weirton, WV
    p. Wheeling, WV
    q. Boardman, OH

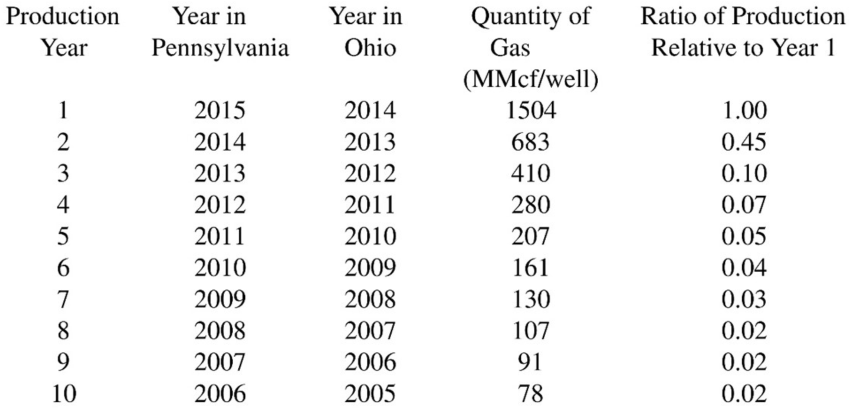
r. Warren, OH
s. Youngstown, OH

B.3 Age Adjusted Well Counts

I calculated the ratios of production between the first year of a well's life and later years using data from the `US Energy Information Administration`. The EIA provides decline curve parameters for 15 counties in Pennsylvania and Ohio and a calculator that applies the parameters to estimate monthly natural gas production volumes. A decline curve is a model of a well's production over time that is derived from historic production data, and is based on three parameters: (1) the initial production rate, averaged over the first thirty days and expressed as thousands of cubic feet per day, (2) an initial rate of production decline, and (3) a hyperbolic decline parameter.

I take a weighted average of the three parameters across the 15 counties, using the number of wells in the county at the end of 2016 as weights. I then input the parameters into the calculator to produce annual production amounts by well age, which I display in Table B4. Because of the amount of time it takes for a well to begin contributing revenues varies across the two states, the table also aligns the year that well is drilled with the production year for which it contributes revenues in 2016. For example, a well drilled in 2013 in Ohio contributes property tax revenues for its second year of production in 2016. Because the ratio of production in year 2 to year 1 is .45 (683 mmcf/1504mmcf), any well drilled in 2013 contributes .45 to its jurisdiction's value of $\triangle Wells_i$.

Table B4: Age Adjustment Ratios



Production Year	Year in Pennsylvania	Year in Ohio	Quantity of Gas (MMcf/well)	Ratio of Production Relative to Year 1
1	2015	2014	1504	1.00
2	2014	2013	683	0.45
3	2013	2012	410	0.10
4	2012	2011	280	0.07
5	2011	2010	207	0.05
6	2010	2009	161	0.04
7	2009	2008	130	0.03
8	2008	2007	107	0.02
9	2007	2006	91	0.02
10	2006	2005	78	0.02

B.4 Approved Spending Categories in Pennsylvania

Pennsylvania Act 13 or 2012 enumerates thirteen approved expenditure categories that it states are "associated with natural gas production." They are:

1. Roadways, bridges and public infrastructure
2. Water and sewer projects
3. Emergency services
4. Greenways, trails, and parks
5. Water reclamation projects
6. Tax reductions
7. Affordable housing projects
8. General government expenditure including records management, geographic information systems and information technology
9. Health and human services
10. Judicial services
11. Planning initiatives
12. Oil and gas career and technical training
13. Saving for future expenditures on one of the other twelve projects

Of the money that goes to local governments, around 80 percent is through disbursements to municipal and county governments with wells, or municipalities without wells but in counties with wells. The remaining 20 percent is through grant programs administered by state agencies. Local governments can apply for grants. While some of the grant money goes to jurisdictions with drilling, much of it does not. The grants fund predetermined projects in eight categories:

1. Reclamation of abandoned mines
2. The collection of baseline water quality data
3. Flood mitigation projects,
4. Greenways, trails, and parks
5. Reclamation of abandoned wells

6. Sewage facilities
7. Watershed restoration
8. Affordable housing and rental assistance

B.5 Alternate Pre-Trend Analysis

In this appendix, I use an event study-style model as an additional test of my identifying assumption that pre-2011 trends in the outcomes are not correlated with 2011 to 2016 drilling intensity, Specifically, the model is:

$$\begin{aligned}\triangle Y_i = \delta_0 + \beta_1(\text{`}01 - \text{`}06) + \beta_2(\text{`}11 - \text{`}16) + \beta_3(\text{`}01 - \text{`}06 \times \triangle Wells_i) \\ + \beta_4(\text{`}06 - \text{`}11 \times \triangle Wells_i) + \beta_5(\text{`}11 - \text{`}16 \times \triangle Wells_i) + \triangle \varepsilon_i,\end{aligned} \tag{7}$$

For the purpose of testing the identifying assumption, the coefficients of interest are β_3 and β_4. For each outcome that I examine in the second essay, I estimate the equation twice (once for each state), and present the results in Tables B5 and B6.

I start with column 2 of Table B5a as an example. The insignificant coefficients β_3 ("01-06 X Wells") and β_4 ("06-11 X Wells") indicate that for Pennsylvania municipalities and prior to the treatment period, trends in transfer revenues are not statistically correlated with 2011 to 2016 drilling intensity. This suggests that drilled and undrilled municipalities experienced similar trends in transfers prior to drilling, and the significant and positive coefficient β_5 ("11-16 X Wells") is driven by drilled municipalities receiving the Impact Fee. Across panels a and b and across columns we see a similar relationship: β_3 and β_4 are insignificant and indicative of parallel trends between drilled and undrilled municipalities, and β_5 is significant (or nearly so) for the outcomes associated with drilling (e.g., in Ohio, municipal property tax revenues increase over the period 2011 to 2016 as shale wells enter the tax base). An exception is with β_3 ("01-06 X Wells") in column 5 of panel b, where it appears that drilled Ohio municipalities are paying down more debt over the period 2001 to 2006 than undrilled municipalities, but this is not a substantial concern because in the preceding results I find no relationship between drilling and municipal debt in Ohio, and β_4 ("06-11 X Wells") shows a parallel trend between drilled and undrilled municipalities in the period just before drilling began.

Table B6 also shows insignificant β_3 and β_4 coefficients across the majority of school district outcomes considered in the study. There are two exceptions. Drilled districts in Pennsylvania experience greater growth in fund balances over the 2001 to 2006 period than undrilled districts, but in the preceding results I find no relationship between drilling and district fund balances in Pennsylvania, and β_4 ("06-11 X Wells") shows no divergent trends between drilled and undrilled districts in the period just before drilling. Similarly, the significant β_3 in column 3 of panel b is not a large concern, because there is no relationship between drilling and school debt in the period just before drilling, and the increase in debt of \$52.82 per well in the treatment period vastly outweighs the incidental divergence in trends between drilled and undrilled districts that occurred between 2001 and 2006.

Table B5: Alternate Pre-Trend Analysis, Municipalities

(a) Pennsylvania

	(1) Property Tax Rev.	(2) Transfer Rev.	(3) Road Expen.	(4) Fund Bal.	(5) Debt
01-06 X Wells	1.11	-0.45	0.38	1.91	-2.86
	(0.7)	(0.9)	(1.9)	(2.3)	(2.5)
06-11 X Wells	-0.31	1.28	0.88	-0.61	3.48
	(0.5)	(2.8)	(1.8)	(2.1)	(2.8)
11-16 X Wells	-0.06	11.12***	8.00***	7.64**	-2.64
	(0.4)	(1.9)	(1.9)	(2.6)	(2.7)
R-squared	0.05	0.02	0.04	0.05	0.01
N	1287	1287	1287	1287	1287

(b) Ohio

	(1) Property Tax Rev.	(2) Transfer Rev.	(3) Road Expen.	(4) Fund Bal.	(5) Debt
01-06 X Wells	0.14	-0.29	0.11	-6.20	-10.84*
	(0.4)	(0.8)	(0.7)	(4.0)	(5.0)
06-11 X Wells	0.09	0.04	-0.71	5.03	5.74
	(0.3)	(0.9)	(0.8)	(5.2)	(5.6)
11-16 X Wells	1.42*	0.45	-1.09	5.48	2.16
	(0.7)	(1.0)	(0.8)	(5.5)	(5.3)
R-squared	0.05	0.03	0.05	0.02	0.01
N	567	567	567	567	567

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. The outcomes are in thousands of dollars. All models include controls for the number of wells drilled in the jurisdiction prior to 2011, population in the 2010 census, the distance to the nearest principal city of an MSA, and the share of the jurisdiction's land area in a Census-designated urbanized area.

Table B6: Alternate Pre-Trend Analysis, School Districts

(a) Pennsylvania

	(1) Property Tax Rev.	(2) Fund Bal.	(3) Debt	(4) Res. Tax Rate
01-06 X Wells	10.51	35.56**	13.41	0.04
	(8.1)	(12.1)	(15.6)	(0.1)
06-11 X Wells	-8.36	10.78	-1.79	-0.03
	(7.4)	(19.2)	(18.5)	(0.0)
11-16 X Wells	2.71	-19.63	-7.76	0.06
	(5.6)	(15.9)	(13.4)	(0.0)
R-squared	0.40	0.03	0.02	0.11
N	294	294	294	255

(b) Ohio

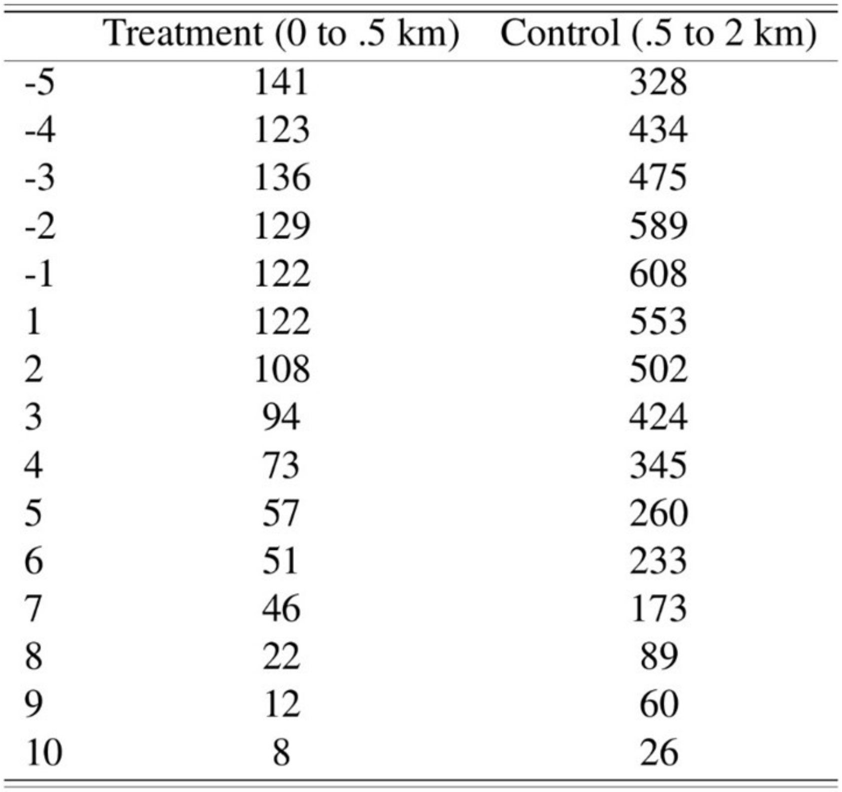
	(1) Property Tax Rev.	(2) Fund Bal.	(3) Debt	(4) Res. Tax Rate	(5) Com. Tax Rate
01-06 X Wells	-0.59	10.84	6.63*	0.00	0.00
	(2.2)	(10.3)	(3.2)	(0.0)	(0.0)
06-11 X Wells	3.63	17.09	2.88	0.01	0.01
	(2.1)	(12.1)	(3.0)	(0.0)	(0.0)
11-16 X Wells	34.15***	81.15***	52.82***	-0.02***	-0.02**
	(5.5)	(23.9)	(10.8)	(0.0)	(0.0)
R-squared	0.73	0.28	0.30	0.10	0.10
N	129	129	129	126	126

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors in parentheses. The outcomes are in thousands of dollars. All models include controls for the number of wells drilled in the jurisdiction prior to 2011, enrollment in the 2011 census, the distance to the nearest principal city of an MSA, and the share of the jurisdiction's land area in a Census-designated urbanized area.

Appendix C Essay 3

C.1 Additional Tables

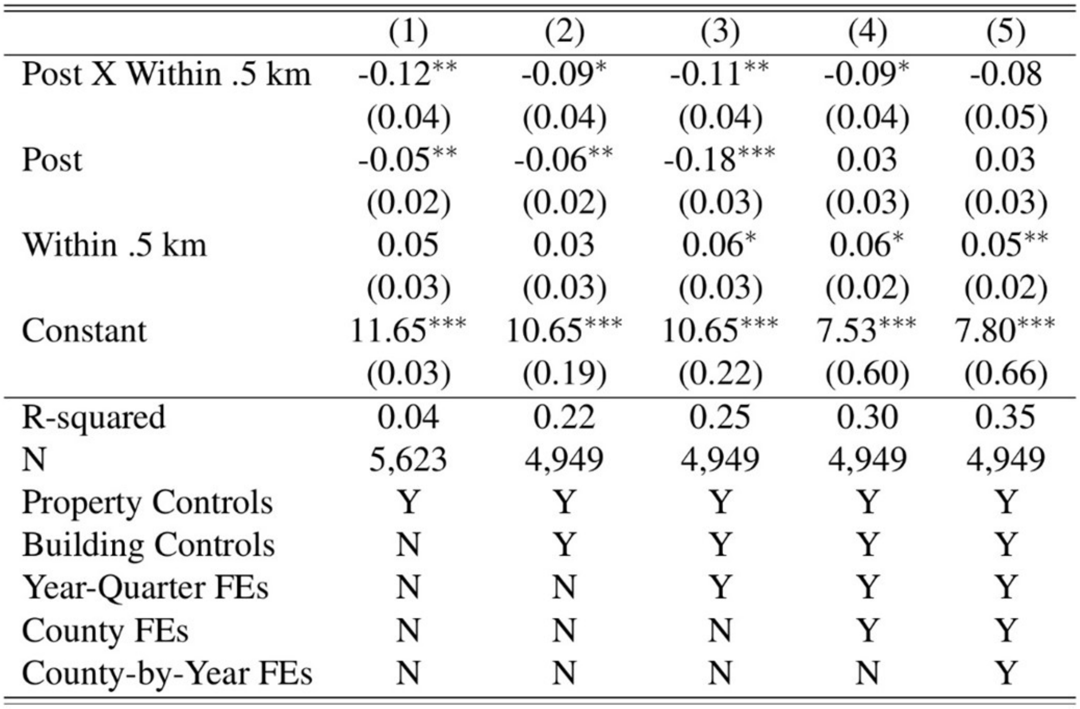
Table C1: Number of Transactions by Normalized Time and Treatment Group



	Treatment (0 to .5 km)	Control (.5 to 2 km)
-5	141	328
-4	123	434
-3	136	475
-2	129	589
-1	122	608
1	122	553
2	108	502
3	94	424
4	73	345
5	57	260
6	51	233
7	46	173
8	22	89
9	12	60
10	8	26

Note: Here the numbers in the left hand column represent the time in years when the transaction took place, relative to conclusion of construction of the nearest CREZ segment (e.g., -1 means that transaction took place within 365 days before the construction completion date).

Table C2: Effect Within .5 km, Five Year Post Period



	(1)	(2)	(3)	(4)	(5)
Post X Within .5 km	-0.12**	-0.09*	-0.11**	-0.09*	-0.08
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Post	-0.05**	-0.06**	-0.18***	0.03	0.03
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Within .5 km	0.05	0.03	0.06*	0.06*	0.05**
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Constant	11.65***	10.65***	10.65***	7.53***	7.80***
	(0.03)	(0.19)	(0.22)	(0.60)	(0.66)
R-squared	0.04	0.22	0.25	0.30	0.35
N	5,623	4,949	4,949	4,949	4,949
Property Controls	Y	Y	Y	Y	Y
Building Controls	N	Y	Y	Y	Y
Year-Quarter FEs	N	N	Y	Y	Y
County FEs	N	N	N	Y	Y
County-by-Year FEs	N	N	N	N	Y

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors are presented, and are clustered by county in columns 4 and 5.

Table C3: Effect on Encumbered Properties, Five Year Post Period

	Verified Intersection (1)	Radius l.t. Distance (2)	Radius l.t. Distance - 50m (3)
Post X Encumbered	-0.21*	-0.13*	-0.08
	(0.08)	(0.06)	(0.04)
Post X Within .5 km	-0.08	-0.08	-0.07
	(0.05)	(0.05)	(0.05)
Post	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)
Within .5 km	0.05**	0.05**	0.05**
	(0.02)	(0.02)	(0.02)
Encumbered	0.03	-0.00	-0.01
	(0.09)	(0.10)	(0.06)
Constant	7.80***	7.80***	7.79***
	(0.67)	(0.67)	(0.67)
R-squared	0.35	0.35	0.35
N	4,949	4,949	4,949
Number Encumbered	69	74	120
Number Encumbered Post	27	29	53

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. The model specifications and standard errors are identical to column 5 in Table 3.4. In this table, the columns differ on how encumbrance is operationalized. In column 1, the encumbered binary equals 1 only if I could verify that a CREZ line crossed the transacted property by mapping the lines and TNRIS parcel polygons. I additionally classify properties as encumbered if their inferred radius is less than their distance to the CREZ lines (column 2), or less than their distance to the CREZ lines minus 50 meters (column 3).

Table C4: Effect Within .5 km, 3 Year Pre and Post Period

	(1)	(2)	(3)	(4)	(5)
Post X Within .5 km	-0.17***	-0.15***	-0.14**	-0.12*	-0.11*
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Post	-0.02	-0.01	-0.14***	0.04	0.08*
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Within .5 km	0.10**	0.08**	0.09**	0.07*	0.06**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Constant	11.66***	10.59***	10.26***	7.58***	7.94***
	(0.03)	(0.22)	(0.20)	(0.35)	(0.42)
R-squared	0.03	0.21	0.25	0.29	0.33
N	3,862	3,429	3,429	3,429	3,429
Property Controls	Y	Y	Y	Y	Y
Building Controls	N	Y	Y	Y	Y
Year-Quarter FEs	N	N	Y	Y	Y
County FEs	N	N	N	Y	Y
County-by-Year FEs	N	N	N	N	Y

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. Robust standard errors are presented, and are clustered by county in columns 4 and 5.

Table C5: Effect on Encumbered Properties, 3 Year Pre and Post Period

	Verified Intersection (1)	Radius l.t. Distance (2)	Radius l.t. Distance - 50m (3)
Post X Encumbered	-0.49	-0.32	-0.23
	(0.29)	(0.18)	(0.15)
Post X Within .5 km	-0.09*	-0.09*	-0.08
	(0.04)	(0.04)	(0.04)
Post	0.08*	0.08*	0.08*
	(0.03)	(0.03)	(0.03)
Within .5 km	0.05**	0.05*	0.04*
	(0.02)	(0.02)	(0.02)
Encumbered	0.30	0.19	0.12
	(0.17)	(0.09)	(0.08)
Constant	7.96***	7.95***	7.94***
	(0.42)	(0.42)	(0.42)
R-squared	0.33	0.33	0.33
N	3,429	3,429	3,429
Number Encumbered	39	44	75
Number Encumbered Post	20	22	38

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The outcome is the natural log of transaction prices in real 2018 dollars. The model specifications and standard errors are identical to column 5 in Table 3.4. In this table, the columns differ on how encumbrance is operationalized. In column 1, the encumbered binary equals 1 only if I could verify that a CREZ line crossed the transacted property by mapping the lines and TNRIS parcel polygons. I additionally classify properties as encumbered if their inferred radius is less than their distance to the CREZ lines (column 2), or less than their distance to the CREZ lines minus 50 meters (column 3).

C.2 Creating Alternate Routes

I identify routes that were canceled and modified by PUCT using two sources. First, the final oversight report that RS&H (2014) compiled for PUCT contains maps of two line segments that connected the Kendall County substation and the Newton Substation in Lampasas County. I digitize these two segments in ArcGIS by connecting the Kendall substation with the Gillespie County substation with one straight line, and the Gillespie County substation and the Newton Substation with another straight line. Second, the Clear View Alliance (2009) provides a map of the proposed "McCamey D-Kendall-Gillespie" line, which was rejected in favor of a line that ran to the south through Kerr county. Cohn and Jankovska (2020) discuss this proposed route at length and note that in official dockets, PUCT ordered the utility to identify a route that ran along an existing 138 kV right-of-way. I use the Clear View Alliance map to identify this existing right-of-way in the HIFLD data, which runs from Kendall and north through Gillespie, Mason, and Menard counties, and digitize it as an alternate CREZ route.

Because the canceled and modified routes only contain 19 transactions within .5 km that are not also within .5km of the actual CREZ lines, I digitize placebo routes near 16 actual segments that account for 93 percent of treatment observations. I do this by connecting their endpoints (which are substation locations) by perfectly straight lines, which serve as my placebo lines. Specifically, I connect:

1. The Carrollton-Upfield Substation in Dallas County to the Denton West Interchange in Denton County
2. The Roanoke Substation in Denton County to the Eagle Mountain Substation in Tarrant County
3. The Hicks Substation in Tarrant County to an unnamed substation near Boonsville in Wise County
4. The Parker Substation in Parker County to the Comanche Peak Substation in Somervell County
5. The Comanche Peak Substation in Somervell County to the Everman Substation in Tarrant County

6. An unnamed substation in Navarro County near Corbet to an unnamed substation near Abbott in Hill County
7. An unnamed substation near Abbott in Hill County to an unnamed substation near Iredell in Bosque County
8. The Brown substation in Brown County to the Killeen Substation in Killeen County

C.3 Counting Affected Properties

I superimpose the TNRIS data over the CREZ lines in ArcGIS, and find that there are 38,322 parcels across 80 counties that are within .5 km of the lines. I collapse the parcels to the property level using a unique property identifier. There are 2,208 parcels with a missing property id, and I trim them down to 2,148 properties based on the share of parcels in each county with complete data that are collapsed.

TNRIS does not provide data for twelve counties that host CREZ lines: Kerr, Mason, Crockett, Coleman, Montague, Clay, Cottle, Jack, Oldham, Hardeman, Briscoe, and Castro. For each of these twelve, I measure the total length in meters of the transmission lines that they contain, and also measure the total length in two neighboring counties with non-missing parcel data. For each missing county, I calculate the average number of encumbered properties per meter and the average number of unencumbered properties per meter across its neighbors, and multiply these averages by the length of lines in the missing county. This yields estimates of the number of missing encumbered properties and the number of missing unencumbered properties within .5 km of the lines.

The property use code field is missing for 5,303 inferred properties in missing counties and for 14,509 properties in other counties. Because I want to limit my analysis to residential and agricultural properties, I use the properties with non-missing use codes to calculate the share of properties in each county that are residential and agricultural. I calculate two separate sets of shares, one for encumbered properties and one for unencumbered properties. I apply the shares within county and encumbrance status to estimate the number of properties with missing use codes that are agricultural and residential. There are 56 counties where all affected properties have missing use codes. For these counties, I apply the shares of residential and agricultural properties from the closest neighboring county with non-missing codes.

These steps yield an estimated 37,695 residential and agricultural properties within .5 km across 92 counties: 22,774 are agricultural (7,710 encumbered and 15,064 nearby), and 14,921 are residential (656 encumbered, and 14,265 nearby).

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