An American Resource Curse?

Evidence from Eastern Coal Country

Elizabeth Mitchell Elder

Hoover Institution

Stanford University

emelder@stanford.edu

August 16, 2024

. * The author is a Hoover Fellow at the Hoover Institution at Stanford University. I gratefully acknowledge extensive feedback from Gabriel Lenz, Laura Stoker, Tali Mendelberg, Graeme Blair, Elizabeth Thom, Margaret Weir, David Broockman, Mark Schwartz, Steven White, and participants at the 2021 California Workshop on Empirical Political Science, UC Berkeley's Research Workshop on American Politics, the 2022 Colloquium on the American Political Economy, and the 2022 Research Conference on the American Political Economy.

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Abstract

Evidence suggests that local economies grow more slowly in areas of the United States dependent on resource extraction. Building on literature on the "resource curse," I present evidence that the consequences are also political: local governments in areas dependent on a resource industry shape public policy to the demands of that industry, leading to underinvestment in government capacity and public goods. Focusing on the case of Eastern Coal Country in the 20th century United States, I present evidence that the coal industry hindered the growth of local government capacity. Former mining areas' current decline may be rooted in these historical developments. This case is a particularly stark manifestation of a broader problem embedded in the "fiscal federalism" of the United States: when localities' economies are dominated by an elite group with coherent interests, local governments must cater to those interests—even when the elites' demands keep local governments poorly-funded and weak.

Word Count: 9,671

1 Introduction

Literature on the resource curse suggests that places with economies centered on resource extraction develop less effective, less democratic political institutions. Judged against these expectations, the United States is considered a success story: despite heavy resource extraction in the early 1900s, a diversified national economy and husbanding of the profits through effective institutions allowed the country to prosper and democratization to continue (David and Wright 1997; Wright 1990).

I argue that viewing the United States as having avoided the resource curse misunderstands the nature of business power in American politics. In a federal system, *local* governments control many policy outcomes of importance to corporate actors, and *local* economies are far more prone than the national economy to being dependent on the success of a single industry. If dependence on mining shaped political institutions in the United States, local governments are the best place to look for evidence.

Indeed, the local areas of the United States that provided the coal fueling its 20th century prosperity—in general, the coal fields of Appalachia and the Illinois Basin—show clear signs of an economic resource curse. Coal-producing areas are poor and underdeveloped compared to their neighbors. Their economies were buffeted by the fluctuations of coal prices for 150 years and have now been debilitated by the near-disappearance of coal employment. Few coal-producing counties have successfully developed other industries to replace mining.

This paper provides evidence that coal-producing areas in Appalachia and the Midwest have also been subject to a political resource curse. Local governments in mining areas are lower in capacity than their neighbors because their economies historically depended on

resource extraction. The profits earned from mining accrued mainly to relatively few private elites; the gains to be had from mining incentivized these elites to invest in controlling local governments. Mining interests received many policy concessions from local governments, including low taxes on mining activity. As a result, local governments in coal country were and remain chronically underfunded and underdeveloped compared to their neighbors.

The political resource curse is a particularly stark example of a broader problem embedded in the "fiscal federalism" of the United States: when localities' economies are dominated by an elite group with coherent interests, local governments must cater to those interests—even when the elites' demands keep local governments weak and poorly-funded, as may be the case when the dominant elite is an industry group with more interest in preventing taxes and interference than in the provision of services. Local governments, which represent smaller constituencies with a less diverse ecosystem of economic and political interest groups than higher levels of government, are vulnerable to capture by a single powerful interest in a way that higher levels of government are not. The results presented here suggest that the potential harms of this capture go beyond control over policy outcomes—capture by economic interests can undermine the very capacity of institutions of local government.

This paper proceeds in two parts. First, I asses how the case of eastern coal country fits within the resource curse paradigm, generate predictions for how coal's dominance affected local political institutions, and discuss the plausibility of these predictions using qualitative work from the region.

Second, I test the hypothesis that areas of Appalachia and the Illinois Basin with economies dominated by coal mining developed smaller, lower-capacity local governments. Using a differences-in-differences design, I demonstrate that coal-dominated counties' local govern-

ments fell behind their neighbors in size starting when the coal industry arrived. A complementary descriptive analysis suggests that while coal-dominated areas' governments caught up to other areas' in public employment by the postwar period, they continued to collect less in tax revenue and spend less money into the 21st century. This suggests that, in American coal country as in resource-dependent places around the world, dependence on extractive resources leads to weak government institutions.

2 Theory

Countries with economies that depend on resource extraction are, on average, poorer than countries that depend on other sectors or have more diversified economies. This pattern was first empirically documented by Sachs and Warner (1995), and it has since spurred a large body of work by an ever-growing set of researchers (see Badeeb, Lean, and Clark 2017; Frankel 2010; Van der Ploeg 2011, for reviews). Some disagreement remains, but a consensus has developed that the economies of resource-dependent countries grow more slowly than others. This is the "resource curse."

Why might this be? Initially, explanations centered on purely economic consequences of resource extraction, like its effects on currency value and investment in other sectors of the economy (Sachs and Warner 1995). However, most analysts now agree that there is a political dimension to the resource curse.

Resource extraction pumps money into the pockets of whomever—be they corporate investors, government officials, or rebel groups—can claim a share of the profits. This money can affect political processes in a number of ways; for example, it can buy support for

authoritarian (or democratic) regimes, bribe government officials for favorable policy, or spur conflict over territorial control (see for example Brollo et al. 2013; Dunning 2008; Ross 2015, for a review). Different effects occur in different contexts, but on average, countries with resource-dependent economies have more corrupt and less democratic governments than other countries (Deacon and Rode 2015). This is the "political resource curse."

In the late nineteenth and early 20th centuries, the United States was heavily engaged in resource extraction. It was responsible for 39% of the world's coal production in 1913 and was the world's leading producer of many other natural resources (David and Wright 1997). The United States seems to have avoided the resource curse; it was and remains a prosperous and democratic country, because the national economy remained diversified even during its period of greatest resource extraction. However, the regions that produced the country's coal at its resource-extracting peak were largely dependent on mining, and they remain poorer and less developed than their neighbors. It was here, I argue, that the resource curse had its bite: while the country overall did not suffer an economic or political resource curse, coal-producing areas did.

2.1 An American Resource Curse?

Since observers first noted that many countries blessed with rich natural resource deposits were, counterintuitively, fairly poor, substantial evidence has accumulated about when and why this is true. Applying this evidence to the case of local coal mining in the United States is not always straightforward: much of it relies on comparing countries, not subnational units, and much of it focuses on oil extraction. But taken together, this body of work suggests an

economic and political resource curse could exist in the case of interest here.

As a starting point, the basic empirical pattern that indicates a "resource curse" is present in U.S. coal localities. Douglas and Walker (2017) find that Appalachian counties endowed with greater coal resources experienced lower income growth than otherwise similar but less coal-rich counties. Matheis (2016), examining a longer time period and larger set of counties, shows similar findings: after an initial boost to the population and economy, coal mining led to less growth over time. So, there is direct evidence that coal mining-dependent localities in the United States experienced slower long-run economic growth than they would have had without mining. What might explain this, given what is known about how the resource curse works elsewhere?

Central to the economic story of the resource curse is that resource extraction alone cannot power an economy in the long term. Natural resources are inevitably exhausted. For an endowment of coal, oil, or diamonds to make a place durably more prosperous, the extraction must spur or fund the development of more "renewable" sectors of the economy, like manufacturing. In the United States as a whole, this is in fact what happened: the massive extraction of coal enabled the development of a large manufacturing sector in regions adjacent to mining areas, fueling national economic growth for decades.

But many countries with large resource endowments are not able to develop other sectors of their economy. One reason for this is the so-called "Dutch Disease:" the growth of extractive industries tends to crowd out investment in other parts of the economy[1]. Another

1. In its original formulation, the Dutch Disease referred to growth in the resource-extracting country's exchange rate brought on by international demand for its resource, making investments in other industries relatively less competitive (Badeeb, Lean, and Clark

is that economies dependent on the price of a single commodity tend to be quite volatile, and this volatility makes it difficult for governments, firms, and workers to plan capital investments that could grow other sectors of the economy (Van der Ploeg and Poelhekke 2009).

To illustrate how these market effects could apply at the local level in the United States, imagine a county with substantial coal deposits. When coal prices are high, competition and thus wages for coal workers are high, drawing many people in the local economy into the mines and boosting local incomes. This would make retaining workers difficult for a local manufacturing firm unable to compete with coal's flush wages; the firm would struggle to produce goods as cheaply as a firm in a place without wage pressure from coal. A local government's coffers might benefit from the area's economic boom, and they could plan improvements to the local schools with this windfall. In a matter of weeks, though, the price of coal could plummet. The manufacturing firm's ability to hire and the government's ability to fund a school would suddenly reverse. This volatility, and the prospect of resource booms making other industries less profitable, make it difficult for resource-endowed areas to maintain public and attract private investments to diversify their economies.

Thus, the well-known economic mechanisms for the resource curse could apply to the case of coal communities in the United States. There is suggestive evidence of these from prior work: Matheis (2016) finds that coal growth crowds out investment in manufacturing over the long term, and Douglas and Walker (2017) find that educational attainment—an indicator of investment by government or citizens in skills applicable outside coal mining—is

2017) Applied to the subnational level, a similar pattern can occur due to rising wages in the resource sector (Douglas and Walker 2017)

lower in mining areas than otherwise-similar parts of Appalachia.

But these economic pathways alone are increasingly seen as insufficient to explain lower economic growth in resource-endowed areas. This is in part because countries that developed their extractive industries *after* developing strong political institutions have seen few negative effects from their endowments. Countries without strong preexisting institutions, though, see worse economic growth. What's more, places with weak institutions can find themselves in something of a trap: resource extraction can undermine efforts to strengthen them. In particular, resource extraction can preserve incumbent elites' power (especially authoritarians), increase corruption, and spur civil conflict[2] (Ross 2015).

Might the political aspects of the resource curse apply to local governments in mining areas of the United States? The answer is not at all clear. Though a variety of mechanisms for political effects have been proposed, almost all rely on one condition: that governments profit from resource extraction. Governments earning substantial revenue (through taxation or, especially, direct ownership of the resource) from resource extraction can use this money to buy support, with bribes or with public goods, without raising taxes from their citizens. Governments that need not extract much in taxes from their citizens have less reason to develop bureaucratic capacity, and they have less reason to spend in ways that are responsive to citizen demands. Public officials drawn into office by the prospect of privately capturing

2. I will set aside the notion of civil conflict here, as the United States had sufficiently strong institutions at the time coal mining began to prevent the emergence of organized groups competing for control of resources. Interestingly, Couttenier, Grosjean, and Sangnier (2017) find evidence that *interpersonal* violence is higher in parts of the United States where mineral resources were discovered before the arrival of formal institutions, but these places are almost entirely west of the region of interest here.

some of these resource revenues tend to have worse incentives and qualities.

The link between the political resource curse and these revenue effects is quite strong: the "rentier effects" that result are considered the curse's primary mechanism in the theoretical literature (Deacon and Rode 2015; Robinson, Torvik, and Verdier 2006), and some empirical work finds that resource extraction hurts institutions most, or even only, when resources are government-owned (Andersen and Ross 2014; Luong and Weinthal 2010). In the eastern United States, though, virtually no coal resources were owned by governments (David and Wright 1997). The region's mineral wealth was largely owned by large corporations located in other parts of the country. What's more, local governments had no ability to tax resource extraction and little ability to tax resource wealth until the mid-to-late 20th century. In other words, there was no excess resource revenue "cursing" officials in mining areas with the ability to use poorly-monitored public funds for personal or political gain.

The absence of resource rents accruing to governments and officials in the eastern United States closed off two pathways for their development: a "cursed" path, in which graft-motivated officials freely spent torrents of coal wealth to maintain their hold on power, undermining institutional development and public accountability; but also a "blessed" path, in which strong institutions channeled excess coal wealth into public programs encouraging long-term growth, perhaps even strengthening citizens' relationship with their government.

Instead, I argue, mining areas developed low-capacity governments because the accrual of resource rents to *private* actors allowed those actors enough political influence to prevent governments from developing the ability to tax them. Local governments in the eastern United States lacked the capacity to tax coal production and wealth when the industry arrived. This incapacity was locked in when coal companies, by dominating local economies

and profiting nearly untaxed by local governments, accrued the political power to prevent changes to the status quo. In this way, we can still understand resource rents—albeit accruing to private, not public, actors—as preventing the development of fiscal capacity. But unlike its traditional expression, where government coffers remain filled due to resource rents despite this lack of capacity, U.S. mining areas' governments remained poor.

The next section provides some historical background on the case of coal mining in the eastern United States in order to ground this argument and provide initial evidence before turning to a quantitative test.

2.2 The Case: Eastern Coal Country

Six states accounted for 83% of the United States' coal production in 1919: Pennsylvania, West Virginia, Kentucky, Illinois, Indiana, and Ohio (U.S. Census Bureau 1922). Though coal production today has moved westward, for the first half of the 20th century, the heart of coal country was in Appalachia and the Illinois Basin.

Before coal production became large-scale and mechanized, coal-producing areas were generally sparsely populated by subsistence farmers. For example, Logan County, West Virginia became one of the nation's largest coal producers in the 1900s, with 75% of its non-farm labor force employed in the coal industry in 1920. In 1880, however, the coal industry had not yet arrived. There was one incorporated town; the county's total population was 7,300, and its local governments employed two people. Between 1880 and 1920, as the region's coal industry developed, Logan County's population grew by more than 5 times to more than 41,000—more than live there today.

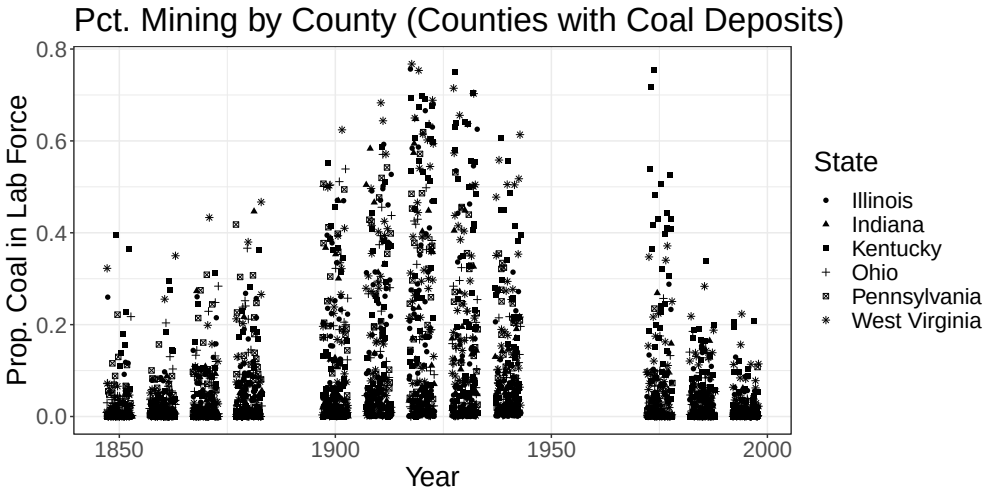

Figure 1: **Trends in County Coal Employment.** Each point represents the proportion of a county's non-farm labor force employed in the coal industry in a year. Dots are jittered along the horizontal axis (time). Only counties with some coal deposits are included.

The period of coal counties' greatest population growth coincides with the period of highest coal employment, the decades around 1920. Figure 1 shows the proportion of the labor force employed in the coal industry between 1850 and 1995[3] in counties with some coal deposits. Though a handful of counties had coal-focused economies already by the 1880s, the period between 1900 and 1930 saw the largest number of counties focus their economies on mining. Some of these places remained invested in coal into the 1970s, but by the end of the twentieth century, even the most coal-focused places saw 20% or less of their workforce in the industry.

The growing populations of coal mining areas were settled in distinctive ways. Even as these areas became more prosperous and populous, they remained rural in character; coal

3. In 1940 and earlier, data is from the United States Census; in 1975 and later, data is from the Quarterly Census of Employment and Wages.

miners and their families often lived near the mines where they worked, in remote areas with few other options for employment (Green 2011). Many miners—60% in West Virginia, up to 80% in Eastern Kentucky—lived in "company towns," settlements built and owned by the coal companies to house mine workers. These towns were often unincorporated and remote, far from centers of population and with limited access to transportation networks.

As is often the case in resource-rich areas, many coal-producing counties in Appalachia and the Illinois Basin developed economies specializing in coal mining, to the exclusion of other industries (Sachs and Warner 1995). How might an economy dependent on mining have affected governing institutions in eastern coal country, absent the rentier effect mechanism so common in other resource-dependent areas?

First, mining specialization is associated with the concentration of economic power (Isham et al. 2005). Resource extraction requires capital investment, but not an especially large or skilled labor force[4], and mined resources are generally exported to other areas for processing and use. Therefore, the profits of resource industries flow fairly narrowly to investors and ownership of mining companies, not broadly across a large labor force or multiple sectors of the economy—or, as discussed above, to governments. As resource extraction comes to dominate an area's economy, economic power becomes concentrated in the area in the hands of relatively few industry elites.

Second, because resource extraction tends to dominate the economies of areas that contain resource deposits (Sachs and Warner 1995), extractive industries are more likely to be

4. Coal mining in the United States through the first half of the twentieth century relied on a reasonably large but low-skilled labor force; other resource industries and modern coal mining rely on smaller but higher-skilled labor forces.

the dominant economic interest in their jurisdiction than are other industries. Governments in resource-rich areas often depend on the jobs and money mining companies bring. In return, mining companies depend on governments in resource-rich areas for favorable policy, since they generally cannot move their operations in search of a better policy environment. This means mining companies had the power and incentive to invest in controlling policy outcomes.

Historical evidence from eastern coal country corroborates the idea that coal elites were especially economically powerful in their areas. Ownership of land and mining rights was concentrated in relatively few corporations (Gaventa 1982; Goldberg and Power 1972; Leistritz and Voelker 1975). Wages for mine workers were relatively low, and there were few options for alternative employment in mining areas. In coal towns, especially when unincorporated, mining companies had substantial control over residents' housing, the prices of goods, and service provision (Green 2011). Even in more conventionally-structured towns, though, the upper and middle classes were either directly involved in mining ventures or had commercial interests that aligned with mining (Gaventa 1982), limiting the development or articulation of political dissent.

Coal companies supplemented these forms of economic power with outright political corruption. Perhaps because they lacked leverage over local governments since they could not credibly threaten to move their business elsewhere, or perhaps because of political competition from unions, coal companies used an array of tactics to strengthen their political influence. Vote buying seems to have been common in coal towns, sometimes through the mid-20th century (Hevener 2002; Ricketts 1998; Roberts 1904; Walls and Stephenson 1972), and companies used the press and legal system to suppress organized opposition (Kiffmeyer

1998; Ricketts 1998).

This power should have allowed mining companies to exert substantial influence over local government policy. I argue they used this influence to limit the capacity of local governments—that is, the ability of governments to collect revenue and implement public policy.

Though preventing government capacity may seem in some ways to be counter to coal elites' preferences—corporations need infrastructure for transport, management needs schools for their children, etc.—I argue that in places where a single industry is the primary source of economic activity, industry elites would rather construct public goods themselves than pay the tax revenue to allow local governments to do so. There are strictly financial reasons for this: in economically diverse areas, even tax-averse companies can support increasing fiscal capacity because doing so would allow governments to ensure other companies are paying their fair share of public goods. In mining areas, where there were few other taxpayers, this logic does not apply.

But there are also political reasons for mining companies to keep capacity out of the hands of governments. Doing so vests decision-making power over the nature of public goods in the company; what's more, it prevents the local government from developing the fiscal or administrative capacity to construct these public goods itself, capacity which could threaten the company's autonomy if it developed. In other words, coal companies had a preference for governments that lacked the personnel to create and enforce policies that could limit companies' autonomy, even if the public demanded those policies—a logic similar to that of slaveholders in the post-Reconstruction South (Suryanarayan and White 2021a).

The policy area of criminal justice illustrates this logic well. Coal companies were very

interested in using the police and courts to arrest, jail, and prosecute union organizers, environmental activists, and journalists (Caudill 1962; Fishback 1995; Gaventa 1982; Kiffmeyer 1998). Though police were almost always allied with coal companies in these goals, there are a few cases of sheriff's offices or other police positions being held by union- or worker-sympathetic personnel, with serious consequences for coal companies (Prosser 1973). Rather than allowing places to build the capacity of public police and sheriff's departments, which could slip out of coal companies' control with a single election, coal companies often employed their own private police corps or funded officers in local police departments that they could then control.

Another illustration of the preference for funding public goods rather than being taxed for them is the case of unincorporated coal towns. Coal corporations' reasons for constructing coal towns were myriad, but their prevalence reflects that companies would rather construct towns from scratch, paying the upfront costs to build public services and infrastructure and governing them themselves, than cede land, tax money, and control to an elected local government to construct them.

Coal companies faced an institutional environment conducive to avoiding taxation. When the coal industry was most dominant, localities largely depended on the property taxes they collected for funding (Peterson et al. 1981); cities and counties had few (or no) other options for revenue, including severance taxes and resource rents, so they could only tax coal through taxing its property value. Governments had few resources to spend on and little expertise in valuing mineral wealth, so companies could easily hide, underreport, or dispute the value of their holdings (see, e.g., "Fair Assessment of Coal Lands Difficult: Kentucky Mining Counties Profit By Inability of Taxing Bodies to Determine Value of Property Known

Only to Experts" 1920; "Is Coal Paying Fair Share?" 1965). This status quo allowed coal companies to minimize property taxes for decades in many mining areas.

I expect that lower government capacity continued even after the coal industry declined. If coal interests were able to keep property tax revenues low, local governments would have been less capable of building strong and effective institutions during a crucial period for their growth and professionalization—not only because of these places' developmental stage, but also because of the advances in local government capacity taking place across the country during the Progressive Era. This early disadvantage in capacity would limit local governments' ability to grow once the industry left, taking its tax revenue with it.

In studying government capacity, I will focus on the outcomes of local government employment, revenue, and spending. Though capacity has many other dimensions, these three represent important aspects of a local government's ability to set public policy: setting and collecting taxes, employing personnel to carry out government functions, and spending money on public policy are all core functions.

In the empirical sections that follow, I use quantitative analyses to test whether developing a coal-dependent economy leads to smaller local governments. These analyses build on—and are no substitute for—extensive work by regional specialists on the relationship between coal mining and government capacity. This is an old topic in qualitative, historical, and public policy work on eastern Coal Country.

There is a widespread sense, substantiated by local evidence (Smith, Ostendorf, and Schechtman 1978, Appalachian Land Ownership Task Force, 1983), that coal companies pay little in taxes, and that this has led to underinvestment in roads, schools, and healthcare (Caudill 1962; Gaventa 1982). In fact, in the 1970s, the West Virginia State Tax Department

took the official position that "the coal industry's support of local government and schools, through property taxes, has not been realistic given the extent of the industry's mineral and fee property holdings" (as quoted by the Appalachian Land Ownership Task Force, 1983).

This provides valuable support for the notion that coal companies have the will and the means to succeed in keeping local governments low in capacity. In the analyses that follow, I seek to complement this evidence in three ways: by comparing local government size in counties across the entire region of Appalachia and the Illinois basin; by studying the timing of the emergence of the gap in government size between coal-and non-coal counties; and by employing quantitative methods to rule out alternative explanations for the size of coal areas' governments.

3 Empirics

3.1 Data and Measures

To test the hypothesis that a coal-dependent economy led to weaker government institutions in the coalfields of the Eastern United States, I draw on data on economic and political conditions at the county level. Counties are a mutually exclusive and exhaustive unit of geography—that is, all land in this region is part of one and only one county—and are small enough in the states of interest that county-level data captures fairly local conditions. I limit my analysis here to the Eastern coal country region discussed above, which includes the six states of Illinois, Indiana, Kentucky, Ohio, Pennsylvania, and West Virginia.

I measure a county's dependence on coal using employment data drawn from the full-

count Censuses from 1850-1940, the period covering the peak of coal employment in this region. For each county, at each Census, I divide the number of people employed in the coal mining industry by the total number of non-farm, non-domestic laborers in the county. This measure captures the extent to which coal provides the livelihoods of county residents and therefore reflects the power of coal, relative to the area's other industries, in the local economy.

I measure the size of local government institutions using two kinds of data. First, using the same 1850-1940 full-count Census data, I calculate the number of people in each county whose industry of employment is classified as "local government administration." This includes people whose full time job (or the job that provided most of their income) was provided directly by the local government, including police officers, firefighters, lamplighters, clerks, assessors, judges, and elected officials, but excluding postal workers and teachers.

Second, the Census of Local Governments provides information on the revenue from property taxes, expenditures, and employees of local governments in the United States over the period from 1957 to 2002. Earlier versions of the survey were conducted roughly decennially from 1880-1932. I sum the property tax revenue, expenditures, and employment of local governments across all local governments in a county—including the county government and any municipal governments—as measures of local government strength. These data also measure the total value of all property in each county for each year; since the revenue measure captures revenues from property taxes, this directly measures the county's tax base.

The three sections that follow all assess the relationship between coal industry dominance and local government size. The first presents simple descriptive statistics on the

size of local governments in coal and non-coal counties over time. The second presents a difference-in-differences analysis, showing that counties where the coal industry developed abruptly diverged from non-coal counties in government size at the time the industry arrived. The third extends these results by showing the relationship between coal industry size and local government employment, revenue, and spending throughout the 20th century. Results available in the appendix section A.2 replicate these findings using an instrumental variables design.

3.2 Government Growth in Coal and Non-Coal Counties

Local governments in the United States grew substantially between 1850 and 1940, and even more dramatically after World War II. A first step in understanding the relationship between a coal-dependent economy and government size is to compare the trajectories of local government growth among counties with and without sizeable coal industries.

To simplify this comparison, I limit the sample to two types of counties: those with large coal industries in between 1910 and 1930 (defined as having 20% or more of their non-farm labor force working in the coal industry in at least one of these years; n=116) and those without appreciable coal industries (less than 1% in the industry; n=270).

I capture government size using the number of local government employees in each county. However, coal and non-coal counties are located in different states with different roles for local governments, and the groups also differ in population over time. To account for this flexibly, I measure government size using the residuals of an OLS regression of the logged number of government employees on a three-way interaction between county's logged population, a set

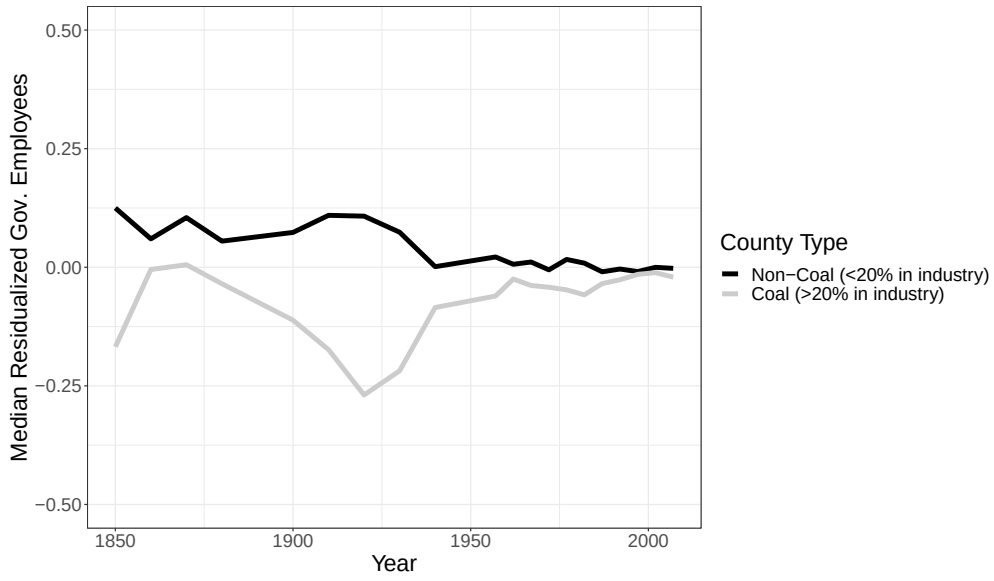

Figure 2: **Government Employment in Coal and Non-Coal Counties Over Time.** On the y-axis is a measure of government employment after accounting for state and population differences. The x-axis shows time. Lines represent residualized government employment in the median coal and non-coal county in each year.

of state fixed effects, and a set of period fixed effects. This gives a measure of the county's local government size, after accounting for differences in population and state between coal and non-coal counties over time.

Figure 2 presents the residualized government employment measure in the median coal and median non-coal county between 1850 and 2002. Early in this period, the median coal county employed fewer people in government than the median non-coal county. This gap widens steadily after 1860, with coal counties' governments employing fewer people; the gap is largest around 1920, and it all but closes as the twentieth century ends. Throughout the 20th century, coal counties' local governments employed fewer people than non-coal counties, after accounting for differences in state and population.

This figure suggests that counties which became heavy coal producers diverged from other places in government employment around the time the coal industry was becoming most powerful, from the early 1900s through the 1920s. However, this is insufficient to conclude that the growing coal industry was the cause of the divergence. Areas could have selected into specializing in coal production for many reasons, and this selection process, rather than the specialization itself, could have produced differences in governments between coal and non-coal counties. The next section takes up this concern.

3.3 Differences in Differences Analysis

As the coal industry expanded over the late 19th and early 20th centuries, some coal-producing areas experienced steady and gradual growth in mining employment. In other areas, the industry arrived abruptly. For example, in 1910, nineteen of the 10,500 residents of Harlan County, Kentucky worked in the coal industry. By 1920, 5,267 worked in coal mining, representing 69% of the county's non-farm labor force.

In all, there are 23 counties in Appalachia and the Illinois Basin that developed dominant coal industries over a short time period. Each of these counties, between two consecutive Censuses in the 1850-1940 period, went from less than 5% of their labor force in coal mining to at least 20%, and eventually reached at least 40%. In all cases, coal became the county's largest industry at some point in (and often throughout) the period between 1880 and 1940. For the purposes of this analysis, this group of 23 counties serves as the "treatment group": they began untreated (i.e. no coal industry) and, at an identifiable point in time, received the "treatment" of developing a large, dominant coal industry.

To assess the relationship between coal specialization and local government size, I compare changes in pre- and post-treatment government size in these treated counties to changes in government size at the same time for counties that did not receive the treatment; that is, a differences in differences design. I define the untreated counties as those in which no more than 5% of the labor force worked in the coal industry at any point between 1850 and 1940 (n=280)[5].

The outcome of interest in this analysis is the same as in the previous section: a measure of the total number of local government employees in each year, residualized for state, population, and time differences. However, to capture the relationship between these variables and government size absent industry intervention, the regression model used to produce the outcome here is trained only on control counties. I expect that after the coal industry arrives in treated counties, governments in treated counties (as measured with this residualized figure) will grow more slowly than governments in control counties.

The key identifying assumption of this analysis is that of "parallel trends:" the potential outcomes under the control condition of both groups of counties move in parallel over the time period being studied. Here, this means that if the treated counties had never developed a coal industry, their governments would have grown at the same rate as the governments of control counties. Within states and years, and accounting for population size, I argue this assumption is plausible; as long as they are being compared to similarly-populated control units with the same state-delegated responsibilities, there is no reason to expect coal counties' governments to have grown differently from their neighbors absent intervention. Appendix

5. Section A.6 in the Supplemental Information includes a map of the treatment and control counties in this analysis.

Section A.3 presents tests that show no signs of pretreatment trends towards divergence, and in Appendix Section A.5, I construct several alternative control groups that make this assumption even more plausible and find similar results.

The coal industry arrived in each of the treated counties between 1870 and 1920. The earliest time period in which outcome data is available is 1850, allowing for the observation of pre-treatment trends in the outcome even for the earliest-treated units. Outcome data is available at 5 or 10 year increments through 1997 (except the 1940-1957 period), and to observe how treated and control units' governments grew over the course of the 20th century, I test for differences at every point following treatment. To account for the different timing of treatment receipt and the measurement of effects at multiple time points after treatment, I implement a difference in differences design with staggered treatment assignment and multiple time periods.

In some of the treated counties, the coal industry did not survive long after the downturn of the 1920s, and in most, coal employment accounted for less than 10% of the labor force by the 1980s. However, I consider treatment status here to be irreversible—that is, once a county is "treated" by developing a dominant coal industry, it remains treated in all following years, regardless of the industry's size in later periods. If I am correct that the presence of a dominant coal industry in the early 20th century shaped the development of local governments durably, even after the industry leaves, it cannot be considered comparable to a unit that never received the treatment at all.

I implement this analysis in `R` using the `did` package and the method described in Callaway and Sant'Anna (2020). Callaway and Sant'Anna's method breaks the treated observations into groups based on when each unit is treated and calculates treatment effects for

each time at which outcomes are observed. It then combines these group-time effects into weighted average treatment effects at each time point for which outcomes are observed for all the groups in my data, where the weights depend on the number of observations in each group[6].

Figure 3 presents the results. Each point represents the average treatment effect for groups at each time pre- and post-treatment; the bars represent bootstrapped 95% confidence intervals clustered at the county level. The x-axis shows the time relative to treatment. The leftmost two points, then, test for differences in differences between treatment and control groups 20 and 10 years before treatment occurs. These estimates are insignificant, suggesting trends in government employment in treatment and control units are not diverging before treatment is received.

The black coefficients in Figure 3 represent the average treatment effect across groups at each time point after treatment. The borderline significant negative coefficient at time 10 suggests that treated units, between the Census ten years before being treated and the Census ten years after being treated, gained less in residualized government employment than did control units over those same years. The estimates from further after treatment compare the change in treated and control units between their final pretreatment period and the period elapsed since treatment occurred. Though the statistical significance of the

6. This differs from a conventional two-way fixed effects method for estimating effects from a difference in differences design in three ways: first, it disaggregates estimates at different lengths of treatment exposure; second, it removes comparisons between already-treated units and those becoming treated; and third, it weights group-time effects by the number of observations, but not the variance in treatment assignment. See Callaway and Sant'Anna (2020) for further details.

estimates vary, coefficients remain negative until around 40 years after treatment, at which point estimates generally become close to 0.

A pooled test of the treatment effect over the periods from 0 to 40 years post-treatment suggests a significant effect of $-.30$ $(-0.53, -0.06)$ points. This represents a one standard deviation decrease in government size in treated units relative to control units. To put this in perspective, consider two Pennsylvania counties that differed by .3 points on the same scale in 1900: Berks and Lancaster, which both had about 159,000 residents at the time. Berks County's governments employed 117 people, while Lancaster's employed 156. This difference of about 25 employees per 100,000 residents is similar to the estimated treatment effect of the emergence of a large coal industry on a coal county.

Results available in the Supplemental Information repeat these tests excluding counties without any coal deposits and excluding counties with slaveowners to ensure the comparisons do not reflect other factors that could have set treated and control counties on different trajectories of government spending around this time. The results are robust to these different samples.

These results suggest that the arrival of a dominant coal industry depressed the growth of local governments in coal-producing areas. Several decades after the arrival of the industry, coal-dominated counties' government employment had returned to near parity with employment other places. Given the substantial decline in coal employment over the second half of the twentieth century, it is possible the dwindling of the coal effect is because the coal industry had left these areas. However, for decades after the industry arrived, coal mining areas showed distinctively slow government growth for their population size and states.

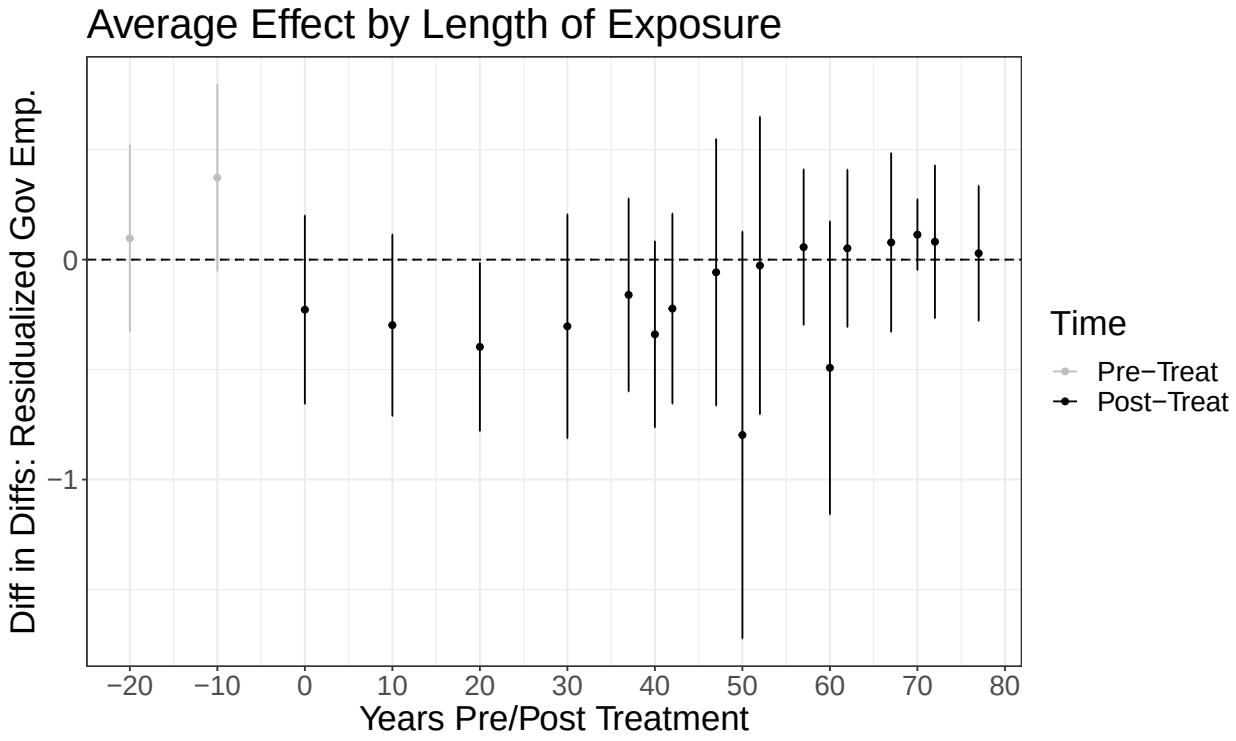

Figure 3: **Differences in Differences: Government Employment in Coal and Non-Coal Counties Over Time.** On the y-axis is the difference between treated and control groups in the difference in government employment from the previous time period. The measure of government employment is logged and residualized for state and population differences. The x-axis shows time relative to treatment; -20 represents 20 years before receiving treatment, while 20 represents 20 years afterwards. Bars represent bootstrapped 95% confidence intervals.

3.4 Extension: Correlational Results

In this section, I move away from the goal of causal inference and test the descriptive claim that counties with coal-dependent economies have weaker governments by a broader set of measures. The differences in differences analysis in the previous section was enabled by the long-term panel data available on government employment; data is more limited for other important features of local governments, including property tax revenue and expenditures. The difference-in-differences analysis also limited the sample of coal counties to 23 units with a distinctive set of useful features. A broader descriptive analysis can explore the relationship between a continuous measure of coal industry size in all counties and local governments in all the region's counties.

I test the expectation that coal-producing counties have weaker local governments by regressing measures of local government capacity on my labor force measure of coal dependence. I use three measures of local government capacity: number of government employees, property tax revenue collected, and total expenditures, collected at intervals between 1880 and 1992.

I regress each government outcome on the proportion of the county's non-farm, non-domestic labor force employed in the coal mining industry. I include state fixed effects to account for between-state differences in government structure, and to scale the outcomes to the population and funding base of the area, I control for logged population and logged total property value in the year of each government outcome. All outcome measures are logged. Observations are weighted by their population. The regression models are weighted least squares with standard errors calculated using the HAC adjustment proposed by Kelly (2020)

to account for spatial autocorrelation between observations.

The control variables I include in these models to make the local government outcomes more comparable across counties—population, total property value, and state—complicate the interpretation of these relationships. For example, there is suggestive evidence that coal companies influenced local officials to ensure their property was assessed at lower-than-appropriate values for tax purposes (Caudill 1962); controlling for property value could therefore be suppressing some of the relationship between coal dependence and tax revenue. However, given the often-substantial differences in population and property value between coal- and non-coal counties, the well-documented relationship between resource dependence and prosperity (James and Aadland 2011), and the differences in local government institutions between coal-heavy and less-coal-heavy states, these state, property value, and population measures are more helpful in creating useful comparisons than they are detrimental to interpretation.

For dates between 1880 and 1940, when the coal industry employed substantial proportions of the labor force in many counties in the region, I predict government outcomes in a year using coal employment from the most recent census. This captures the contemporary relationship between coal dependence and local government at a particular time. For outcomes measured starting in the 1950s, after coal employment has become negligible in most places, I predict government outcomes using coal employment in 1920, the industry's peak. This captures the longer-run relationship between coal dependence and local government after the industry's strength has waned.

Figure 4 presents the result for government employment. Each point represents the coefficient on the proportion of county labor force in the coal industry in a regression of the

logged number of government employees on coal employment, logged total population, logged property value, and state fixed effects. From around 1900-1940, counties more dependent on the coal industry had local governments that employed fewer people. By the 1950s, counties that had been more dependent on coal had similarly-sized local governments to others, though in some years the relationship remains negative and significant.

The results in Figure 4 echo those in the differences in differences analysis in the previous section: in the initial years after coal became powerful, coal counties had smaller local governments, but coal and non-coal counties converged in government employment as time passed and the industry employed fewer people.

Figure 5 presents the relationship between coal employment and property tax revenue. Again, the results suggest a negative association: counties with employment more concentrated in the coal industry collect less revenue from property taxes, even after accounting for their population size, property tax base, and state. Unlike in the case of government employment, however, this relationship is uneven in the early years but persists into the late 20th century, long after coal employment in the region declined.

Finally, Figure 5 presents the relationship between coal employment and total local government spending. This outcome measure is available at a more limited set of time points, but when it is available, the results are consistent with those from the models of property tax revenue: even after coal employment has declined, former coal-dependent counties' local governments spend less money.

Taken together, these results suggest that local governments in areas with more of their labor force employed by the coal industry tend to employ fewer people, collect less revenue, and spend less money. Patterns diverge in the later time periods between government

Figure 4: **Coal Employment and Government Employment.** Points represent the coefficient on the proportion of county labor force in the coal industry in a regression of the logged number of government employees on coal employment, logged total population, logged property value, and state fixed effects. Bars represent 95% confidence intervals with standard errors calculated with HAC adjustment for spatial clustering.

Figure 5: **Coal Employment and Property Tax Revenue.** Points represent the coefficient on the proportion of county labor force in the coal industry in a regression of the logged amount of government revenue from property taxes on coal employment, logged total population, logged property value, and state fixed effects. Bars represent 95% confidence intervals with standard errors calculated with HAC adjustment for spatial clustering.

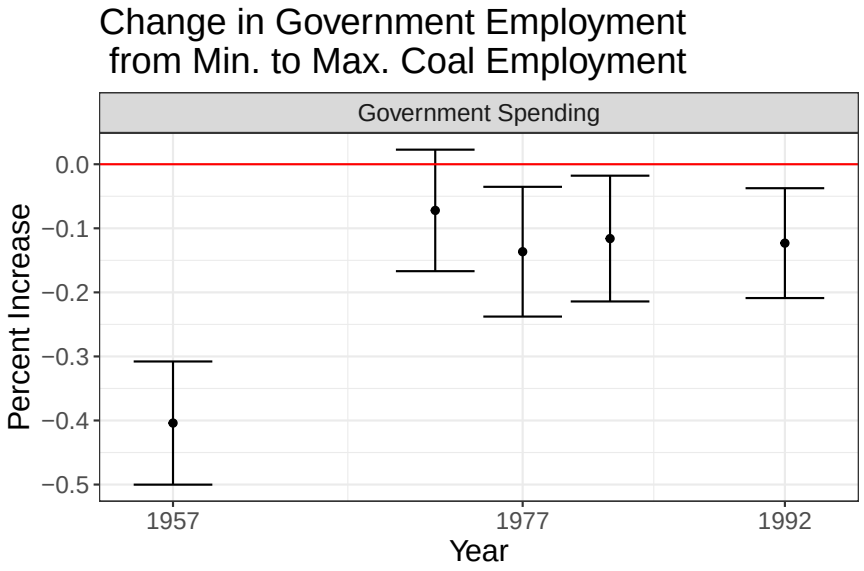

Figure 6: **Coal Employment and Government Spending.** Points represent the coefficient on the proportion of county labor force in the coal industry in a regression of the logged amount of government spending on coal employment, logged total population, logged property value, and state fixed effects. Bars represent 95% confidence intervals with standard errors calculated with HAC adjustment for spatial clustering.

employment and government spending and revenue: though employment in coal counties recovers after the industry declines, more coal-dependent counties continue to collect less in revenue decades afterwards.

4 Conclusion

In this paper, I have presented a combination of descriptive and causal analyses that document the relationship between a coal-dominated economy and local government capacity in the case of Eastern Coal Country in the United States. Drawing on literature on the resource curse, I predicted that localities with economies focused on coal extraction would have smaller, weaker local governments than comparable localities in different economic circumstances.

Comparable coal and non-coal counties employed similar numbers of people in local government until around 1880, the point at which the coal industry began to expand dramatically in this region. By the late nineteenth and early 20th centuries, counties with larger proportions of their labor force employed in the coal industry tended to have local governments that employed fewer people and collected less revenue. By the postwar period, when coal employment had declined dramatically, local governments in coal areas no longer employed fewer people, but they continued to collect less in revenue and spent less money.

A differences in differences analysis of government employment suggests a causal interpretation of these patterns. Using a sample of counties in which the coal industry developed suddenly at an identifiable time, I demonstrate that coal and non-coal counties, on a similar trajectory of government employment before the industry arrived, diverged after the indus-

try arrived. This suggests that the arrival of a large coal industry caused local government growth to slow.

Results available in the appendix section A.2 extend these findings using an instrumental variables design, using the raw tonnage of coal deposits in a county as an instrument for the size of its coal industry. The results do not systematically differ from those in these section. This suggests that lower government capacity in coal areas cannot be explained solely by some areas with coal deposits "choosing" to specialize in coal for reasons that could be correlated with government capacity, lending further support to the causal interpretation in the difference in differences design.

These results align with past work showing that dependence on resource extraction can lead to weakened government institutions. Applying literature on the resource curse to the case of coal in Appalachia and the Illinois Basin, I have argued that profits from resource extraction accrue to relatively few industry elites, who can invest these gains in extracting preferred policy from governments in mining-heavy areas. These preferred policies include low taxes and high autonomy for the industry, which constitute an overall preference for weaker local governments in mining areas. The results presented here suggest that in this context, mining elites were successful.

Coal is not the only industry that dominated local economies in this region. In other parts of Appalachia and the Midwest, substantial portions of counties' labor forces were employed in steel mills, automobile factories, and other manufacturing establishments. These industries also constructed company towns and avoided taxation by local governments. In other work, I test whether the model of industry dominance and local government explored here extends to cases beyond resource extraction.

Today, in many former coal-producing areas east of the Mississippi, mining is drawing to a close. Few of these places have developed diverse economies in the wake of coal employment's decline; economic, educational, and health outcomes in many are poor. A possible implication of the results presented here is that this state of affairs was not inevitable. The coal industry hindered the growth of local governments, perhaps preventing investment in human capital and infrastructure that could have better prepared these areas for the inevitable point when the coal was exhausted. Future work should examine more closely the link between local government capacity and the ability to weather industrial decline.

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A Supplemental Information

Contents

A.1 Map of Coal Employment

Figure 7 shows the spatial distribution of coal employment in the six-state region of interest in this paper in 1920. The counties in which coal employed the largest proportion of people are concentrated in two bands: the coal deposits of the Illinois basin, stretching across Illinois into Western Kentucky and north into Western Indiana; and Appalachia, stretching from the anthracite coalfields of northeastern Pennsylvania to the southwest through West Virginia and Ohio into Eastern Kentucky.

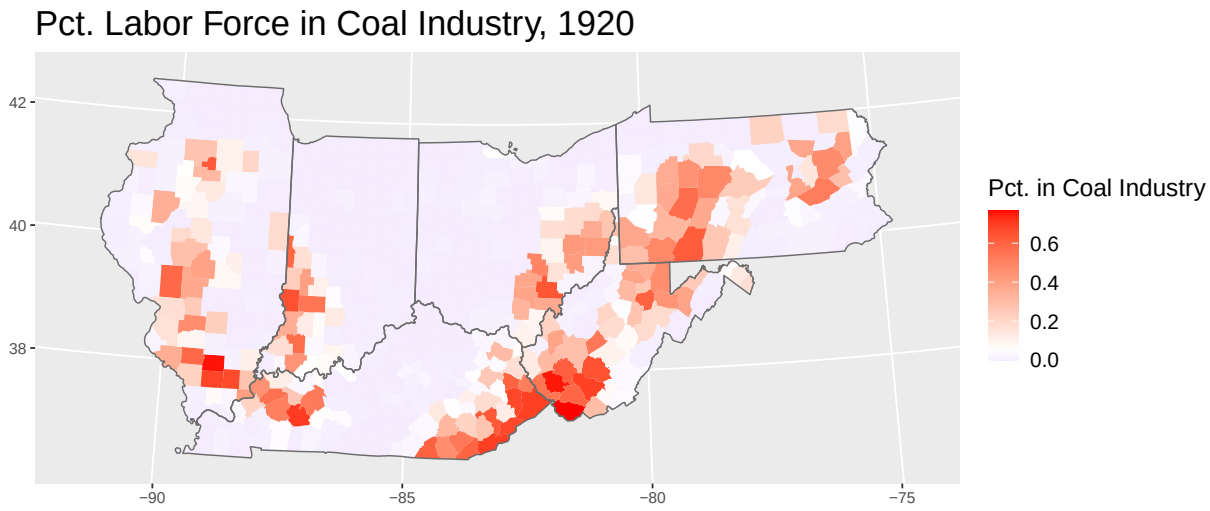


Figure 7: **Map of Coal Employment.** The color of each county represents the proportion of its non-farm, non-domestic labor force employed in the coal industry in 1920.

A.2 Instrumental Variables Analysis

The descriptive results presented in Section 3.4 suggest that counties more dependent on coal employment had smaller local governments. The differences in differences analysis in Section 3.3 supports a causal interpretation of this relationship in the case of government employment: for 30-40 years after the coal industry arrived, coal dependence depressed the growth of the local government workforce. However, the descriptive results for local government revenue and spending suggest a longer-term effect, with coal areas' governments

remaining poorer well into the late 20th century.

Is the long-term association between coal employment and government revenue/spending indicative of a causal relationship? The data available for these measures do not allow for a differences in differences design in this case. However, an instrumental variables analysis can provide a test of an important alternative explanation: that underdeveloped counties select into specializing in coal production, and this selection explains why these counties' governments are poorer later on.

Counties need substantial coal deposits to develop an economy that specializes in coal mining; the tonnage of coal deposits in a county is therefore an excellent predictor of the proportion of a county's workforce employed in coal ($F = 299$ in 1920). For a measure of coal deposits, I rely on the United States Geographical Survey's USCOAL database. USCOAL was an effort to measure the amount of coal resources present in all deposits in the United States. Compiled from a variety of surveys in sources between the 1950s and 1980s, USCOAL contains, when possible, an estimate of the total tonnage of coal originally present in each deposit; otherwise, it contains an estimate of the coal remaining in the ground in the 1950s. Aggregated to the county level, this data provides an estimate of the total coal resources available in a county.

In terms of relevance, coal deposits are a strong instrument for coal employment. The other key criterion for a good instrument is the exclusion restriction: are coal deposits associated with local government size only through their relationship with coal employment? It seems plausible that coal in the ground cannot affect local governments other than through the mining industry that develops to extract it.

One potential exclusion restriction concern is that in the United States, coal-endowed

areas tend to be relatively rugged and therefore remote. Because of spatial clustering in these variables, variation in coal deposits could be shared with variation in ruggedness, which could violate the exclusion restriction if ruggedness affects government size other than through the coal industry—a plausible concern. Though coal deposits are associated with rugged terrain in this region, this association disappears when accounting for state differences in ruggedness, so the inclusion of state fixed effects in these regressions should address this concern.

By instrumenting for coal-industry dependence with coal deposits, I am only using the variation in coal dependence that can be explained by tonnage of coal deposits when estimating the relationship between coal dependence and local government size. This removes the possibility that some coal-endowed counties choose to specialize in coal, while others don't, in ways that could explain the relationship between coal dependence and local government size.

To estimate the relationship between local government size and the instrumented measure of coal dependence, I regress the three measures of local government size discussed in the main text—number of government employees, amount of property tax revenue, and total amount of spending—on state fixed effects, population size, property tax base, and the proportion of a county's nonfarm labor force employed in the coal industry, instrumented by the tonnage of coal deposits in the county. As in the models in the main text, all observations are weighted by 1880 population size, and standard errors are calcluated using the HAC adjustment proposed by Kelly (2020). From 1880-1940, the independent variable is measured in the same year as the outcome and controls; starting in 1957, the independent variable is coal employment in 1920.

Results are presented in Figure 8, Figure 9, and Figure 10. Each figure presents the coefficient on coal employment in the instrumental variables model described above, as well as the OLS results from the main text for comparison. The coefficients in the instrumental variables models are generally less precisely estimated than the OLS coefficients (especially in earlier years, when the coal deposit instrument is weaker), but in most cases, the two are similar in magnitude; in a substantial majority of cases, the IV coefficients are within the confidence interval of the OLS estimates.

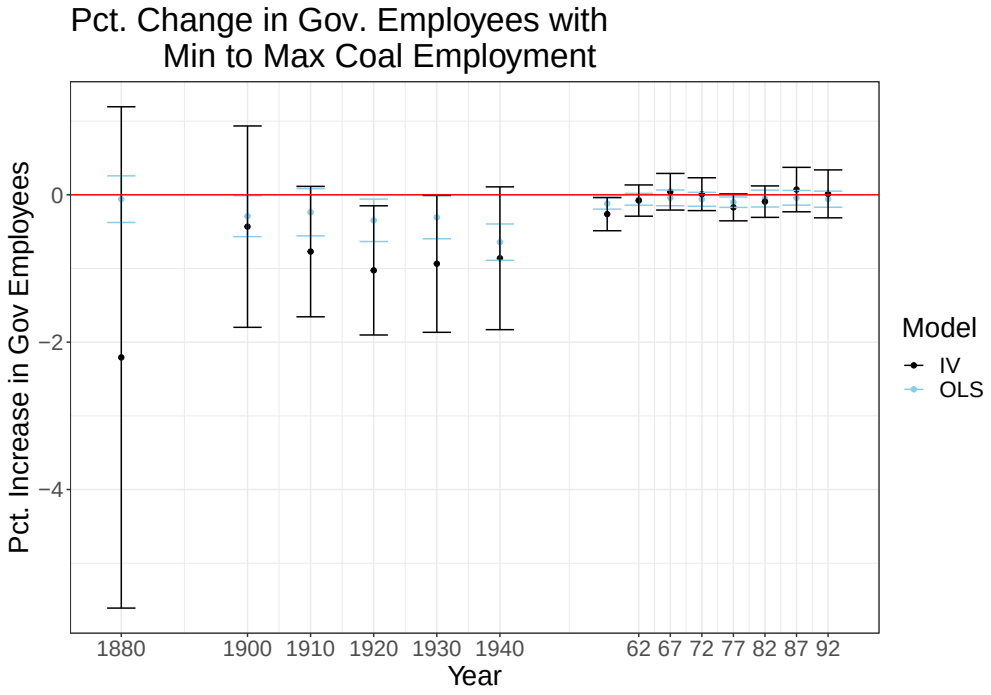


Figure 8: **IV Results: Employment.** Points and lines in black represent coefficients and 95% confidence intervals on coal employment, instrumented using coal deposits, in predicting the number of government employees in a county, controlling for state, population, and property value. Gray points and lines represent the OLS regression estimates presented in the main text for comparison.

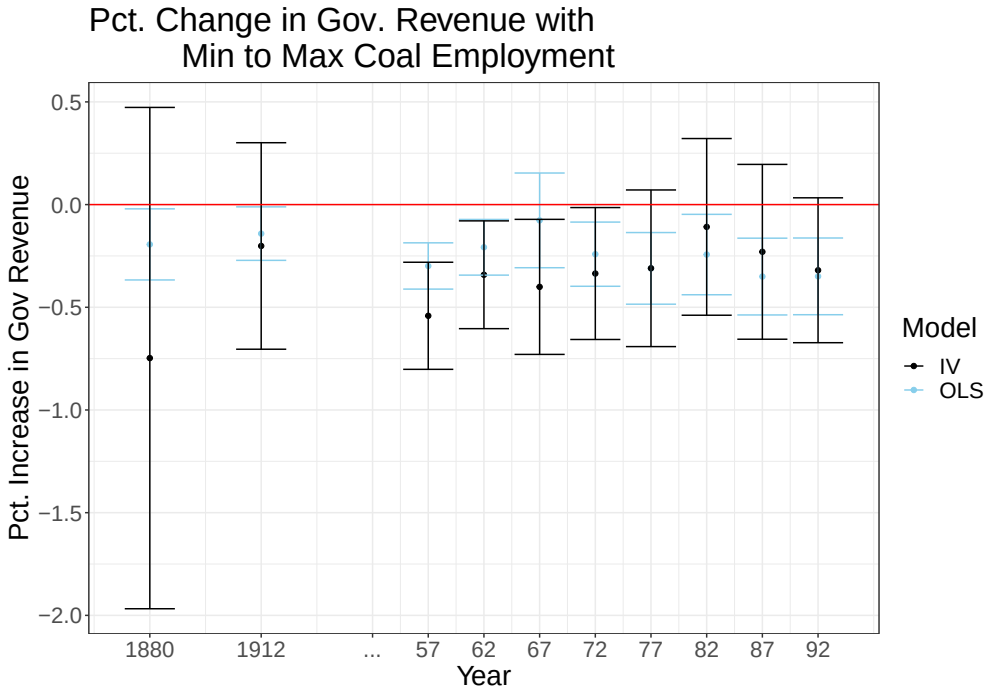
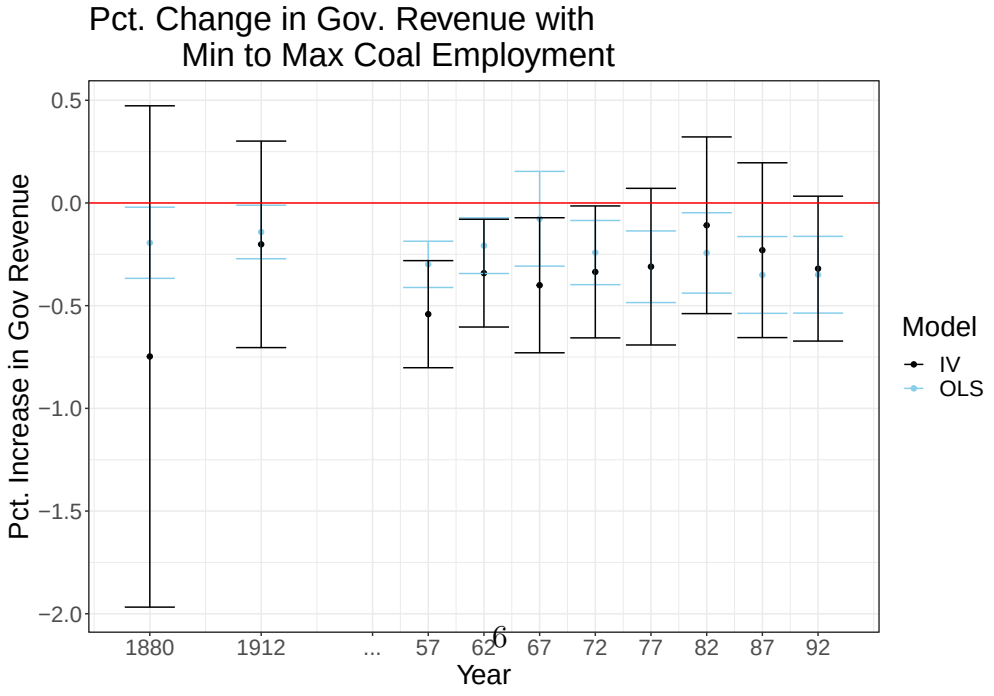


Figure 9: **IV Results: Revenue.** Points and lines in black represent coefficients and 95% confidence intervals on coal employment, instrumented using coal deposits, in predicting the total property tax revenue for local governments in a county, controlling for state, population, and property value. Gray points and lines represent the OLS regression estimates presented in the main text for comparison.



In general, the results of the instrumental variables models point to the same conclusion as those of the OLS results discussed in the main text. Counties with more coal employment had local governments with fewer employees through 1940, but not consistently afterwards; they collected less revenue consistently over time; and they spent less money in the 1950s, though perhaps not later on.

These results suggest that the descriptive relationship between coal employment and local government size cannot be explained by coal-endowed counties selecting into specializing in resource extraction for reasons that lead to smaller local governments.

A.3 Different in Differences: Pretrend Checks

The key assumption in the difference-in-differences analysis in the main text is that of parallel trends: absent the arrival of the coal industry in treated counties, government size in coal and non-coal counties would have moved in parallel. One way to test the plausibility of this assumption is to look for divergence in slopes between treated and control units before the treatment occurs. If units were moving in parallel before the treatment arrived, it could be more plausible they would have continued to move in parallel if the treatment never occurred.

To test for the presence of pre-treatment trends, I estimate treatment effects for each treatment cohort for each of its pre-treatment time periods with available data. Effects are estimated using the same methods as the true treatment effects reported in the paper, using the `did` package from Callaway and Sant'Anna (2020). That is, for the 1880 treatment cohort, for example, I calculate whether there is a significant difference-in-differences as if the treatment occurred in 1870 and in 1860. Figure 11 plots the results for each cohort and

year. Though three of the fifteen individual cohort-years approach statistical significance, no cohort shows consistent positive or negative effects, and the pretrend estimates on average center at zero. I conclude that there are no clear signs of consistent pretreatment trends.

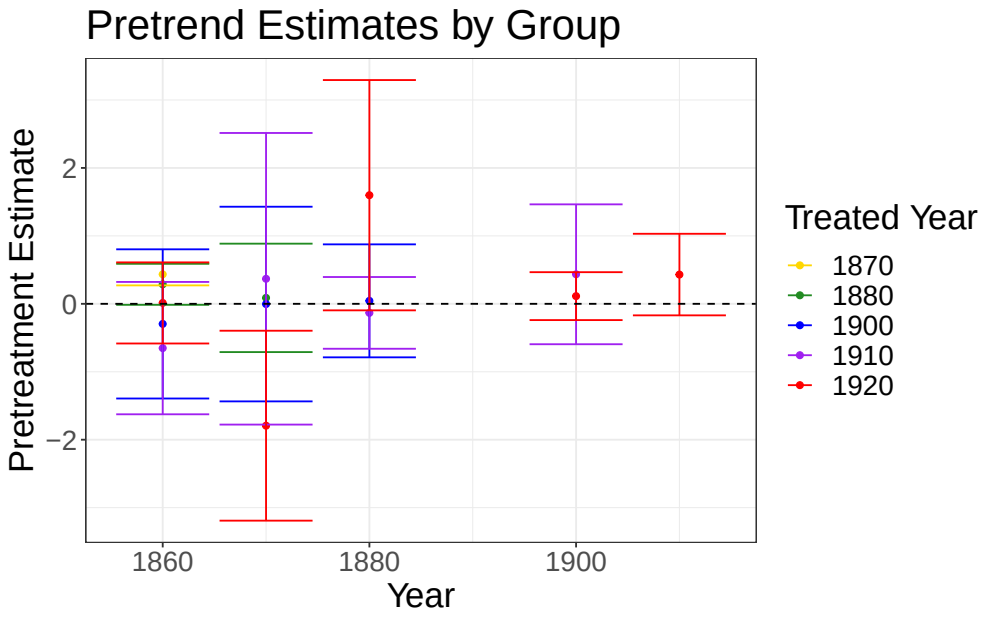


Figure 11: Pre-treatment Diff in Diff Estimates

A.4 Difference in Differences: Effects by Cohort

The difference in differences analysis presented in the main text shows the average effect of coal industry onset on government employment over time, combining information across five different "treatment cohorts:" that is, groups of counties that developed a coal industry in a particular year. Figure 12 shows estimates for each cohort separately.

The trend reflected in the averaged results is visible in each group, with the exception of the 1900 cohort, though the standard errors and exact estimates differ. Before treatment onset, there is no clear pattern of differences in trends between treatment and control counties.

Starting in the first year of treatment and continuing for 30-40 years, governments in treated counties grow more slowly than governments in control counties. This trend disappears, and in some cohorts reverses, as distance from treatment increases.

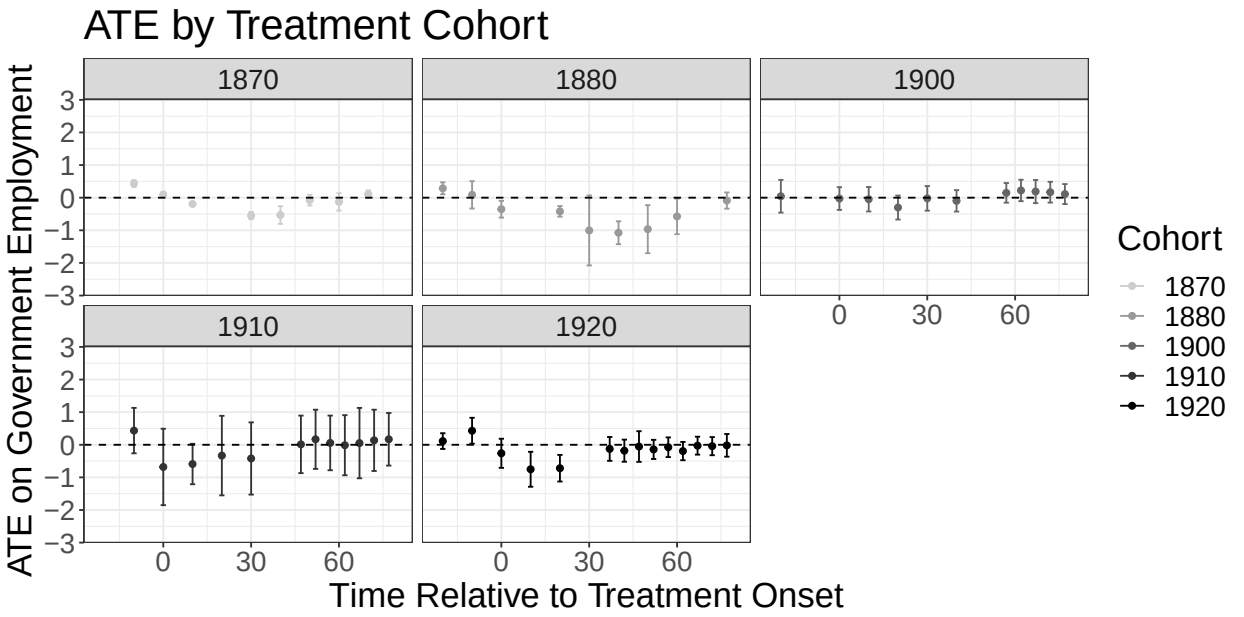


Figure 12: **Differences in Differences: Government Employment in Coal and Non-Coal Counties Over Time, By Treatment Cohort.** On the y-axis is the difference between treated and control groups in the difference in government employment from the previous time period, with different estimates for each group of counties based on the year in which the industry arrived. The measure of government employment is logged and residualized for state and population differences. The x-axis shows time relative to treatment; -20 represents 20 years before receiving treatment, while 20 represents 20 years afterwards. Bars represent bootstrapped 95% confidence intervals.

A.5 Difference in Differences: Robustness Checks

The differences-in-differences analysis presented in Section 3.3 of the main text uses all counties with a coal employment proportion that never exceeds 5% as the comparison group for the 23 treated counties of interest. Though the government employment trends pre-treatment appear similar in the treated and control groups, it remains possible that the trends in the potential outcomes in the treated and control groups under control diverged at the time of treatment. To check the plausibility of this concern, I now repeat the differences-in-differences analysis using more limited samples that address potential differences between treated and control units[7].

One potential concern is slavery. Kentucky and West Virginia, the two heaviest coal-mining states in the sample, were also the only two states in which slavery was common. Local governments in slaveholding areas were changing in the late 1800s in the processes of Reconstruction and Redemption (Suryanarayan and White 2021b), and it is possible that the paths of slave- and non-slave areas diverged at similar times to the coal industry's arrival. I therefore repeat the analysis using only counties with no slaves counted in the 1860 Census[8] using data from Acharya, Blackwell, and Sen (2018). The results are presented in Figure 13; they are quite similar to the results using the full sample.

Another potential concern is that coal-endowed areas in this region are often rugged and remote. These areas may have been set on different trajectories than the rest of the region

7. See Section A.6 for a map of the treatment counties and those used as comparison counties here and in the main text.

8. This eliminates a number of control counties in Kentucky and West Virginia, as well as 6 of the 23 treatment counties.

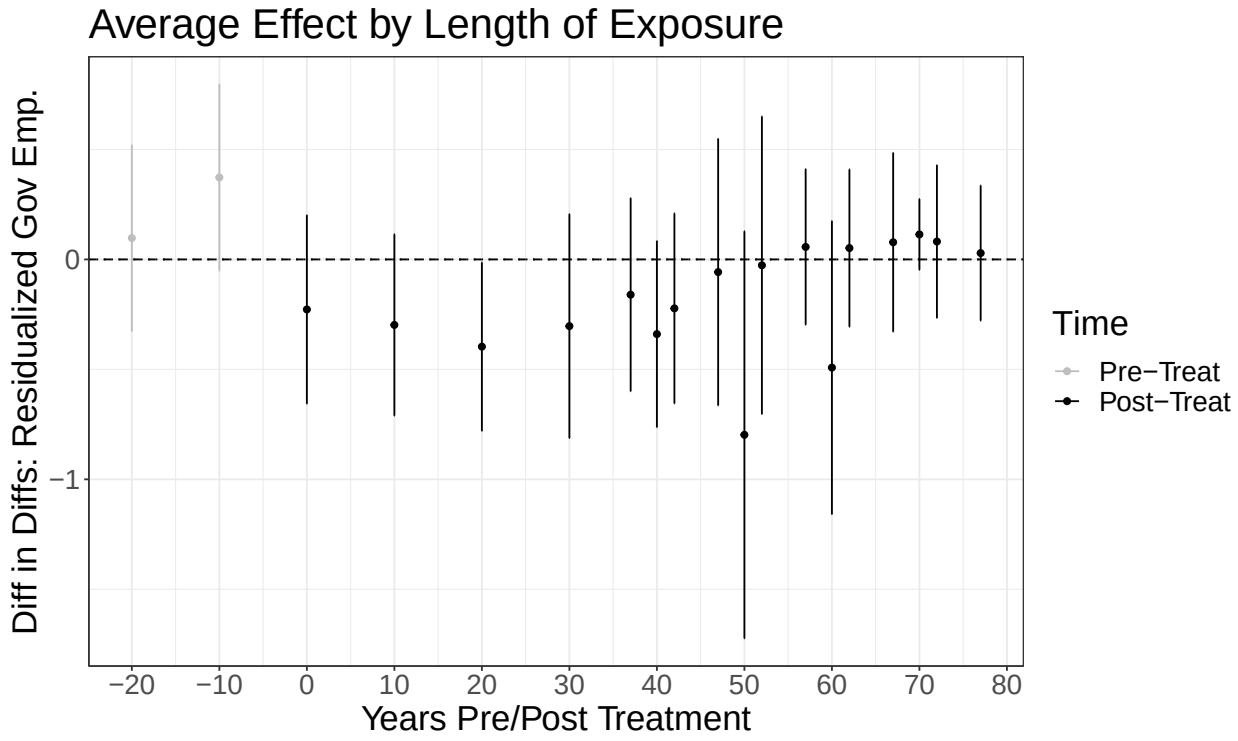


Figure 13: **Differences in Differences: Government Employment in Coal and Non-Coal Counties Over Time, Non-Slave Counties Only.** On the y-axis is the difference between treated and control groups in the difference in government employment from the previous time period. The measure of government employment is logged and residualized for state. time, and population differences. The x-axis shows time relative to treatment; -20 represents 20 years before receiving treatment, while 20 represents 20 years afterwards. Bars represent bootstrapped 95% confidence intervals.

as industrialization progressed around the turn of the century. To address this concern, I repeat the differences-in-differences analysis using only counties with some amount of coal deposits, using data from the USGS (control n = 41; further details can be found in the Instrumental Variables section of the SI). The results can be found in Figure 14. Again, results are similar to those in the full sample.

Finally, the differences in differences analysis presented in the main text relies on a group of 23 counties that quickly developed dominant coal counties. This quick development allows me to precisely identify the time at which these coal counties were first "treated" with the industry, which is useful for analysis. However, limiting the analysis to this small a group of counties makes the analysis vulnerable to the concern that only one or a few counties could be driving the result.

To address this concern, I repeat the pooled analysis discussed the main text (combining periods 0-40 years after treatment) 23 times, each time leaving out one of the treated counties. The results of these analyses are presented in Figure 15. Where the full sample pooled analysis ATT was $-.30$, the "leave-one-out" analyses range from $-.37$ to $-.24$. This suggests that the estimated treatment effect is robust to slight variations in the treatment sample, and a single treated observation is not producing the observed results.

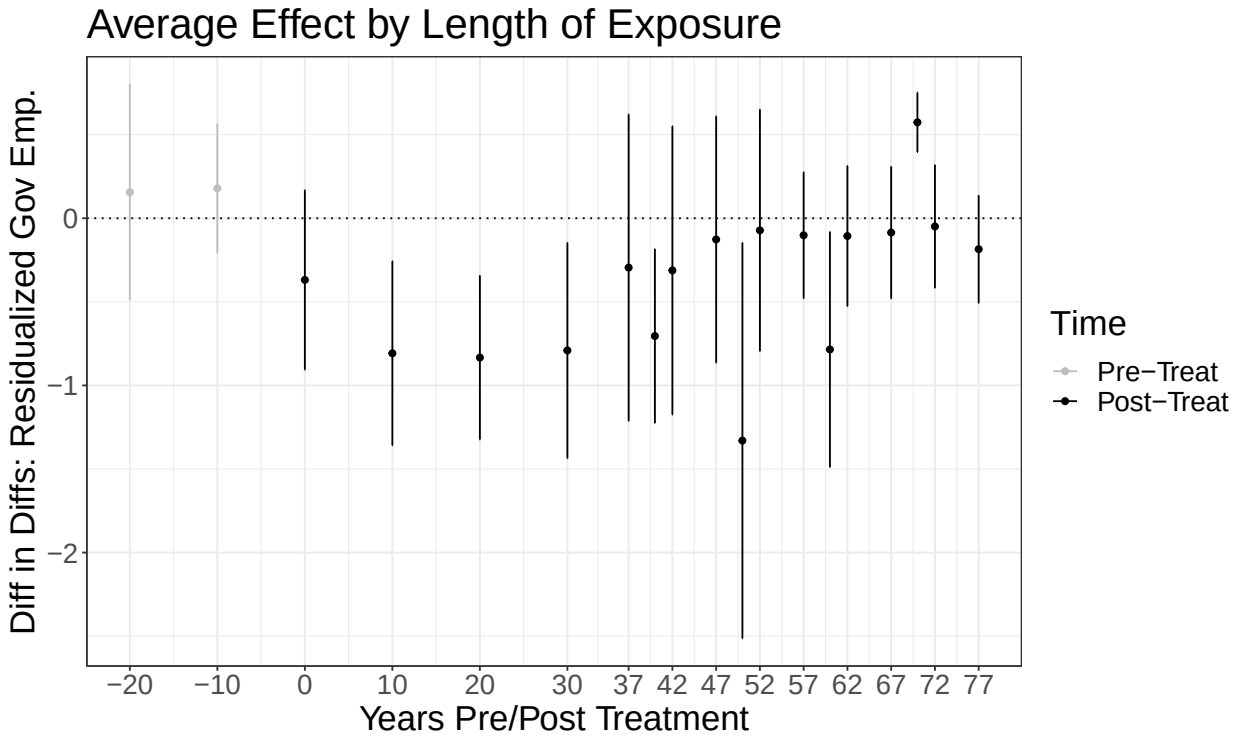


Figure 14: **Differences in Differences: Government Employment in Coal and Non-Coal Counties Over Time, Coal-Endowed Counties Only.** On the y-axis is the difference between treated and control groups in the difference in government employment from the previous time period. The measure of government employment is logged and residualized for state and population differences. The x-axis shows time relative to treatment; -20 represents 20 years before receiving treatment, while 20 represents 20 years afterwards. Bars represent bootstrapped 95% confidence intervals.

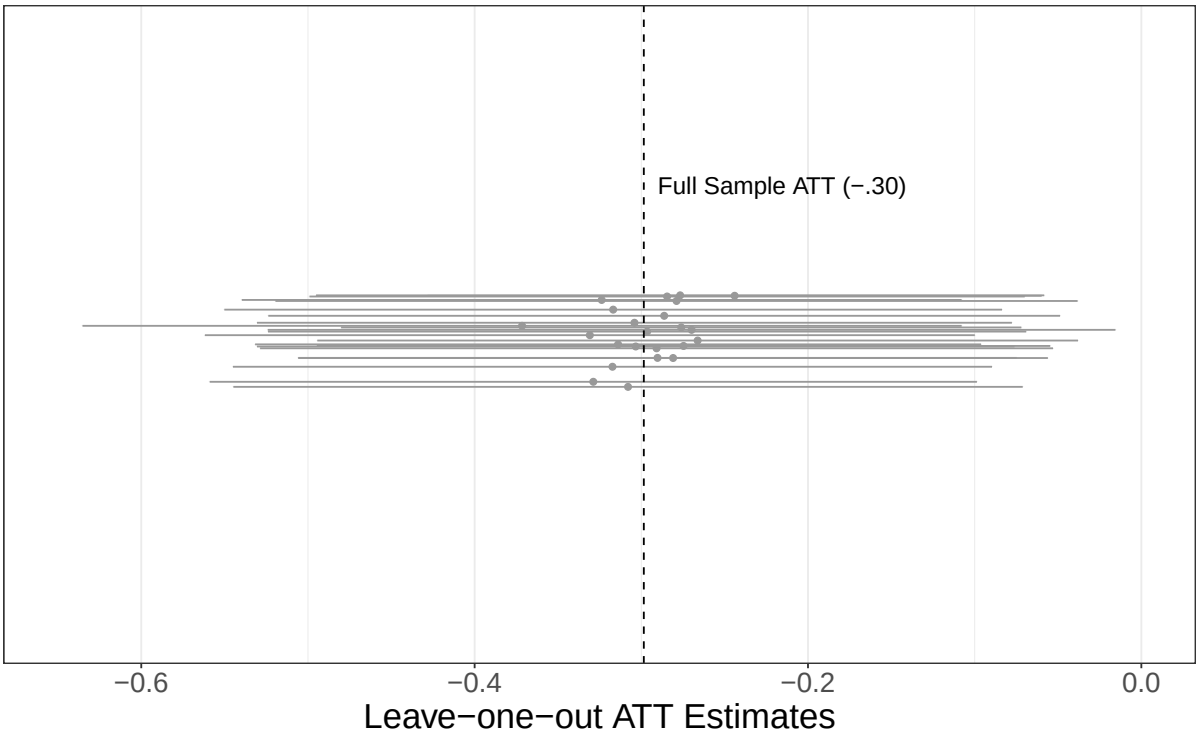


Figure 15: **Differences in Differences: Government Employment in Coal and Non-Coal Counties Over Time, Coal-Endowed Counties Only.** On the y-axis is the difference between treated and control groups in the difference in government employment from the previous time period. The measure of government employment is logged and residualized for state and population differences. The x-axis shows time relative to treatment; -20 represents 20 years before receiving treatment, while 20 represents 20 years afterwards. Bars represent bootstrapped 95% confidence intervals.

A.6 Characteristics of Treated and Comparison Counties: Differences in Differences

First, to clarify the choice of treatment counties for the difference in difference analysis, Figure 16 shows the trajectories of coal employment over time in all counties in the region in which coal employed at least 40% of the labor force at any point between 1850 and 1995.

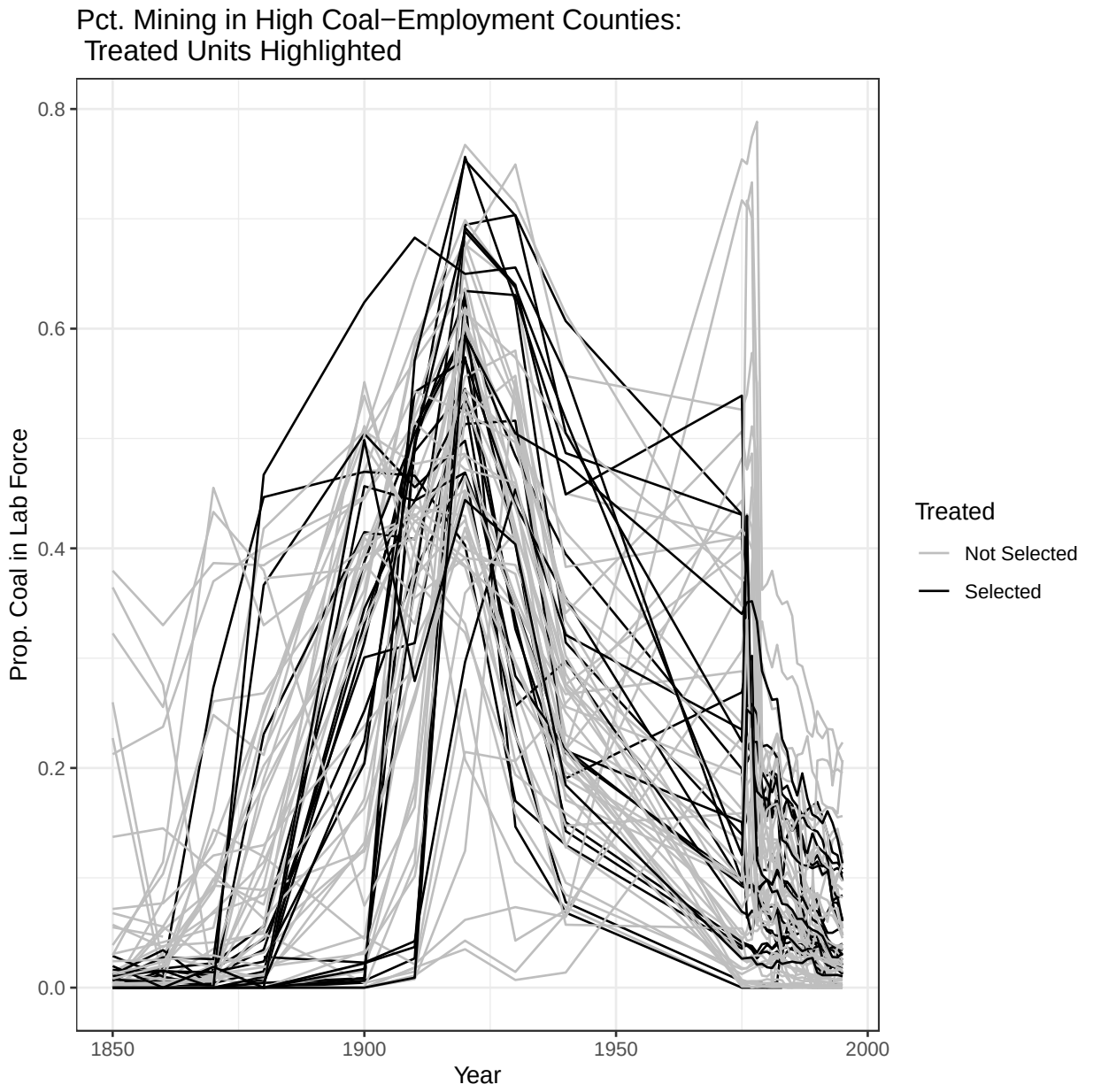


Figure 16: **Coal employment in high-coal counties.** Lines show the proportion of each county's labor force in the coal industry at each year, for all counties with at least 40% of their labor force in the coal industry at some point over this period. Treatment counties in DiD analysis are highlighted in black.

The treatment counties used in the difference in differences analysis are shown in black,

while all other high-coal-employment counties are shown in gray. The treatment counties stand out for the nearly-vertical lines indicating the sudden (i.e. between two decennial Censuses) emergence of a large coal industry in the late 1800s or early 1900s. The coal industries in these treated counties peaked and declined at different times, though most were highest between 1900 and 1920.

High-coal-employment counties that were not used as treatment comparisons were rejected for one of three reasons. First, they did not allow for the comparison of pre-treatment trends, because data were missing or because their industries arrived too early. Second, their coal industries developed too gradually, not allowing the identification of a single time period at which the industry arrived. Third, counties whose industries developed after 1940 were rejected because of a lack of coal employment data between 1940 and 1975, which makes it impossible to identify when the industry arrived. I dropped a final county because its coal industry employed a substantial proportion of its labor force in only one Census, reasoning that this time would have been too short to allow a durable effect of the industry on governance.

Selected characteristics of the counties in the treated group and control group, as well as all counties not used in either, can be found in Table 17. The median treated county was substantially smaller than control and other counties in 1870, but had surpassed them in population by 1920; the groups had similar populations by 1970. No group's median county had a noticeable proportion of its non-farm labor force working in coal in 1870, but the groups diverged in 1920 and afterwards. The number of government employees per 10,000 residents in the median treated and other counties remain lower than in the median control county throughout the period. Finally, the table reports the proportion of the median county's

labor force employed by its largest industry, regardless of what the largest industry was; the median control county's employment was less concentrated than treated and other counties, especially in 1920.

Next, to illustrate the comparisons being made in the differences in differences analysis in Section 3.3 in the main text, as well as the robustness checks in the previous section, Figure 18 maps the counties in the "treatment" and comparison groups.

In Figure 18, the red counties are the 23 "treatment" counties in the difference in difference analysis; all are places that rapidly developed coal industries at an identifiable point in time between 1870 and 1920. All the counties in various shades of blue were included as control counties in the analysis presented in the main text; these are the counties in the region that never employed more than 5% of their workforce in coal between 1850 and 1940. The non-shaded counties had middling coal employment or coal employment that grew gradually, and they are not included as comparisons in any of the difference in difference analyses.

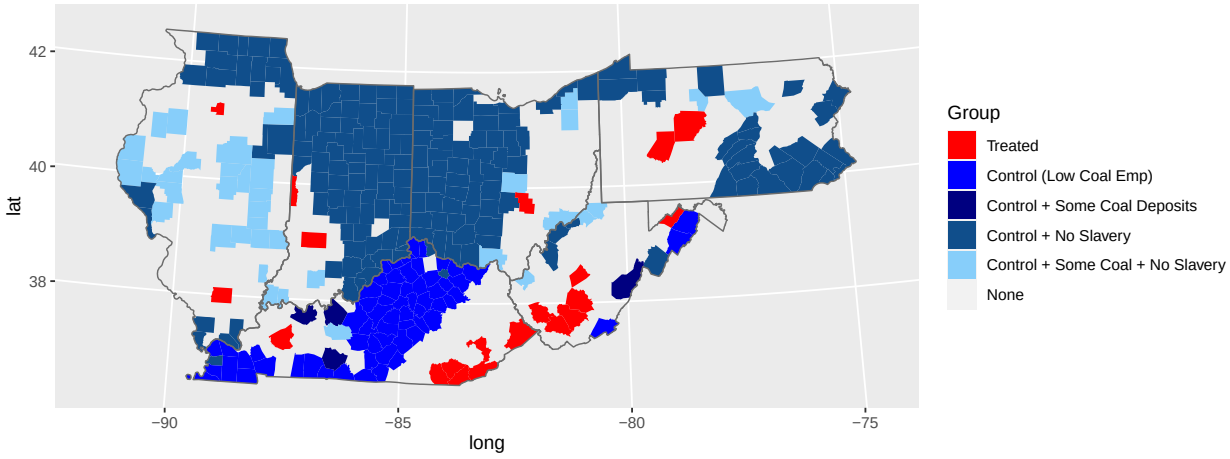


Figure 18: **Counties Used in Difference in Differences comparisons.**

Group:	Control	Other	Treated
N	280.00	221.00	23.00
Population			
1870	14040.00	13119.00	7575.00
1920	22573.50	23979.00	30819.00
1970	28765.00	25721.00	28315.00
Pct. Working in Coal			
1870	0.00	0.01	0.00
1920	0.00	0.13	0.57
1975	0.00	0.02	0.10
Gov. Emp. Per 10k Residents			
1870	5.50	4.52	3.16
1920	8.55	6.87	5.52
1972	339.30	321.02	320.94
Prop. in Largest Industry			
1880	0.07	0.09	0.09
1920	0.13	0.25	0.58

Figure 17: Characteristics of the median countries in the control group, treated group, and "other", counties in neither group.

The different shades of blue represent different categories of counties based on the variables used in the robustness checks in the previous section: slaves per household (from Acharya, Blackwell, and Sen 2018) and presence of coal deposits. Counties of the color labeled "Control (Low Coal Emp)", mostly located in non-coal-producing parts of Kentucky, were only included as control counties in the main text and were eliminated from both robustness checks for slaveholding and a lack of coal deposits.

Many counties in Indiana and Ohio (and several elsewhere) had no coal deposits, but were not slaveholding areas, so they were included as controls in the analysis eliminating slave areas. A handful of places in Kentucky and West Virginia had coal deposits but held slaves, so they were included in the test limited to coal-endowed counties. Finally, a group of counties largely in Illinois had both coal deposits and no slaves and were eligible for both robustness-test comparisons.