Do Universities Improve Local Economic Resilience?*

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December 17, 2021

Abstract

We use a novel identification strategy to investigate whether regional universities make their local economies more resilient to adverse economic shocks. Our strategy is based on state governments assigning normal schools (to train teachers) and insane asylums to counties between 1830 and 1930. Normal schools later became much larger regional universities while asylum properties mostly continue as small state-owned psychiatric health facilities. Because site selection criteria were similar for these two types of institutions, comparing counties assigned a normal school versus an insane asylum identifies the effect of a regional university. We find that having a regional university roughly offset the negative effects of exposure to manufacturing declines, and we attribute a significant share of this resilience to the resilience of regional public university spending.

*We would like to thank Alex Bartik, Tim Bartik, Lea Immel, Alex Whalley, and participants at the University of Illinois Young Applied Faculty Lunch, the Urban Economics Association European Meeting, the Urban Economics Association North American Meeting, and the Society of Labor Economists Annual Meeting.

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Local economies are routinely subject to adverse changes that can lead to employment losses and lower incomes. As such, economists and policymakers are interested in factors that make a local economy more resilient, hoping to avoid those negative consequences (Lin, 2012; Martin, 2012; Wolman et al., 2017; Bartik, 2018). Significant attention is paid to the presence of a university (Hartt, Zwick and Revington, 2019; Maxim and Muro, 2020, 2021). For example, Pittsburgh's well-known universities are sometimes credited with the city's resilience to the Rust Belt decline (Andes et al., 2017; Armstrong, 2021).[1]

Using a novel identification strategy, we investigate whether a regional university improves local economic resilience. Our strategy utilizes the placement of normal schools and insane asylums in the late 19th and early 20th centuries. We argue and show that state governments assigned these institutions to counties using similar criteria. Most normal schools grew into regional universities that were a large part of the local economy, while most insane asylums were converted into psychiatric health facilities and remained small in size.

The social reform movements of the 19th century included expanding education and advocating better care for those with mental illness. As a product of the education movement, local governments established community schools, creating widespread demand for qualified teachers. To meet this demand, many states established normal schools to train teachers according to the "norm" for good teaching (Labaree, 2008). At the same time, the "moral treatment" movement advocated more compassionate care for those with mental illnesses, with the objective of facilitating recovery. This contributed to states constructing insane asylums. The locations of both normal schools and insane asylums were political decisions, in which proximity and ease of access to population centers were important factors. The states also desired locations with sufficient property and natural beauty, to achieve each institution's goals (Humphreys, 1923; Kirkbride, 1854). States often chose locations for multiple normal schools as well as multiple asylums.[2]

[1]This gets discussed in the press as well, with titles such as "The Mystery of Pittsburgh: How Some Shrinking Cities are Thriving in the New Economy" and "From Rustbelt to Brainbelt." (Henderson, 2018; The Economist, 2020).

[2]This contrasts with states choosing only one location for the capital or flagship university.

In the early 20th century, normal schools and insane asylums were similar in size relative to county population. But in the mid-20th century, most normal schools became regional state colleges and universities, and students became a large share of the county population. Today, these universities typically focus on undergraduate and master's level education and are not as research intensive as flagship state universities. In 1980, roughly the starting point of our empirical analysis, they awarded roughly 42 percent of all bachelor's degrees in the U.S.[3] In contrast to the normal schools, asylums never grew large, and most of the asylum properties continue as state-owned psychiatric health facilities.

This history allows us to identify the effects of universities by comparing the resilience of counties which were assigned normal schools versus counties that were assigned asylums. Our identification assumption is that the asylum counties are a good counterfactual for what would have happened in the normal counties had the normal schools not turned into regional universities. We argue the two types of counties were selected on similar observable and unobservable criteria. We also assume that the presence of an insane asylum does not have direct effects on resilience, beyond the resilience effects from having a normal school that never transformed to a university. This seems plausible given they have remained at about the same (small) size since the early 20th century.

We define an economy as resilient if it avoids some or all of the negative effects of a typically adverse shock. We focus on resilience to manufacturing decline, although we additionally consider other resilience, which is of general interest to economists and policymakers.[4] Since U.S. manufacturing employment's peak in the 1970s, there have been large declines, first concentrated in the Rust Belt, but geographically broad since 2000. This has led to consternation about adverse impacts on local economies (Autor, Dorn and Hanson, 2013; Pierce and Schott, 2016). We find that normal counties are more resilient to the negative

[3] This is based on all Title-IV universities in the United States that report data to IPEDS in 1980. See Appendix A for details.

[4] We acknowledge that some of the mechanisms that enable resilience to persistent decline of a major industry may differ from those enabling resilience to short-run business cycle fluctuations, and we discuss these in our mechanisms section.

effects of manufacturing exposure, losing less employment, income, and population during and following the decline. In fact, the universities provide nearly full resilience, so every job lost due to additional manufacturing exposure in asylum counties is not lost in normal counties. We also see nearly full resilience when looking at population and earnings. In additional analysis, we show universities also enable resilience to the mining employment decline in the 1980s, and to the business cycle.

We then consider the mechanisms through which this resilience occurs. One possibility is that the regional public university itself is resilient, expanding in adverse economic conditions due to increased demand from students, or declining less due to relatively stable allocations from the government. We present evidence that regional public university size, measured by expenditures, is resilient to economic shocks. The resilience of university spending growth is roughly 15 percent of the overall resilience. This is likely an underestimate because we do not measure student spending or university expenditures on construction.

As the regional public university grows (or declines less) in response to an adverse shock, this has indirect effects as well. One primary spillover would be a spending multiplier that comes through increases in local demand. Consistent with this hypothesis, we find that much of the resilience is concentrated in non-tradable sectors. Using typical multipliers from the literature, the university spending channel can explain a significant share of the overall resilience.

Another possible mechanism is that universities increase the education level of the economy, at baseline or as a reaction to the economic shock. We find that the bachelor's degree share in normal counties is higher than in asylum counties immediately preceding the manufacturing decline. There is less of a literature estimating the causal effects of education levels on resilience, but the correlations are consistent with education levels being a quantitatively-important mediator.

Our focus on non-research-intensive universities contrasts with much of the literature on the economic impacts of universities. However, we see this treatment effect as particularly policy relevant. First, as we have mentioned, these universities award a large fraction of bachelor's degrees in the United States. Second, states may be unlikely to start or close a flagship research university, but more likely a regional university. For example, current regional university consolidation and program reduction discussions are ongoing in Pennsylvania and Wisconsin (Quinton, 2020).[5] Further, regional universities are more heavily reliant on state budgets, and so the value of these universities is annually policy relevant.[6] Indeed, there has been considerable discussion in recent years surrounding the societal value, financial sustainability, and the future of regional public universities (see McClure and Fryar (2020), Maxim and Muro (2020), and Seltzer (2019)). While their role as an anchor institution in local communities is often cited, there is very little work to our knowledge estimating the causal local economic impacts of these public, non-research-intensive universities.[7]

Our paper contributes to an important and growing literature studying the relationship between universities and local economic growth. Many of these papers focus on the relationship between universities and innovation, while others consider the effect on growth more broadly (Aghion et al., 2009; Andersson, Quigley and Wilhelmson, 2004; Andrews, 2021; Bartik and Erickcek, 2008; Cantoni and Yuchtman, 2014; Feng and Valero, 2020; Hausmann, 2020; Kantor and Whalley, 2014, 2019; Valero and Reenen, 2019).[8]

We make two contributions to this literature. First, none of these papers consider whether universities improve resilience to negative economic shocks. Most related to our work on resilience is Glaeser and Saiz (2004), which shows a relationship between manufacturing

[5]In April 2020, the Chancellor of the Vermont State Colleges proposed permanently closing three campuses due to additional COVID-19 losses, and he resigned soon afterwards due to backlash (Seltzer, 2020; Quinton, 2020).

[6]In 2013, the average state appropriation to non-selective four-year public universities was 26.2 percent of the average total spending at these universities. This reliance on state appropriations is much lower among selective public universities. For 35 selective four-year public universities, this was 18.9% (Deming and Walters, 2017).

[7]An important consideration regarding external validity is that our paper studies resilience to manufacturing and mining declines and recessions, none of which had direct negative effects on universities. In fact, enrollment is known to grow during times of economic decline. Thus, our results do not speak to resilience to shocks that negatively affect universities.

[8]Moretti (2004) studies the relationship between the supply of college graduates and wages, using the presence of a land-grant institution as an instrument.

exposure and shifting away from manufacturing, which is stronger in areas with higher initial bachelor's degree share. However Feyrer, Sacerdote and Stern (2007) do not find that Rust Belt counties with higher bachelor's share were more resilient to the Rust Belt shock.

Second, we present a novel identification strategy to address endogenous university locations. Our comparison of normal to asylum counties contrasts with some of the other identification strategies used in this literature, including comparing to runners-up locations (Andrews, 2021), budgetary shocks (Aghion et al., 2009; Kantor and Whalley, 2014), and legislative changes incentivizing university research (Hausmann, 2020).

Our identification strategy is most similar to Andrews (2021), and we view the two strategies as complementary, especially given our different outcomes of interest. One of the biggest differences is that we identify the effects of different types of universities, as Andrews (2021) includes primarily research-intensive universities. This is particularly important given his focus on patents and our focus on resilience. Second, while our sample is not hand-matched, it is larger. We have over 200 normal schools counties and nearly 130 asylum counties, giving us more statistical power. Finally, all counties in our control group are given a similarly-sized state institution, rather than being only runners-up. Andrews (2021) is also interested in the effect of universities relative to counties with a "consolation prize," but has only 27 counties in his sample for this exercise.

Our paper proceeds as follow. In Section 1, we discuss the historical placement of normal schools and asylums that leads to our empirical strategy. We then show that while normal schools' placement was highly selected, there is no reason to suspect it was differently selected than the placement of insane asylums. We describe the effect of normal school assignment on recent enrollment, education levels, and industry composition in Section 2. We investigate the resilience of local labor markets in response to manufacturing declines in Section 3, and with respect to other shocks in Appendices B and C. We consider the mechanisms through which universities improve resilience in Section 4. Section 5 concludes.

1 Normal Schools and Asylums: History and Identification Strategy

During the early and middle part of the 19th century there was strong support for the establishment of public institutions aimed at social improvement and reform. This included development of normal schools, as well as asylums for the mentally ill (Grob, 2008). In this section we describe these institutions, as well as qualitative historical evidence suggesting very similar site selection criteria.

Demand for teachers grew rapidly in the early to mid 19th century, as local communities began operating elementary and eventually secondary schools (Labaree, 2008). This demand was met through the establishment of state normal schools to train teachers. The first state normal school was opened in 1839, and by 1930 the number had reached 209 (Ogren, 2005). State governments typically established multiple normal schools across the state.

In *The Factors Operating in the Location of State Normal Schools*, Humphreys (1923) argued that political considerations were the most important factor determining normal school locations. Other factors included demand for instruction (e.g. the local population), geographic accessibility, financial and land donations, location of existing schools, and natural beauty. As of 1923, about half of normal schools in the United States had been located directly by the state legislature. The other locations were chosen by commissions authorized by the legislature (Humphreys, 1923).

We focus on normal schools because they evolved to become important state universities. By the early 20th century, legislatures began changing some normal schools to teachers colleges, allowing them to grant bachelor's degrees. These colleges broadened, reducing teacher education to a smaller component. Through the 1950s many state legislatures renamed teachers colleges as state colleges. From the 1950s through the 1970s, many obtained university status (Labaree, 2008). For example, Southern Illinios University, Northern Illinois University, Eastern Illinois University, and Western Illinois University all started as normal schools. The distribution of opening years and years in which they were converted to state colleges, can be seen in Figure 1a.

We digitize historical university-level enrollment data, and see that the evolution from normal school to regional university was correlated with large enrollment increases (Figure 1b).[9] In 1934, normal school enrollment was about 2 percent of the county's population. By 1970, enrollment in universities that began as normal schools was well over 10 percent of county population, and has been relatively constant since.

We now describe the history of state insane asylums and why asylum counties are a good control group for normal school counties. The establishment of state insane asylums accelerated in the 1830s, as part of a movement to provide therapeutic and compassionate care that would facilitate recovery (Grob, 2008). Many of these state asylums were built according to the Kirkbride architectural plan, which stressed the importance of picturesque environments, large natural spaces for recreation, and stately buildings.[10] This plan also stressed that asylums be close to population centers and roads and railroads connected to patients' home cities (Kirkbride, 1854).

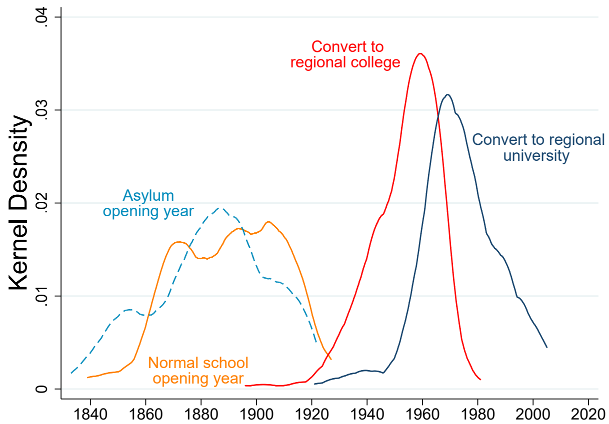
These goals resulted in institutions with buildings and grounds quite similar to normal schools. This history meant that site selection criteria was similar, both institutions were desired by local communities, and both were sources of pride at the time they were built.

Importantly, a given state was often choosing locations for normal schools and insane asylums around the same time, as can be seen in Figure 1a.[11] Indeed, Humphreys (1923) shows that location decisions for these two types of institutions were sometimes considered

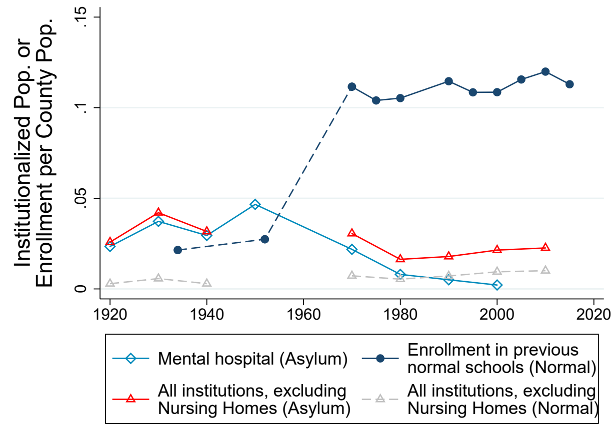
[9] We digitize historical university-level enrollment data in the 1933-1934 academic year using the *Biennial Survey of Education, 1932-1934* (Foster et al., 1937), and in 1952 using *American Universities and Colleges, Sixth Edition* (American Council on Education, 1952). Total university enrollments in 1970 and 1975 are collected from *Higher Education General Information Survey (HEGIS)* (United States Department of Education. National Center for Education Statistics., 1998, 1999). Total enrollments from 1980 to 2015 are collected from IPEDS (U.S. Department of Education, National Center for Education Statistics, 2020).

[10] Thomas Kirkbride, the developer of this plan, believed that good architecture was critical for curing mental illness (Yanni, 2007).

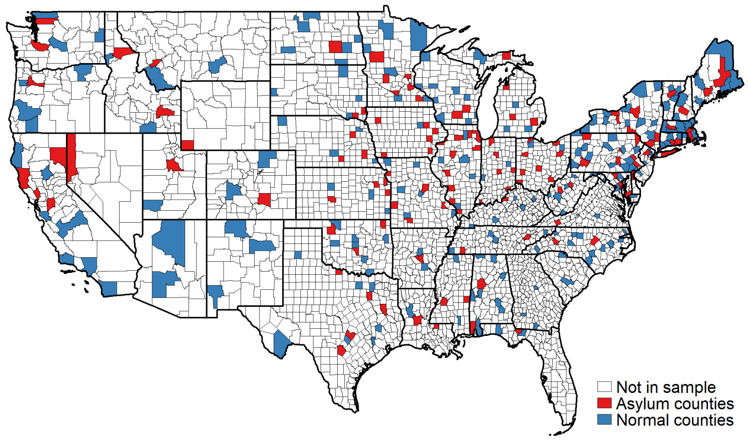
[11] For roughly one third of the states in our sample, asylums and normal schools opened within six years, and for more than half of the states they opened within eleven. The average difference between the opening dates of the first asylum and the first normal school within the same state was approximately 16 years.



(a) Asylum and Normal School Opening Years



(b) Asylum and Normal School Size Over Time



(c) Locations of Normal Schools and Asylums

Figure 1: **History of Normal Schools and Insane Asylums.** Figure (a) shows opening years for normal schools and asylums. We use an Epanechnikov kernel with a five-year bandwidth for density estimation. The year in which previous normal schools convert to state colleges and state universities is defined to be the year that the school's name changes to college and university respectively. Figure (b) shows average enrollment in normal schools (or in colleges that had been normal schools) per county population in normal counties. We also show average institutionalized population per county population for both normal and asylum counties. Depending on the year, institutionalized population includes population in mental institutions, correctional institutions, institutions for the elderly, handicapped, and poor, juvenile facilities, and nursing/skilled nursing facilities. College enrollment in Maine and Vermont is missing in 1952; however, using a balanced sample yields a similar figure. Figure (c) shows a map of the locations of the normal and asylum counties in our sample. See footnotes 9 and 14 for data sources.

concurrently, and were relevant for political negotiations. This further supports the idea that locations of one versus another type of institution may have been due to randomness in political negotiations.[12] Further, states were often choosing locations for several normal schools and several asylums.[13]

In addition to collecting historical enrollment data for normal schools, we also collect data on historical population in insane asylums to compare the size of these two types of institutions. This represents another test that these institutions were expected to provide similar advantages to their counties, and may have required similar political influence.[14] Figure 1b shows that in the early 20th century, enrollment at normal schools and population in asylums were similar relative to county population.

An article from the *Kankakee Gazette*, written in August 1877 when the city was assigned an asylum, helps illustrate these points.[15] The article is titled "Got It: Knew We Would—Couldn't Be Otherwise!: The Eastern Insane Asylum Located at Kankakee." Reflecting the desirability of insane asylums, it says "Our citizens received the news in a spirit of jubilee, and on Friday evening there was a bonfire, band music... and speeches..." The article also reflects the importance of scenery, describing the place for the asylum as "just outside of the city limits on the south side of the river—a desirable location," and details the 351 acres that will be contributed to the asylum. The article describes one of the benefits for the city is that "the construction of a $200,000 building will give employment to laborers..."[16]

[12]Humphreys (1923) writes that Oklahoma was locating 11 state institutions in one year (including normal schools and an asylum), and the "opportunities for political 'deals', combinations, and 'trading' were exceptional."

[13]For 29 of the 49 states in our sample, they established more than one normal school and more than one asylum.

[14]We collect historical data on population in insane asylums at the county level from various sources. We obtain institutional population by institution type from the decennial censuses of 1920 through 1940 using 100% counts from IPUMS (Ruggles et al., 2021). Because 100% counts are not available in 1950, we instead collected asylum-level resident population data using a 1950 publication of The Council of State Governments (Council of State Governments, 1950).

[15]We thank the Abraham Lincoln Presidential Library and Museum for scanning the microfilm for us during the pandemic.

[16]The article also mentions other advantages: "In addition to the advantages which an institution of the size of the new asylum must confer upon the place of its location to a greater or less extent, the impression which exists abroad to a certain degree that Kankakee is a low marshy place, now stands refuted in the most public manner."

The article lists the other finalists for the asylum: "Decatur, Bloomington, Champaign, Urbana, Danville, Paxton, and Pontiac," showing that it was a political process that mainly considered small population centers in central Illinois. Finally, the article also discusses the importance of political influence, crediting "the great services of Messrs. Bonfeid and Taylor, our representatives in the upper and lower houses of the legislature." We provide more examples from historical newspapers in Appendix F.2.

Several additional facts support our identification assumption, that assignment of a normal school instead of an asylum was as good as random. First, as we will discuss, roughly 17 percent of counties that were assigned asylums also were assigned normal schools (13 percent of normal counties had asylums). This suggests similar selection criteria for the two types of institutions. Second, in many cases asylum counties were runners-up locations for public colleges and universities (including normal schools), as documented in Andrews (2021).[17] In the opposite direction, one example is Bloomington, IL, which was assigned a normal school and was a top contender for an asylum.[18] Andrews (2021) also presents evidence suggesting Tuscon, Arizona had actually wanted the asylum, but a flood delayed their delegation's lobbying trip to the capital and they instead received the state university.

Many of the asylum properties continue to be owned by the state, and continue as psychiatric health facilities. Others have been acquired by universities or are used as correctional facilities (Hoopes, 2015). While the size of the institutionalized population fell from 1950 to 1980 during the deinstitutionalization movement, average institutionalized persons per population in asylum counties was still nearly double that of normal counties in 2010. As Figure 1b shows, the per capita population in non-nursing home institutions has fallen only modestly.

[17] Specifically, of the 62 high-quality public college site selection experiments identified by Andrews (2021), 17 had runners-up that were asylum counties. However, most of these experiments involved land grant institutions rather than normal schools. Andrews (2021) discusses consolation prizes at length, and also argues that which location ended up with which institution was "as good as random".

[18] The normal school in Bloomington became Illinois State which is technically in Normal, Illinois, but Bloomington-Normal is a single metro area and both are in McLean County. In fact, Normal used to be known as "North Bloomington," and the name Normal is taken from "Illinois State Normal School," the original name of Illinois State.

Hence, when we compare normal counties to asylum counties, we are comparing counties that were assigned a modestly-sized institution between 1830 and 1930. However, while the normal schools later became colleges that grew dramatically, the asylums and the mental health and correctional facilities to which they were converted remained modestly sized. Furthermore, as can be seen in Figure 1b, much of the asylum closure preceding this conversion happened prior to the period we study, which starts around 1980.

1.1 Need for and Validity of Empirical Strategy

In this section, we show the importance and validity of our identification strategy. Consistent with our description above, we show normal schools were not randomly assigned; however, they were chosen based on the same criteria as insane asylums. We also show that in the more recent time period we study, the geographic advantages enjoyed by counties with normal schools are similar in counties with asylums.

Our historical data on normal schools come from Ogren (2005), which includes the school's location, opening year, and years corresponding to name changes.[19] There were 209 normal schools across 204 counties, opened between 1839 and 1930, with median opening year of 1891 (Figure 1a).

We digitize data on asylums' geographic locations and opening years from the 1923 special census of "institutions of mental disease" (Furbush et al., 1926). Our identification comes from states randomly choosing some counties to receive normal schools and some to receive asylums, around the same time. Thus we exclude five asylums that were established before 1830. This yields a sample of 160 asylums from 151 counties.[20]

For counties with both normal schools and asylums, we define these as normal counties, though for robustness we exclude them. There were 25 such counties, out of the 204 counties

[19]Using the city and state of the normal school, we identified the county using StatsAmerica (Indiana Business Research Center, 2020).

[20]The opening years and locations were extracted from Table 64 and Table 104 of the book. Seventeen of these asylums did not have opening years in the 1923 Census. We obtained these opening years from government websites or other open sources. Sources for each missing opening year are available upon request.

with normal schools. This yields a total of 204 normal counties and 126 asylum counties. Figure 1c shows the geographic distribution of normal and asylum counties in our sample.

We start by comparing normal school counties to all other counties, based on geographic variables and county characteristics in 1840, before almost all of the normal school locations were chosen.[21] As the above discussion makes clear, normal school counties were chosen by state legislatures and commissions, raising concerns they were placed in counties that have persistent economic advantages.

Panel A of Table 1 explores whether normal counties have significant geographic advantages over a typical county. We test whether they are closer to big cities, based on the following quantity, inspired by a gravity model:

$$\log \text{Nearby Population}_i = \log \sum_{j \neq i} \frac{\text{Population}_j}{\text{Distance}_{ij}}$$

where i is the county of interest and j is summed over all other counties. The quantity is large if nearby counties have high population. We measure this quantity in 1980.

The point estimates, comparing column (1) to (3), suggest normal school counties are significantly closer to big cities than a typical county, and regression analysis with state fixed effects in column (5) confirms this. This is consistent with historical descriptions of the criteria. However, asylum counties appear to be comparable to normal counties. While the average normal county is closer to the state capital and has more water coverage than the average county (columns 1 and 3), this appears accounted for by being in different states (column 5). There is some evidence that asylum counties are closer to state capitals, with the difference significant at the 10 percent level (column 4).[22]

[21] In constructing the balance tables with historical 1840 or 1920 data we crosswalk the NHGIS codes to 1990 FIPS codes using the crosswalk in Eckert et al. (2020), to account for changing county boundaries over time. There are not meaningful changes in county boundaries for our sample of normal and asylum counties when using the more recent BEA data.

[22] We think it unlikely that this slight imbalance on proximity to the state capital will bias our results. However, even if being close to a state capital did confer some advantage regarding the resilience of a county, the bias would be in the opposite direction of our results.

Table 1: Covariate Balance: Normal School, Asylum, and All Other Counties

	(1)	(2)	(3)	(4)	(5)
	Variable Means			Difference in Means With State FE	
	Normal	Asylum	All others	(1) - (2)	(1) - (3)
Panel A: Geographic Characteristics					
Log Nearby Population	13.02	13.31	11.75	-0.21	0.97***
	(1.29)	(1.41)	(1.28)	(0.17)	(0.08)
Within 150 Miles of State Capital	0.48	0.52	0.42	-0.11*	-0.02
	(0.5)	(0.5)	(0.49)	(0.06)	(0.03)
Water Coverage	6.23	6.35	4.39	-0.04	-0.83
	(12.96)	(12.44)	(11.06)	(1.34)	(0.92)
Panel B: Characteristics in 1840					
Log Population	9.71	9.51	8.99	-0.02	0.27***
	(1.38)	(1.59)	(1.22)	(0.13)	(0.08)
Insane and Idiot Share	0.08	0.08	0.08	0.00	-0.01
	(0.07)	(0.08)	(0.1)	(0.01)	(0.01)
Log Manufacturing Capital Stock	12.02	11.87	10.51	-0.18	0.49***
	(2.07)	(2.07)	(1.86)	(0.19)	(0.14)
Urban Share	6.72	9.22	1.40	-3.15	2.71**
	(16.13)	(20.31)	(8.11)	(2.56)	(1.15)
Sectoral Employment Share					
Agriculture	73.94	68.65	82.47	5.16	-1.57
	(21.3)	(23.51)	(20.39)	(3.22)	(1.47)
Commerce	2.97	3.82	2.48	-0.69	0.09
	(5.42)	(5.22)	(5.03)	(0.78)	(0.18)
Learned Professions and Engineers	2.25	4.20	2.00	-1.85	0.12
	(3.51)	(11.16)	(4.05)	(1.42)	(0.12)
Manufacturing	17.38	19.51	10.53	-1.94	2.19*
	(14.44)	(15.66)	(12.85)	(1.66)	(1.31)
Mining	0.73	0.79	0.60	-0.21	0.04
	(4.02)	(1.74)	(2.49)	(0.19)	(0.28)
Non-ocean Navigation	1.30	2.24	1.24	-0.81**	-0.15
	(3.32)	(5.71)	(3.95)	(0.39)	(0.16)
Ocean Navigation	1.43	0.79	0.68	0.33	-0.73*
	(7.01)	(2.17)	(5.05)	(0.76)	(0.43)

Notes: This table shows summary statistics for normal, asylum and all other counties. Nearby population is based on 1980 population and a gravity model. Columns (1) through (3) show variable means and standard deviations in parentheses. Column (4) and column (5) display estimates from regressing each variable on the normal county indicator with state fixed effects. Column (4) contains normal and asylum counties and column (5) contains normal and all other counties. In columns (4) and (5) we report standard errors clustered at the state level in parentheses. Sample sizes vary across variables due to missing data for some counties. For the variables in Panel A, there are 204 normal counties and 126 asylum counties. For log nearby population there are 2776 other counties, while for the other two variables in Panel A there are 2779 other counties (the three additional counties are counties that were renamed after 1980 or did not exist in 1980). We are missing many counties in Panel B as their current states were not yet states in 1840, and so data were not available. For log population, insane and idiot share, and urban share, there are 145 normal counties, 92 asylum counties, and 1729 other counties. For Log Manufacturing Capital Stock there are 137 normal counties, 89 asylum counties, and 1581 other counties. For all sectoral employment shares there are 143 normal counties, 90 asylum counties, and 1677 other counties. See text for details.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

In Panel B, we find that the locations of normal schools were highly selected based on their 1840 characteristics. For these variables we are restricted to states east of the Mississippi River, as well as a few others, as most of the Western states were not yet states at this point.[23] Normal counties had significantly larger populations than all other counties, were more urban, and had more manufacturing. All of these are consistent with proximity to population centers being an important criteria for the placement of normal schools. However, despite these advantages relative to the average county, normal counties and asylum counties look very similar.

Table 1 underscores the need for a strong control group for normal school counties, and shows that asylum counties are a strong control group. We confirm this in Appendix Table A12, where we show that the same patterns existed in 1920 as well. Of note, a large majority of the variables differ between normal counties and all other counties, but there are no variables that differ between normal and asylum counties at the 5 percent level. There are several reasons to look at 1920: first, by 1920, we have data on all the counties; second, it lets us confirm that the normal schools and asylums did not have differential effects on the counties prior to the normal schools growing into regional universities; and third, it allows us to look at different variables than in 1840.

2 Normal Schools' Effects on Local Economies

History suggests normal school counties should have a higher probability of having a regional public university. In this section, we show this is indeed true, and we look at the other effects of normal schools on the 1980 labor market. While we are primarily interested in whether a regional university causes a local economy to be more resilient, these effects give important context.

[23]States with available data include Minnesota, Missouri, Arkansas, Louisiana, parts of Iowa, and everything east of the Mississippi River. We look at similar variables for all states in 1920 in Table A12.

2.1 Effect on University Presence and Enrollment

In Table 2 Panel A, we look at the effect of normal school assignment on the county's higher education sector. We estimate regressions with state fixed effects, and an indicator for having been assigned a normal school. The sample is composed of the counties that were assigned either a normal school or an asylum. We interpret the coefficient on the normal county indicator as the causal impact of having a normal school on y. All the y variables are measured in 1980, but patterns are similar in 2000 or 2015.

The first row shows the effect of having a normal school on someday having a normal school that turns into a regional college or university. Mechanically, asylum counties have zero such colleges. The effect is 0.93, implying the vast majority of normal schools did turn into regional universities.[24] Throughout the paper, we report the reduced-form effect of having a normal school. Thus, if there is a positive effect of regional universities and we wished to interpret the coefficients as the effect of having a regional university, we will obtain a slight underestimate, as some normal schools do not become universities.

Some asylum counties do have four-year public colleges, and the second row shows the average number in an asylum county is 0.44. This average is higher by 0.69 colleges in normal counties. In the third and fourth rows, we see that other types of colleges are potentially crowded out by regional colleges, including four-year private colleges and two-year colleges. These differences are not statistically significant, although if you add them all together, the point estimate is close to zero, implying there is roughly no difference in the total number of colleges in normal counties.

Because there is no difference in the total number of colleges, we might think the effect of having been assigned a normal school is small. However, regional universities created from normal schools are much larger in size than other universities.

[24]There were two universities that were previously normal schools that existed as regional colleges or universities in 1980, but their data are not included in the 1980 IPEDS data. We include their counties as having a normal school converted to a regional college in row 1 of Table 2, but data for these universities are not included in the remainder of the table since they were not reported in 1980.

Table 2: County Characteristics in 1980

	(1) Variable Means Normal	(2) Variable Means Asylum	(3) Difference in Means With State FE (1) - (2)
Panel A: County-level Higher Education Sector			
Has regional college formerly normal school	0.91	0.00	0.93***
	(0.29)	(0.00)	(0.02)
Total public four-year colleges	1.11	0.44	0.69***
	(0.67)	(0.88)	(0.12)
Total private four-year colleges	1.39	1.94	-0.45
	(3.27)	(4.62)	(0.53)
Total two-year colleges	0.97	1.16	-0.22
	(2.17)	(2.17)	(0.31)
Enrollment as % of population	11.72	4.56	8.41***
	(9.23)	(5.51)	(1.59)
Full-time enrollment as % of population	8.52	2.97	6.48***
	(7.4)	(4.34)	(1.26)
Total degrees awarded as % of population	3.04	0.93	2.47***
	(2.77)	(1.41)	(0.5)
Bachelor's degrees awarded as % of population	1.43	0.39	1.23***
	(1.38)	(0.69)	(0.25)
% Population over 25 with Bachelor's degree	16.57	15.02	2.04**
	(4.79)	(6.1)	(0.86)
% Population over 25 with 1-3 years college	15.40	15.01	0.57
	(3.89)	(3.97)	(0.35)
Panel B: County Characteristics			
Total population (1,000 ppl)	226.05	266.76	-26.28
	(601.44)	(560.22)	(80.55)
% Population growth 1950 to 1980	51.66	50.07	-0.13
	(65.51)	(72.6)	(9.89)
Total employment (1,000 ppl)	119.70	151.42	-21.32
	(342.4)	(359.61)	(51.34)
% Total employment growth 1950 to 1980	96.03	99.15	-7.64
	(99.35)	(91.42)	(13.2)
Civilian LFPR, age $\geq$ 30	57.22	57.78	-0.04
	(5.21)	(5.51)	(0.7)
Unemployment rate, age $\geq$ 30	4.75	4.54	0.21
	(1.83)	(2.05)	(0.22)
Poverty rate 1979, age $\geq$ 40	11.49	9.94	0.56
	(5.53)	(4.4)	(0.49)
Log median income, full-time year-round female workers, 1979	2.35	2.41	-0.03***
	(0.12)	(0.12)	(0.01)
Log median income, full-time year-round male workers, 1979	2.89	2.95	-0.03*
	(0.15)	(0.14)	(0.01)
% manufacturing employment	16.45	17.67	-1.49*
	(8.69)	(8.26)	(0.82)
% retail employment	16.31	15.49	1.13***
	(2.58)	(2.82)	(0.31)
Per capita personal transfer receipts	1,211.60	1,224.76	-25.48
	(245.82)	(299.63)	(41.74)

Notes: Columns (1) and (2) show means and standard deviations in parentheses. For panel A, column (1) includes 204 normal counties, and column (2) includes 126 asylum counties. Data are from NHGIS and IPEDS. Column (3) displays coefficients from regressing each variable on the normal county indicator with state fixed effects. Regressions in column 3 consist of 204 normal counties and 126 asylum counties. For panel B, the table shows summary statistics for normal and asylum counties using 1980 BEA and NHGIS data. Using log total population and log total employment as dependent variables also yields estimates that are not significantly different from zero (coefficient and standard error for log total population are 0.19 and 0.17, and coefficient and standard error for log employment are -0.20 and 0.19). Using log per capita government transfers also yields statistically insignificant coefficients. For each variable in panel B except percent retail employment, civilian LFPR, unemployment rate, poverty rate, and log median income, the regression consists of 200 normal counties and 126 asylum counties. For percent retail employment, the regression consists of 199 normal and 126 asylum counties. For civilian LFPR, unemployment rate, and log median income, the regression consists of 204 normal counties and 126 asylum counties. BEA data are missing for four normal counties that are small in size. In column 3 we present standard errors clustered at the state level in parentheses.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Using university-level enrollment data from the Integrated Postsecondary Education Data System (IPEDS), enrollment at all Title-IV-eligible universities located in asylum counties is equivalent to roughly 5 percent of the county population. In a normal county, this is higher by more than 8 percentage points—two and a half times larger than in asylum counties. Most of this difference is driven by full-time enrollment which is seen in the next row. Similarly, the number of degrees awarded is more than three times as high in normal counties compared to asylum counties, and that is at least partially driven by bachelor's degrees. This table shows that having been assigned a normal school greatly increases the likelihood of having a large and public university in the county.[25] As we will discuss, both of these may affect resilience.

Based on the 1987 Carnegie classification of Institutions of Higher Education, none of the regional universities converted from normal schools were Research I, and less than 2 percent were Research II institutions. Nearly 83 percent were listed as Comprehensive I or II, which are universities that are committed to graduate education through the master's degree (but not through the doctorate degree).

As a final measure of education, we use U.S. Census data to look at the share of the population over 25 that has a bachelor's degree in 1980. We find an additional 2 percent of the population has a bachelor's degree.[26] This effect is nontrivial, given a mean of 15% in asylum counties. However, this is not much larger than the number of bachelor's degrees awarded in every year. So while normal schools do increase the education level of the county, the effect is small compared to the number of students, consistent with students mostly leaving the county after graduation. There is no difference in the share of the population with one to three years of college.

Consistent with students leaving after graduation, normal school counties have a much

[25] The larger enrollment in normal school counties is driven by the university that was converted from a normal school. Of the 183 normal counties with normal schools that were transformed to regional universities and reported data to IPEDS in 1980, total enrollment at all universities was 12.8 percent of the population on average. Enrollment at the previous normal schools in those counties was 10.5 percent of population on average.

[26] Andrews, Russell and Yu (2021) also find universities have a positive effect on educational attainment.

larger share of 18 to 23 year olds (of the over 25 population), but the rest of the age distribution looks indistinguishable (Appendix Figure A9).

2.2 Effect on Population, Earnings, and Local Industry Mix

We also analyze the effect of normal school assignment on local economic characteristics in 1980, near the starting point of much of our analysis (Table 2 Panel B). Asylum counties have larger population and employment, although the differences are not statistically significant. They are also not on different trends over the 1950 to 1980 period.[27] The civilian labor force participation rate and the unemployment rates, for those above 30, are indistinguishable. Similarly, the poverty rates for those over 40 are not statistically distinguishable. We focus on the above 30 or above 40 demographic to avoid the effect from students who are not working. We show these data without restricting to this older demographic in Appendix Table A11.

Full-time year-round workers, both male and female, have a slightly higher median income in asylum counties, which is statistically significant. One possible reason for this is small differences in sectoral composition. We look at major industry categories based on SIC codes in 1980 using data from the Bureau of Economic Analysis. These cover most industry-county combinations, although there are some missing values. For presentation purposes, we show only industries for which the industry share differs by more than 0.5 percent between normal and asylum counties, or is statistically different.

The most statistically significant difference is that the share employed in retail trade is roughly 1.1 percentage points higher in normal counties relative to asylum counties. This is intuitive, as retail stores might serve a large student body. We also find a smaller manufacturing share.[28] All industries, including NAICS industries in 2001, are available in Appendix

[27]This is consistent with the finding in Andrews (2021) that population growth is relatively similar in counties receiving a university and those receiving a consolation prize (including an asylum).

[28]In 2001, we see a similar pattern, with normal counties having a higher share employed in retail trade, and accommodation and food services, which was not its own category in 1980. Interestingly, we see a smaller share employed in wholesale trade. Though not statistically significant, we do see more government

Tables A13 and A14.

We find no significant differences in per capita personal transfer receipts using BEA data. We also see no differences in unemployment benefits in particular (Appendix Table A11).

Overall, in this section we find that while normal schools have a large effect on the higher education presence in a county, there is little evidence that students stay around after graduating. Further, the conversion of normal schools to regional universities did not have a large effect on the local economy, when looking at growth from 1950-1980 and comparing to asylum counties. While there were large enrollment increases in normal counties, this was a period of national economic growth. Both normal and asylum counties are positively selected, and we do not see that the growth of the regional universities led to differential population growth. This may not be surprising given our focus on regional universities, which are less research intensive. As a result, we would expect them to be less likely to create differentially agglomerative economies relative to other positively selected counties.

3 Resilience to Manufacturing Declines

In this section we analyze the impact of exposure to manufacturing shocks, and whether this differs for normal counties relative to asylum counties. We focus on two episodes of pronounced manufacturing declines: declines in the Rust Belt starting around 1980 and national declines starting in the year 2000.[29]

The manufacturing declines starting around 1980 were the first persistent decline in U.S. manufacturing in the post-WWII period, and they followed a 20-year period in which U.S. manufacturing employment had grown substantially. These declines were highly concentrated in the Rust Belt. We focus on this setting as it allows us to study resilience to persistent manufacturing decline, starting from the initial point of this persistent decline.

and less manufacturing employment.

[29]In Appendices B and C, we consider two other types of resilience. First, we look at whether regional universities make local economies resilient to the decline in mining employment after 1981. We also look at whether regional universities cause local economies to be resilient to the business cycle.

The manufacturing declines in the 2000s were very large, persistent, and in all regions of the country. For these reasons, we focus on resilience to these declines as well. However, unlike the Rust Belt declines, for many areas these declines were accelerations of previous persistent declines in the 1980s and 1990s.

Using our sample of normal and asylum counties, we estimate:

$$y_i = \beta_1 \text{Normal}_i + \beta_2 \text{Mfg Exposure}_i + \beta_3 \text{Normal}_i \times \text{Mfg Exposure}_i + \alpha_s + X_i\gamma + \epsilon_{it} \qquad (1)$$

where y_i measures long-run percentage growth of various outcomes, including employment, population, earnings, and per capita personal transfer receipts; α_s is a state fixed effect; and X_i includes controls. Our data on outcomes are from the BEA.[30]

Equation (1) estimates the reduced-form effect of having a normal school, which includes a small fraction (9%) of normal school counties that do not currently have a regional university (Table 2 Panel A). Thus, if there is a positive effect of regional universities, equation (1) will slightly underestimate this effect. We address this more directly with an IV specification in the robustness section.

We estimate separate specifications for the Rust Belt and national manufacturing declines. For the Rust Belt shock, Mfg Exposure$_i$ is the 1978 share of county employment in manufacturing, the year manufacturing employment peaked in the U.S. and before a period of permanent decline in the Rust Belt states.[31] We limit this analysis to Rust Belt states, given differences in manufacturing trends across regions (see Figure A8). Following Alder, Lagakos and Ohanian (2019), we define Rust Belt states as Illinois, Indiana, Michigan, New

[30]Several counties have missing values for manufacturing exposure in 2000, and several are dropped because there is only one normal or asylum county in the state. This yields a regression sample of 198 normal counties and 122 asylum counties when looking at manufacturing declines in the 2000s. All of the counties in the Rust Belt sample are in states that have at least two normal counties and two asylum counties. In the regression sample for the 2000s manufacturing decline, 310 of the 320 counties are in states with at least two normal and two asylum counties. Excluding the four states that have only normal counties yields very similar results.

[31]Rust Belt states did experience declines in manufacturing employment following the 1969 and 1973 recessions, but there were important recoveries afterwards. By 1978, manufacturing employment in Rust Belt states was 91 percent of 1969 manufacturing employment after dropping to 84 percent in 1975.

York, Ohio, Pennsylvania, West Virginia, and Wisconsin. States outside the Rust Belt experience large manufacturing declines especially starting in 2000. If we included these states, the 1978 manufacturing share may not capture exposure to the large declines starting in 2000. As we discuss below, we also show results that do not restrict to Rust Belt states.

For the national manufacturing declines starting in 2000, Mfg Exposure$_i$ is the share employed in manufacturing in the year 2000. Because all census regions experience a manufacturing decline starting in 2000 (see Figure A8), for these specifications we look at normal and asylum counties throughout the U.S. and growth from 2000-2018.

The coefficient β_2, which we expect to be negative, measures the impact of additional manufacturing exposure in asylum counties. The coefficient β_3 measures the differential impact of manufacturing exposure in normal counties. We are interested in whether $\beta_3 > 0$, implying that universities helped mitigate the negative impact of manufacturing exposure.

To improve precision, we include controls for log population in 1950 and log population in 1980 when studying the acceleration of manufacturing declines in 2000. For the Rust Belt regression we use log population in 1950 and in 1978 given we study growth from 1978 to 2018. If growth is persistent, then including the controls will reduce the standard error. Given that Table 2 shows no differential population growth from 1950 to 1980 in normal versus asylum counties, we do not expect that these controls would affect the point estimates. We show evidence of this in the tables to come. Given the small number of states in the Rust Belt specification, we present unclustered standard errors robust to heteroskedasticity, as well as p-values based on randomization inference.[32] For the 2000s manufacturing decline, in which we have 44 states, we present standard errors clustered at the state level, as well as p-values based on randomization inference.

One potential concern might be that having a university in the normal school counties indirectly affects the asylum counties, possibly through governments diverting resources to the university from other parts of the state. In the case of these fiscal spillovers, we think

[32]We permute within states the assignment of normal counties, among our sample of normal and asylum counties. This is based on 1000 permutations.

these are likely to be small because they would draw from many counties throughout the state. Moreover, the coefficients can still be interpreted as the effect of having a regional university in that county relative to a counterfactual of having a regional university somewhere else in the state (rather than a counterfactual of having no regional university).

3.1 Effects of Exposure to the Rust Belt Manufacturing Shock

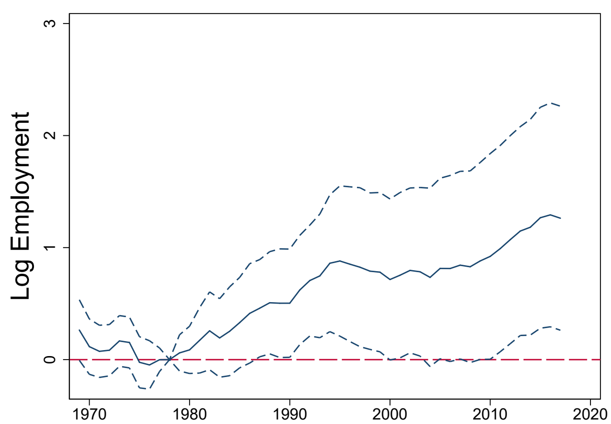
We start by estimating a version of equation (1) that allows us to observe how the differential effect of manufacturing exposure in normal counties evolves over time, and whether there are differences by manufacturing exposure preceding the adverse manufacturing shock.

We estimate:

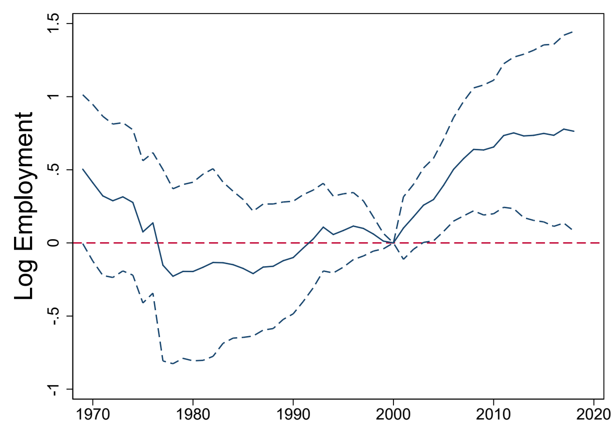
$$\begin{gathered}\mathrm{y}_{it} = \beta_t \mathrm{Normal}_i \times \mathrm{Mfg\ Exposure}_i + \kappa_t \mathrm{Mfg\ Exposure}_i + \rho_t \mathrm{Normal}_i + \\ \lambda_t \mathrm{Ln(Pop\ 1950)}_i + \psi_t \mathrm{Ln(Pop\ 1980)}_i + \alpha_{st} + \gamma_i + \epsilon_{it}\end{gathered}$$

We note that the coefficient $\hat{\beta}_{2018}$ in this specification would be the same as the coefficient $\hat{\beta}_3$ in equation (1) if the outcome in (1) was difference in log employment between the base year and 2018, rather than the percent employment growth over those years. The coefficients β_t in this specification quantify the differential effect of baseline manufacturing share on employment in normal relative to asylum counties, relative to the base year (1978 for the Rust Belt analysis and 2000 for the 2000s manufacturing declines). We include state-year fixed effects, to compare normal and asylum counties within the same state. As in equation (1) we allow employment growth to also be a function of population growth between 1950 and 1980 (1978 for the Rust Belt shock).

Figure 2a shows increasing the 1978 share employed in manufacturing has a differentially positive (less negative) effect on log employment in normal relative to asylum counties in the same state. This differential effect starts just after U.S. manufacturing reaches its peak in 1978. This provides important evidence that results are not driven by persistent differences between normal and asylum counties, that pre-dated manufacturing shocks. The differential



(a) Rust Belt, Changes from 1978



(b) All Counties, Changes from 2000

Figure 2: **Differential Effect of Manufacturing Exposure on Normal Counties Relative to Asylum Counties.** Panel A shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 1978 share employed in manufacturing. Effects are relative to 1978, and include county and state-year fixed effects, lower-level terms, and interactions between year fixed effects and ln(population, 1950), and separately ln(population, 1978). Panel B shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 2000 share employed in manufacturing. Effects are relative to 2000, and include county and state-year fixed effects, lower-level terms, and interactions between year fixed effects and ln(population, 1950), and separately ln(population, 1980). Dotted lines are 95 percent confidence intervals, with standard errors clustered at the county level in Panel A and at the state level in Panel B.

effect of exposure in normal counties grows through the late 1980s, and then again after the 1990 recession. This differential effect then remains flat, before growing again around the Great Recession. Increasing 1978 manufacturing share by 10 percentage points in asylum counties reduces the 1978-2018 difference in log employment by .15 log points (based on the coefficient κ_{2018}, not shown in the plot). But in normal counties, this additional exposure has a statistically significantly less negative effect, and the total effect is close to zero, given the coefficient β_{2018} is roughly 1.3. Thus, raising manufacturing share by .1 yields a differentially positive effect of .13 log points in normal counties, counterbalancing the negative effect of .15 log points in asylum counties. Appendix Figure A10 shows similar plots for the other outcome variables.

Table 3 shows the results from specification (1). We find that regional universities improved resilience to negative manufacturing shocks. Column 1 shows that among asylum counties in Rust Belt states, increasing 1978 manufacturing share by 10 percentage points is associated with 1978-2018 percent employment growth lower by 24 percentage points. However, the effect of exposure is statistically significantly smaller among normal counties. Strikingly, the magnitude suggests no negative impact of exposure, implying these universities enabled full resilience to the negative impacts of additional exposure. Including state fixed effects implies the differential effect of exposure in normal counties is not statistically significant, though the magnitude is still quite large. Controlling for 1950-1978 population growth leads to an R-squared that is over two and a half times as large, and a substantial reduction in the standard error so that the estimate is now significant at the 5% level.[33] The point estimate increases modestly, but the increased significance is driven by the reduction in the standard error.[34]

[33] Appendix Figure A14 shows binned scatter plots of residualized employment growth on manufacturing exposure, separately for normal and asylum counties. These plots show that residualized employment growth is falling over much of the range of manufacturing exposure for asylum counties, but this relationship is much flatter for normal counties.

[34] Appendix Table A16 shows that adding additional county-level control variables measured in 1980 or pre-1980 leads to a very similar coefficient, statistically significant at the 5 percent level, and a 21 percent increase in the R-squared.

An important context for our results in Table 3 is that the size of the manufacturing decline is comparable in normal and asylum counties. We do not see a statistically significant difference in the employment growth of the manufacturing sector (Appendix Table A10). In other words, the difference in resilience that we see in Table 3 is due to differential growth in sectors outside of manufacturing.

We also see statistically significant differential effects of manufacturing exposure on population growth, average earnings, and per capita transfer receipts in normal relative to asylum counties. These effects also imply nearly full resilience. Effects on transfer receipts appear to be concentrated in the "retirement and other" transfer receipt category (which includes Social Security, Medicare, and Medicaid, as well as other receipts), and income maintenance benefits, although the latter is not statistically significant.[35] The differentially smaller growth in transfers in high-manufacturing normal relative to asylum counties suggests government funding for education may reduce the need for government funding in other areas.

3.2 Effects of Exposure to Manufacturing Declines Starting in 2000

Our results for the whole country over the 2000 to 2018 period are largely consistent with what we found for the Rust Belt over 1978 to 2018.

Figure 2b shows increasing the 2000 manufacturing employment share has a differentially positive (less negative) effect on log employment in normal relative to asylum counties, starting just after the national manufacturing declines of 2000. This differential effect of exposure continues to grow through 2018. Increasing 2000 manufacturing share by 10 percentage points in asylum counties reduces the 2000-2018 difference in log employment by .1 log points (based on the coefficient κ_{2018}, not shown in the plot). But in normal counties, this additional exposure has a statistically significantly less negative effect, and the total effect is close to zero, given the coefficient β_{2018} is roughly .8. Thus, raising manufacturing share by .1 yields a differentially positive effect of .08 log points in normal relative to asylum counties,

[35]Income maintenance includes Supplemental Security Income, the Earned Income Tax Credit, Supplemental Nutritional Assistance, and other programs.

Table 3: The Differential Effect of Manufacturing Share on Normal and Asylum Counties

Panel A: The Rust Belt Shock and Differential 1978-2018 Changes

Y = % Growth	Employment	Empl.	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 1978	-2.426**	-1.992**	-2.374***	-1.122**	-5.489***	8.787***
	(0.977)	(0.937)	(0.552)	(0.545)	(1.536)	(2.308)
Normal*Mfg. Share, 1978	2.527**	1.692	1.967***	1.167*	4.446***	-7.759**
	(1.132)	(1.136)	(0.726)	(0.656)	(1.561)	(3.227)
Observations	103	103	103	103	103	103
R-Squared	0.078	0.219	0.543	0.426	0.326	0.524
State FE	N	Y	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	N	N	Y	Y	Y	Y
P-value, randomization inference	.016	.102	.009	.069	.001	.051

Panel B: 2000 Manufacturing Shock and Differential 2000-2018 Changes

Y = % Growth	Empl.	Empl.	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 2000	-1.498***	-1.397***	-1.192***	-0.599**	-0.666***	0.563**
	(0.384)	(0.446)	(0.426)	(0.264)	(0.127)	(0.242)
Normal*Mfg. Share, 2000	0.920*	1.088**	0.994**	0.595*	0.294	-0.230
	(0.486)	(0.520)	(0.486)	(0.346)	(0.228)	(0.300)
Observations	325	320	320	320	320	320
R-Squared	0.056	0.311	0.378	0.475	0.467	0.563
State FE	N	Y	Y	Y	Y	Y
Control for 1950-1980 Pop. Growth	N	N	Y	Y	Y	Y
P-value, randomization inference	.054	.021	.025	.061	.223	.564

Notes: Dependent variable in Panel A and Panel B is $(Y_t/Y_{t-1}) - 1$. In Panel A, robust standard errors are in parentheses, and in Panel B standard errors clustered at the state level are in parentheses. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978). Columns that include controls for 1950-1980 population growth include Ln(Population, 1950) and Ln(Population, 1980) as additional control variables. P-values based on randomization inference are obtained by permuting within states the assignment of normal counties, among our sample of normal and asylum counties. This is based on 1000 permutations.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

counterbalancing the negative effect of .1 log points in asylum counties.[36] Appendix Figure A11 shows similar plots for the other outcome variables.

Column 1 of Table 3 shows that among asylum counties, increasing 2000 manufacturing share by 10 percentage points is associated with 2000-2018 percent employment growth lower by 15 percentage points. However, among normal counties that effect is statistically significantly smaller, by 9.2 percentage points. The results are slightly larger and significant at the 5 percent level when including state fixed effects, and similar when including controls for 1950-1980 population growth.[37] We again see similar results for population growth.

As in the Rust Belt results, Appendix Table A10 shows that the resilience is primarily driven by industries outside of manufacturing.

In asylum counties, higher manufacturing exposure is correlated with lower growth in earnings per job and higher growth in per capita transfer receipts. While the point estimates suggest these changes are less adverse in normal counties, they are not precisely estimated.

3.3 Robustness

Our first robustness check is to analyze resilience to manufacturing decline in the entire country starting in 1978. U.S. manufacturing employment peaked in 1978, and declined over the following decades, with accelerated declines in the 2000s. As an additional specification we analyze 1978 manufacturing exposure and growth from 1978-2018 in all counties. This specification may capture additional declines over this longer time period, rather than limiting our analysis to 2000-2018. Appendix Table A15 shows that when looking at all counties,

[36]While not statistically significant, we see some evidence that the magnitude increases much more modestly surrounding the 1990 recession. Correlation between manufacturing share in 2000 and manufacturing share in 1978 will imply that counties exposed to the accelerated declines of the 2000s were also exposed to earlier declines. If regional universities increase resilience, then this will imply a differential effect of 2000 manufacturing share before 2000. Most notably, Rust Belt counties experienced declines in the 1980s as well as 2000s. Excluding the Rust Belt counties from Figure A11 yields a flatter line before 2000.

[37]Appendix Table A16 shows that including additional county-level controls measured in 1980 or pre-1980 yields a very similar coefficient, statistically significant at the 5% level, and a 26% increase in the R-squared. Appendix Figure A14 shows binned scatter plots of residualized employment growth on manufacturing exposure, separately for normal and asylum counties. These plots show that residualized employment growth is falling over much of the range of manufacturing exposure for asylum counties, but this relationship is much flatter for normal counties.

the differential effect of 1978 manufacturing exposure on employment, population, and average earnings per job growth from 1978-2018 was statistically significantly less negative in normal counties than in asylum counties. The estimated effect on per capita personal current transfer receipts was also more negative in normal counties, but in this case the result is not statistically significant.[38]

A second robustness check uses a discrete measure of manufacturing intensity instead of the continuous measure. Appendix Tables A19 and A20 show similar patterns using an indicator for above median manufacturing share in 1978 and 2000 respectively, though the effects are less precisely estimated and not statistically significant.

As we discussed above, some normal counties do not have universities. Further, some asylum counties do have public universities. As a result, our reduced-form specification will underestimate the resilience effects of a regional public university. As an alternative specification, we analyze the impact of having a four-year regional public university on resilience, using historical assignment of a normal school as an instrument. As expected, this specification yields larger effects.[39] (Tables A21, A22).

Results are also robust to dropping counties that were assigned normal schools and asylums (Tables A23 and A24). Appendix F.17 presents results using a matching strategy to identify a control group for normal counties. These results differ from our main results, which we believe underscores the importance of our identification strategy.

While we believe the identification assumptions are less likely to hold, we additionally compare counties receiving public research universities. Appendix F.16 shows the negative impact of 2000 manufacturing exposure is substantially smaller in counties that were as-

[38]Appendix Table A18 shows the results by transfer component.

[39]As a proxy for having a four-year regional public university, we identify counties with four-year public universities in 1987 that are of the same 1987 Carnegie classification as the normal schools that were converted to regional public universities. This excludes Research I universities for example, and the distribution of Carnegie classifications among our set of regional public universities is very close to the distribution among the normal schools that were converted to regional universities. Our main specification interacts presence of a university with manufacturing share. Following Wooldridge (2002), we use two-stage least squares, instrumenting for two endogenous variables: presence of a regional public university and this interacted with manufacturing share. The instruments are normal school assignment, and this interacted with manufacturing share.

signed public research universities between 1830 and 1930 relative to asylum counties. The magnitude is about 20 percent larger than the differential effect in normal counties, but this difference is also not statistically significant.

Finally, we also consider whether regional public universities improve resilience to other shocks. Appendix B shows universities enable resilience to the 1980s mining decline (though the resilience is closer to two thirds, rather than full), and Appendix C show universities enable resilience to the business cycle. While the immediate impact of the recession is similar in normal and asylum counties, we find that by the second year after a business cycle peak, impacts on employment and income are less severe in normal counties.

4 Mechanisms

In this section, we consider the mechanisms through which universities make local economies more resilient. Here, we focus on three potential mediators: university spending growth, the baseline share of the population with a bachelor's degree, and the growth of that share. We illustrate these channels using a directed acyclic graph (DAG) in Appendix Figure A7, and our evaluation of these mechanisms follows the mediation analysis framework from the statistics literature.[40]

To preview our findings, we show that university spending growth is a quantitatively important mediator. We also find that the baseline bachelor's share is higher in normal versus asylum counties, for the average-manufacturing-share county. Unlike spending, we have no causal estimates from the literature of the impact of bachelor's share on income growth. Instead, we descriptively show that controlling for baseline bachelor's share and its interaction with manufacturing share, leads to a modest to substantial reduction in the estimated differ-

[40]For example, see Imai, Keele and Yamamoto (2010), and Heckman and Pinto (2015) for a presentation of econometric mediation analysis. We will at times use the mediation analysis terminology. A mediator is an intermediate outcome through which causality flows, i.e. B is a mediator if A causes B and B causes C. A moderator is a separate variable that affects how much another independent variable affects an outcome, i.e. B is a moderator if the effect A has on C depends on B. In Section 3, we analyzed whether having a normal school moderates the effect of manufacturing share employment.

ential resilience in normal counties. This is consistent with universities facilitating resilience through their impact on local education levels.

4.1 Spending Growth

We hypothesize that historical normal school assignment reduces, or moderates, the negative effect of manufacturing exposure on university spending, and this partially explains the differential resilience of income growth in normal counties. There are several reasons why university spending in normal counties may be more resilient to manufacturing decline. Student demand at regional public universities may be more resilient than at private universities, due to potential tuition differences. Alternatively, funding for these universities may be more resilient because they are public.

We obtain university spending data from IPEDS, which includes wages and benefits, as well as operational expenses, but excludes capital outlays such as construction, and we aggregate to the county level.[41] We measure the change in real university spending as a fraction of initial county-level personal income, deflating nominal levels by CPI. Because our objective is to understand the impact of university spending on the local economy, we focus on universities whose enrollment is not predominantly distance education.[42]

We estimate the following:

$$\frac{\Delta \text{Spending}_i}{\text{Income}_{i,t-1}} = \beta_1 \text{Mfg Share}_i \times \text{Normal}_i + \beta_2 \text{Mfg Share} + \beta_3 \text{Normal}_i + \beta_4 X_i + \epsilon_i \tag{2}$$

X_i are the same controls as in equation (1). This regression tests whether having a normal school reduces, or moderates, the negative effect of manufacturing exposure on university

[41] Operational expenses include expenses for janitorial services, building maintenance, groundskeeping, and security, as well as other categories.

[42] Specifically, we restrict to universities at which less than 50 percent of total enrollment was enrolled exclusively in distance education in 2018. When looking at changes in spending over time, we include only these universities (based on the 2018 measure) in each year, and aggregate at the county-year level. Our results are robust to using spending at all universities, regardless of the extent of distance education (Table A8).

spending growth.

To quantify university spending's role as a mediator, we could estimate the following:

$$\frac{\Delta \text{Income}_i}{\text{Income}_{i,t-1}} = \delta_1 \text{Mfg Share}_i \times \text{Normal}_i + \delta_2 \text{Mfg Share} + \delta_3 \text{Normal}_i + \delta_4 \frac{\Delta \text{Spending}_i}{\text{Income}_{i,t-1}} + \delta_5 X_i + \eta_i \tag{3}$$

If we believed that δ_4 were the causal impact of university spending growth on income growth, we could identify the mediating effect as the product of β_1 and δ_4. The required identifying assumption to interpret δ_4 causally is typically called the "sequential ignorability" assumption, and requires that spending growth be uncorrelated with the error term, conditional on the other terms. While we have argued throughout the paper that β_1 is identified, university spending growth in equation (3) may be correlated with other variables that are correlated with income growth. Thus, assigning a causal role to spending growth is challenging in this regression. Therefore, our preferred estimate of the mediation effect is β_1 times the literature's estimate of the local multiplier on spending.[43]

We find that university spending is differentially resilient in normal counties to manufacturing declines (Table 4 column (1)). When the dependent variable is university spending growth the coefficient on Normal $\times$ Mfg Share is significantly positive, for both the Rust Belt and the 2000s shocks. Comparing the coefficients in Table 4 column (1) to our baseline result, the resilience of university spending is about 15 percent of the total resilience.

When the dependent variable is income growth (equation (3), the coefficient on university spending is positive, but imprecisely estimated (Table 5 column (2)).[44] However, as we discussed, university spending is likely endogenous, and we do not interpret this estimate

[43]One might imagine that spending growth could play a moderating role in addition to the mediating role it is given here. While we think this is second-order, we do show this specification in Appendix D.

[44]One might wonder if other types of government would provide similar resilience. In Table A10, we look at whether State and Local government employment grows differently in response to manufacturing exposure in normal versus asylum counties. In the 2000s shock, we find that State and Local government employment is more resilient in normal counties, suggesting there is not some other type of government employment that is equally resilient in asylum counties. For the Rust Belt shock, we do not find such a result, but it is significantly noisier.

as the causal effect of university spending on income growth. Instead, we borrow from the literature on the local spending multiplier. These papers, from a variety of settings, present a range of multiplier estimates from 1.25 to 5 or more.[45] Taking a multiplier of two, near the bottom of this range, suggests that university spending is responsible for thirty percent of the resilience. One good reason to suspect a multiplier greater than one is that when we present a decomposition of resilience by sector in Appendix E, we see an increase in employment across a variety of non-tradable sectors.

This analysis does not include differential resilience of student spending in normal counties or any multiplier from this spending, implying it presumably underestimates universities' contribution to resilience. We also likely underestimate the total effect because the data do not include university spending on construction projects. We show in Table A10 that construction employment contributed to the estimated resilience. Taking the share of construction spending from Delaney and Doyle (2014), if construction spending were everywhere proportional to other spending, we would want to increase our estimates by about 40 percent. In this case the resilience of university spending would explain more than forty percent of the resilience.

Overall, once we take into account local multipliers and that our expenditure data do not include student or construction spending, we conclude that the fraction of the resilience coming from university spending is substantially larger than 15 percent and probably close to half.

[45]We focus on studies that identify longer-run multipliers, given that we study long-run growth. There are a number of economic impact studies of universities which tend to report short-run multipliers based on regional economic models (Leslie and Slaughter, 1992). Siegfried, Sanderson and McHenry (2007) review and address problems associated with many of these studies. Even though they address concerns about these studies, they report that of the 19 studies of expenditure multipliers, the range was 1.3 to 2.5 with a median of 1.7. Moretti (2010) presents a multiplier from skilled tradable jobs to local goods and services (3.5), Weinstein (2018) from financial services (6), Moretti and Wilson (2014) from biotech (13.5), Marchand (2012) from energy jobs (2), Black, McKinnish and Sanders (2005) from coal (1.25), and Zou (2018) from military contractions (2.2).

Table 4: Mediation Analysis, Effect of Normal Schools and Manufacturing on Potential Mediators

Panel A: Rust Belt, 1978-2018

	(1) Univ. Spending Growth	(2) Bachelor's Share, 1980	(3) Change in BA Share
Normal	0.0478**	0.0187*	-0.00194
	(0.0210)	(0.0109)	(0.00744)
Manufacturing Share, 1978	-0.441*	-0.390***	-0.214***
	(0.245)	(0.133)	(0.0696)
Normal × Manuf Share, 1978	0.480*	0.283**	0.129
	(0.277)	(0.133)	(0.0805)
Observations	103	103	103

Panel B: All Counties, 2000-2018

	(1) Univ. Spending Growth	(2) Bachelor's Share, 2000	(3) Change in BA Share
Normal	0.0123***	0.0185**	0.00119
	(0.00381)	(0.00773)	(0.00260)
Manufacturing Share, 2000	-0.168**	-0.405***	-0.0934**
	(0.0667)	(0.116)	(0.0364)
Normal × Manuf Share, 2000	0.130*	0.00439	-0.000636
	(0.0684)	(0.127)	(0.0491)
Observations	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Manufacturing share is demeaned, so the coefficient on Normal can be interpreted as the average effect on the potential mediator. University spending growth and Change in BA share in Panel A are measured from 1980 to 2018. In Panel B, they are measured from 2000 to 2018. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

4.2 Bachelor's Share

We also hypothesize that having a normal school increases the education level of the county, and this reduces, or moderates, the negative effect of manufacturing exposure on employment (as depicted in Figure A7). For example, local economies with higher education levels may more easily shift sectoral composition in response to a negative shock. The effect of regional public universities on the BA share may also be increased, or moderated, by higher manufacturing exposure (drawn in a dashed line in Figure A7c). For example, higher manufacturing share counties may have lower demand for bachelor's degrees which would lower the bachelor's degree share, except if there is a local university which lowers the cost.[46]

Previous literature has also studied the bachelor's degree share, and its role in economic resilience and growth, while also acknowledging that the bachelor's share is not exogenous (Glaeser and Saiz, 2004; Feyrer, Sacerdote and Stern, 2007; Shapiro, 2006). While we show the causal effect of regional public universities on the bachelor's share, we describe the relationship between bachelor's share and resilience more descriptively.

We measure the bachelor's share in the over 25 population using Census data obtained from NHGIS. For the Rust Belt analysis, we use the bachelor's share in 1980. For the 2000s, we use the bachelor's share in 2000. We estimate equation (4), demeaning manufacturing share, so that the coefficients on Normal can be interpreted as the effect for the average-manufacturing-share county.

$$\text{BA Share}_i = \beta_1 \text{Normal}_i + \beta_2 \text{Mfg Share}_i + \beta_3 \text{Mfg Share}_i \times \text{Normal}_i + \beta_4 X_i + \epsilon_i \qquad (4)$$

As with the main analysis of our paper, we interpret β_1 as the causal effect of regional public universities on the BA share. To test whether this causal effect of universities on BA share is what explains the differential resilience in normal counties, we could estimate the

[46] Alternatively, agents may have been forward-looking and invested in education, if it was accessible, knowing that manufacturing would eventually decline.

following:

$$\frac{\Delta \text{Income}_i}{\text{Income}_{i,t-1}} = \delta_1 \text{Mfg Share}_i \times \text{Normal}_i + \delta_2 \text{Mfg Share}_i \times \text{BA Share}_i + \delta_3 \text{BA Share}_i + \delta_4 \text{Mfg Share}_i + \delta_5 \text{Normal}_i + \delta_6 X_i + \eta_i \quad (5)$$

We could then calculate the contribution of the BA share to resilience by multiplying β_1 and δ_2 and adding β_3 times δ_3.[47] However, we treat this very cautiously, given that the BA share is not exogenous. As previous research has discussed, it may be correlated with other variables that are also correlated with income growth, such as the economy's age distribution, amenities, and other unobservable variables.[48] We nonetheless find it informative to estimate equation (5), and observe descriptively whether the differential impact of manufacturing exposure in normal counties disappears after controlling for the bachelor's share and the differential effect this may have in high manufacturing counties.

Finally, exposure to manufacturing decline may negatively affect *growth* in the bachelor's degree share, and this may affect resilience. College-educated individuals may move in response to the negative shock, and local universities may have lower enrollments and thus lower likelihood of graduates remaining in the area. However, this negative effect of manufacturing exposure on BA share growth may be less negative in normal counties, for example because the universities are more resilient in these counties (Figure A7b).[49]

We measure the change in the BA share from 1980 or 2000 until the 2014-2018 ACS sample. We estimate equation (4) but instead use BA share growth as a dependent variable, to test whether manufacturing exposure less negatively affects BA share growth in normal counties.

[47] Using mediation-analysis terminology, we could calculate the moderating effect that is mediated by the BA share and the mediating effect of the BA share that is moderated by manufacturing.

[48] Using the mediation analysis terminology, this violates the sequential ignorability assumption.

[49] Using mediation-analysis terminology, we are interested in whether BA share *growth* serves as a mediator that is moderated by having a normal school.

We could then calculate the mediating effect of BA share growth by estimating:

$$\frac{\Delta \text{Income}_i}{\text{Income}_{i,t-1}} = \delta_1 \text{Mfg Share}_i \times \text{Normal}_i + \delta_2 \text{BA Share Growth}_i + \delta_3 \text{Mfg Share}_i + \delta_4 \text{Normal}_i + \delta_5 X_i + \eta_i \quad (6)$$

Because BA share growth and baseline BA share may be correlated, we estimate a single regression controlling for both channels. As above, we treat this very cautiously, given that BA share growth is not exogenous, and may be correlated with other variables.

We find that historical normal school assignment increases the bachelor's share preceding the shock, for both the Rust Belt and the 2000s shocks (Table 4, column 2). For average-manufacturing share counties, the bachelor's degree share is higher by roughly two percentage points in normal relative to asylum counties. As we saw in Table 2, this is a nontrivial difference, given the average in asylum counties in 1980 is roughly 15%. In 2000, the average in asylum counties is roughly 22%. If there is a causal impact of bachelor's share on resilience, the larger share in normal counties would partly explain why these counties are more resilient. For the Rust Belt shock, the differential bachelor's share in normal counties increases with, or is moderated by, manufacturing share.

For the Rust Belt, baseline bachelor's share is a statistically significant predictor of income growth (Table 5 Panel A column (3)), and this effect does not vary significantly with manufacturing share. The coefficient on Normal × Mfg Share falls by about one third, after controlling for the baseline bachelor's share and its interaction with manufacturing share (comparing columns 1 and 3).[50]

For the 2000s shock, neither the baseline bachelor's share nor the interaction with manufacturing share is a statistically significant predictor of income growth, though they are both positive, and they are jointly significant at the 1% level. Including these as covariates

[50] If we were to treat the coefficient on bachelor's share as a causal effect, we could also calculate its mediating effect by multiplying the coefficient on the interaction in Panel A column (2) with the coefficient on Bachelor's Share in Column (3), which totals 1.23. This is about a third of the total resilience (3.67).

Table 5: Mediation Analysis, Correlations between Mediators and Outcome

Panel A: Rust Belt, 1978-2018

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Income Growth	(5) Income Growth
Manufacturing Share, 1978	-3.823**	-3.564***	-0.812	-2.202**	0.244
	(1.465)	(1.304)	(2.187)	(1.041)	(1.883)
Normal × Manuf Share, 1978	3.496**	3.213**	2.316*	2.521**	2.175**
	(1.452)	(1.329)	(1.179)	(1.100)	(1.026)
Univ. Spending Growth		0.588			
		(0.611)			
Bachelor's Share, 1980			4.165**		1.588
			(1.768)		(1.462)
BA Share, 1980 × Manuf Share, 1978			-8.527		-12.35
			(13.61)		(11.43)
Change in BA Share				7.577***	6.742***
				(1.864)	(1.595)
Observations	103	103	103	103	103

Panel B: All Counties, 2000-2018

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Income Growth	(5) Income Growth
Manufacturing Share, 2000	-0.993**	-0.915**	-1.030	-0.562*	-0.865
	(0.398)	(0.417)	(0.976)	(0.299)	(0.775)
Normal × Manuf Share, 2000	0.825*	0.764	0.732	0.828*	0.726
	(0.481)	(0.485)	(0.575)	(0.415)	(0.504)
Univ. Spending Growth		0.464			
		(0.400)			
Bachelor's Share, 2000			0.542		0.00510
			(0.740)		(0.709)
BA Share, 2000 × Manuf Share, 2000			1.842		2.084
			(5.108)		(4.523)
Change in BA Share				4.604***	4.445***
				(1.368)	(1.511)
Observations	320	320	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Manufacturing share is demeaned, so the coefficient on non-interacted terms can be interpreted as the average effect. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

reduces the coefficient on Normal × Mfg Share by about 11% (comparing columns 1 and 3).[51]

While the baseline bachelor's share is higher in normal versus asylum counties, for the average-manufacturing-share county the BA share growth is not (Table 4 column (3)). However, for Rust Belt counties there is some evidence that the normal-asylum difference in BA share growth increases with manufacturing share. The interaction term has a point estimate of 0.129, which is not quite statistically significant. To the extent that there is a causal impact of BA share growth on income growth, this could explain some of the overall resilience we see in normal counties. In column 6, we see that including both baseline BA share and BA share growth as potential mediators and moderators still explains about one third of the overall resilience (3.496), if we were to interpret these as causal effects.

In Appendix D.3, we investigate whether historical normal school assignment affects baseline industry shares, and whether this explains why normal counties are more resilient. Overall, we find no evidence that industry composition plays a substantial mediating role. While not a formal mediation analysis, we also look at the composition of the resilience by sector in Appendix E. While we think that some industries may play a mediating role, we cannot disentangle which industries grow in a way that causes broader growth versus which industries are growing because of the local economic growth. We find employment gains across a variety of sectors, especially non-tradables, suggestive of a spending multiplier.[52]

In sum, our results suggest that university spending growth accounts for more than one-third of the differential resilience in normal counties. We also find that the baseline bachelor's share is higher in normal versus asylum counties, for the average-manufacturing-share county. Unfortunately, unlike spending, we have no causal estimates from the literature

[51] If we were to interpret the coefficient on $BAShare \times MfgShare$ as the causal moderating effect of BA share, it would be small for the 2000 shock (0.0185 times 1.842) compared to the total moderating effect of a normal school (0.825), although the standard error is also very large.

[52] We also find that the health care sector grew more in the 2000s, consistent with our motivating example of Pittsburgh, where its universities and hospitals are given credit as the industries that led its resilience. However, more broadly, health care is a great example of an industry that could be leading the growth or one that is expanding because the city is growing, so it does not lend itself to a formal mediation analysis.

on the impact of bachelor's share on income growth. Controlling for baseline bachelor's share and its interaction with manufacturing share, we see a reduction in the estimated differential resilience in normal counties (coefficient on Normal $\times$ Mfg Share), roughly one third in the case of the Rust Belt and 11% in the case of the 2000s shock. These results are consistent with bachelor's share improving local economic resilience; however, as with the prior literature we are cautious about interpreting these as causal impacts.

5 Conclusion

We investigate whether regional universities make the local economy more resilient to economic shocks, using a novel identification strategy: comparing counties assigned normal schools to counties assigned insane asylums by state governments in the 19th and early 20th centuries. Overall, we find that regional universities do increase local resilience to recent adverse shocks.

Would creating a new regional public university today provide similar resilience to future shocks? Our mechanisms analysis helps answer this question of external validity. We show that a significant percentage of the resilience comes from resilient public university spending. As a result, a new university would intuitively yield similar effects, provided it makes similar budget decisions and receives similar revenues, including state funding. We also showed that regional universities increase the share of the local population with a bachelor's degree. This may take some time to achieve.

A second observation about external validity is that we compare counties with universities to those that received other important public institutions. If public institutions increase resilience, these counties will be more resilient than the average county, even though they do not have universities. While only speculative, it seems intuitive that placing universities in areas without important public institutions may have larger effects.

As we showed in Table 2, normal and asylum counties have similar numbers of colleges overall, implying something specific about regional public universities provides resilience. The obvious candidates are that these universities are public and they are substantially larger relative to the population, both of which are consistent with our mechanisms analysis. We hope further research will continue to provide insight here. This also relates to the question of whether expanding universities on the intensive margin would have similar effects, and whether there are returns to scale in university size. Our strategy identifies the extensive margin, and so these are policy-relevant questions for future research.

We think our findings fit into a much larger discussion of the costs and benefits of funding universities. Policy proposals often cite local economic development and resilience as one reason to increase support for universities. A recent example is Maxim and Muro (2021), which advocates for federal funding for regional universities as a way to help distressed communities. We contribute to this discussion by providing causal estimates that can help policymakers quantify one side of the trade-offs.

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A Data Appendix

This appendix provides a description of our data sources and the construction of key variables used in our paper.

A.1 College-year roster

Our analysis includes data on university characteristics at the county level. We construct these data by first constructing an institution-by-year roster using IPEDS data, comprised of two-year and four-year Title IV institutions. We then construct and match institution-by-year variables to the roster, including total degrees granted, student enrollments, financial variables, employees, and staff in the fall semester.

Step 1: Define characteristics

For each institution, we defined its control (public or private), type (two-year or four-year) and Title IV accreditation. These characteristics are obtained from the annual "Institutional Characteristics" survey provided by the Integrated Postsecondary Education Data System (IPEDS, U.S. Department of Education, National Center for Education Statistics (2020)) .

Public institutions: An institution is considered public if its *"control"* is "Public only" or "Combination of public and private."

Two-year and four-year institutions: In 1980, an institution is considered two-year if its *"type"* is "Two year," "2-year branch campus of a multi-campus university" or "2-year branch campus of other 4-year multi-campus inst", and an institution is four-year if its *"type"* is "University (must offer at least two first professional programs)," or "Other four year." If an institution is neither two-year nor four-year, then it is categorized as a less than two-year institution. Similarly, from 1984 to 2017, we define an institution as a two-year institution if its *"iclevel"* is "At least 2 but less than 4 years below the Baccalaureate" or "Below the Baccalaureate." We define an institution as four-year if its *"iclevel"* is "4 or more years (Baccalaureate or higher degree)" or "Baccalaureate or higher degree." Institutions with *"iclevel"* equal to "Below Associates Degree" are categorized as less than two-year institutions.

Title IV institutions: From 1986 to 1997, we classify a college as a Title IV institution if it is eligible for any of "Financial Aid," "Veteran Administration Educational Benefits," "Pell Grants," "Supplementary Education Opportunity Grants," "Stafford Loans," "College Work Study Program," "National Direct Student Loan," "Higher Education Assistance Loan," or "Other Federal Student Financial Aid Programs." From 1998 to 2015, we use the variable *"opeflag"* to identify Title IV institutions, if they are coded as "Participates in Title IV federal financial aid programs." We did not include in this classification institutions coded as "Branch campus of a main campus that participates in Title IV" nor "Deferment only - limited participation." We fill missing Title IV information in 1980, 1984, and 1985 with Title IV information in 1986.

Step 2: Fill missing values

Given each institution is often present multiple times across the years, we filled missing values using values of the same institution in the next year. We began with filling missing county, institution name, state, and city using the values in the next year. Then, we filled missing FICE code, public school indicator and zip code using values of the same institution in other years. Particularly, when filling county names and zip code, we further required the other observation to be listed as being located in the same city. This ensures we do not impute the wrong locations for institutions that moved.

Next, we imputed missing county FIPS using the FIPS of the same institution in other years. First, we identified institutions that have the same non-missing FIPS for all observations. For these institutions, we imputed the missing FIPS using the FIPS in neighboring years. For those institutions that had inconsistent FIPS across years, we imputed using other observations for the same institution, as long as the city and state were the same.

Some institutions in our sample were listed as "system," which is a single administrative body that controls two or more institutions.[53] We identified an institution as an observa-

[53] The definition of "institution system" can be found on IPEDS Data Collection System (U.S. Department of Education, National Center for Education Statistics, 2020*b*).

tion for the administrative system if its name contained "System" or similar words. Before proceeding to the next step, we dropped (1) institutions that were not eligible for Title IV programs; (2) institutions that were below two-year or types were "non-response" or "administrative unit;" and (3) institutions that reported as a system.

Step 3: Fill missing FIPS codes

First, we identified observations that were still missing FIPS.

Second, we used zip code to cross walk to FIPS. Note that not all observations in our sample had ZIP codes. Moreover the ZIP code to county FIPS crosswalk is not necessarily one-to-one. We only crosswalk ZIP to FIPS for observations with a one-to-one ZIP to FIPS matching.

Third, there were still 1991 observations missing a FIPS code out of 166,513 observations, and we filled them in by hand following the procedures below:

1. If an observation contained county name, we identified its FIPS using County FIPS Codes from USDA website (United States Department of Agriculture, 2020).

2. If an observation did not have a county name but had a city name, we first identify county name using its city name on STATSAMERICA (Indiana Business Research Center, 2020), then we identified the FIPS code.

Finally, we pooled together observations that were not originally missing FIPS, those for which we used ZIP to match to FIPS codes, and those for which we filled in FIPS by hand. At this stage, all the observations in our sample matched with a county FIPS code.

A.2 Degrees

The institution-by-year degree data were obtained from the IPEDS "Completions" survey U.S. Department of Education, National Center for Education Statistics (2020). We used the variable "awlevel" from the raw table to identify the degree level. Depending on the year, we defined associate's degrees, bachelor's degrees and post-bachelor's degrees in the following way:

- *Associate's degree*: "Associate degree creditable toward bachelor's degree" (1980), "Associate degree not creditable toward bachelor's degree" (1980), "Associates degrees" (1984 to 2017).
- *Bachelor's degree*: "Bachelor's degree."
- *Post-Bachelor's degree*: "Masters degrees," "Intermediate degrees," "Doctors degrees," "First-professional degrees," "Post-masters certificate," "Doctors degree (old degree classification)," "First-professional degree (old degree classification)," "First-professional certificate (old degree classification)," "Doctors degree - research/scholarship," "Doctors degree - professional practice," and "Doctors degree - other (new degree classification)."

For each institution in a given year, we extracted the total degrees granted to male and female students for each degree level and summed together to get the total degrees granted (the total degrees from 2008 to 2017 were reported in the survey, and so we used this variable instead). We merged the degree data to the institution roster and kept the matched institutions.

To verify the validity of the data construction, we compared the total degrees awarded at universities in our roster with the 2019 NCES Digest of Education Statistics.[54] Figure A1a and Figure A1b show that total degrees awarded at the universities in our roster based on our construction is very similar to that reported by the NCES Digest of Education Statistics.

[54]We compare with the 2019 NCES Digest of Education Statistics.(U.S. Department of Education, National Center for Education Statistics, 2019*b*) For post-BA degrees, we restrict to years before 2011, as after this time doctoral degrees include many degrees that were previously classified as first-professional degrees.

(a) Associate degrees (b) BA and post BA degrees

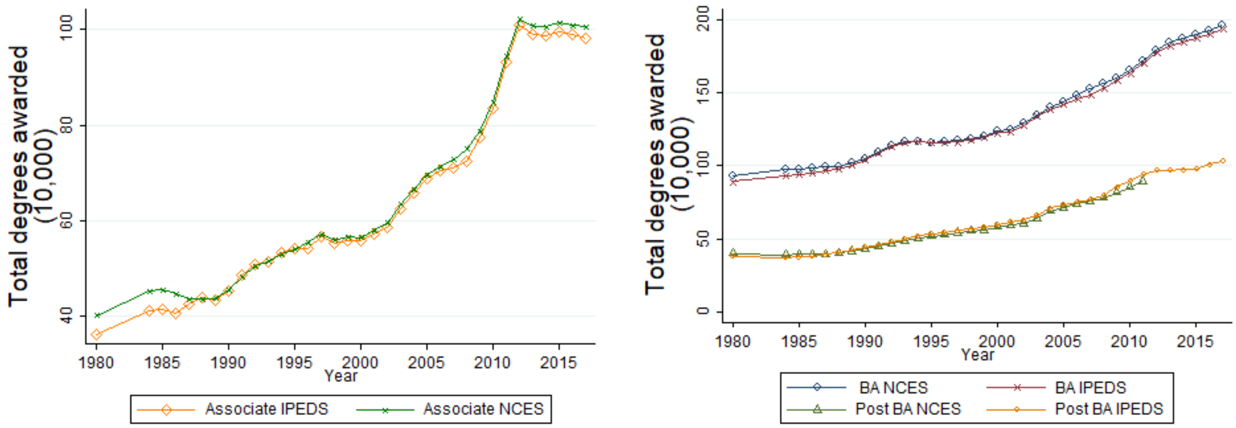

Figure A1: Comparing Sample Degrees and the NCES Reported Total

Notes: Figure A1a compares total associate's degrees by year in our university roster and total numbers reported by the NCES Digest of Education Statistics. Figure A1b compares total bachelor's and post bachelor's degrees by year in our university roster and total numbers reported by the 2019 NCES Digest of Education Statistics.

A.3 Enrollment

We obtained institution-by-year enrollment from IPEDS "Fall Enrollment" survey U.S. Department of Education, National Center for Education Statistics (2020). For each observation, we defined full-time and part-time enrollment separately using variable "line" in the raw data. Then, we calculated total full-time and total part-time enrollment by summing up male and female enrollment of each enrollment type. The total enrollment is the sum of total full-time and part-time enrollment. To generate the sample enrollment data, we merge institution-by-year enrollment to our roster and kept matched institutions.

A.4 University Employment

We measure growth in university employment from 1989 to 2018 for Rust Belt counties, and from 2001 to 2018 for all counties. We obtain university employment data from the IPEDS

Fall Staff survey U.S. Department of Education, National Center for Education Statistics (2020). The IPEDS Fall Staff survey starts in 1987, but more than half of the 1987 survey was imputed, and so we use staff in 1989 as the survey was administered every two years at that time. We use staff in 2001 when analyzing growth for all counties. We calculate total fall staff as the sum of total full-time and part-time staff.

Aggregating staff at all universities in our roster by year yields similar results to NCES publications of total employees at degree-granting universities in 2001 and 2018. In 2001, our total is 3,044,873 and the NCES total is 3,083,353. In 2018, our total is 3,883,766 and the NCES total is 3,923,374 (U.S. Department of Education, National Center for Education Statistics, 2019*a*).

We aggregate total staff at all universities in our roster at the county level, and use this measure in our analysis.

A.5 University finance

We obtain data on university expenditures from the IPEDS "Finance" survey, which starts in 1980. We calculate growth in county-level university expenditures from 1980 to 2018 for Rust Belt counties, and from 2000 to 2018 for all counties. There is some change over time in the expenditure variables. In 1980 we use current funds expenditures (B19). In 2000 public universities, private-for-profit universities, and private-not-for-profit universities report differently. We use total current funds expenditures and transfers (B223) for public institutions, total expenses for private-for-profit and private-not-for-profit institutions (F3E07 and F2E121 respectively). In 2018, we use total expenses and deductions current year total for public institutions reporting using GASB (F1C191). We use total expenses for public institutions reporting using FASB and for private-not-for-profit institutions (F2E131), and for private-for-profit institutions (F3E071).

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B Resilience to Mining Declines

In addition to the manufacturing decline, there was a large decline in mining employment during the 1980s, led primarily by a decrease in the oil and coal industries. In this section, we analyze whether regional universities provided local economic resilience to the counties with large mining employment shares in 1981, the year in which mining employment peaked. During the 1980s, mining employment fell by nearly 50 percent from its 1981 peak. By 2003, at its trough, it was around 40 percent of its 1981 peak.

Our strategy to determine if regional universities cause resilience to mining declines is similar to the strategy we used in the main text for manufacturing declines. The only difference is that instead of using the 1978 manufacturing share, we look at the 1981 mining share, and we use a base year of 1981.

Table A1: **Resilience to the 1981 Mining Employment Decline**

	(1) Employment	(2) Employment	(3) Employment	(4) Population	(5) Earnings	(6) Transfers
Mining Share, 1981	-1.656**	-3.675**	-3.290***	-2.651***	-1.551***	1.610***
	(0.629)	(1.478)	(0.901)	(0.550)	(0.194)	(0.366)
Normal * Mining Share, 1981	0.444	2.374	2.185**	1.427**	0.236	-1.048***
	(0.775)	(1.563)	(1.006)	(0.583)	(0.204)	(0.388)
Observations	326	321	321	321	321	321
State Fixed Effects	N	Y	Y	Y	Y	Y
Control for 1950-1980 Pop. Growth	N	N	Y	Y	Y	Y

Notes: Dependent variables are measured in log growth, 1981-2018. Standard errors clustered at the state level in parentheses. Mining share is a fraction of total employment in the county, as measured in the BEA data. Columns that include controls for 1950-1980 population growth include log 1950 and log 1980 population as additional control variables. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Table A1 shows our results. When controlling for state fixed effects and 1950 and 1980 log population, as we do in our manufacturing analysis, an increase in 1981 mining share leads to a large decline in employment over the subsequent 37 years. The point estimate indicates that for asylum counties, having an additional one percent of 1981 employment in mining leads to a 3.3 percent decline in jobs. However, this adverse impact of exposure is nearly two thirds smaller in counties that were assigned normal schools. Without the population controls, the coefficients are similar, but the standard errors are larger and the

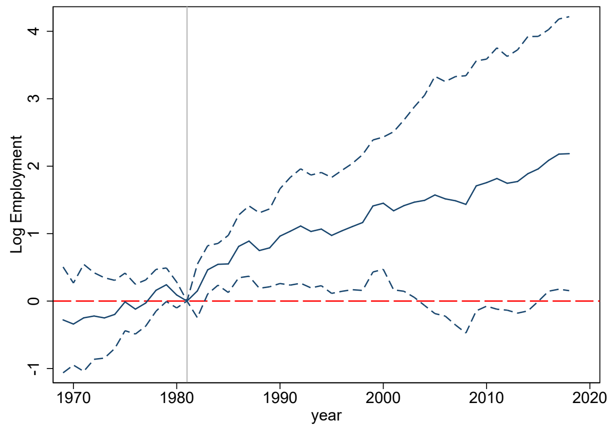


Figure A2: **Differential Effect of 1981 Mining Exposure on Normal Counties Relative to Asylum Counties.** This plot shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 1981 share employed in mining. Effects are relative to 1981, and include county and state-year fixed effects, lower-level terms, and interactions between year fixed effects and log 1950 population and separately log 1980 population. Dotted lines are 95 percent confidence intervals, with standard errors clustered at the state level.

effect is not statistically significant.

Consistent with our results on employment, we find a similar effect for population, with mining share causing a significant decline in population, and this adverse impact is smaller in normal school counties. Unlike the results from our manufacturing analysis, there is no significant resilience effect on earnings. The effect on transfers is consistent with our manufacturing results.

Figure A2 shows event-study results. We plot the differential effect of 1981 mining exposure in normal counties, relative to 1981. The differential effect in normal counties arises immediately after 1981, which is when mining employment was declining most rapidly, but the effect gets gradually larger throughout the entire timeframe. There does not seem to be a differential effect before 1981.

B.1 Mechanisms

To study the mechanisms, we estimate the same regressions as we do for the manufacturing analysis, but using 1981 as a base year and the 1981 mining share as our economic shock.

Table A2: **Resilience to the 1981 Mining Employment Decline, Effect on Potential Mediators**

	(1) Univ. Spending Growth	(2) Bachelor's Share, 1980	(3) Change in BA Share
Normal	0.0464***	0.0197***	0.00337
	(0.0111)	(0.00640)	(0.00582)
Manufacturing Share, 1981	-0.379***	-0.265***	-0.0997*
	(0.126)	(0.0672)	(0.0528)
Normal × Mining Share, 1981	0.198	0.00991	-0.0242
	(0.121)	(0.0730)	(0.0633)
Observations	321	325	325

Notes: Regressions include state fixed effects. Standard errors clustered at the state level. Regressions include controls for Ln(Population, 1950) and Ln(Population, 1980). $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

The university spending results are noisy, but largely consistent with our findings on manufacturing. While we do not find a statistically significant effect of the interaction of normal and mining share on university spending growth, the point estimate is large and positive (Table A2 column (1)). Bachelor's share may also be a mediating moderator, in that we find a positive effect that a normal school has on bachelor's share (Table A2 column (2)), and bachelor's share interacted with the mining share is large and positive, although statistically insignificant (Table A3 column (3)). However, unlike with manufacturing, we do not find much of a role for the bachelor's share to be a moderating mediator. Once we begin to also consider the change in BA share, the results are too noisy to meaningfully interpret. If anything, the mediating effect is negative.

When we look at the industry composition of resilience, we see similar results as we do in our manufacturing analysis. Here, we focus on the 1981-2000 changes in employment in a variety of industries, and we look at the growth in employment in that industry as a percent of total 1981 employment, so that it is a decomposition.[55] Of the effects significant at the 5 percent level, we see that services explains the largest share of the resilience, followed by state and local government, and then construction. Significant amounts of resilience are

[55]For mining employment, a significant number of counties are missing in 2000 that were not missing in 1981. For this exercise, our sample is the group of counties for which the growth rate of interest is available.

Table A3: **Resilience to the 1981 Mining Employment Decline, Correlation of Potential Mediators with Outcomes**

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Income Growth	(5) Income Growth
Manufacturing Share, 1981	-2.103*	-1.410	-3.136	-1.061	-0.197
	(1.166)	(1.204)	(2.031)	(0.870)	(1.677)
Normal × Mining Share, 1981	2.563*	2.202	2.000	2.815**	3.296***
	(1.339)	(1.368)	(1.355)	(1.146)	(1.221)
Univ. Spending Growth		1.829*			
		(1.007)			
Bachelor's Share, 1980			3.366**		-2.979*
			(1.597)		(1.680)
BA Share, 1980 × Mining Share, 1981			13.73		-10.99
			(13.69)		(11.73)
Change in BA Share				12.86***	14.13***
				(2.012)	(2.356)
Observations	321	321	321	321	321

Notes: Regressions include state fixed effects. Standard errors clustered at the state level. Regressions include controls for Ln(Population, 1950) and Ln(Population, 1980). $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

coming from construction, services, and state and local government. The point estimate on retail is also positive. The resilience is not coming from preventing the decline in mining, but rather comes through other sectors, especially services. This would be consistent with the university having spillovers to local non-tradable sectors.

Table A4: **Resilience to the 1981 Mining Employment Decline, Employment by Sector**

	(1) Emp Growth	(2) Mining	(3) Constr.	(4) Manuf.	(5) Retail	(6) FIRE	(7) Services	(8) Federal	(9) State & Local
Mining Share, 1981	-3.501***	-0.598***	-0.316***	-0.212	-0.394***	-0.158*	-1.031***	-0.00867	-0.401***
	(0.600)	(0.143)	(0.0462)	(0.131)	(0.109)	(0.0890)	(0.179)	(0.0159)	(0.0939)
Normal * Mining Share, 1981	1.925**	-0.0102	0.156**	0.0960	0.221*	0.0779	0.818***	-0.0142	0.294**
	(0.778)	(0.130)	(0.0738)	(0.146)	(0.126)	(0.0896)	(0.280)	(0.0231)	(0.145)
Observations	321	185	314	320	320	313	318	321	321
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control for 1950-1980 Pop. Growth	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Regressions include state fixed effects. Standard errors clustered at the state level. Regressions include controls for Ln(Population, 1950) and Ln(Population, 1980). $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

C Cyclicality of the Local Economy

In this appendix, we turn to another measure of resilience, resilience to the business cycle. We investigate whether economic growth during recessions is different in normal relative to asylum counties. Our approach is to compare the movement of economic variables around the years in which NBER-defined recessions begin: 1980, 1990, 2001, and 2007.[56] We estimate the local projection

$$y_{i,t+r} - y_{it} = \beta_r \text{Recession}_t \times \text{Normal School}_i + \alpha_{st} + \epsilon_{i,t} \tag{7}$$

where y is the outcome of interest and r ranges from -3 to 4, Recession$_t$ is 1 in a year the NBER defines as a business cycle peak, and Normal School$_i$ is an indicator variable for the county having a normal school. We include state-year fixed effects, α_{st}. We use the sample of normal and asylum counties as previously defined. The coefficients β_r trace out the difference between normal counties and asylum counties in the years around a recession.[57]

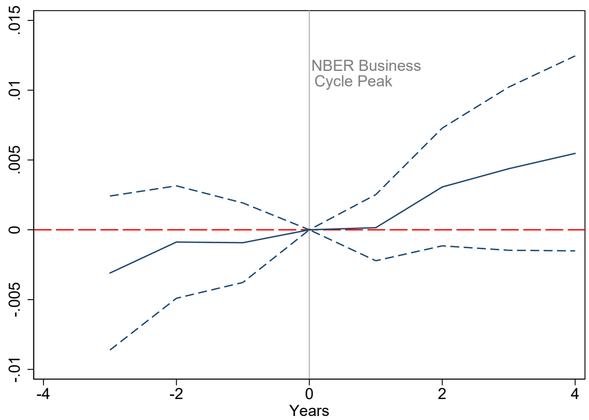
Note that the baseline regression does not include a term for Normal School$_i$ or a county fixed effect. The rationale for this is to compare the growth rate of normal counties to asylum counties in years around recessions, without also comparing them to growth rates in other years. We plot several years before and after the recession, so that whether the effect is cyclical should be easy to see in a figure. Therefore, the specification gives a description of how normal counties grow compared to asylum counties in the years around a recession.

We cluster standard errors by state-year, which is effectively as state-recession, since only the growth rates from recession years are used to calculate the β_r. This accounts for

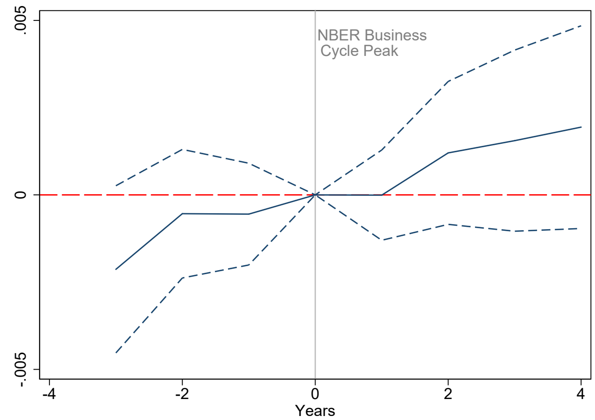
[56] We consider the 1981 recession to be an extension of the 1980 recession. In principle, the BEA data would allow us to include the 1969 and 1973 recessions, but we prefer to start when normal schools' conversions to universities have largely finished and they are established within their counties.

[57] This specification is a local projection (Jordà, 2005). An alternative would be to estimate an event-study distributed-lag model, which would lead to the exact same regression coefficients in some cases. For example, we could limit our dataset to only include the 3 years around a business cycle peak, and estimate a regression with six coefficients for β_{-3} to β_3 (without a β_0), and with a county fixed effect. The main downside is that we cannot extend it beyond 3 years because the the 2000 recession and the 2007 recession would start to overlap.

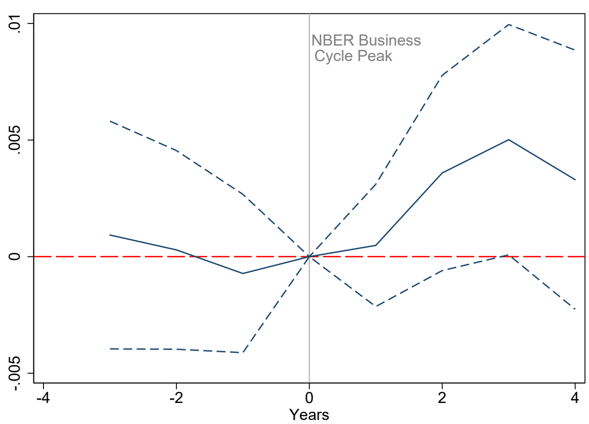
correlations in outcomes across counties within states, but assumes that different recessions are effectively independent observations.[58] While there are in theory efficiency gains from estimating the β_r coefficients jointly, Jordà (2005) shows that such gains are very small, so we estimate each β_r in its own regression.



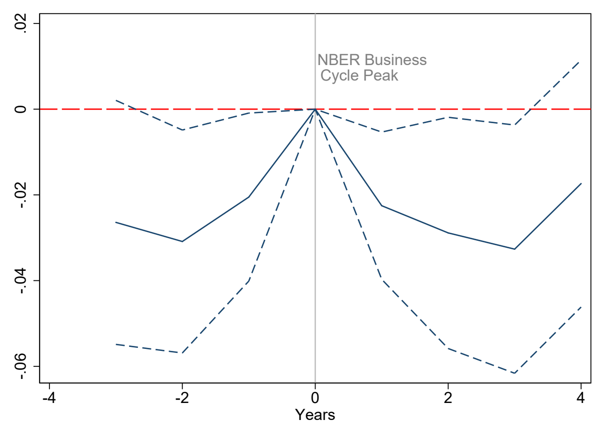
(a) Log Employment



(b) Employment to Population Ratio



(c) Log Personal Income



(d) Log Unemployment Insurance

Figure A3: **Local projections before and after recession years.** Plots show coefficient estimates from equation (7), and indicate the differential in normal counties in the log-change in the indicated variable since the business cycle peak. Regressions include state-year fixed effects. Dashed lines are 95 percent confidence intervals. Standard errors are clustered at the state-year level (effectively state-recession level because only the growth rates from recession years are used to calculate the coefficients). Economic variables come from the BEA.

The results of this regression are shown in Figure A3, for various outcomes y. For log

[58] A more conservative approach would be to cluster by state due to concerns that even across the recessions, there is a correlation in growth rates. In that case, the results in Figure A3 are not significant, even at the 10 percent level.

employment, employment-to-population ratio, and log per capita personal income, normal counties grow faster between the first and second year after a business cycle peak. This effect is significant at the 10 percent level. We do not see evidence that normal and asylum counties are experiencing differential growth leading up to the recession, although there are increases in the coefficient between $t-3$ and $t-2$ for log employment and the employment to population ratio. The magnitude of the effect is about half a percent for employment and income after three or four years. For comparison, employment growth in our sample is about 1.2 percent per year. For log unemployment insurance, normal counties have significantly less growth in the years after a recession. Interestingly, they have more growth relative to asylum counties in the years prior to a recession, also indicative of the fact that normal counties seem to be less cyclical overall.

It is interesting to note that for most of these variables, the biggest differential growth occurs in the second year after the business cycle peak. For many of the recessions, that is a time where the economy—measured by GDP—has started to recover, although the number of jobs nationally is still shrinking or stagnant.

The cyclicality is also apparent in comparing the outcomes year-by-year, from the regression

$$y_{it} = \beta_t \text{Normal}_i + \alpha_{st} + \epsilon_{it}$$

We show the plot of β_t in Appendix Figure A4, and there is clear cyclicality in the within-state difference of normal and asylum counties.

For robustness, we also estimate the same regression as 7, but include county fixed effects, which allows for differences in average growth rate of y across counties.

$$y_{i,t+r} - y_{it} = \beta_r \text{Recession}_t \times \text{Normal School}_i + \alpha_{st} + \alpha_i + \epsilon_{i,t} \tag{8}$$

In this specification, β_r measures the difference in the average within-county change in growth during recessions, in normal relative to asylum counties. This focuses on the differences

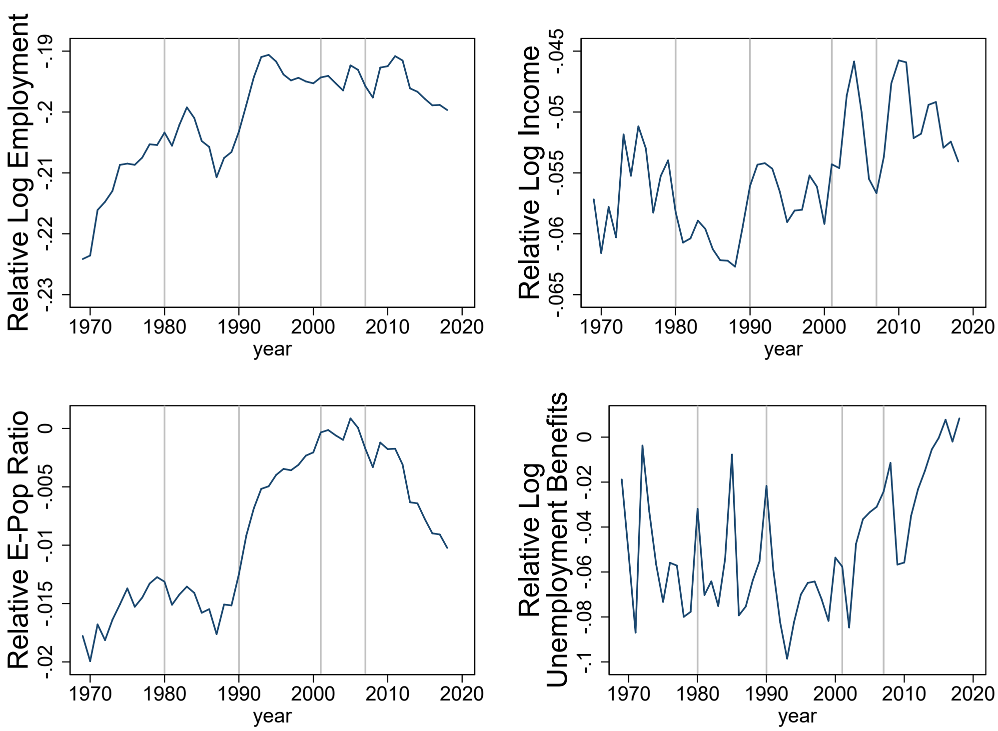
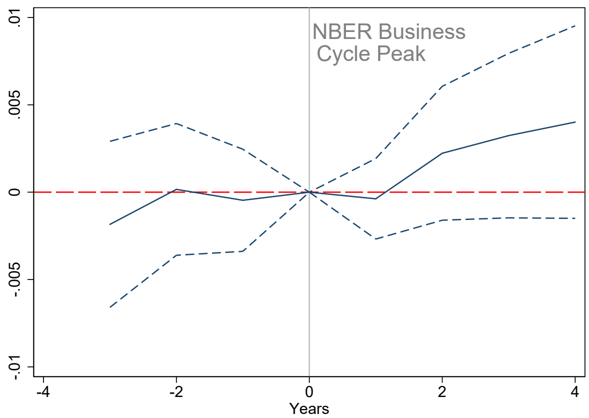
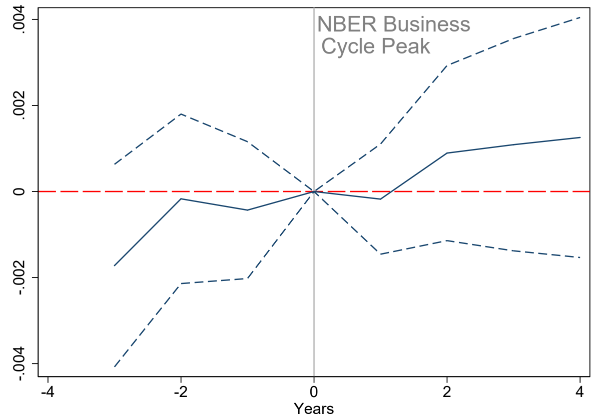


Figure A4: **Relative economic outcomes by year, normal versus asylum.** Each point represents the coefficient estimate on Normal county from a regression of the economic outcome on an indicator for a normal county, with state fixed effects. Gray lines indicate an NBER business cycle peak.

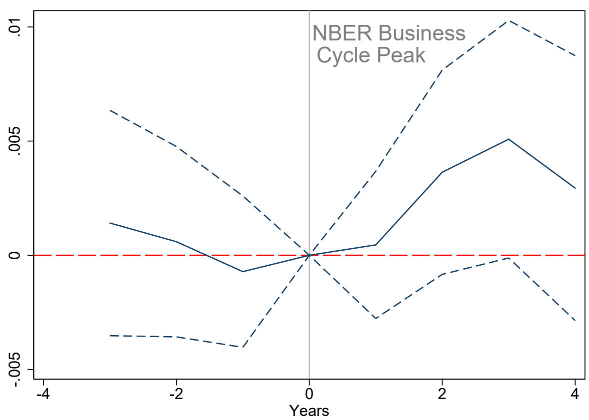
between normal and asylum counties that arise during recessions, as we measure these differences relative to the average growth rate of the county.[59] These results are shown in Appendix Figure A5, and are largely indistinguishable from Figure A3.



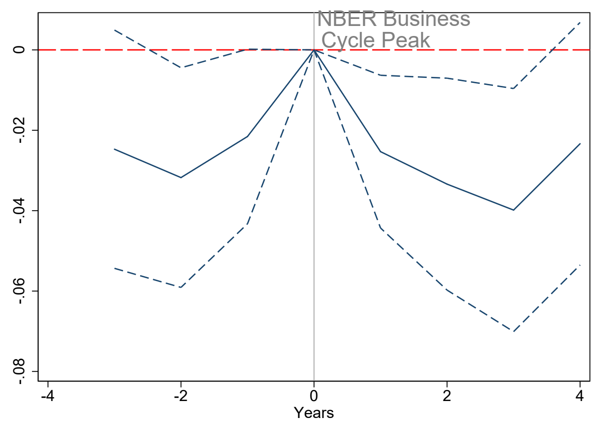
(a) Log Employment



(b) Employment to Population Ratio



(c) Log Personal Income



(d) Log Unemployment Insurance

Figure A5: **Local projections before and after recession years, with county fixed effects.** Plots show coefficient estimates from equation (8), and indicate the differential in normal counties of the change in the indicated variable since the business cycle peak, relative to the average growth rate of the county. Regressions include state-year and county fixed effects. Dashed lines are 95 percent confidence intervals. Standard errors are clustered at the state-year level. Economic variables come from the BEA.

[59] An alternative way to think about this specification is that we are still comparing the effect of normal schools and asylums on the growth rate of the county around a recession, but now we are looking at the (r-year) growth rate after the recession in excess of the average (r-year) growth rate in that county.

C.1 Enrollment Increases During Recessions

Is the resilience mechanism for business cycles the same as it was for the manufacturing decline? Looking at the cyclicality of university finance is difficult as the data are missing for many counties in some key years, such as 2002 and 2009. Instead, we look at enrollment as a percentage of the population. We estimate a specification similar to equation (7), but use the number of students enrolled as a fraction of the population as the left-hand side variable.[60]

Figure A6 shows a clear increase in enrollment as a percent of population during the first few years after a business cycle peak. This suggests that universities are expanding relative to their population during the recession, and based on our previous analysis the direct and indirect effects of that growth presumably help the resilience of the local economy.

The setting and available data make it harder to estimate what percentage of resilience the spending channel might explain, but the qualitative evidence suggest that it is likely at work.

[60] The business cycle peaks used for the regression are 1980, 1990, 2001, and 2007, as in the main regression. The regressions prior to the peak do not include the 1980 recession because the student data starts in that year.

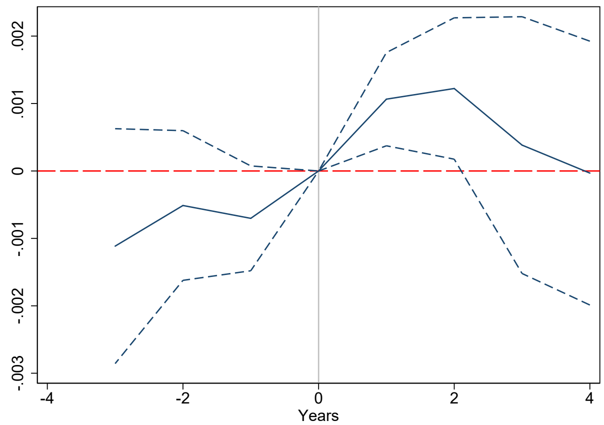


Figure A6: **Local projections of enrollment per capita, before and after recession years.** Each point indicates the differential change in normal counties in enrollment per capita since the business cycle peak on whether a county has a normal school. Regressions include state-year fixed effects. The figure uses the same measure of enrollment as in Section 4, which excludes enrollment at primarily online universities. Dashed lines are 95 percent confidence intervals. Standard errors are clustered at the state-year level (effectively state-recession level because only the growth rates from recession years are used to calculate the coefficients).

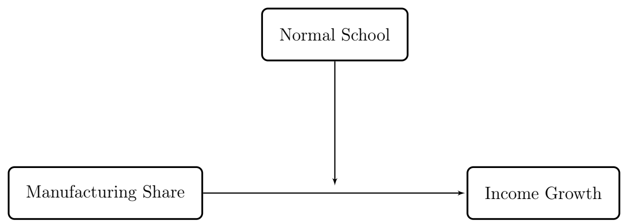
D Supplemental Mediator Analysis

D.1 Directed Acyclic Graphs

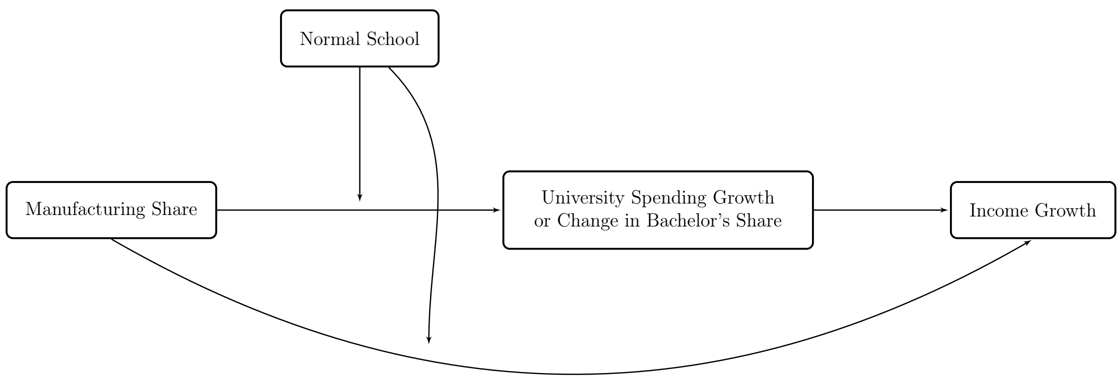
In Figure A7, we show directed acyclic graphs that illustrate the moderating and mediating relationships we hypothesize and analyze in Section 4. Arrows that point at boxes indicate a causal effect. Arrows that point at other arrows indicate a moderating effect.

D.2 Employment as an Outcome

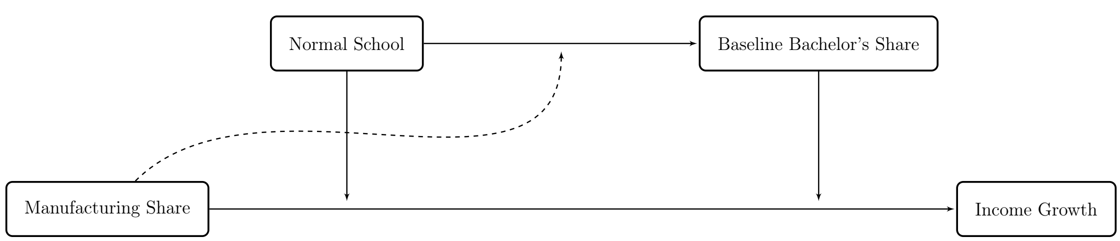
In Tables A5, we repeat the mediation analysis using employment growth as the main outcome of interest instead of income growth. The results are largely similar, although including BA share and its interaction with manufacturing share leads to a smaller reduction in the coefficient on $Normal \times MfgShare$ (roughly 10 percent instead of one-third) for the Rust Belt shock.



(a) Baseline moderation model



(b) Moderating a Mediator model



(c) Mediating a Moderator model. The dashed line represents an extension to such a model in which the mediating relationship is also moderated.

Figure A7: Directed Acyclic Graphs. Arrows that point at boxes indicate a causal effect. Arrows that point at other arrows indicate a moderating effect.

Table A5: Mediation Analysis, Correlations of Mediators with Outcomes, Robustness to Employment Outcome

Panel A: Rust Belt, 1978-2018

	(1) Emp. Growth	(2) Emp. Growth	(3) Emp. Growth	(4) Emp. Growth	(5) Emp. Growth
Manufacturing Share, 1978	-2.374***	-2.393***	-1.997*	-1.448**	-1.228
	(0.552)	(0.605)	(1.121)	(0.553)	(0.973)
Normal × Manuf Share, 1978	1.967***	1.988**	1.745**	1.410**	1.643**
	(0.726)	(0.771)	(0.785)	(0.679)	(0.743)
Univ. Spending Growth		-0.0434			
		(0.282)			
BA Share, 1980 × Manuf Share, 1978			-0.435		-3.217
			(5.778)		(4.805)
Bachelor's Share, 1980			0.784		-1.093
			(0.961)		(0.956)
Change in BA Share				4.326***	4.910***
				(1.024)	(1.168)
Observations	103	103	103	103	103

Panel B: All Counties, 2000-2018

	(1) Emp. Growth	(2) Emp. Growth	(3) Emp. Growth	(4) Emp. Growth	(5) Emp. Growth
Manufacturing Share, 2000	-1.192***	-1.136**	-1.413*	-1.413*	-1.285*
	(0.426)	(0.452)	(0.796)	(0.796)	(0.647)
Normal × Manuf Share, 2000	0.994**	0.951*	0.884	0.884	0.879*
	(0.486)	(0.499)	(0.540)	(0.540)	(0.516)
Univ. Spending Growth		0.335			
		(0.358)			
Bachelor's Share, 2000			0.228	0.228	-0.190
			(0.568)	(0.568)	(0.523)
BA Share, 2000 × Manuf Share, 2000			2.248	2.248	2.437
			(4.082)	(4.082)	(3.789)
Change in BA Share					3.459***
					(1.249)
Observations	320	320	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

D.3 Industry Composition as a Mediator

In this section, we consider whether the county's industry composition might serve as a mediator, as in Figure A7c. In particular, we know from Tables A13 and A14 that there are some small differences in the industry composition of the normal and asylum counties.

We first estimate a version of equation (4) where the dependent variables are the sectoral shares. We show only the industries for which there are significant normal-asylum differences in this regression, as well as government. We then estimate a version of equation (5), where we instead include the sectoral shares as independent variables, only for the industries with significant differences in the first set of regressions. In the average-manufacturing-share county, the retail employment share is roughly 1 percentage point higher in normal counties (column 4 of Table A6). For the average-manufacturing-share county, there is no significant difference in government share between normal and asylum counties (column 3), although the normal-asylum difference increases with manufacturing exposure.

However, the estimated differential resilience in normal counties is not any smaller when including the retail and government employment shares and their interaction with manufacturing share. This evidence is consistent with our main results not being driven by differences in sectoral composition at baseline.

For the 2000s shock, the average-manufacturing-share normal county has higher retail, and accommodation and food services employment shares in 2000, and lower finance employment shares, and these differences do not vary significantly with manufacturing share. Wholesale share is marginally significant in Table A14, but not once we include the controls from the baseline regression.

Descriptively, differential resilience in normal counties does not appear explained by differences in sectoral composition at baseline, as there is an increase in the coefficient on $Normal \times MfgShare$.

We additionally test whether industry composition is a mediator by assuming that each industry contributes to income growth in proportion to its national growth rate. To test

Table A6: Mediation Analysis, Industry Share

Panel A: Rust Belt, 1978-2018

	(1) Government Share, 1978	(2) Retail Share, 1978
Normal	0.000435	0.00998*
	(0.0122)	(0.00533)
Manufacturing Share, 1978	-0.621***	-0.0111
	(0.142)	(0.0681)
Normal × Manuf Share, 1978	0.326**	-0.0957
	(0.162)	(0.0732)
Observations	103	103

Panel B: All Counties, 2000-2018

	(1) Wholesale Share	(2) Retail Share	(3) Finance & Ins. Share	(4) Acc. & Food Service Share	(5) Gov't Share	(6) Shift-Share GDP Growth
Normal	-0.00157	0.00429*	-0.00295*	0.00977***	0.000221	0.00631
	(0.00162)	(0.00238)	(0.00172)	(0.00238)	(0.00669)	(0.00458)
Manufacturing Share, 2000	0.00337	-0.0647	-0.0662***	-0.111***	-0.475***	0.0905
	(0.0223)	(0.0418)	(0.0226)	(0.0344)	(0.121)	(0.0540)
Normal × Manuf Share, 2000	-0.0130	0.0438	-0.0219	-0.0296	0.111	-0.0549
	(0.0232)	(0.0439)	(0.0284)	(0.0441)	(0.158)	(0.0753)
Observations	320	320	320	320	320	16900

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

this, we create a shift-share prediction of GDP growth in each county.[61] It is not affected by the presence of a normal school, nor does including it and its interaction with manufacturing share yield a smaller coefficient on $Normal \times MfgShare$ when the dependent variable is income or employment growth (Table A6).

[61] The shift-share growth is based on GDP shares of broad NAICS industries in 2001, using national growth rates from 2001-2018. In a few cases, the BEA data is censored, meaning the shares do not add up to 1. We assign the national average GDP growth rate to this residual. We do not implement this analysis for the Rust Belt shock given the change in industry definitions over time.

Table A7: Mediation Analysis, Industry Share

Panel A: Rust Belt, 1978-2018

	(1) Income Growth	(2) Income Growth	(3) Emp. Growth	(4) Emp. Growth
Manufacturing Share, 1978	-3.823**	-11.88*	-2.374***	-4.090
	(1.465)	(6.120)	(0.552)	(2.850)
Normal × Manuf Share, 1978	3.496**	3.500***	1.967***	2.248***
	(1.452)	(1.290)	(0.726)	(0.795)
Retail Share, 1978		-7.678**		-2.860*
		(2.944)		(1.597)
Retail Share, 1978 × Manuf Share, 1978		41.24		1.110
		(33.00)		(17.94)
Government Share, 1978		-2.670**		-1.534**
		(1.114)		(0.736)
Government Share, 1978 × Manuf Share, 1978		-1.343		2.554
		(7.498)		(5.109)
Observations	103	103	103	103

Panel B: All Counties, 2000-2018

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Emp. Growth	(5) Emp. Growth	(6) Emp. Growth
Manufacturing Share, 2000	-0.993**	-0.509	4.891	-1.192***	-1.225	3.703
	(0.398)	(1.690)	(8.786)	(0.426)	(1.513)	(7.692)
Normal × Manuf Share, 2000	0.825*	1.296**	0.898*	0.994**	1.293**	1.030**
	(0.481)	(0.553)	(0.473)	(0.486)	(0.549)	(0.473)
Wholesale Share, 2001		-1.126			-0.533	
		(1.003)			(0.789)	
Wholesale Share, 2001 × Manuf Share, 2000		33.27*			22.77	
		(17.37)			(15.46)	
Retail Share, 2001		-0.769			-1.171*	
		(0.755)			(0.659)	
Retail Share, 2001 × Manuf Share, 2000		-6.019			-10.79	
		(16.23)			(13.23)	
Finance and Insurance Share, 2001		0.817			0.0317	
		(1.263)			(1.637)	
Finance and Insurance Share, 2001 × Manuf Share, 2000		-37.00**			-13.26	
		(16.40)			(15.19)	
Accomodation and Food Service Share, 2001		0.522			-0.299	
		(1.180)			(1.049)	
Accomodation and Food Service Share, 2001 × Manuf Share, 2000		-3.912			4.142	
		(20.15)			(15.53)	
Gov't Share, 2001		-1.514***			-1.239***	
		(0.336)			(0.335)	
Government Share, 2001 × Manuf Share, 2000		0.0958			0.470	
		(3.908)			(3.357)	
Shift-Share GDP Growth			0.327			-0.188
			(0.342)			(0.411)
Shift-Share GDP Growth × Manuf Share, 2000			-4.465			-3.684
			(6.618)			(5.778)
Observations	320	320	320	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

Table A8: Mediation Analysis, University Spending Growth, All Universities (including distance-learning)

Panel A: Rust Belt, 1978-2018

	(1) Income Growth	(2) Univ. Spending Growth (All Univ.)	(3) Income Growth
Manufacturing Share, 1978	-3.823**	-0.436*	-3.545***
	(1.465)	(0.248)	(1.305)
Normal × Manuf Share, 1978	3.496**	0.476*	3.192**
	(1.452)	(0.280)	(1.330)
Univ. Spending Growth (All Univ.)			0.638
			(0.616)
Observations	103	103	103

Panel B: All Counties, 2000-2018

	(1) Income Growth	(2) Univ. Spending Growth (All Univ.)	(3) Income Growth
Manufacturing Share, 2000	-0.993**	-0.167**	-0.902**
	(0.398)	(0.0723)	(0.411)
Normal × Manuf Share, 2000	0.825*	0.121	0.759
	(0.481)	(0.0737)	(0.478)
Univ. Spending Growth (All Univ.)			0.541
			(0.380)
Observations	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

D.4 Alternative Measure of University Spending

In Section 4, we measure university spending among universities with mostly in-person students. In this section, we show the mediation results are not substantially different if we use spending reported to IPEDS of all universities, regardless of their distance enrollment.

The results are shown in Table A8, and they are similar to Table 4. The magnitudes are comparable, and the significance levels are the same.

D.5 Growth as a Mediator

In the main analysis, we only consider university spending growth and BA share growth as mediators, without letting them moderate the effect of manufacturing. We think that is the most intuitive way to think about them. However, it could be that the local multiplier depends on the state of the economy, or that the effect of BA share growth on the economy depends on the industries. For that reason, in Table A9 we present Table 5 including the interaction of the growth terms with manufacturing share. None of the new terms are statistically significant. As in the main text, we would be very hesitant to interpret any of these coefficients causally.

Table A9: Mediation Analysis, Correlations between Mediators and Outcome

Panel A: Rust Belt, 1978-2018

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Income Growth
Manufacturing Share, 1978	-3.823**	-3.357***	-0.826	0.321
	(1.465)	(1.193)	(1.812)	(2.170)
Normal × Manuf Share, 1978	3.496**	3.247**	2.591**	2.183**
	(1.452)	(1.365)	(1.138)	(1.053)
Univ. Spending Growth		1.174		
		(1.612)		
Univ. Spending Growth × Manuf Share, 1978		-2.811		
		(5.271)		
Change in BA Share			7.316***	6.698***
			(1.743)	(1.621)
Change in BA Share × Manuf Share, 1978			-11.42	-1.609
			(18.15)	(16.15)
Bachelor's Share, 1980				1.600
				(1.486)
BA Share, 1980 × Manuf Share, 1978				-11.60
				(10.81)
Observations	103	103	103	103

Panel B: All Counties, 2000-2018

	(1) Income Growth	(2) Income Growth	(3) Income Growth	(4) Income Growth
Manufacturing Share, 2000	-0.993**	-0.979**	-0.0229	-0.509
	(0.398)	(0.411)	(0.646)	(0.611)
Normal × Manuf Share, 2000	0.825*	0.641	1.056**	0.929*
	(0.481)	(0.530)	(0.502)	(0.517)
Univ. Spending Growth		-0.450		
		(1.201)		
Univ. Spending Growth × Manuf Share, 2000		9.341		
		(10.79)		
Change in BA Share			5.785**	5.933*
			(2.465)	(3.053)
Change in BA Share × Manuf Share, 2000			-12.33	-15.21
			(13.82)	(17.83)
Bachelor's Share, 2000				-0.204
				(0.839)
BA Share, 2000 × Manuf Share, 2000				3.716
				(5.370)
Observations	320	320	320	320

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. Manufacturing share is demeaned, so the coefficient on non-interacted terms can be interpreted as the average effect. Robust standard errors are presented in Panel A, and standard errors clustered at the state level in Panel B. Regressions in Panel A include controls for Ln(Population, 1950) and Ln(Population, 1978), while regressions in Panel B include controls for Ln(Population, 1950) and Ln(Population, 1980). See text for details.

E Effects by Industry

In this section, we test the extent to which differential resilience of employment is explained by the tradable or nontradable sectors. One possible explanation for our results is that the manufacturing in normal counties is different than in asylum counties, and was not affected as much by these shocks. This would suggest differential employment effects in manufacturing. Another explanation is that university spending creates a local multiplier, implying effects in nontradable sectors. We estimate the following specification:

$$\frac{\Delta \text{ Sectoral Employment}_{it}}{\text{Total Employment}_{i,t-1}} = \beta_1 \text{Normal}_i + \beta_2 \text{Mfg Exposure}_i + \beta_3 \text{Normal}_i \times \text{Mfg Exposure}_i + \alpha_s + X_i \gamma + \epsilon_{it} \tag{9}$$

If we had a full breakdown of employment by sector, we could add up all the βs from the different sectors and they would sum to the total employment result. Because of the change from SIC to NAICS classifications in 2000, we split our analysis of the Rust Belt shock into growth from 1978-2000 and growth from 2001-2018.

Column 1 of Table A10 shows that increasing 1978 manufacturing share by 10 percentage points has a differentially positive effect on percent employment growth in normal counties from 1978-2000 of nearly 11 percentage points, relative to asylum counties. Differential effects on employment growth in services account for nearly 30 percent of this effect, retail accounts for approximately 17 percent, and federal government for roughly 6 percent. Effects for the remaining industries are smaller and/or less precise. Importantly, we cannot rule out that the effect of exposure on manufacturing growth was the same in normal and asylum counties. This implies resilience is not driven by differential exposure to the broad shock, or by differential resilience of the manufacturing sector to the same shock.[62]

Panel B shows the analogous results analyzing exposure to the 2000 manufacturing declines.[63] We limit the regressions to a sample of 249 counties, with nonmissing sectoral

[62] Appendix Table A29 shows the results from 2001 to 2018 for Rust Belt counties.

[63] There is considerably more missing sectoral employment data when using the NAICS classifications post-2000. To maximize our sample size, we analyze percent growth in average employment from 2001-2004

employment across our sectors of focus.[64] We find significant and substantial effects in retail, services, government, and construction. We find no differential effect on manufacturing growth. We also see important effects in industries which had more missing data, and so were not included in these main results (see Appendix Tables A27 and A28). In particular, we see evidence suggesting effects on health employment.

to 2015-2018. This allows us to include counties which are missing employment for a given sector in 2001 but not in 2002, for example. We adjust the multi-year averages based on the particular years that make up the average in each county, using national industry employment. For example, we compute the average national employment in retail from 2001-2004, and then the average from 2002-2004. We compare the 2002-2004 average to the 2001-2004 average to get a multiplier. We adjust county-level average retail employment for which the county only has retail data in 2002-2004 rather than in all four years. If national employment in 2002-2004 is 1.005 times average retail employment in 2001-2004, then for counties with retail data in only 2002-2004, we divide by 1.005. This averaging allows us to include 10 to 20 percent more counties.

[64]Even with multi-year averages, some counties are still missing employment data for some sectors. These 249 counties have employment for sectors that are nonmissing for nearly all counties (at least 99 percent after the multi-year averaging), as well as employment in professional, scientific, and technical services, and administrative and support services (which are covered for 90 percent of Rust Belt counties after the averaging).

We include these services sectors because they seem especially relevant. The connection between higher education and professional, scientific, and technical services, is intuitive and has been explored in other work (e.g. Andrews, 2021). Administrative and support services includes firms that provide services to other firms, for example cleaning, groundskeeping, and security. Given the growth in contracting out these types of occupations (see Weil 2014), differential growth of universities may be captured by state and local employment as well as administrative and support services. For completeness, we show results for all sectors, not restricting to a fixed sample of counties, in Appendix Tables A27 and A28.

Table A10: Differential Employment Growth in Normal Counties, by Sector

A: Rust Belt, 1978-2000										
$Y = \frac{\Delta \text{Empl}_{j,t}}{\text{Empl}_{t-1}}$	All	Constr.	Mfg.	Transp.	Whole-sale	Retail	FIRE	Serv.	Fed. Gov.	State & Local
Mfg. Share, 1978	-1.249***	-0.050	-0.342***	-0.010	0.001	-0.168*	-0.134***	-0.373**	-0.040	-0.069
	(0.359)	(0.034)	(0.111)	(0.045)	(0.028)	(0.092)	(0.044)	(0.152)	(0.027)	(0.103)
Normal*Mfg., 1978	1.091**	0.075	0.140	0.023	0.030	0.190*	0.048	0.321*	0.062**	-0.065
	(0.500)	(0.047)	(0.135)	(0.051)	(0.036)	(0.110)	(0.057)	(0.177)	(0.027)	(0.132)
Observations	103	102	103	98	101	103	103	103	103	103
R-Squared	0.646	0.431	0.547	0.424	0.572	0.463	0.412	0.526	0.164	0.426

B. All Counties, 2001-2018										
$Y = \frac{\Delta \text{Empl}_{j,t}}{\text{Empl}_{t-1}}$	All	Constr.	Mfg.	Retail	Finan. & Insur.	Real Estate	Prof. Serv.	Admin Serv.	Fed.	State & Local
Mfg. Share, 2000	-1.126***	-0.034	-0.172***	-0.069	-0.090	-0.047*	-0.089**	-0.065**	-0.020**	-0.081**
	(0.378)	(0.028)	(0.052)	(0.043)	(0.057)	(0.026)	(0.037)	(0.025)	(0.008)	(0.037)
Normal*Mfg., 2000	1.075**	0.061*	0.025	0.096**	0.078	0.022	0.071	0.099***	0.034**	0.125***
	(0.408)	(0.031)	(0.052)	(0.047)	(0.065)	(0.025)	(0.044)	(0.035)	(0.013)	(0.044)
Observations	249	249	249	249	249	249	249	249	249	249
R-Squared	0.432	0.431	0.444	0.400	0.238	0.413	0.339	0.407	0.281	0.311
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: *** p-value ≤ .01, ** p-value ≤ .05, * p-value ≤ .1. Robust standard errors in parentheses in panel A. Standard errors clustered at the state level in parentheses in panel B. All regressions include state fixed effects. Panel A includes controls for Ln(Population, 1950) and Ln(Population, 1978). Panel B includes controls for Ln(Population, 1950) and Ln(Population, 1980). Panel B includes only counties with non-missing industry employment for these listed industries. Dependent variable is the change in sectoral employment for sector j from t-1 to t relative to total county employment in t-1. See text for details.

F Robustness Appendix

F.1 Manufacturing Decline by Census Region

In Figure A8, we show that the decline in manufacturing was primarily concentrated in the Rust Belt, which overlaps the Midwest and Northeast Census regions, starting in 1978, but was widespread throughout the country starting in 2000.

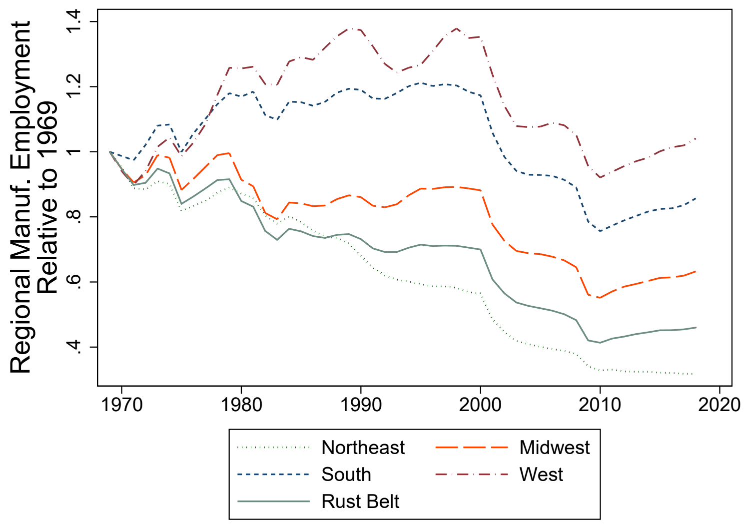


Figure A8: Manufacturing Employment by Census Region, Relative to 1969

F.2 Historical Newspapers

Asylums were desired generally, not only in Kankakee. When the Western Illinois Insane Asylum was awarded, a newspaper article in a rival town had a headline "Rock Island Got It... The New Insane Asylum to be Located at the East Moline Site—Monmouth Made a Good Fight but Failed to Get It" (Warren County Democrat, 1895).

It was common to allege that other places only won asylums because of shady political dealings, which reflects that cities and towns wanted the asylums. For example, when Anna was awarded the Southern Illinois Insane Asylum, a rival town's newspaper ran a story with the subheadlines "Is There Anything Rotten in the State of Denmark?—And Several Other

Very Blunt Questions" alleging that Anna was chosen over rival Jonesboro to benefit corrupt politicians (The Cairo Evening Bulletin, 1869).

Once the asylums were awarded, newspapers were eager to have them built. In Alton, Illinois, a newspaper headline declared they were "In a Hurry for Insane Hospital" (Alton Evening Telegraph, 1915). Finally, newspapers also thought there were benefits years later. During the era of deinstitutionalization, the Dixon State Hospital was described as "A Vital Force in the Economic and Social Life in Dixon" (Dixon Evening Telegraph, 1951).

An additional example is that after the Northern Illinois Insane Asylum was awarded to Elgin, a rival town's newspaper attributed it to "obvious partiality shown by the commissioners in favor of Elgin, even justifying suspicions of corruption... in further view of the palpable fact that the location at Elgin comes short in several very important particulars..." (Ottawa Free Trader, 1869).

F.3 1980 Balance Table Extended

Table A11 shows 1980 balance on a few other variables that were not included in Table 2.

Table A11: County Characteristics in 1980

	(1)	(2)	(3)
	Variable Means		Difference in Means With State FE
	Normal	Asylum	(1) - (2)
Civilian LFPR	59.56	61.00	-0.98
	(4.96)	(5.3)	(0.61)
Employment as % of population	55.54	57.06	-1.08
	(5.4)	(5.89)	(0.71)
Unemployment rate	6.83	6.58	0.23
	(2.3)	(2.64)	(0.28)
Poverty rate in 1979	14.90	11.91	2.03***
	(7.56)	(5.79)	(0.67)
Average earning per job ($1,000)	13.23	14.06	-0.52*
	(2.7)	(2.67)	(0.28)
Per capita UI compensations	74.74	83.49	-0.22
	(47.5)	(53.87)	(4.37)

Notes: Columns (1) and (2) show means and standard deviations in parentheses. The table shows summary statistics for normal and asylum counties using 1980 BEA and NHGIS data. Using log average earnings per job as the dependent variable the coefficient is .043 with standard error of .021. Using log per capita UI compensation also yields statistically insignificant coefficients. For poverty rate in 1979, average earning per job, and per capita UI compensations, the regression consists of 200 normal counties and 126 asylum counties. For civilian LFPR, employment as % of population, and unemployment rate the regression consists of 204 normal counties and 126 asylum counties. In column 3 we present standard errors clustered at the state level in parentheses.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.4 Age Profile

In Figure A9, we show the difference in age profile between normal and asylum counties. We estimate the regression of the population of a specific age, divided by the over 25 population, and regress that on a dummy for having a normal school, as well as state fixed effects, in different years. In each year, we see a huge spike in the ages corresponding to college, but very little effect on the ages after college.

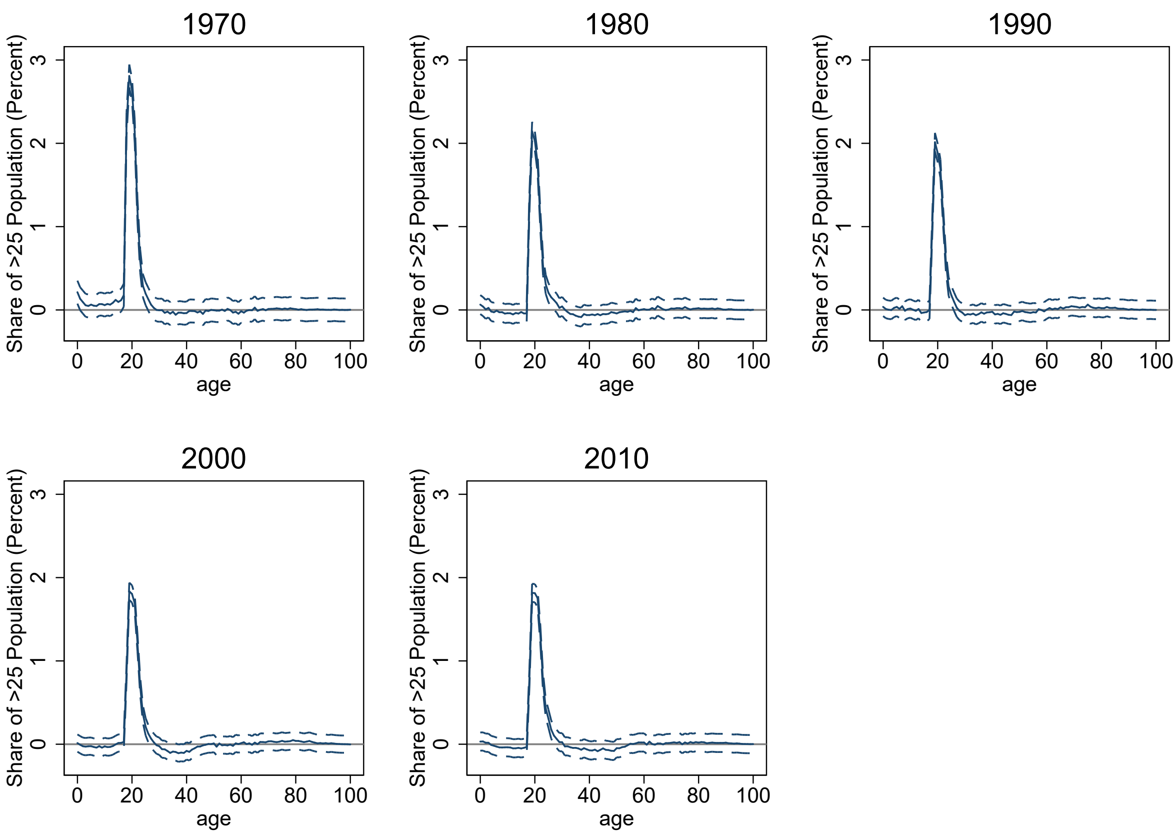


Figure A9: **Age Profile in Different Years.** Each point in the plots is the coefficient on Normal county, from a regression of age share on normal county, including state fixed effects. Dashed lines are 95 percent confidence intervals, based on standard errors clustered at the state level. Age share is defined as the population of a specific age divided by the total population over 25 years old. Population by age data is from NHGIS.

F.5 1920 Balance

In this section, we show that there is covariate balance in 1920. This is after the establishment of normal schools and asylums, but before they turned into regional universities. Another advantage is that the data covers the entire country, rather than the subsample we were able to look at in 1840. In column (4), we show the coefficient from a regression with state fixed effects. The lack of significance indicates that normal and asylum counties are comparable. In column (5), we reiterate that other counties are not a good control group because there are large deviations in baseline characteristics.

Table A12: Covariate Balance in 1920: Normal School, Asylum, and All Other Counties

	(1)	(2)	(3)	(4)	(5)
		Variable Means		Difference in Means With State FE	
	Normal	Asylum	All others	(1) - (2)	(1) - (3)
Log Population	10.66	10.86	9.65	-0.14	0.69***
	(1.102)	(1.154)	(0.946)	(0.153)	(0.091)
Urban Share	39.97	46.60	16.05	-3.02	18.086***
	(28.395)	(26.917)	(22.389)	(3.929)	(2.572)
Male Share	50.98	51.35	51.95	-0.26	-0.894***
	(1.838)	(2.578)	(2.38)	(0.283)	(0.176)
African-American Share	10.14	7.01	12.12	0.47	2.154***
	(18.186)	(13.671)	(19.643)	(0.607)	(0.703)
White Foreign-Born Share	10.18	11.18	6.38	-1.228*	0.946***
	(9.402)	(8.573)	(7.719)	(0.665)	(0.356)
Population Density	0.37	1.27	0.07	-0.99	0.17
	(1.762)	(9.574)	(0.541)	(1.179)	(0.113)
Log Manufacturing Establishments	4.63	4.86	3.45	-0.18	0.737***
	(1.333)	(1.37)	(1.027)	(0.17)	(0.084)
Average Manufacturing Wage	1,035.13	1,071.53	972.40	-7.58	24.73
	(231.487)	(252.168)	(284.351)	(27.886)	(15.754)
Manufacturing Share	6.88	7.78	3.62	-0.968*	1.271***
	(6.586)	(6.202)	(5.288)	(0.575)	(0.334)
Log Value Added, Manufacturing	15.18	15.58	13.26	-0.32	1.211***
	(2.133)	(2.156)	(1.944)	(0.243)	(0.125)
Log Value of Crops	15.30	15.42	14.87	0.04	0.391***
	(1.21)	(0.997)	(1.19)	(0.141)	(0.097)
Log Value of Farm Property	16.94	17.15	16.49	0.04	0.437***
	(1.085)	(0.962)	(1.084)	(0.117)	(0.083)

Notes: This table shows county characteristics in 1920 for normal, asylum and all other counties. Data are from NHGIS. Columns (1) through (3) show variable means and standard deviations in parentheses. Column (4) and column (5) display estimates from regressing each variable on the normal county indicator with state fixed effects. Column (4) contains normal and asylum counties and column (5) contains normal and all other counties. In columns (4) and (5) we report standard errors clustered at the state level in parentheses. Sample sizes vary across variables due to missing data for some counties. For all but the four manufacturing variables, there are 204 normal counties and 126 asylum counties. For the four manufacturing variables, there are 203 normal counties and 125 asylum counties. For demographic variables, there are 2779 other counties. For log manufacturing establishments, average manufacturing wage, manufacturing share, and log value added in the manufacturing sector there are 2617, 2618, 2616 and 2615 other counties. For the agriculture variables, there are 2778 other counties.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.6 Industry Balance

In Table A13, we show the effect of having a normal school on all the major industry categories (SIC) in 1980. As we claim in the main text, the only significant ones are retail and manufacturing.

Table A13: **Effect on Industry Employment (Percent of Total Employment) in 1980**

	(1)	(2)	(3)
	Variable Means		Difference in Means With State FE
	Normal	Asylum	(1) - (2)
Agriculture	0.9210	0.7760	0.0278
	(1.337)	(0.846)	(0.14)
Mining	1.3100	0.9550	0.2973
	(3.424)	(2.722)	(0.392)
Construction	4.7730	4.8970	-0.2018
	(1.371)	(2.251)	(0.241)
Manufacturing	16.4490	17.6660	-1.4947*
	(8.693)	(8.261)	(0.824)
Transportation and Public Utilities	4.3300	4.3660	0.0523
	(2.036)	(1.555)	(0.191)
Wholesale Trade	4.0100	3.9990	0.0121
	(1.723)	(1.763)	(0.211)
Retail Trade	16.3120	15.4890	1.1275***
	(2.581)	(2.818)	(0.313)
Finance, Insurance, and Real Estate	6.0120	6.2700	-0.1564
	(2.226)	(2.152)	(0.285)
Services	19.9380	20.0050	0.1544
	(4.37)	(5.179)	(0.663)
Government	19.7980	19.8840	-0.3873
	(8.12)	(8.684)	(1.078)
Observations	200	126	326

Notes: This table shows mean and standard deviation of industry share in 1980 using BEA data. Column (3) displays coefficients from regressing each variable on the normal county indicator with state fixed effects. The sample size for each variable varies. For agriculture, we have 195 normal and 124 asylum counties. For mining we have 193 normal and 124 asylum counties. For construction, manufacturing, FIRE, and government we have 200 normal and 126 asylum counties. For Transportation and services we have 198 normal and 126 asylum counties. For wholesale and retail trade we have 199 normal and 126 asylum counties. The robust standard errors are clustered at the state level and reported in parentheses in column 3.
$*p < 0.10, **p < 0.05, ***p < 0.01$

In Table A14, we show the effect of having a normal school on all the major industry categories (NAICS) in 2001. Of note, normal counties have less wholesale trade, more retail trade, and more accomodation and food services.

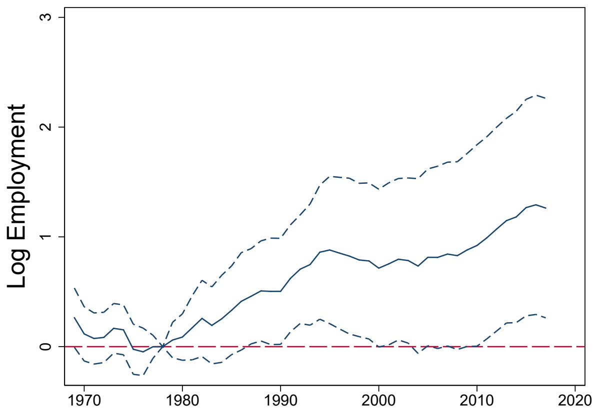
Table A14: **Effect on Industry Employment (Percent of Total Employment) in 2001**

	(1)	(2)	(3)
	Variable Means		Difference in Means With State FE
	Normal	Asylum	(1) - (2)
Forestry	0.7950	0.6010	0.0114
	(1.182)	(1.255)	(0.214)
Mining	0.8210	0.7930	-0.1343
	(1.692)	(1.966)	(0.283)
Utilities	0.4180	0.3970	0.0008
	(0.521)	(0.36)	(0.071)
Construction	5.7750	5.8730	-0.2329
	(1.631)	(1.879)	(0.228)
Manufacturing	10.1440	11.3080	-0.7670
	(5.125)	(5.772)	(0.813)
Wholesale Trade	3.0090	3.3090	-0.3943**
	(1.291)	(1.407)	(0.19)
Retail Trade	11.9710	11.3900	0.5812**
	(1.912)	(1.968)	(0.24)
Transportation and Warehousing	3.1050	3.0700	0.0664
	(1.959)	(1.53)	(0.244)
Information	1.6860	1.7080	-0.0091
	(0.915)	(1.035)	(0.145)
Finance and Insurance	3.6520	4.0420	-0.3998*
	(1.874)	(2.046)	(0.237)
Real Estate	2.6750	2.6510	-0.0250
	(0.968)	(0.918)	(0.118)
Professional Services	4.4620	4.6410	-0.1905
	(2.392)	(2.514)	(0.315)
Management of Companies	0.7980	0.7630	0.1108
	(0.858)	(0.75)	(0.155)
Administrative Services	4.6690	4.7000	-0.0666
	(1.809)	(1.855)	(0.262)
Educational Services	1.6500	1.6950	-0.0917
	(1.527)	(1.534)	(0.167)
Health Care	10.1430	10.3800	-0.1836
	(2.871)	(3.408)	(0.428)
Arts and Entertainment	1.6990	1.5600	0.1029
	(1.093)	(0.657)	(0.09)
Accommodation and Food Services	7.1270	6.1320	0.9833***
	(2.252)	(1.743)	(0.213)
Other Services	5.5290	5.3900	0.1476
	(0.869)	(0.847)	(0.103)
Government	17.7680	16.9830	0.8502
	(6.945)	(7.261)	(0.968)

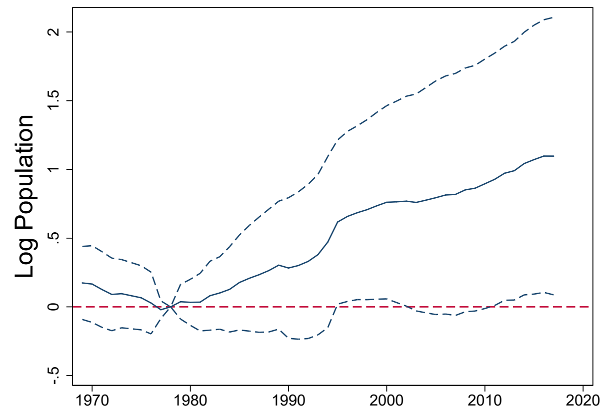
Notes: This table shows mean and standard deviation of industry share in 2001 using BEA data. Column (3) displays coefficients from regressing each variable on the normal county indicator with state fixed effects. The sample size for each variable varies. For forestry, we have 106 normal and 70 asylum counties. For mining, we have 129 normal and 79 asylum counties. For utilities we have 129 normal and 82 asylum counties. For construction and real estate, we have 200 normal and 126 asylum counties. For manufacturing, we have 199 normal and 124 asylum counties. For wholesale trade we have 184 normal and 114 asylum counties. For retail and government, we have 200 normal and 126 asylum counties. For transportation, we have 148 normal and 93 asylum counties. For information, we have 194 normal and 122 asylum counties. For finance and other services, we have 197 normal and 124 asylum counties. For professional services we have 169 normal and 111 asylum counties. For management of companies, we have 152 normal and 92 asylum counties. For administrative services, we have 172 normal and 106 asylum counties. For educational services, we have 170 normal and 104 asylum counties. For health care, we have 171 normal and 104 asylum counties. For arts and entertainment, we have 195 normal and 121 asylum counties. For accommodation and food services, we have 197 normal and 121 asylum counties. The robust standard errors are clustered at the state level and reported in parentheses.
$*p < 0.10, **p < 0.05, ***p < 0.01$

F.7 Event Studies, Other Outcomes

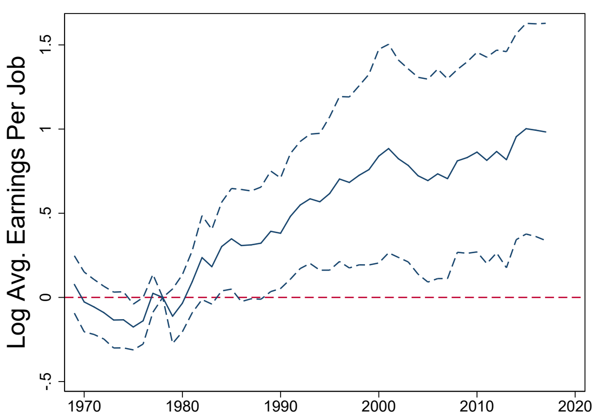
Figure A10 shows the event study specification for outcomes other than employment for the Rust Belt.



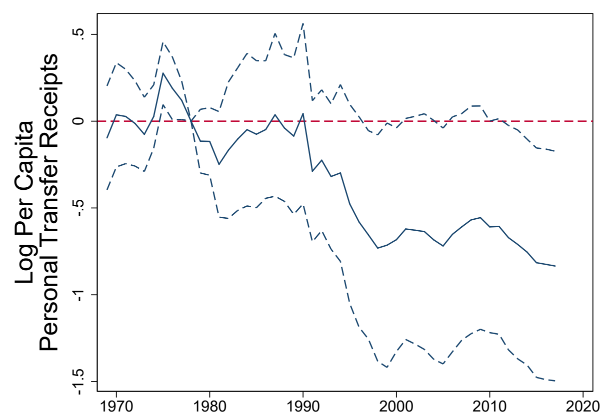
(a) Ln(Employment)



(b) Ln(Population)



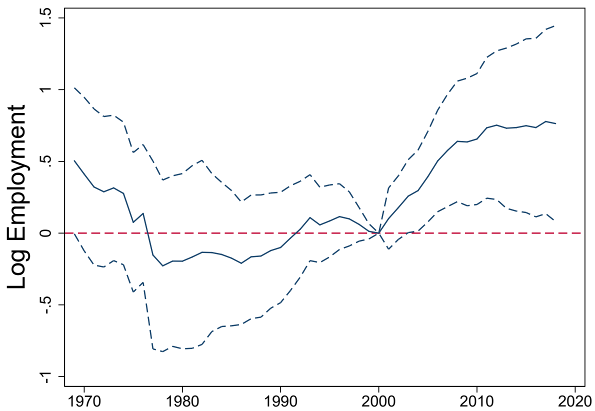
(c) Ln(Avg. Earnings Per Job)



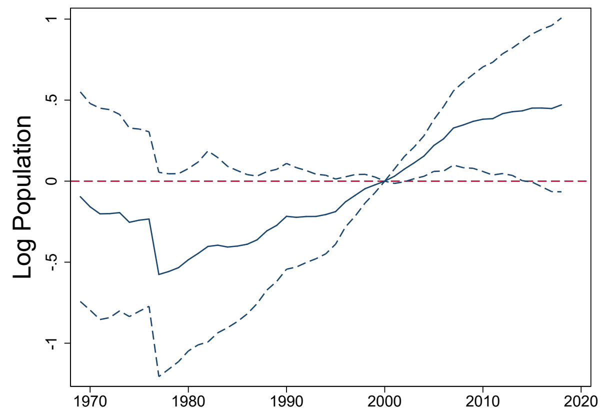
(d) Ln(Per Capita Personal Transfer Receipts)

Figure A10: **Differential Effect of 1978 Manufacturing Exposure on Normal Counties Relative to Asylum Counties in Rust Belt States.** This plot shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 1978 share employed in manufacturing. Effects are relative to 1978, and include county and state-year fixed effects, lower-level terms, and interactions between year fixed effects and ln(population, 1950), and separately ln(population, 1978). Dotted lines are 95% confidence intervals, with standard errors clustered at the county level.

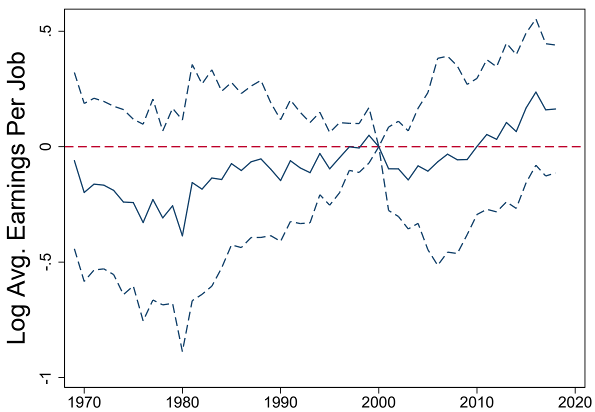
Figure A11 shows the event study specification for other outcomes in the 2000's shock.



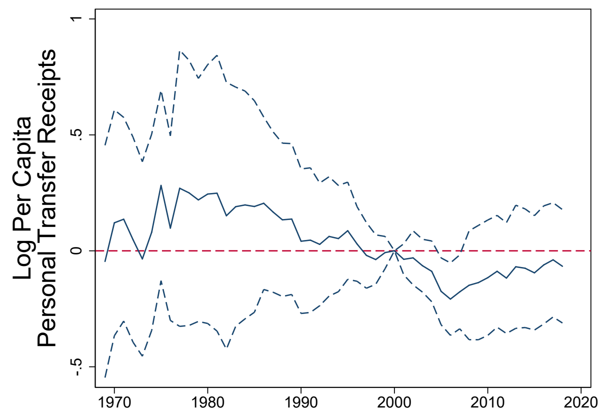
(a) Ln(Employment)



(b) Ln(Population)



(c) Ln(Avg. Earnings Per Job)



(d) Ln(Per Capita Personal Transfer Receipts)

Figure A11: **Differential Effect of 2000 Manufacturing Exposure on Normal Counties Relative to Asylum Counties** This plot shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 1980 share employed in manufacturing. Effects are relative to 2000, and include county and state-year fixed effects, lower-level terms, and interactions between year fixed effects and ln(population, 1950), and separately ln(population, 1980). Dotted lines are 95% confidence intervals, with standard errors clustered at the county level.

F.8 University Spending Event Studies

Figures A12 and A13 show the event study specification of the effect that normal counties have on the resilience of university spending.

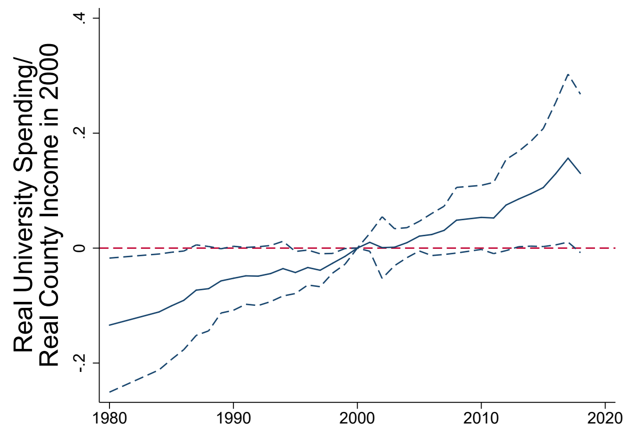


Figure A12: **Differential Effect of 2000 Manufacturing Exposure on Normal Counties Relative to Asylum Counties.** This plot shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 2000 share employed in manufacturing. Effects are relative to 2000, and include county and state-year fixed effects, and lower-level terms. Dotted lines are 95% confidence intervals, with standard errors clustered at the state level. Spending data are not available in 1981-1983, or in 2009. Controls include interactions between year fixed effects and ln(population, 1950) and between year fixed effects and ln(population, 1980). The increasing trend from 1980 to 1990 is consistent with exposure to manufacturing declines in 2000 being correlated with exposure to the 1980s shock.

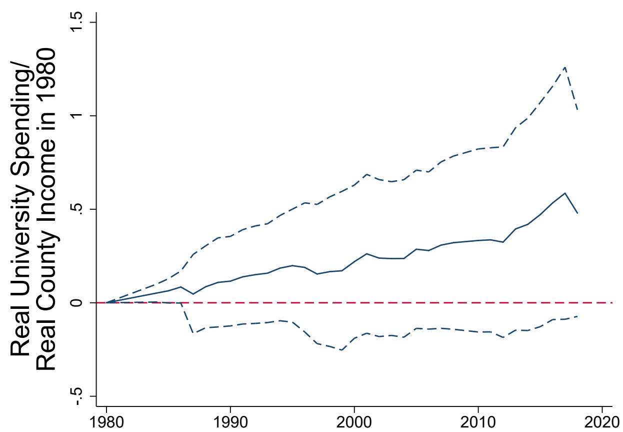


Figure A13: **Differential Effect of 1978 Manufacturing Exposure on Normal Counties Relative to Asylum Counties.** This plot shows coefficients on the interaction between the year indicator, whether the county had a normal school, and 1978 share employed in manufacturing. Effects are relative to 1980, and include county and state-year fixed effects, and lower-level terms. Dotted lines are 95% confidence intervals, with standard errors clustered at the county level. Spending data are not available in 1981-1983, or in 2009. Controls include interactions between year fixed effects and ln(population, 1950) and between year fixed effects and ln(population, 1978).

F.9 Robustness of Main Results: All Counties, 1978-2018

Instead of focusing only on the Rust Belt, Table A15 shows the effects of manufacturing share on the main outcomes of interest for all counties of the United States.

Table A15: **1978 Manufacturing Exposure and Differential 1978-2018 Changes in Normal Counties**

Y = % Growth	Employment	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 1978	-1.704*	-0.967	-2.924***	4.237**
	(0.905)	(0.617)	(0.686)	(1.815)
Normal*Mfg. Share, 1978	1.800*	1.536**	1.511**	-3.199
	(0.995)	(0.681)	(0.722)	(2.587)
Observations	321	321	321	321
R-Squared	0.493	0.486	0.347	0.542
State FE	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	Y	Y	Y	Y

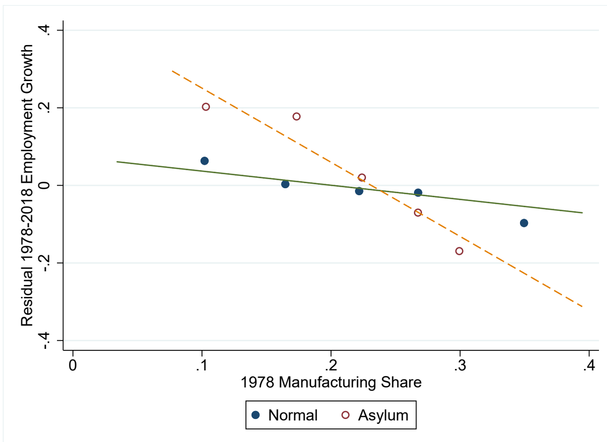
Notes: Robust standard errors in parentheses. Dependent variable is $(Y_t/Y_{t-1})-1$. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978). $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.10 Residualized Scatter Plots

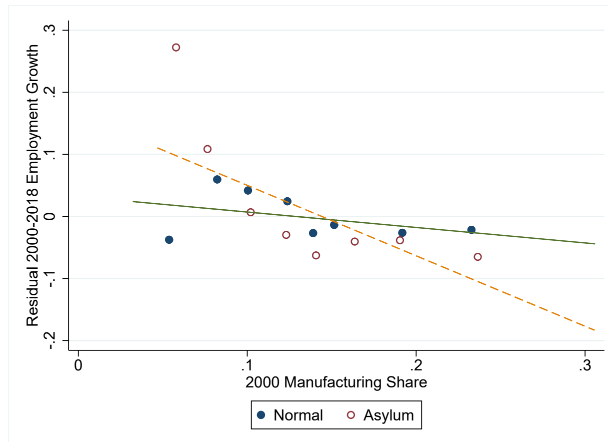
Figure A14d shows the residualized and binned scatter plots of employment growth and manufacturing share. Such plots could show the resilience of counties in response to manufacturing declines in a non-linear way, although the sample is not big enough to show a clearly non-linear relationship.

Figure A14: **Residualized Employment Growth and Manufacturing Share: Binned Scatterplots**

Residualized Employment Growth, Controlling for Earlier Population Growth

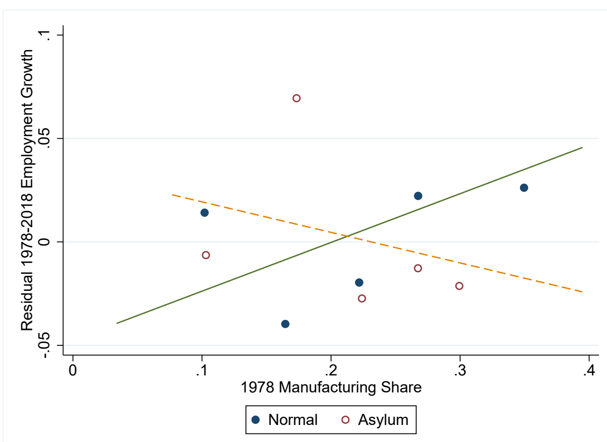


(a) Rust Belt States, 1980-2018

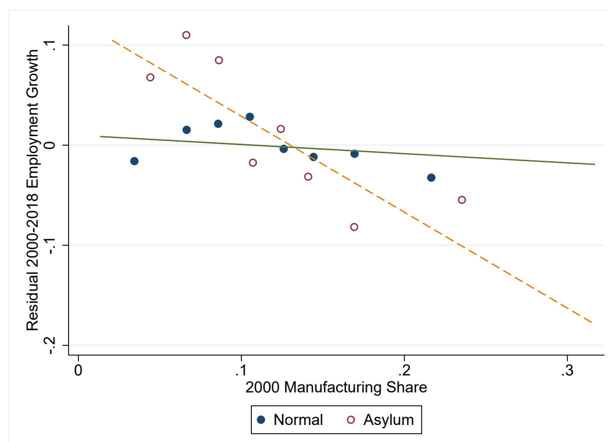


(b) Whole U.S., 2000-2018

Residualized Employment Growth, Full Set of Control Variables



(c) Rust Belt States, 1980-2018



(d) Whole U.S., 2000-2018

This plot shows the results from regressing employment growth on a set of control variables, separately for normal and asylum counties. We then show the binned scatterplots of the residuals from those regressions on manufacturing exposure. In the top half of the figure we control for log population in 1950 and log population in 1978 (for the Rust Belt) or log population in 1980 (for the whole U.S.). In the bottom half of the figure, the control variables include the population variables above, as well as share of the population with a bachelor's degree in 1980, log of average earnings in 1969 and in 1978, log of average per capita transfers in 1969 and 1978, log of per capita income in 1969 and 1978, log of nearby population in 1980 based on a gravity model, percent water coverage, and an indicator for whether the county is within 150 miles of the state capital.

F.11 Additional Controls

Table A16 shows the robustness of the main result, by adding additional controls.

Table A16: **Employment Growth Specifications with Additional Controls**

Y = Employment Growth	% Growth 1978-2018/100	% Growth 2000-2018/100
Manufacturing Exposure	-1.686**	-1.439**
	(0.692)	(0.566)
Normal*Mfg. Exposure	2.049**	1.082**
	(0.783)	(0.513)
Observations	103	320
R-Squared	0.657	0.475
Mfg. Exposure Year	1978	2000
State Fixed Effects	Y	Y
Controls for Pre-Period Population Growth	Y	Y
Other Controls	Y	Y

Notes: Robust standard errors in parentheses in column 1, and clustered at the state level in column 2. Dependent variable is $(Y_t/Y_{t-1}) - 1$. Column 1 includes controls for Ln(Population, 1950) and Ln(Population, 1978), and column 2 for Ln(Population, 1950) and Ln(Population, 1980). Other controls include share of the population with a bachelor's degree in 1980, log of average earnings in 1969 and in 1978, log of average per capita transfers in 1969 and 1978, log of per capita income in 1969 and 1978, log of nearby population in 1980 based on a gravity model, percent water coverage, and an indicator for whether the county is within 150 miles of the state capital.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.12 Transfers, by Type of Transfer

Tables A17 and A18 breaks the result on transfers into various components. In particular, the point-estimates suggest all the major subcomponents exhibit some resilience, even though most are not statistically significant.

Table A17: **1978 Manufacturing Exposure and Differential Growth in Transfers in Normal Counties**

Y = % Growth	Per Capita Transfers		
	Income Maintenance	Unempl. Insurance	Retirement & Other
Manufacturing Share, 1978	14.148***	2.206**	8.263***
	(5.079)	(1.012)	(2.712)
Normal*Mfg. Share, 1978	-9.059	-1.286	-7.500*
	(6.113)	(1.172)	(3.827)
Observations	103	103	103
R-Squared	0.655	0.412	0.427
State Fixed Effects	Y	Y	Y
Controls for 1950-1978 Population Growth	Y	Y	Y

Notes: Robust standard errors in parentheses. Dependent variable is $(Y_t/Y_{t-1})-1$. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978). Retirement & Other includes medical benefits as well as several other categories.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A18: **2000 Manufacturing Exposure and Differential Growth in Transfers in Normal Counties**

Y = % Growth	Per Capita Transfers		
	Income Maintenance	Unempl. Insurance	Retirement & Other
Manufacturing Share, 2000	1.803***	-0.631	0.461*
	(0.479)	(0.456)	(0.268)
Normal*Mfg. Share, 2000	-0.102	-0.367	-0.197
	(0.743)	(1.062)	(0.333)
Observations	320	320	320
R-Squared	0.684	0.589	0.557
State Fixed Effects	Y	Y	Y
Controls for 1950-1980 Population Growth	Y	Y	Y

Notes: Standard errors clustered at the state level in parentheses. Dependent variable is $(Y_t/Y_{t-1}) - 1$. Columns that control for 1950-1980 population growth include controls for Ln(Population, 1950) and Ln(Population, 1980). Retirement & Other includes medical benefits as well as several other categories.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.13 Alternative Specification: Above Median Manufacturing Share

Tables A19 and Table A20 show the robustness of our main specification to defining exposure to the manufacturing shock using a dummy variable for having an above median share in manufacturing employment in the base year. While the results are not as significant as they were in our main specification, the point estimates generally indicate that normal counties are more resilient.

Table A19: **The Rust Belt Shock and Differential Changes from 1978-2018 in Normal Counties, Using Above-Median Manufacturing as Exposure**

Y = % Growth	Employment	Population	Earnings per Job	Per Capita Transfers
Normal County	-0.148	-0.091	-0.476**	0.393
	(0.097)	(0.094)	(0.207)	(0.426)
Above Median Manufacturing Share, 1978	-0.219**	-0.079	-0.668***	0.432
	(0.084)	(0.076)	(0.207)	(0.388)
Normal*Above Median Mfg. Share, 1978	0.157	0.072	0.581**	-0.132
	(0.120)	(0.104)	(0.237)	(0.489)
Observations	103	103	103	103
R-Squared	0.511	0.407	0.296	0.474
State FE	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	Y	Y	Y	Y

Notes: Robust standard errors in parentheses. Above Median Manufacturing Share, 1978 is an indicator for whether the county's share employed in manufacturing was above the median for normal and asylum Rust Belt counties in 1978. Dependent variable is $(Y_t/Y_{t-1}) - 1$. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978).
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A20: **2000 Manufacturing Exposure and Differential Changes from 2000-2018 in Normal Counties, Using Above-Median Manufacturing as Exposure**

Y = % Growth	Employment	Population	Earnings per Job	Per Capita Transfers
Normal County	-0.0653	-0.0266	-0.0180	-0.0453
	(0.0675)	(0.0382)	(0.0268)	(0.0290)
Above Median Manufacturing Share, 2000	-0.125*	-0.0577	-0.0766***	0.0378
	(0.0629)	(0.0397)	(0.0256)	(0.0319)
Normal*Above Median Mfg. Share, 2000	0.105	0.0592	0.0397	0.00239
	(0.0739)	(0.0482)	(0.0352)	(0.0364)
Observations	320	320	320	320
R-Squared	0.373	0.472	0.465	0.560
State FE	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	Y	Y	Y	Y

Notes: Robust standard errors in parentheses. Above Median Manufacturing Share, 2000 is an indicator for whether the county's share employed in manufacturing was above the median for normal and asylum counties in 2000. Dependent variable is $(Y_t/Y_{t-1})-1$. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978).

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.14 Instrumental Variables Specification

Tables A21 and A22 run a regression similar to our main specification but instead use normal schools as an instrument for having a regional public university. We define regional public universities as any county with a four-year public university and have a similar Carnegie classification as normal schools in 1987. The point estimates are larger than our baseline specification, as it is adjusting for the fact that there is less than a one-for-one first stage effect of having a normal school on having a regional university.

Table A21: **The Rust Belt Shock and Differential Changes from 1978-2018 in Counties with Public Universities, IV estimation**

Y = % Growth	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 1978	-3.216***	-1.641***	-7.435***	12.225***
	(0.773)	(0.492)	(1.956)	(4.168)
Regional Public Univ.*Mfg. Share, 1978	2.717***	1.623***	6.165***	-10.783**
	(1.011)	(0.515)	(1.932)	(4.786)
Observations	103	103	103	103
R-Squared	0.413	0.307	0.104	0.293
State FE	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	Y	Y	Y	Y

Notes: Standard errors clustered at the state level in parentheses. Dependent variable is $(Y_t/Y_{t-1}) - 1$. Columns that include controls for 1950-1978 population growth include Ln(Population, 1950) and Ln(Population, 1978) as additional control variables. Coefficients are two-stage least squares estimates, using historical assignment of a normal school as an instrument for the presence of a regional public university, and the interaction between normal county and manufacturing share as an instrument for the presence of a regional public university interacted with manufacturing share. We define counties with regional public universities as counties which had a four-year public university in 1987, that are of the same 1987 Carnegie classification as the normal schools that were converted to regional universities. This excludes Research I universities for example, and the distribution of Carnegie classifications among our set of regional public universities is very close to the distribution among the normal schools that were converted to regional universities.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A22: **2000 Manufacturing Exposure and Differential Changes from 2000-2018 in Counties with Public Universities, IV estimation**

Y = % Growth	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 2000	-1.391**	-0.698**	-0.719***	0.533*
	(0.543)	(0.330)	(0.167)	(0.285)
Regional Public Univ.*Mfg. Share, 2000	1.317**	0.775*	0.385	-0.254
	(0.667)	(0.459)	(0.290)	(0.378)
Observations	325	325	325	325
R-Squared	0.137	0.178	0.080	0.291
State FE	Y	Y	Y	Y
Control for 1950-1980 Pop. Growth	Y	Y	Y	Y

Notes: Standard errors clustered at the state level in parentheses. Dependent variable is $(Y_t/Y_{t-1}) - 1$. Columns that include controls for 1950-1980 population growth include Ln(Population, 1950) and Ln(Population, 1980) as additional control variables. Coefficients are two-stage least squares estimates, using historical assignment of a normal school as an instrument for the presence of a regional public university, and the interaction between normal county and manufacturing share as an instrument for the presence of a regional public university interacted with manufacturing share. We define counties with regional public universities as counties which had a four-year public university in 1987, that are of the same 1987 Carnegie classification as the normal schools that were converted to regional universities. This excludes Research I universities for example, and the distribution of Carnegie classifications among our set of regional public universities is very close to the distribution among the normal schools that were converted to regional universities.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.15 Dropping Normal and Asylum Counties

In Tables A23 and A24, we show the main results, but drop counties that have both a normal school and asylum, rather than counting them as counties with normal schools.

Table A23: **The Rust Belt Shock and Differential Changes from 1978-2018 in Normal Counties, Dropping Counties with Normal Schools and Asylums**

Y = % Growth	Employment	Empl.	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 1978	-2.426**	-2.002**	-2.350***	-1.155**	-5.648***	8.882***
	(0.979)	(0.921)	(0.558)	(0.558)	(1.524)	(2.393)
Normal*Mfg. Share, 1978	2.464**	1.673	2.020**	1.029	4.380***	-7.099**
	(1.217)	(1.274)	(0.809)	(0.727)	(1.569)	(3.413)
Observations	94	94	94	94	94	94
R-Squared	0.083	0.214	0.540	0.411	0.347	0.525
State FE	N	Y	Y	Y	Y	Y
Control for 1950-1978 Pop. Growth	N	N	Y	Y	Y	Y

Notes: Robust standard errors in parentheses. Dependent variable is $(Y_t/Y_{t-1})-1$. Columns that control for 1950-1978 population growth include controls for Ln(Population, 1950) and Ln(Population, 1978).
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A24: **2000 Manufacturing Exposure and Differential Changes from 2000-2018 in Normal Counties, Dropping Counties with Normal Schools and Asylums**

Y = % Growth	Empl.	Empl.	Empl.	Population	Earnings per Job	Per Capita Transfers
Manufacturing Share, 2000	-1.498***	-1.391***	-1.175***	-0.593**	-0.652***	0.543**
	(0.384)	(0.447)	(0.432)	(0.271)	(0.129)	(0.253)
Normal*Mfg. Share, 2000	0.896*	1.066*	1.100**	0.645*	0.309	-0.154
	(0.506)	(0.542)	(0.490)	(0.364)	(0.222)	(0.340)
Observations	301	296	296	296	296	296
R-Squared	0.056	0.305	0.376	0.476	0.467	0.537
State FE	N	Y	Y	Y	Y	Y
Control for 1950-1980 Pop. Growth	N	N	Y	Y	Y	Y

Notes: Standard errors clustered at the state level in parentheses. Dependent variable is $(Y_t/Y_{t-1})-1$. Columns that include controls for 1950-1980 population growth include Ln(Population, 1950) and Ln(Population, 1980) as additional control variables.
$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

F.16 Differential Effect in Counties with Research Universities

Almost none of the universities converted from normal schools are designated as research universities in recent years. Thus, our main identification strategy does not allow us to identify whether research universities improve resilience, for example through spillovers, innovation, and entrepreneurship. However, as an additional exercise we estimate differential effects of manufacturing exposure in counties with public research universities, established between 1830 and 1930. This allows us to estimate a specification relying on similar identification assumptions as equation (1). We compare counties that were assigned state universities that were research universities (based on the 1987 Carnegie Classification), to counties that were assigned state normal schools, to counties that were assigned state asylums, all during the period from roughly 1830 to 1930.

We believe the identification assumptions for research universities may be less likely to hold than for normal schools. Most importantly, in many cases there is only one research university per state, and so the selection into becoming a research county may have been quite different than for becoming a normal or asylum county, for which multiple counties were often chosen across the state. We also have limited power to identify an effect, given the small number of counties with research universities.[65]

We estimate (1), and include an additional interaction between manufacturing exposure and an indicator for having a public R1 or R2 university established between 1830 and 1930. Because we are estimating the effect of manufacturing exposure in research university counties, we need more than one research university county per state if we include state fixed effects. Only about one third of states have more than one research university based on our definition. As a result, we instead include census division fixed effects, and include our additional county level control variables.[66]

[65]Our regressions include only 56 research university counties, compared to 199 normal school counties. Note this differs from the full 204 normal counties as five normal counties have missing manufacturing share in 2000.

[66]This specification yields similar results to our main specification in Table 3 if we include only normal and asylum counties (Appendix Table A25).

The point estimates suggest the negative impact of 2000 manufacturing exposure is substantially smaller in research counties than in asylum counties, though this is not statistically significant (Appendix Table A25). The magnitude is about 20 percent larger than the differential effect in normal counties, but this difference is also not statistically significant. The research output of the university does not appear to dramatically further contribute to resilience, but again we treat this as more suggestive. As we will discuss, this is consistent with our findings in the mechanisms section to follow.[67]

Table A25: Manufacturing Exposure and Differential Growth in Normal Counties and Research University Counties

Dependent Variable	Employment	Employment	Employment	Employment
Manufacturing Share	-1.475*	-1.460	-1.141**	-1.202**
	(0.759)	(0.883)	(0.485)	(0.462)
Normal*Mfg. Share	2.128**	2.081**	0.841*	0.799*
	(0.833)	(0.903)	(0.452)	(0.442)
Research Univ.*Mfg. Share		0.303		0.962
		(1.381)		(0.634)
Observations	103	110	325	362
R-Squared	0.617	0.602	0.309	0.296
States	Rust Belt	Rust Belt	All	All
Exposure Year	1978	1978	2000	2000
Census Division Fixed Effects	Y	Y	Y	Y
Controls for Pre-Period Population Growth	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include census division fixed effects. Research university counties are those with a public R1 or R2 university in the county, based on 1987 Carnegie Classifications, established between 1830 and 1930. Manufacturing share is measured in 1978 for columns 1 and 2, and in 2000 for columns 3 and 4. The sample includes only Rust Belt states in columns 1 and 2, but all states in columns 3 and 4. Robust standard errors are presented in columns 1 and 2, and standard errors clustered at the state level in columns 3 and 4. Controls for pre-period population growth include log population in 1950 and 1978 in columns 1 and 2, and in 1950 and 1980 in columns 3 and 4. Other controls include share of the population with a bachelor's degree in 1980, log of average earnings in 1969 and in 1978, log of average per capita transfers in 1969 and 1978, log of per capita income in 1969 and 1978, log of nearby population in 1980 based on a gravity model, percent water coverage, and an indicator for whether the county is within 150 miles of the state capital. See text for details.

[67] We present the results for Rust Belt states for completeness, but there are only 12 research university counties in this specification.

F.17 Comparison to a Matching Strategy

As an alternative to our control group of asylum counties, we use a matching procedure to identify control counties. We use nearest neighbor matching on 1920 population, as well as propensity score matching with the following variables used to predict likelihood of being assigned a normal school: 1920 population, 1920 urban population share, and 1920 manufacturing employment as a share of population.[68] For both, we specify exact matching on state. The matching procedure and results are described in detail in Appendix Table A26. For the Rust Belt shock, the results are similar to our main results. However, for the 2000 manufacturing decline, the negative effect of manufacturing exposure in the control counties using the matches is substantially smaller than the negative effect of exposure in the asylum counties. As a result, the differential effect is smaller in normal counties, and not precisely estimated.

These matching results suggest selection on unobservables for asylum counties relative to other counties observationally similar to normal counties in 1920. Given our qualitative and quantitative evidence that selection was similar for normal and asylum counties, this suggests the matched control group yields biased effects and further underscores the importance of our identification strategy.

[68] We use 1920 as this allows us to include all states in our sample.

Table A26: Manufacturing Exposure and Differential Growth in Normal Counties, Matching

Rust-Belt Exposure and Growth

Dependent Variable	Employment	Employment
Mfg. Exposure	-1.219**	-2.416***
	(0.590)	(0.552)
Mfg. Exposure*Normal	0.673	1.770**
	(0.711)	(0.692)
Observations	110	110
R-Squared	0.611	0.552
State Fixed Effects	Y	Y
Controls for 1950-1978 Population Growth	Y	Y
Matching Variables	Ln(Pop 1920)	Ln(Pop 1920)
		1920 Urban Pop. Share
		1920 Mfg. Empl. per Pop.

2000 Manufacturing Exposure and Growth

Dependent Variable	Employment	Employment
Mfg. Exposure	-0.683***	-0.474**
	(0.207)	(0.187)
Mfg. Exposure*Normal	0.390	0.242
	(0.333)	(0.271)
N	394	392
R-Squared	0.382	0.415
State Fixed Effects	Y	Y
Controls for 1950-1980 Population Growth	Y	Y
Matching Variables	Ln(Pop 1920)	Ln(Pop 1920)
		1920 Urban Pop. Share
		1920 Mfg. Empl. per Pop.

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. All regressions include state fixed effects. The sample in column 1 includes normal counties, as well as nearest-neighbor matches based on Ln(1920 population), with exact matching on state. In column 2, we estimate a linear probability model in which the dependent variable is an indicator for being a normal county, and the predictors are state fixed effects, Ln(1920 population), 1920 urban population share, and 1920 manufacturing employment over population. We calculate the predicted probability of being a normal county based on the 1920 characteristics, and identify the nearest-neighbor match for each normal county, with exact matching on state. In both columns we implement matching without replacement, and cluster standard errors at the level of the matched set, following Abadie and Spiess (2021). See text for details.

F.18 Effects by Industry

As a robustness check for the analysis in Appendix E, Table A27 shows the entire industry decomposition for the Rust Belt specification. Tables A28 shows the entire industry decomposition for the all counties specification.

Table A27: **Differential Employment Growth in Rust Belt Normal Counties, by Sector**

Y = (Average 2001-2004)-(Average 2015-2018) Employment Growth	All	Forestry	Mining	Utilities	Constr.	Mfg	Wholesale	Retail	Transport.	Info	Finance and Insurance
Manufacturing Share, 1978	-0.552**	-0.0003	0.026	-0.007	-0.028*	-0.049	0.003	-0.020	0.002	-0.040**	-0.048
	(0.224)	(0.004)	(0.042)	(0.006)	(0.015)	(0.051)	(0.017)	(0.022)	(0.032)	(0.019)	(0.032)
Normal*Mfg. Share, 1978	0.532**	0.0003	0.043	0.008	0.045*	-0.060	0.032	0.025	-0.007	0.045**	0.020
	(0.239)	(0.005)	(0.055)	(0.007)	(0.024)	(0.062)	(0.022)	(0.025)	(0.037)	(0.020)	(0.033)
Observations	103	57	66	70	102	103	92	103	84	100	102
R-Squared	0.328	0.249	0.582	0.093	0.307	0.264	0.182	0.280	0.244	0.163	0.118

Y = (Average 2001-2004)-(Average 2015-2018) Employment Growth	Real Estate	Prof./Tech. Services	Mgmt. Companies	Admin Services	Ed Services	Health	Arts	Accom./ Food	Other Services	Federal	State and Local
Manufacturing Share, 1978	-0.046***	-0.080***	-0.016	-0.076***	-0.022*	-0.066	-0.031***	-0.002	-0.027***	-0.007	-0.052
	(0.013)	(0.021)	(0.022)	(0.025)	(0.013)	(0.042)	(0.009)	(0.034)	(0.009)	(0.008)	(0.062)
Normal*Mfg. Share, 1978	0.037**	0.071***	0.026	0.080***	0.030*	0.063	0.017	0.010	0.034***	0.010	0.061
	(0.016)	(0.023)	(0.022)	(0.029)	(0.015)	(0.052)	(0.011)	(0.035)	(0.013)	(0.010)	(0.065)
Observations	102	93	82	93	89	89	100	101	101	103	103
R-Squared	0.305	0.372	0.238	0.245	0.135	0.333	0.394	0.237	0.462	0.157	0.350

All regressions include state fixed effects, and controls for Ln(Population, 1950) and Ln(Population, 1978). Robust standard errors in parentheses.

Table A29 shows the industry decomposition, but only for counties that do not have missing industry employment, rather than making the adjustments to increase the sample size.

Table A28: **Differential Employment Growth in Normal Counties, by Sector**

Y = (2001-2004)-(2015-2018) Employment Growth	All	Forestry	Mining	Utilities	Constr.	Mfg	Wholesale	Retail	Transport.	Info.	Finance and Insurance
Manufacturing Share, 2000	-1.065***	0.0119	0.00397	-0.00150	-0.0256	-0.176***	-0.0199	-0.0231	0.000642	0.000986	-0.0549
	(0.347)	(0.00765)	(0.0546)	(0.00385)	(0.0188)	(0.0331)	(0.0150)	(0.0331)	(0.0321)	(0.0122)	(0.0348)
Normal*Mfg. Share, 2000	0.847**	-0.0180*	0.0112	0.00289	0.0486**	0.00179	0.0492**	0.0303	0.0193	0.00562	0.0421
	(0.409)	(0.0101)	(0.0872)	(0.00515)	(0.0235)	(0.0492)	(0.0207)	(0.0418)	(0.0390)	(0.0134)	(0.0446)
Observations	320	160	194	211	317	319	289	320	246	311	318
R-Squared	0.405	0.339	0.460	0.229	0.435	0.407	0.244	0.361	0.275	0.189	0.213

Y = (2001-2004)-(2015-2018) Employment Growth	Real Estate	Prof./Tech. Services	Mgmt. Companies	Admin Services	Ed Services	Health	Arts	Accom./ Food	Other Services	Federal	State and Local
Manufacturing Share, 2000	-0.0331*	-0.0930***	-0.00708	-0.0437*	-0.0321***	-0.116***	-0.0274***	-0.0300	-0.0305*	-0.0423**	-0.0592
	(0.0172)	(0.0306)	(0.0168)	(0.0248)	(0.0116)	(0.0241)	(0.0101)	(0.0256)	(0.0157)	(0.0199)	(0.0437)
Normal*Mfg. Share, 2000	0.000486	0.0763**	0.00438	0.0791**	0.0417***	0.0964***	0.0138	0.000845	0.0276	0.0593***	0.0642
	(0.0171)	(0.0366)	(0.0180)	(0.0365)	(0.0154)	(0.0338)	(0.0129)	(0.0327)	(0.0181)	(0.0208)	(0.0496)
Observations	317	285	241	281	266	267	308	310	313	320	320
R-Squared	0.369	0.335	0.259	0.409	0.276	0.480	0.318	0.396	0.425	0.203	0.304

All regressions include state fixed effects, and controls for Ln(Population, 1950) and Ln(Population, 1980). Standard errors are clustered at the state level.

Table A29: Differential Employment Growth in Normal Counties, by Sector

A: Rust Belt, 1978-2000										
$Y = \frac{\Delta Empl_{s,t,t-1}}{Empl_{t-1}}$	All	Constr.	Mfg.	Transp.	Whole-sale	Retail	FIRE	Serv.	Fed. Gov.	State & Local
Mfg. Share, 1978	-1.249***	-0.050	-0.342***	-0.010	0.001	-0.168*	-0.134***	-0.373**	-0.040	-0.069
	(0.359)	(0.034)	(0.111)	(0.045)	(0.028)	(0.092)	(0.044)	(0.152)	(0.027)	(0.103)
Normal*Mfg., 1978	1.091**	0.075	0.140	0.023	0.030	0.190*	0.048	0.321*	0.062**	-0.065
	(0.500)	(0.047)	(0.135)	(0.051)	(0.036)	(0.110)	(0.057)	(0.177)	(0.027)	(0.132)
Observations	103	102	103	98	101	103	103	103	103	103
R-Squared	0.646	0.431	0.547	0.424	0.572	0.463	0.412	0.526	0.164	0.426
B. Rust Belt, 2001-2018										
$Y = \frac{\Delta Empl_{s,t,t-1}}{Empl_{t-1}}$	All	Constr.	Mfg.	Retail	Finan. & Insur.	Real Estate	Prof. Serv.	Admin Serv.	Fed. Gov.	State & Local
Mfg. Share, 1978	-0.762***	-0.019	-0.096*	-0.043	-0.087*	-0.037**	-0.074***	-0.085***	-0.005	-0.005
	(0.231)	(0.020)	(0.056)	(0.026)	(0.045)	(0.017)	(0.024)	(0.026)	(0.005)	(0.037)
Normal*Mfg., 1978	0.673**	0.036	-0.020	0.051*	0.046	0.027	0.064**	0.093***	0.009	0.012
	(0.267)	(0.029)	(0.069)	(0.030)	(0.041)	(0.020)	(0.025)	(0.028)	(0.006)	(0.044)
Observations	85	85	85	85	85	85	85	85	85	85
R-Squared	0.351	0.251	0.329	0.364	0.177	0.296	0.414	0.278	0.117	0.416
C. All Counties, 2001-2018										
$Y = \frac{\Delta Empl_{s,t,t-1}}{Empl_{t-1}}$	All	Constr.	Mfg.	Retail	Finan. & Insur.	Real Estate	Prof. Serv.	Admin Serv.	Fed.	State & Local
Mfg. Share, 2000	-1.126***	-0.034	-0.172***	-0.069	-0.090	-0.047*	-0.089**	-0.065**	-0.020**	-0.081**
	(0.378)	(0.028)	(0.052)	(0.043)	(0.057)	(0.026)	(0.037)	(0.025)	(0.008)	(0.037)
Normal*Mfg., 2000	1.075**	0.061*	0.025	0.096**	0.078	0.022	0.071	0.099***	0.034**	0.125***
	(0.408)	(0.031)	(0.052)	(0.047)	(0.065)	(0.025)	(0.044)	(0.035)	(0.013)	(0.044)
Observations	249	249	249	249	249	249	249	249	249	249
R-Squared	0.432	0.431	0.444	0.400	0.238	0.413	0.339	0.407	0.281	0.311
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: *** p-value $\leq$ .01, ** p-value $\leq$ .05, * p-value $\leq$ .1. Robust standard errors in parentheses in panels A and B. Standard errors clustered at the state level in parentheses in panel C. All regressions include state fixed effects. Panels A and B include controls for Ln(Population, 1950) and Ln(Population, 1978). Panel C includes controls for Ln(Population, 1950) and Ln(Population, 1980). Panels B and C include only counties with non-missing industry employment for these listed industries. Dependent variable is the change in sectoral employment from t-1 to t relative to total employment in t-1. See text for details.