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Essays in the Consequences of Occupational Regulation

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by

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Abstract

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Occupational regulation affects many people across many aspects of life. Most anyone knows someone affected or themselves are affected by some form of occupational regulation. This dissertation research investigates the consequences of occupational regulation across three different areas of study: economic history, urban and regional economics, and health policy.

The first chapter investigates the historic licensing of emigrant agents. In the period following the US Civil War, firms wished to capitalize on the availability of African American labor. To do so they hired emigrant agents, also known as labor agents, to hire and help with the migration of individuals from the South. Faced with out-migration at the hands of the labor force, some southern states licensed the profession as a substantial barrier to practice. I use linked full-count US Censuses to determine the effect that licensing emigrant agents had on the individual probability of migration both out of state, and out of the South. A difference-in-differences analysis on the border counties of North and South Carolina suggests that the licensing of emigrant agents reduced the probability of migration out of the South by more than 1 percentage point.

The second chapter deals with cross-border competition and the effects of licensing massage therapists. Occupational licensing has been shown to have many pervasive economic effects. Licensing restricts competition, which causes wage premiums, potentially induces rent seeking, and ultimately results in consumers having to pay high prices through both channels of reduced supply and producers passing on increased cost of doing business. Licensing laws are passed at the state level; and thus, there can be considerable variation across states. Should there be much economic activity at state borders, this would be inconsequential. Yet, the existence of metropolitan areas spanning state borders begs the question of what effects can restricting competition be when competitive substitutes are easily available. This theory is tested using major MSAs that cross state borders and data from the American Community Survey to show how the differing licensing schemes affect the incomes of practicing massage therapists. Ultimately, it appears that the effect of easily available substitutes of massage therapists in the border state mutes the effect of the wage premium that would be caused by a more restrictive licensure scheme. Not only do wage premiums not appear in geographically adjacent states, it is especially missing in border MSAs.

The third chapter is joint work with Dr. Bobby W. Chung of St. Bonaventure University and presents an analysis of the effect of expanding scope of practice for nurse practitioners. As a response to the Covid-19 Pandemic many states choose to expand nurse practitioner scope of practice. We analyze the effects of the expansion of scope of practice on daily Covid related mortality using a synthetic control design. Our results suggest that expanding scope of practice for NPs reduced daily Covid related mortality and was most effective in non-rural areas. The total effect of expanding scope of practice is a reduction of hundreds of deaths over a 30 day period compared to the synthetic control.

This is for Jeff, I miss you every day and I know you'd be pumped to see it finished . . .

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Finally, all mistakes in this dissertation are certainly at the fault of either Kira Jane or Riesling. They are both great cats, but terrible empiricists.

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Chapter 1

Licensing Emigrant Agents: Occupational Regulation and Migration Effects on African Americans

1.1 Introduction

After the Civil War and the passing of the 13th Amendment newly freed African Americans, now able to freely seek gainful employment from any firm, became available for hire. For firms, this created a profit opportunity as these individuals did not expect to be paid as high of wages as native-born whites or European immigrants (Whatley, 1993). To recruit this new labor resource, firms hired emigrant agents, sometimes called labor agents or labor recruiters, to travel into the South, recruit large numbers of African American workers, and assist in their migration. Emigrant agents were viewed as such a threat by Southern firm owners and politicians that many states in the South began licensing the profession prohibitively (Bernstein, 1997; Cohen, 1991; Prentice, Kónya, & Prentice, 2018).

Despite anecdotal accounts of the successes of emigrant agents moving mass quantities of African Americans out of the South, there is disagreement on the overall effectiveness of licensing

emigrant agents in preventing migration prior to the Great Migration (Bernstein, 1997; Collins, 1997; Logan, 2009; Prentice et al., 2018). From this disagreement comes a puzzle; if emigrant agents were ineffective in assisting the migration of many African Americans, why prohibitively license them? Or, if licensing emigrant agents was ineffective, why are there many accounts of emigrant agents ceasing to practice and decreases in emigration due to licensing barriers?[1]

Until recently, analysis of African American migration used net-migration rates or small-samples of time-linked historic data (Collins & Wanamaker, 2014; Prentice et al., 2018). This paper contributes to the existing literature by using linked samples of full-count US Censuses from 1870, 1880, and 1900 which provide a more accurate account of African American migration over census periods. Net migration numbers tend to underplay the amount of interstate migration happening over the time period after the Civil War as most states were experiencing both large amounts of out- and in-migration. In addition to simple natural experiments of states licensing over different periods of time, this paper also focuses on border analyses for additional causal evidence and investigates an especially novel natural experiment first noted by Prentice et al. (2018). North and South Carolina each passed emigrant agent laws in 1891 that licensed at the same fee, $1000 per county. In 1893 North Carolina's Supreme Court deemed the state's emigrant agent licensing law unconstitutional on the grounds that it was not being evenly applied across the state and that the profession did not pose a threat to the public and the fee was unreasonable (Alilunas, 1937). South Carolina retained their law. Under this methodological design, it appears licensing emigrant agents reduced an individual's probability of migration out of the South by more than 1 percentage point.

This paper proceeds as follows. Section 2 provides an overview on emigrant agent history. Section 3 is a summary of the related literature. Section 4 discusses the natural experiment of North and South Carolina. Section 5 covers the methodological approach and data. Section 6 contains results. Section 7 discusses robustness. Section 8 concludes.

[1]See The Atlanta Constitution (Atlanta, Georgia) February 25, 1890 ; January 2, 1895; February 3, 1900; The Times-Democrat (New Orleans, Louisiana) March 26, 1914; The Birmingham News (Birmingham, Alabama) June 19, 1917

1.2 History of Emigrant Agents in the United States

Emigrant agents in the United States rose to prominence during the Reconstruction period following the Civil War. Labor became scarcer in the South after the war as newly freed men and women began the process of developing bargaining power over work, building communities, and pursuing personal goals that were not possible during the era of slavery (Foner, 1988). Cotton planters in the Southeast, sugar cane farmers in Louisiana, railroads out West, coal fields in Eastern Kentucky and West Virginia, etc. were all desirous of African Americans as a less expensive labor source (Bernstein, 1997; Cohen, 1991; Lewis, 1989). To facilitate the recruitment of labor, firms would hire emigrant agents to travel into the South, recruit laborers, and bring them North or West to work (Cohen, 1991). According to Cohen (1991) the occupation first started through efforts and funding of the Freedmen's Bureau. The Bureau was interested in getting African Americans back to work after emancipation and emigrant agents facilitated matching employers with labor. After the Bureau ceased operations, the profession of emigrant agent continued to sustain via the expense of private individuals and firms.

Emigrant agents lowered the transaction costs of moving both in terms of information costs and monetary costs by advertising the opportunities available elsewhere and forwarding money for or outright paying for travel (Bernstein, 1997; Cohen, 1991). Emigrant agents were accredited with the out migration of a number of individuals in southern states; especially after the end of Reconstruction and major mass migrations such as the Great Kansas Exodus of 1879 (Bernstein, 1997; Scroggs, 1917; Windom & Blair, 1919).

To alleviate the tensions of plantation owners and potentially stem the out-migration, southern states began to license the occupation of emigrant agent. The licensing laws put in place were written such that they were facially neutral, not technically targeting any one group, however as Cohen (1991) notes, "everyone knew the aim was to limit black movement."

Scroggs (1917) notes a quote from James L. Orr, Governor of South Carolina in 1866, in which he states that African Americans were invaluable to the production resources of the state and if they

migrated it would "convert thousands of acres of productive land to dreary wilderness" and thus it was his job to discourage migration.

The first state to enact emigrant agent laws was Virginia in 1870, although that law only required a $25 licensing fee, approximately $500 in 2021, which is not an overly intense barrier to practice (Cohen, 1991). Six years later, Georgia enacted a much more significant licensing law requiring a yearly fee of $100 per county per year in which an agent was active. In 1877 the fee was raised to $500 (Alilunas, 1937; Cohen, 1991). Adjusted for inflation that would be a licensing fee of over $14,000 in 2021. If an individual agent wanted to be active in multiple counties, the upfront cost of doing so legally was immense. The 1876 Georgia law marks the first significant barrier to emigrant agents in the South. Following Georgia's example, other states began prohibitively licensing emigrant agents. A summary of the first accounts of licensure through the South can be found in Table 1.

Table 1.1: Years in which Emigrant Agents were First Licensed

State	Year
Virginia	1870
Georgia	1876
Alabama	1877
North Carolina	1891
South Carolina	1891
Lousiana	1894
Florida	1903
Mississippi	1912
Arkansas	1915
Tennessee	1917
Texas	1923

The definition of what explicitly fell under the scope of emigrant agent laws varied across states as well. In some states, anyone who was explicitly recruiting others to move out of the state was an emigrant agent, but their assistants may not have fallen under that label. According to Roback (1984), in Montgomery, Alabama, "Anyone who printed, published, wrote, delivered, posted, or distributed any advertisement, pamphlet, or newspaper persuading people to leave the city..." was considered an emigrant agent and subject to the licensing law. This included assistants as well.

Emigrant agent licensure did not go completely unchallenged. The first appeal occurred in Georgia with Sheppard v. Commissioner and this was followed by challenges in Alabama and North Carolina with Joseph v. Randolph and State v. Moore respectively (Alilunas, 1937). These cases led to different outcomes; the Supreme Court of Georgia ruling the law was not unconstitutional while the supreme courts of Alabama and North Carolina ruling that their laws were unconstitutional. In the case of Joseph v. Randolph, the court felt it "affected the right of free egress from the state" and in State v. Moore the court decided that the occupation was not dangerous to the public interest and the fee was unreasonable (Alilunas, 1937).

The debate over the constitutionality of emigrant agent laws culminated in the case of Williams v. Fears in Georgia which challenged Georgia's 1898 expansion of emigrant agent laws. In the case R.A. "Peg Leg" Williams, a famous emigrant agent, had been arrested for practicing without a license. The case eventually was appealed to the U.S. Supreme Court, and the Court decided that it was not unconstitutional to license emigrant agents and, specifically, that licensing emigrant agents "did not amount to such an interference with the freedom of transit…as to violate the Federal Constitution."[2]

Williams v. Fears set precedent for the constitutionality of emigrant agent licensure and was subsequently followed by a wave of states licensing successfully. Alabama and North Carolina re-licensed in 1903. As well, the precedent set by Williams v. Fears supported two more decisions in South Carolina that challenged the constitutionality of the state's licensure laws, State v. Bates in 1918 and State v. Reeves in 1919 (Alilunas, 1937).

Emigrant agent licensing, among other restrictive laws, remained in place through the Great Migration. The Great Migration, the mass migration of African Americans out of the South, is generally considered to have started in 1910 and marks a major redistribution of the African American population in the United States (Collins, 2021).

[2]Williams v. Fears, 179 U.S. 270 (1900)

1.3 Related Literature

This paper explicitly expands on the literature regarding emigrant agents and African American mobility in the United States. Cohen (1991) discusses the historic account of mobility in the post-Civil war era from the beginning of Reconstruction through the beginning of the Great Migration including suffrage restrictions, recruitment restrictions, and segregation. Bernstein (1997) and Bernstein (2001) discuss the legal aspect of licensing emigrant agents and how the laws were exploited specifically to the detriment of the African American population. In one of the earliest accounts of scholarship on the subject, Alilunas (1937) discusses the various licenses and subsequent court rulings on licensing laws for emigrant agents.

While most accounts of recruitment restrictions, such as those of emigrant agent licensing, and African Americans have been from historic or legal perspectives, some scholars have looked at recruitment restrictions and the effect on migration from an empirical standpoint. Naidu (2010) finds that criminal fines for enticement, recruiting laborers already under contract, led to lower mobility, wages, and returns to experience for black workers; specifically, a 10% increase in enticement fines caused a 0.5% decrease in the probability of migration and between a 0.11% and 0.17% decrease in wage (Naidu, 2010).

Prentice et al. (2018) attempt to empirically test the effects of licensing emigrant agents using the pseudo-natural experiment of Alabama and North Carolina deeming their licensing laws unconstitutional. Using two linked samples of data from the Linked Representative Samples 1870-1880 and 1880-1900 via IPUMS, Prentice et al. (2018) find that emigrant agents had an insignificant effect on the migration of African Americans, and ultimately conclude that licensing emigrant agents was not the cause of the delay of the Great Migration.

The literature on emigrant agents does not only apply to the United States. Harper (2004) summarizes the promotion of immigration to Canada by emigrant agents in Scotland and Ireland in the late 19th Century. Similar to the emigrant agents in the US South, these agents would recruit through large public lectures, advertise Canada through picturesque displays, and would in some

cases accompany those recruited across the Atlantic to secure the recruits work and help them settle in their new home (Harper, 2004).

This paper also contributes to the greater literature on African American emigration, discrimination, and the effects of immigration on economic and social mobility. Despite poor social and economic standing for African American agricultural workers in the South, there was some potential for upward movement in economic status (Alston & Ferrie, 2005). Emigration due to the Great Migration additionally had health effects on African Americans. Using proximity of birthplace to railroads as an IV for migration, Black, Sanders, Taylor, and Taylor (2015), show that African Americans who migrated experienced higher levels of mortality. Higher levels of mortality due to migration implies that while there were economic gains from moving, the professions that individuals were moving into were possibly more dangerous or more hazardous to long term health (Black et al., 2015).

There are multiple hypotheses on why mass migration out of the South did not happen earlier than the Great Migration. Collins (1997) finds evidence that foreign born migration from Europe may have had a crowding-out effect on African American migration North. Logan (2009) shows that while human capital and what appears to be "educational selection" into migration appear to be a major cause, health status played a more significant role than was previously thought. Cohen (1976) notes how the combination of contract laws, vagrancy statutes, criminal surety laws, and convict labor laws led to a system of involuntary servitude, and restricted mobility for African Americans through the late 19th and early 20th centuries.

The interplay of mobility as the power of exit to show displeasure with the current political regime is a popular theme throughout much of the literature. Naidu (2012) shows how suppression of suffrage of African Americans though various barriers including poll taxes and literacy tests resulted in poorer public good provision for African Americans in terms of public schooling and often incited out-migration. Elections at this time were complicated at best if not completely spoiled by fraud and intimidation of the African American populace King (2001a, 2001b). The interplay of voting as a representation of the power of voice and migration as the power of exit for

internal change relates the question to Hirschman (1970). Furthermore, mobility as a mechanism for selecting appropriate public good provision such as education is directly related to Tiebout (1956). As African Americans migrated in search of a better political and social climate as well as for economic purposes they both incited political change where they were leaving and sorted into places that better fit their preferences. Hornbeck and Naidu (2014) show that African American out-migration may have been helpful to the development of the South as well because places that experienced major out migration were forced into investing in agricultural capital and modernized scale of firm operations.

There is plenty of economic evidence of the benefits to migrating both from outside the country and within the United States as well. Abramitzky, Boustan, and Eriksson (2012) show that individuals who have lower economic prospects in Europe were more likely to migrate to the US. Abramitzky, Boustan, Jacome, and Perez (2021) show that historically and currently children of immigrants have higher rates of upward economic mobility as compared to children of US-born individuals. Collins and Wanamaker (2014) expand on the selection idea and apply it to African Americans; while there was some selection into migration, there were large gains for African American migrants similar to that of Abramitzky et al. (2012). Ferrie (2005) shows that the 19th Century United States was more mobile socially and physically than other places and the phenomenon persisted through the cohort of studied individuals who were in their thirties by 1920. Salisbury (2014) notes that in the 19th Century US migration was motivated more by the possibility of upward economic mobility rather than simply higher average wages. Stewart (2012) suggests that migration to US frontier cities was most likely from young, city originating, white collar, and literate individuals and that migration often led to higher quality jobs for immigrants and facilitated mobility between blue- and white-collar jobs. As African Americans migrated North, they were also used as strikebreakers for industrialized jobs Whatley (1993).

Finally, this paper also helps to understand some of the aspects of the discrimination literature. Krueger (1963) shows that theoretically a majority group can benefit economically by discriminating against a minority group. Law and Marks (2009) note that occupational licensing in the

Progressive Era allowed for current practitioners to exclude minorities. Sowell (2019) notes the power of African American labor in the South was what often broke attempts to suppress earnings and specifically notes the use of emigrant agents in recruiting African Americans to move North. Roback (1984) attempts to disentangle whether labor markets were exploitative or competitive in the Jim Crow Era and finds that labor laws were attempts to enforce a cartel of which employers had no other way to sustain due to the bargaining power of African American labor. Shlomowitz (1984) similarly found that there were attempts by farmers to set wages but contemporary articles suggest that labor markets were competitive as the rhetoric often referred to the "theft" of labor among farmers bidding up wages.

1.4 North and South Carolina as a Natural Experiment

The novelty of the North and South Carolina natural experiment was first noted by Prentice et al. (2018). Over the 1870-1880 census periods both states exhibit similar trends in out-migration of African Americans. North and South Carolina both experience out-migration due to emigrant agents. They also had similar political reactions, both choosing to license emigrant agents at $1000 per county in 1891 (Cohen, 1991). The states are a geographic pair as well.

What makes the natural experiment exceptionally unique is that in 1893, the North Carolina Supreme Court declared their emigrant agent licensing law unconstitutional (Alilunas, 1937)(Alilunas 1937). The argument could be made that North Carolina passing a licensing law could have affected South Carolina's legislature's decision to pass a similar law, thus the policy would not be randomly assigned. However, the decision of the North Carolina Supreme Court is a much more random event.

Prentice et al. (2018) additionally point out that a similar natural experiment occurs between Alabama and Georgia as that of North and South Carolina. In 1882 the Alabama Supreme Court declared Alabama's emigrant agent law unconstitutional. However, the argument that out-migration from Alabama exhibits similar trends to that of Georgia is more difficult to make based on the

net and sample migratory trends. Over the two census periods of 1870-1880 and 1880-1900, the net migration rates from Alabama were 0.075 to 0.066 per 10000 and from Georgia were 0.042 and 0.06 (Prentice et al., 2018). Thus, the conditional parallel trends would need to account for a decrease in Alabama's out-migration while Georgia had an increase in out-migration.

1.5 Data and Methodology

The data used in this project consists of linked 100% U.S. Census records between 1870-1880 and 1880-1900. There is no availability of the 1890 census as it was destroyed (O'mahony, 1991). The data consists of all African American males that can be linked between either pair of censuses. These two linked samples are treated as a repeated cross section although there are some individuals that are linked across all three censuses.

The census records are retrieved from IPUMS and are linked using the ABE Exact Method developed by Abramitzky, Boustan, Eriksson, Feigenbaum, and Pérez (2021). For an individual to be linked across two historic data sets according to the ABE Exact Method requires unique first and last names, implied birth year based on age, and birthplace. If there are multiple potential matches then that individual is discarded. According to Abramitzky, Boustan, Eriksson, et al. (2021), automated linking methods such as the ABE Exact Method are correct with hand-linked genealogist records over 95% of the time. In addition, coefficient estimates and parameters of interest are robust when using automated linking methods, regardless of approach.

Using these linked samples provides a much larger data set than has been available to researchers in the past. For example, the North and South Carolina sample contains over 50,000 observations of individual African Americans across the 1870-1880 and 1880-1900 periods as opposed to Prentice et al. (2018) who have just under 600 observations. The first fact that becomes evident when analyzing this data is that net migration rates understate the amount of emigration that is occurring. This data shows that what appears to be a small outflow when looking at net migration may actually be a much larger shuffling or sorting of the African American popula-

tion. Table 2 contains the sample migration rates of men between the ages of 13 and 65 for the 1870-1880 and 1880-1900 periods for states in the South. To visualize the amount of migration occurring within and out of the South, Figure 1 shows migration flows of greater than 100 individuals from any state in the South between 1870 and 1880. There is significant out- and in- migration to most southern states. Of the flows represented in Figure 1, only Alabama and Arkansas had no in-migration greater than 100 individuals, only out-migration. Additionally, the largest migration was from Alabama to Mississippi and consisted of more than 1500 individuals within the sample.

Table 1.2: Sample Migration Rates (per capita)

State	1870 Obs.	1880 Obs.	1870-1880 Migration Rate	Std. Dev	1880-1900 Migration Rate	Std. Dev
Alabama	12846	13600	0.28	(0.48)	0.23	(0.42)
Arkansas	3152	4397	0.37	(0.48)	0.32	(0.47)
Florida	2629	3165	0.33	(0.47)	0.25	(0.43)
Georgia	16615	16514	0.18	(0.39)	0.19	(0.39)
Louisiana	8862	10450	0.27	(0.45)	0.24	(0.43)
Mississippi	11020	14997	0.28	(0.45)	0.26	(0.44)
North Carolina	13156	13199	0.14	(0.34)	0.21	(0.40)
South Carolina	12928	15178	0.13	(0.34)	0.18	(0.38)
Tennessee	6693	7464	0.27	(0.44)	0.30	(0.46)
Texas	5489	8753	0.34	(0.47)	0.22	(0.42)
Virginia	15483	14815	0.23	(0.42)	0.29	(0.45)

To discern the causal effect of licensing emigrant agents on individual probability of migration this paper will use difference-in-differences techniques with the natural experiment of North Carolina deeming their emigrant agent licensure unconstitutional. Summary statistics for North and South Carolina pre- and post-treatment can be found in Table 3. Similarly, Figure 2 illustrates the pre- and post-treatment migrations out of North Carolina and South Carolina.

The potential variables of interest are Migrate and Migrate Out of South. These are determined by what state an individual lives in at time t and t+1. If an individual is living in a different state at t+1 the indicator Migrate takes the value of one, and if the state is not a former Confederate State the indicator Migrate Out of South takes the value of one as well. Family size includes how many family members are reported to be living with the individual at the time of the census. Age

Table 1.3: North Carolina and South Carolina Pre- and Post-treatment

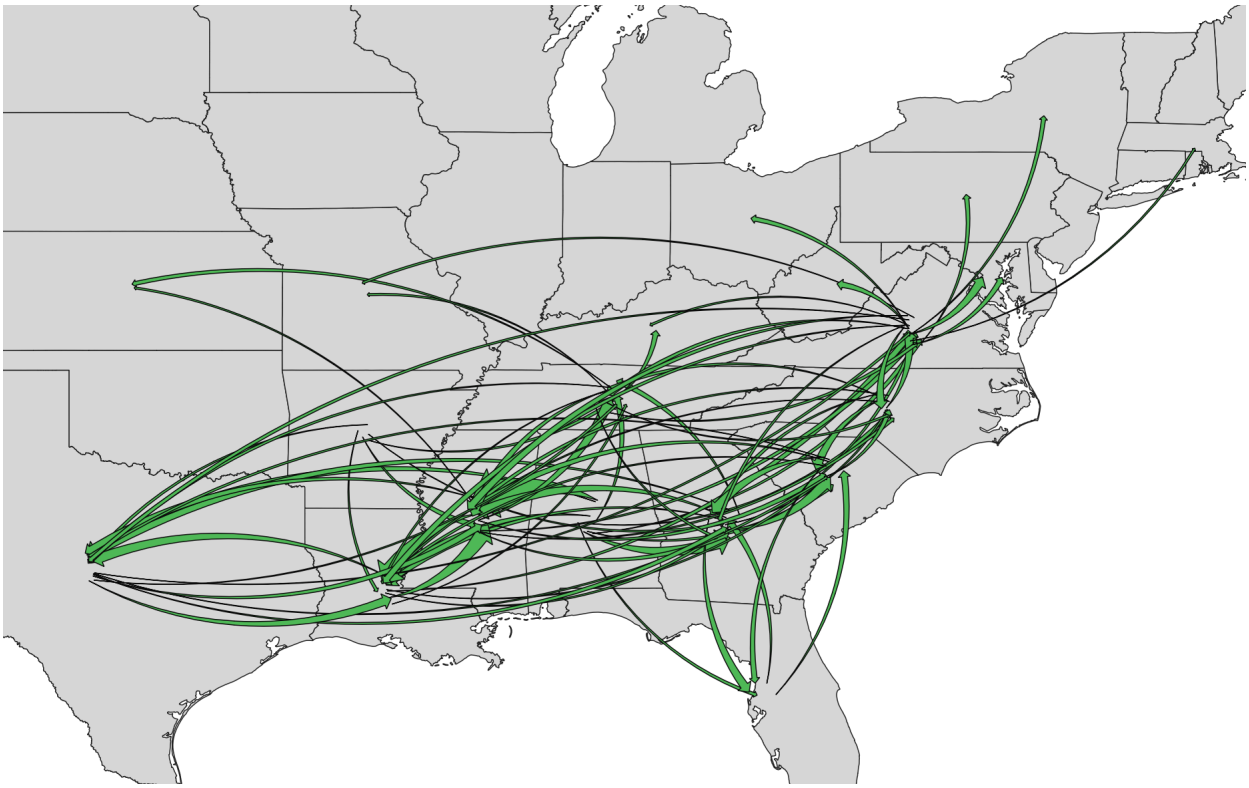
Pre-treatment

	North Carolina			South Carolina		
VARIABLES	N	Mean	SD	N	Mean	SD
Migrate	13,156	0.136	0.343	12,928	0.131	0.337
Migrate Out of South	13,156	0.0215	0.145	12,928	0.00975	0.0982
Family Size	13,156	5.221	3.071	12,928	5.089	2.813
Age	13,156	29.20	13.38	12,928	28.85	13.08
Ever Married	13,156	0.857	0.350	12,928	0.908	0.289
Technical	13,156	0.00258	0.0508	12,928	0.00309	0.0555
Farm	13,156	0.746	0.436	12,928	0.723	0.448
Managerial	13,156	0.00365	0.0603	12,928	0.00379	0.0615
Clerical	13,156	0.000228	0.0151	12,928	0.000309	0.0176
Sales	13,156	0.00144	0.0380	12,928	0.00162	0.0403
Craft	13,156	0.0363	0.187	12,928	0.0295	0.169
Operative	13,156	0.0252	0.157	12,928	0.0160	0.126
Service	13,156	0.0186	0.135	12,928	0.0228	0.149
Large City	13,156	0.0186	0.135	12,928	0.0521	0.222
Illiterate	13,156	0.713	0.453	12,928	0.747	0.435
Distance to Nearest Railroad (miles)	13,156	9.121	10.80	12,928	6.563	6.515

Post-treatment

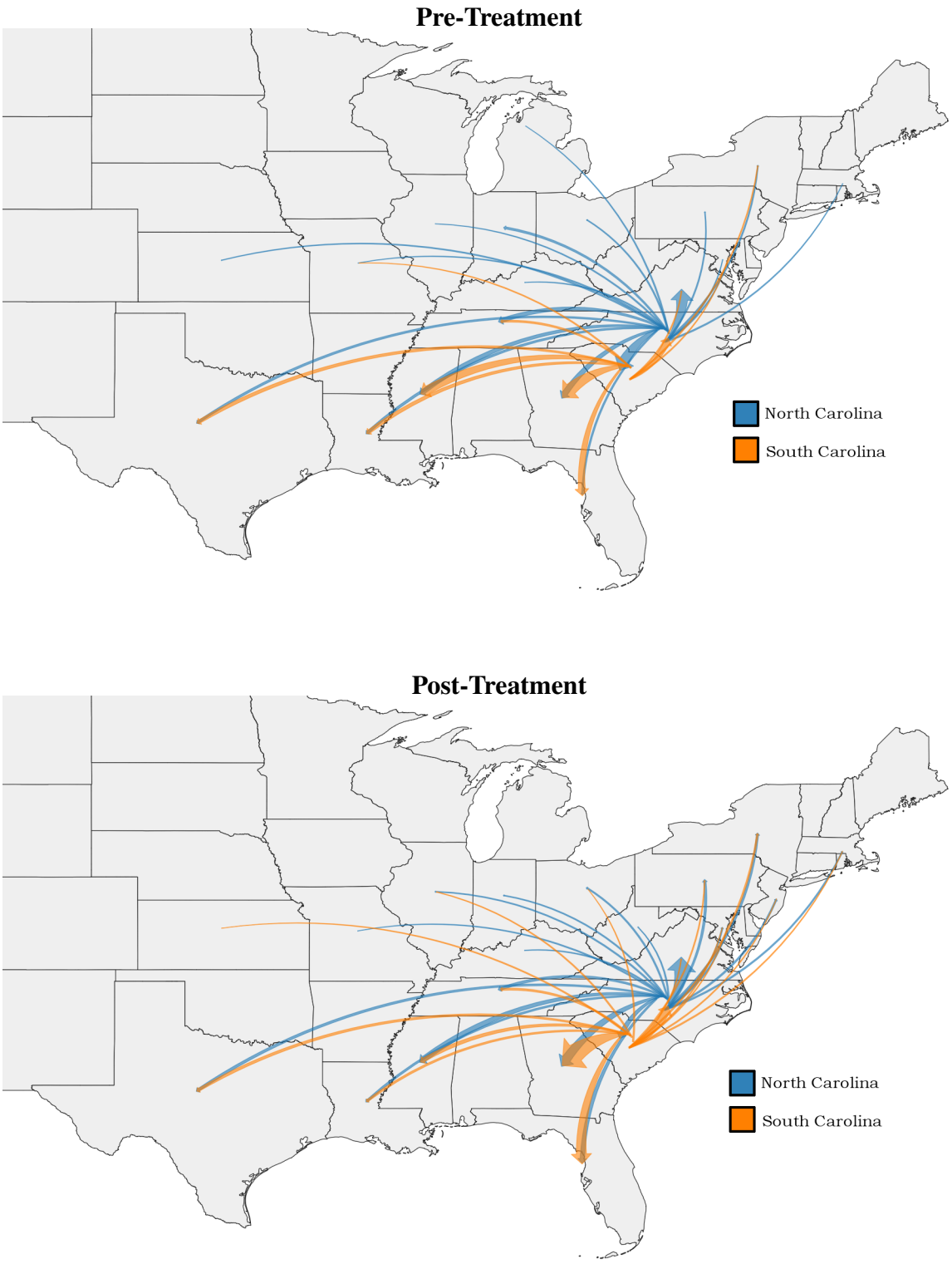
	North Carolina			South Carolina		
VARIABLES	N	Mean	SD	N	Mean	SD
Migrate	13,199	0.213	0.409	15,178	0.176	0.381
Migrate Out of South	13,199	0.0492	0.216	15,178	0.0174	0.131
Family Size	13,199	5.723	3.112	15,178	5.548	2.937
Age	13,199	28.75	12.19	15,178	27.38	11.47
Ever Married	13,199	0.936	0.245	15,178	0.948	0.221
Technical	13,199	0.00606	0.0776	15,178	0.00507	0.0710
Farm	13,199	0.642	0.479	15,178	0.628	0.483
Managerial	13,199	0.00235	0.0484	15,178	0.00198	0.0444
Clerical	13,199	0.000530	0.0230	15,178	0.000593	0.0243
Sales	13,199	0.00129	0.0359	15,178	0.00152	0.0389
Craft	13,199	0.0285	0.166	15,178	0.0274	0.163
Operative	13,199	0.0303	0.171	15,178	0.0214	0.145
Service	13,199	0.0230	0.150	15,178	0.0246	0.155
Large City	13,199	0.0136	0.116	15,178	0.0516	0.221
Illiterate	13,199	0.579	0.494	15,178	0.612	0.487
Distance to Nearest Railroad (miles)	13,199	7.686	8.546	15,178	4.076	4.279

Figure 1.1: Migration Flows From Southeast States 1870-1880



is the individual's age in years. Technical, Farm, Managerial, Clerical, Sales, Craft, Operative, and Service are all indicator variables that deal with what type of occupation an individual has, they are coded based on the Census's OCC1950 classification at the broadest scale. The comparison group of occupation is any non-occupational response which can include missing/unknown and occupations left blank. Large City denotes whether an individual was living somewhere with a population greater than 25,000. Finally, Illiterate is an indicator variable for if the individual could not read or write, this only takes the value of 1 if the individual is explicitly coded as "No, illiterate (cannot read nor write)" and takes the value of zero if the individual can do one of the two, both, or the answer was unknown due to being illegible or blank. Ever Married is a lag variable for if the individual was married, separated, divorced, or a widow/widower. Ever Married cannot be included contemporaneously because marital status was not reported in the 1870 Census. In addition to the individual characteristics available through the censuses this study also uses proximity to railroads as an independent variable. As Black et al. (2015) use proximity of birthplace to railroad lines,

Figure 1.2: Migration From North and South Carolina Pre- and Post-Treatment



and many anecdotal accounts of emigrant agents reference travel by railroad, it is of importance to account for possible rail travel. To account for railroad presence and expansion of railroads across the two time periods, maps from The Association of American Railroads (1948) are digitized and georeferenced with GIS software. Distance from county centroids was then calculated. Therefore, Distance to Nearest Railroad, is a county level variable measuring centroid proximity to railroads.

The model to be estimated is a difference-in-differences linear probability model of the form

$$Migrate_{ij} = \beta_0 + X_i'\beta + \delta_1 D_{TreatState} + \delta_2 D_{TreatPeriod} + \delta_3 D_{TreatState} D_{TreatPeriod} + \epsilon_{ij}$$

Where Migrate is an indicator variable equal to 1 if the individual migrated, regardless of if it was out of state or out of the South, in the period. X is a vector of individual characteristics including age, family size, ever married, large city, occupation classification, literacy, and proximity to railroad. Each individual in South Carolina in 1880 is considered to be treated as South Carolina enacted their law in the 1880-1900 period. Each individual in North Carolina is considered to be not-treated.

One concern may be that North Carolina may not be a reliable counterfactual for South Carolina. Table 4 shows the difference-in-means t-tests for the independent variables in the pre-treatment period. There are small, but statistically significant differences for many of the independent variables. A robustness check where the model is entropy balanced at the first moment is included in section 7.

This study also makes use of the density of the data set made available through 100% Census data which allows for a border analysis of North and South Carolina. The analysis is restricted to just the counties of North and South Carolina that border each other. The sample size becomes more restrictive, but the arguments for a causal relationship are stronger. The border analysis is done with the same model and outcome variables, however in terms of out-of-state migration, migration from North to South Carolina and vice-versa is excluded as that is comparably costless as opposed to other out-of-state migration.

Table 1.4: Difference in Means for Pre-Treatment Independent Variables

	North Carolina	South Carolina		
VARIABLES	Mean	Mean	Diff-in-means	t
Age	29.20	28.85	0.351**	(2.142)
Ever Married	0.857	0.908	0.051***	(-12.812)
Technical	0.00258	0.00309	-0.0005	(-0.773)
Farm	0.746	0.723	0.023***	(4.159)
Managerial	0.00365	0.00379	-0.0001	(-0.180)
Clerical	0.000228	0.000309	-0.0001	(-0.401)
Sales	0.00144	0.00162	-0.0002	(-0.372)
Craft	0.0363	0.0295	0.007**	(3.036)
Operative	0.0252	0.0160	0.009	(5.200)
Service	0.0186	0.0228	-0.004**	(-2.38)
Large City	0.0186	0.0521	-0.034***	(-14.738)
Illiterate	0.713	0.747	-0.035***	(-6.283)
Railroad Distance (miles)	9.121	6.563	2.558***	(23.110)
N = 26,084				

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

1.6 Results

The results for Migrate and Migrate Out of South can be found in Table 5. Estimations (1) and (2) estimate the effects when looking at all of North Carolina and South Carolina, estimations (3) and (4) are the border counties only.

The results found in Estimation (1) are consistent with economic theory. As an individual gets older, they are less likely to migrate but at a diminishing rate, implying that migration is more costly as an individual gets older. Increasing family size, either through a spouse or having children would also increase the cost of migration, and similarly the coefficients are negative and significant. Marriage appears to reduce the probability of migration by about 7.5 percentage points, where each additional family member appears to decrease the probability of migration by just under 4 percentage points. The only occupational classifications that are statistically significant at the 5% level are Farm, Managerial, Operative, and Service. Being a farmer reduces the individual's probability of migration by approximately 1.6 percentage points. Conversely, managerial, operative, and service occupations increase the individual probability of migration by 11.6, 4, and 2.6 percentage points respectively. Being in a large city also accounts for an increase in probability of migration, about

Table 1.5: Linear Probability of Migration

VARIABLES	(1) Migrate	(2) Migrate Out of South	(5) Migrate	(6) Migrate Out of South
Age	-0.00118*	-0.000714***	-0.00116	-0.000662
	(0.000665)	(0.000269)	(0.00184)	(0.000659)
Age^2	1.97e-05**	4.91e-06	1.06e-05	4.55e-06
	(9.65e-06)	(3.74e-06)	(2.63e-05)	(8.83e-06)
Ever Married	-0.0756***	-0.0333***	-0.0685***	-0.0435***
	(0.00648)	(0.00355)	(0.0191)	(0.0110)
Family Size	-0.00393***	-0.000199	-0.00359**	-0.000803
	(0.000538)	(0.000233)	(0.00150)	(0.000604)
Illiterate	-0.00403	-0.00586***	-0.0225**	-0.0105**
	(0.00348)	(0.00149)	(0.00984)	(0.00413)
Farm	-0.0162***	-0.00460***	-0.0240**	0.000571
	(0.00401)	(0.00170)	(0.0118)	(0.00466)
Technical	0.0521*	0.0140	0.0709	-0.0172***
	(0.0284)	(0.0133)	(0.0856)	(0.00565)
Managerial	0.116***	0.0196	-0.0640	0.0647
	(0.0362)	(0.0162)	-0.0766	(0.0782)
Clerical	0.0108	0.00643	0.132	-0.0334***
	(0.0819)	(0.0413)	(0.263)	(0.00604)
Sales	0.0481	0.0307	0.0819	-0.00709
	(0.0472)	(0.0271)	(0.175)	(0.00669)
Craft	0.00343	0.00685	0.0135	0.00109
	(0.0103)	(0.00465)	(0.0342)	(0.0116)
Operative	0.0403***	0.0207***	0.0840**	-0.00307
	(0.0124)	(0.00647)	(0.0408)	(0.0125)
Service	0.0258**	0.0139**	0.0172	0.0176
	(0.0123)	(0.00616)	(0.0408)	(0.0208)
Large City	0.0450***	0.0114**		
	(0.0104)	(0.00483)		
Railroad (miles)	-0.000160	1.68e-05	0.000645	-0.000228
	(0.000199)	(8.75e-05)	(0.000690)	(0.000230)
Treatment State	-0.00331	-0.0103***	0.00281	0.000559
	(0.00426)	(0.00156)	(0.0122)	(0.00416)
Treatment Period	0.0832***	0.0290***	0.0584***	0.0206***
	(0.00473)	(0.00232)	(0.0128)	(0.00549)
Treatment Effect	-0.0352***	-0.0217***	0.0123	-0.0142**
	(0.00631)	(0.00266)	(0.0178)	(0.00690)
Constant	0.248***	0.0729***	0.238***	0.0783***
	(0.0118)	(0.00522)	(0.0341)	(0.0152)
Observations	54,461	54,461	6,160	6,160
R-squared	0.015	0.017	0.018	0.016

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

4.5 percentage points. These results are consistent with Stewart (2012) on migration to frontier cities in that young, white collar, city originating individuals were more likely to migrate. This appears true for migration of African American males out of North and South Carolina in general. Most importantly, the treatment effect is negative and significant, implying that licensing emigrant agents reduces an individual's probability of moving out-of-state by 3.5 percentage points. This is significant at the 1% level.

Estimation (2) is consistent with (1), with smaller coefficients which is potentially due to less migration out of the South as opposed to simply migration out of state. The treatment effect is still negative and significant at the 1% level which implies licensure adversely affected both migration within and out of the South.

Estimations (3) and (4) are the border analyses. Investigating only the bordering counties of North and South Carolina drastically reduces the sample size, however most of the results remain the same. A border analysis makes for a stronger causal argument as the counties face the same relative geography, weather, and distance to other states. This leaves the major defining difference between the counties on either side of the border is the state they are in and subsequently the laws of that state. One noticeable difference is the lack of large cities on the border of the two states. Additionally, the treatment effect is not significant for migration out-of-state but is significant for migration out of the South. In the most restrictive estimation, (4), licensing emigrant agents reduces probability of migrating out of the South by approximately 1.4 percentage points.

These results are also economically significant when taking into account the migration rates out of state in the sample are between 13 and 21 percent for both states. Ultimately, the results support the theory posed that licensing emigrant agents increases uncertainty and therefore increases the cost of migration, reducing observed migration.

Do these results imply that licensing emigrant agents postponed the Great Migration for any amount of time? Probably not. However, the results do imply that without the licensing of emigrant agents, there would have been more out-migration, at least from South Carolina. Considering that would have forced locales and firms to offer better alternatives to African Americans to keep them

from migrating, it is possible that the licensing did allow for a continued mistreatment of African Americans that may not have happened otherwise.

1.7 Robustness

As there are small but statistically significant differences in the means of the independent variables, it is possible that North Carolina may not be a perfect counterfactual for South Carolina.

Hainmueller (2012) provides a method for entropy balancing. The method creates a system of weights such that the reweighted treatment and control groups are more similar, in this case the treatment and control groups reweighted to satisfy similar first moments. As a robustness check the weights from entropy balancing the sample were used in weighted regressions 1-4 in Table 6. These equations are modeled the same as those in Table 4, the first two being the full samples of North and South Carolina and the second two being the border counties.

Weighting the regression according to the entropy balancing weights only strengthens the effects in columns 1 and 2. The treatment effect increases from -3.52 percentage points to -4.10 percentage points for migration out-of-state and from -2.17 percentage points to -2.26 for migration out of the South. For the border analysis the insignificant treatment effect moves closer to zero from 1.23 percentage points to 1.17 percentage points and the treatment effect on migration out of the South decreases slightly from 1.42 percentage points to 1.41 percentage points.

This robustness check suggests that at worst any bias introduced in the results in Table 4 are causing the treatment effect to be understated.

1.8 Conclusion

There has been much debate between the economics and history communities over the effectiveness of licensing emigrant agents as a means to prevent the migration of African Americans from the South after the Civil War. There are many anecdotal accounts of the effectiveness; however, the

Table 1.6: Entropy Balanced Linear Probability of Migration

	(1)	(2)	(3)	(4)
VARIABLES	Migrate	Migrate Out of South	Migrate	Migrate Out of South
Age	-0.00160**	-0.000720**	-0.00191	-0.000757
	(0.000716)	(0.000300)	(0.00186)	(0.000649)
Age^2	2.61e-05**	4.74e-06	1.96e-05	6.31e-06
	(1.04e-05)	(4.13e-06)	(2.66e-05)	(8.81e-06)
Ever Married	-0.0749***	-0.0330***	-0.0618***	-0.0420***
	(0.00694)	(0.00386)	(0.0192)	(0.0109)
Family size	-0.00384***	-0.000108	-0.00350**	-0.000584
	(0.000579)	(0.000262)	(0.00153)	(0.000597)
Illiterate	-0.00435	-0.00685***	-0.0223**	-0.00908**
	(0.00378)	(0.00170)	(0.00997)	(0.00404)
Farm	-0.0141***	-0.00305*	-0.0229*	0.00147
	(0.00426)	(0.00183)	(0.0120)	(0.00448)
Technical	0.0623**	0.0235	0.0446	-0.0172***
	(0.0307)	(0.0182)	(0.0795)	(0.00557)
Managerial	0.105***	0.0217	-0.0956*	0.0358
	(0.0386)	(0.0201)	(0.0504)	(0.0499)
Clerical	0.0755	0.0574	0.292	-0.0309***
	(0.109)	(0.0902)	(0.295)	(0.00579)
Sales	0.0592	0.0173	0.132	-0.00770
	(0.0573)	(0.0232)	(0.208)	(0.00609)
Craft	0.00541	0.000171	0.0163	0.00124
	(0.0119)	(0.00464)	(0.0347)	(0.0105)
Operative	0.0320**	0.0225***	0.0968**	0.00165
	(0.0141)	(0.00768)	(0.0449)	(0.0143)
Service	0.0253*	0.0166**	0.0212	0.0259
	(0.0141)	(0.00808)	(0.0423)	(0.0230)
Large City	0.0606***	0.0213***		
	(0.0131)	(0.00729)		
Railroad (miles)	-2.52e-05	6.86e-05	0.00106	-0.000297
	(0.000239)	(0.000100)	(0.000760)	(0.000188)
Treatment State	-0.00323	-0.0106***	0.00125	0.000425
	(0.00451)	(0.00173)	(0.0124)	(0.00386)
Treatment Period	0.0896***	0.0300***	0.0618***	0.0202***
	(0.00534)	(0.00265)	(0.0132)	(0.00539)
Treatment Effect	-0.0410***	-0.0226***	0.0117	-0.0141**
	(0.00677)	(0.00294)	(0.0182)	(0.00673)
Constant	0.250***	0.0717***	0.240***	0.0757***
	(0.0129)	(0.00590)	(0.0346)	(0.0148)
Observations	54,461	54,461	6,160	6,160
R-squared	0.016	0.018	0.018	0.015

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

effect has not been prominent when investigated using historically available data. Until recently, only net migration rates or very small sample linked census records were accessible.

Using newly developed ABE Exact Method linked census records this paper has shown that licensing emigrant agents could have reduced the probability of migration up to 3.5 percentage points. While this may not be evidence that the Great Migration was postponed due to the licensing, there could potentially have been much more migration had emigrant agents not been licensed.

This study additionally has a cautionary tale. If licensing policy in the past can be written in such a way that it is intentionally harmful to one group of people then policy makers in the present need to be aware that current policy could be unintentionally and disproportionately harmful.

With new data available and more advanced empirical techniques being developed constantly the discussion on the effects of emigrant agents is likely to evolve substantially. Future research could expand the analysis to other applicable states. Specifically, Louisiana and Mississippi can be investigated utilizing this method as Louisiana licensees in 1894 and Mississippi remained unlicensed until 1912. Arkansas and Missouri can similarly be tested as Arkansas licenses in 1915 and Missouri never licenses. Tennessee and Kentucky make for a potential bordered pair as well, Tennessee licensing in 1917 and Kentucky never licensing. Future research into this topic could also include investigating the effects of preventing out-migration on later generations. As father-son pairs can be linked it is possible to carry the effects of licensing emigrant agents forward to census records that report income and other variables of interest.

Chapter 2

Occupational Licensing and Intra-MSA Effects: Massage Therapists in the US

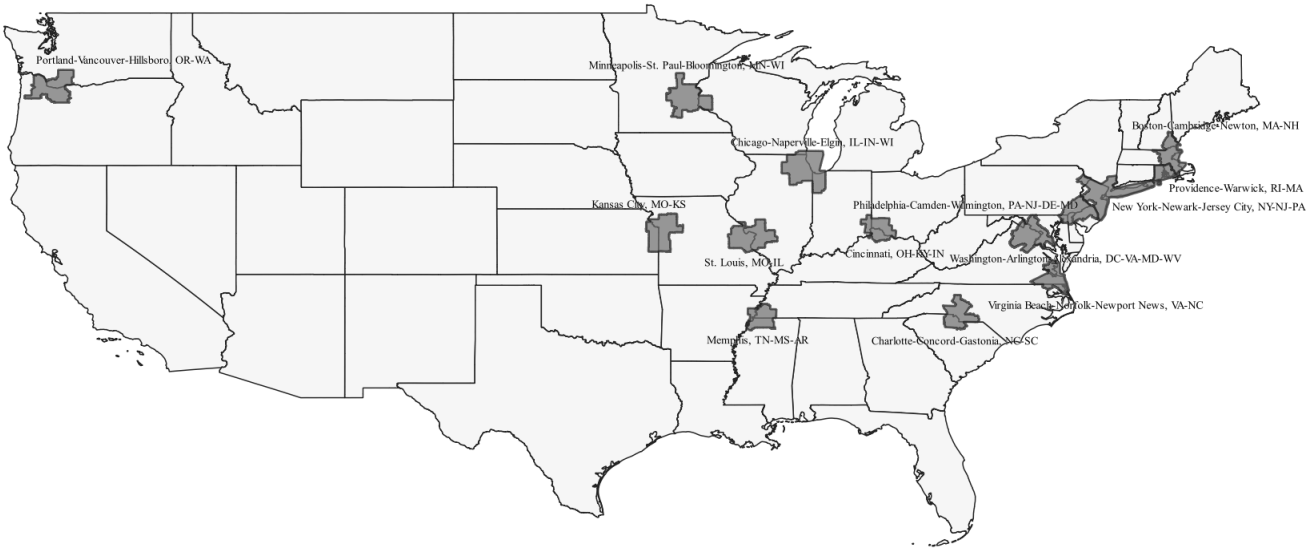
2.1 Introduction

Whether or not an occupation is licensed often has a large effect on the decisions made by those who practice that occupation or those who are deciding on pursuing that occupation. More often than not occupational licensing schemes are decided at the state level. This leads to differing licensing schemes that can cause major effects on licensed industries between states.

This paper discusses how state-level occupational licensing has effects on metropolitan statistical areas that cross state borders. Licensure is a barrier to entry, and restricting entry reduces the supply of massage therapists and causes an increase in price. Licensing can also be a signal of quality to consumers, and they may have increased demand for a service with higher perceived quality, increasing the price. Additionally, occupations that are licensed have a means by which consumers can report complaints to licensing boards, which can result in disciplinary action for individuals who provide intentionally poor or harmful service.

Figure 1 shows the U.S. MSAs with a 2019 population greater than one million people. There are 14 total, half of which are on the East coast. However, there are MSAs that span state borders through the Midwest and one in the Pacific Northwest.

Figure 2.1: U.S. Border MSAs with a 2019 Population Greater than 1 Million People



Theoretically, there could be two possible outcomes from the differing licensure schemes on the incomes of massage therapists within an MSA. One, the licensure could cause a wage premium by increasing barriers to entry. This would result in noticeable differences in practitioner incomes across states. Or two, competition from across the state border, but still within the MSA, could mute these effects. In this case, incomes should be relatively the same on either side of the state border, despite the differences in licensing.

This paper expands on the work done on occupational licensing by Kleiner (2006), and Thornton and Timmons (2013); as well as the work done by Murphy (2016) on economic freedom at state borders. As most of the economic activity done at state borders is driven by MSAs this paper may explain more thoroughly the results presented by Murphy (2016). Additionally, while some work has been done to understand the effects of border labor markets in the occupational licensing literature, no work has shown a model of how the effect may take place. I add to the literature by modeling the local labor market as a circular city with firms facing higher marginal cost on one side of a border and lower marginal costs on the other side.

Section 2 discusses the pertinent literature on the topics of occupational licensing as well as literature of state regulations and economic freedom at state borders, section 3 models a labor market at a state border, section 4 discusses the data collected from the American Community Survey, section 5 discusses the design and results, and section6 concludes.

2.2 Occupational Licensing and Interstate Effects

The economic justification for occupational licensing stems from a problem of asymmetric information (Kleiner, 2006; Shapiro, 1986). The provider of a good or service has knowledge of the quality of the product while those who are consuming have no knowledge, pre-consumption, of product quality. This market failure justifies licensure. Licensure ensures some level of quality has been met, at least at the time the licensure is earned.

Occupational licensing does act as a barrier to entry and ultimately has wage effects (Blair & Chung, 2019; Kleiner, 2006; Kleiner & Krueger, 2013)). However, Kleiner and Krueger (2013) find that governmental certification has a much smaller effect on pay. This could stem from the voluntary nature of certification as opposed to the coercive nature of licensure. Specifically, in the case of massage therapists, Thornton and Timmons (2013) find that wage premiums may be as high as 16.2% in states that license as opposed to states that do not. Blair and Chung (2019) find that licensing causes an average of 17%-27% reduction in labor supply. Adams, Jackson, and Ekelund (2002) test to see whether the wage premiums stem more from the supply-side through restriction of entry or the demand-side through supposed increase in quality.

Because licensing is done at the state level, there is often much heterogeneity in occupational licensing requirements between states (Kleiner & Vorotnikov, 2017). State-run occupational licensing boards set requirements and issue licenses. Unfortunately, these boards are often composed of individuals who already have licenses issued by the boards themselves and thus have incentive to restrict competition further (Allensworth, 2017; Kleiner, 2006). Per Allensworth (2017), this would be considered a major anti-trust issue in the private sector.

There is some evidence that licensing at state borders is also important in the scope of practice literature. Markowitz and Adams (2020) find that increases in Advance Practice Registered Nurse scope of practice leads to a higher likelihood of APRNs being self employed and specifically state, "There is also some evidence that outside options are important with APRNs who live on the border of less restrictive states being more likely to cross a state border for work."

Regulation may also cause other market inefficiencies. Cramer and Krueger (2016) find that rideshare drivers, such as Uber and Lyft, spend a "significantly higher fraction of their time, and drive a substantially higher share of miles" with passengers as compared to taxi drivers and they attribute this in part to inefficient taxi regulations.

It is possible that the wage premiums from licensing may be reduced at or near state borders where competition is higher from unlicensed or less-strictly licensed providers of the same good or service. Murphy (2016) using the Economic Freedom of North America index finds that differing levels of economic freedom at state borders can have effects on income.

In addition to licensing literature there is also precedent for this study in the tax literature. Rohlin, Rosenthal, and Ross (2014) show that differing tax rates affect entrepreneurial activity and the decision of which side of the border to locate on. Regulation and taxation are similar in nature in that they add barriers to doing business.

Of note, this application is only possible in the instance that the good or service being purchased from the licensed occupation can be obtained across state borders. There are applications, such as plumbers, that this does not apply. A plumber can only work in a home in a state in which they are licensed. One cannot take their house across the state border to have plumbing fixed. Essentially this analysis requires an individual to travel to the licensed occupation, not hire the licensee to come to them.

2.3 Modeling Competition at State Borders

To model competition at a state border I use a modified version of the model presented in Vogel (2008). Specifically, I simplify Vogel (2008) by assuming that while there are many firms, there are only two differing marginal costs and that marginal cost difference is driven by the difference in state licensure requirements. Thus, locating on one side of the border comes with a lower marginal cost. This model assumes that the additional licensing affects the marginal cost of production. If licensing means that firms are constantly trying to maintain a minimum quality, this is possible. If licensing is simply a fixed cost, then a simple Cournot model with different fixed costs of entry explains supply restriction.

The market is represented by a circular city model with a unit circumference. Locations along the circumference of the circle are indexed by $z \in [0, 1)$. There is a mass of $L > 0$ consumers that are uniformly distributed around the city. Each customer demands either 1 or 0 units of output inelasitcally.

The utility a consumer gets from consuming a unit of output from firm i located at point z is represented by the utility function:

$$u(z, i) = v - p_i - t \times D(z, i)$$

where v is the common valuation of output, p_i is price, $D(z, i)$ is the shortest arc length separating z firm i and t is the transport cost per unit of distance.

The consumer is assumed to buy from the firm with the lowest location-adjusted price.

$$p_i + tD(z, i)$$

A consumer who chooses not to buy gets utility of zero.

Ultimately, consumer z in a market with a finite set of N producers buys one good from $i \in N$ only if

$$i \in \text{argmin}_{j \in N}\{p_j + D(z, j)\} \text{ and } p_i + tD(z, i) \leq v$$

Assume $n \geq 2$ firms. Each firm has a constant marginal cost of production $k_i \geq 0$ and $\bar{k}$ denotes the average marginal cost of production in the market. For simplicity we will assume $k_i \in \{k_H, k_L\}$ where H represents a highly licensed and therefore high marginal cost state and L represents a low licensed and therefore low marginal cost state.

Additionally we will assume shipping costs are zero as consumers travel to and then consume a service from the business. Vogel (2008) shows how this model can expand to include shipping costs, τ. Therefore, i's cost of producing a good is simply k_H or k_L.

The game consists of $n \geq 2$ firms competing in 2 stages. In the first stage, firms decide on location simultaneously and in the second stage firms decide on price simultaneously. In the location stage, firms choose from locations on the circle $z_i \in [0, 1)$, where $\mathbf{z} \equiv (z_0, ..., z_{n-1})$. In the price stage firms observe locations and then simultaneously choose prices, $p_i \in [0, \infty)$, where $\mathbf{p} \equiv (p_0, ..., p_{n-1})$. A pure strategy for the game is a location choice and the mapping of locations, $\mathbf{z}$, to prices.

Ultimately, Vogel (2008) shows that certain PSNE exist along equilibrium path and the solution for price, market share, and profit are as follows:

$$p_i^* = \frac{t}{n} + \frac{2}{3}\bar{k} + \frac{1}{3}k_i,$$
$$x_i^* = \frac{1}{n} + \frac{2}{3t}(\bar{k} - k_i),$$
$$\pi_i^* = Lt(x_i^*)^2.$$

This is a simplified version of the Vogel (2008) result in considering that there is no delivery cost for firms.

In equilibrium firms that produce at a lower marginal cost are rewarded by being more isolated. Thus, if we consider that firms on either side of the border will be producing a good of the same quality, then the firms on the lower cost side of the border will be more isolated and take up more market share. Theory predicts that the firms on either side will locate equidistantly according to the market share formula, and that firms in the low cost state will have customers that will be on the other side of the border. An interesting consequence of this theory is that low marginal cost

of production is actually associated with fewer firms, not more, as firms that can produce at low location adjusted prices serve larger and larger market share.

For simplicity, imagine a market of two firms. They would locate at opposite sides of the circle, one firm would produce at a lower marginal cost, offer a lower price, and equivalently have a market share of more than half. As the number of firms gets larger, we see that price is mostly affected by $\bar{k}$ and then partially affected by it's own marginal cost, a ratio of 2 to 1. In short, the price a firm charges depends more on the average marginal cost in the market than individual firm marginal cost. If price is driven more by average marginal cost, it would make sense that we would see less of an occupational licensing premium in markets at borders.

For a market with two firms with marginal costs, k_L and k_H there would be:

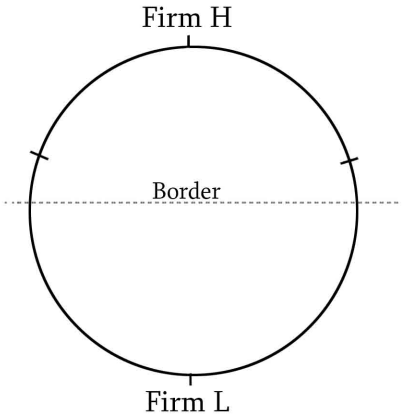
$$p_L^* = \frac{t}{2} + \frac{2}{3}\bar{k} + \frac{1}{3}k_L < p_H^* = \frac{t}{2} + \frac{2}{3}\bar{k} + \frac{1}{3}k_H$$
$$x_L^* = \frac{1}{2} + \frac{2}{3t}(\bar{k} - k_L) > x_H^* = \frac{1}{2} + \frac{2}{3t}(\bar{k} - k_H), \text{ and}$$
$$\pi_L^* = Lt(x_L^*)^2 > \pi_H^* = Lt(x_H^*)^2.$$

If one was to take a market that located exclusively within one state and was therefore governed by one set of licensing laws, $\bar{k}$ would be equal to k_i and firms' prices would simply include a competition and travel cost component, $p_i = \frac{t}{n} + k_i$. It is possible that to see an occupational wage premium accurately would be to have two separate markets from two states with differing licensing laws. At the border, if there is sufficient market activity, there will be spillovers in price, market share, and profit. This model also implies that one might see a gradient of occupational licensing wage premium as the distance from the state border increases, depending on how large you model the market and the cost of travel. Also, The distribution of the market over the border will play a role in what fraction of the firms are high cost versus low cost and thus change $\bar{k}$. A change in the average marginal cost, $\bar{k}$, should not change the wage premium of the local market, $p_H^* - p_L^*$.

With two firms the market would look like Figure 2, with low cost firm Firm L and high cost firm Firm H. Note that the firms locate on opposite sides of the circle and Firm L captures slightly more market share. This solution is similar as an even number of high and low costs firms are in

the market. Low costs firms are spaced out more as they capture more market share. Therefore the model predicts less low cost firms than high cost firms.

Figure 2.2: Two firms with Market Share



One might be concerned with local licensing laws at the city level in this case as well. If a city at the border has local level licensing this should only raise the average marginal cost for all firms in the market not just on one side of the border. This would result in a higher $\bar{k}$ and higher prices compared to other markets in the area. The same would go for any other local market increases in marginal cost like high prices of leasing a business in a city. As long as they are effecting all firms in the market there should be no price change internally.

2.4 Evidence from US MSAs

Table 1 contains a summary of the MSA's used in this analysis. These MSAs cross state borders for only two states, are more than 1 million people in population, have significant differences in licensing, and have enough observations in both states for analysis.

The primary data used is a pooled cross-section of the 2010-2019 ACS 1% samples. The data is collected by the US Census Bureau and was retrieved using the IPUMS database (Ruggles et al., 2020). Specifically, all massage therapists recorded over the 10 year period.

The primary variable of interest is logged hourly total earned income, *lnHourlyWage*. I use this as opposed to income from wages and income from all sources because massage therapists

Table 2.1: Multi-State MSA's Population $>$ 1 million

MSA	States	Population	Difference in Licensing
New York City-Newark-Jersey City	NY-NJ-PA	19,216,182	NY requires \$27 less fees but 500 more hours of experience required
St. Louis	MO-IL	2,803,228	Illinois requires \$5.20 more in fees and 100 more hours of experience
Portland-Vancouver-Hillsboro	OR-WA	2,492,412	Oregon: \$49 more in fees, 125 more hours of experience, one less exam
Kansas City	MO-KS	2,157,990	MO licenses KS does not, MO requires \$169.80 in fees and 500 hours of experience
Providence-Warwick	RI-MA	1,624,578	MA requires \$160 more in fees and 150 more hours of experience

classified as self-employed don't receive wages. Using earned income also excludes income from capital gains or losses. *ln(HourlyWage)* is computed by taking the total pre-tax personal income or losses from all sources, *Income*, dividing by usual hours worked per week *HoursWorked* times fifty-two. Other variables collected are age, as a proxy for work experience, *Age*; the class of the worker, *SelfEmployed*; and indicator variables for PoWPUMA, state, educational attainment, race, and sex.

To understand the interaction of the differing licensure within the MSA and how it compares across states I apply the following two panel models to the two states in which the MSA is part of:

$$log(HourlyWage)_{it} = \beta_0 + \beta_1 Dstate1_{it} + \Gamma\chi_{it} + \alpha_t \tag{2.1}$$

$$log(HourlyWage)_{it} = \beta_0 + \beta_1 Dstate1NotMSA_{it} + Dstate2NotMSA_{it} + Dstate1inMSA_{it} + \Gamma\chi_{it} + \alpha_t \tag{2.2}$$

where χ is a set of indicator variables for gender, race, education, and employment status and α_t is a year fixed effect.

Equation 1 would be the naive test to see if the two states exhibit an occupational licensing wage premium. This requires the two states have significant differences in occupational licensing laws. It is also preferable that the MSAs be large enough for there to be enough observations within on both sides of the border. It is also important that the MSA is somewhat isolated from other state borders such that there aren't multiple regulatory influences.

2.5 Results

Tables 2-6 shows the results of estimating wage premiums in the following MSAs: New York City-Newark-Jersey City, St. Louis, Portland-Vancouver-Hillsboro, Kansas City, and Providence-Warwick. In looking at multiple MSAs in the US there are a few results. Overall, massage therapists working in the MSA make more than those not working in the MSA, In none of the estimations are there statistically significant wage premiums between the two states, and when restricted to the MSA the coefficient estimates are even smaller.

New York doesn't show a wage premium that is statistically significant over New Jersey. This could be because most of the economic activity of the state is being driven by the MSA. Roughly 64% of New York's population lives in the MSA. Thus, it may be better to use a more zoomed-in analysis for the model to make sense. The geographic footprint of the New York-Newark-Jersey City MSA is also large as well. It covers nearly half of the state of New Jersey. New Jersey is actually completely within the New York-Newark-Jersey City MSA or Philadelphia-Camden-Wilmington MSA. So if the theory predicts a gradient of wage premiums from the different MSAs then it would be expected that New Jersey's wage would be getting pulled towards that of Pennsylvania as well as that of New York because of the Philadelphia MSA.

The St. Louis MSA does not appear to have a wage premium. St. Louis is split roughly half and half between Missouri and Illinois geographically. It is isolated from the other MSA's in the area, and the other border MSA in Missori, Kansas City, is bordering with a state that does not license massage therapists.

Table 2.2: Results: New York City-Newark-Jersey City

	(1) NY-NJ Simple	(2) NY-NJ MSA
New York	0.00843	
	(0.0894)	
New York in NYC		0.0641
		(0.116)
New York not in NYC		-0.0513
		(0.101)
New Jersey not in NYC		-0.0486
		(0.121)
Self Employed	-0.0578	-0.0464
	(0.0789)	(0.0812)
Female	0.0672	0.0717
	(0.0780)	(0.0779)
Black	0.159	0.157
	(0.0977)	(0.0980)
Asian	-0.383***	-0.383***
	(0.0921)	(0.0924)
Other Minority	-0.100	-0.100
	(0.159)	(0.159)
Hispanic	-0.0794	-0.0847
	(0.104)	(0.104)
Constant	1.156**	1.196***
	(0.452)	(0.454)
Age and Education	YES	YES
County Income and Population	YES	YES
Year FE	YES	YES
Observations	773	773
R-squared	0.071	0.073

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

Table 2.3: Results: St. Louis

	(1)	(2)
	IL-MO Simple	IL-MO MSA
Illinois	0.0286	
	(0.0942)	
Illinois & St. Louis		0.00305
		(0.177)
Illinois not St. Louis		0.0156
		(0.111)
Missouri not St. Louis		-0.0564
		(0.131)
Self Employed	-0.134*	-0.133*
	(0.0778)	(0.0782)
Female	-0.0154	-0.0184
	(0.0897)	(0.0900)
Black	-0.160	-0.160
	(0.147)	(0.147)
Asian	-0.590***	-0.592***
	(0.156)	(0.156)
Other Minority	-0.252	-0.254
	(0.249)	(0.250)
Hispanic	-0.0411	-0.0420
	(0.191)	(0.192)
Constant	1.592***	1.611***
	(0.374)	(0.379)
Age and Education	YES	YES
County Income and Population	YES	YES
Year FE	YES	YES
Observations	552	552
R-squared	0.079	0.079

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

Table 2.4: Results: St. Portland-Vancouver-Hillsboro

	(1)	(2)
	OR-WA Simple	OR-WA MSA
Oregon	0.114	
	(0.0805)	
Oregon in Portland		0.0390
		(0.121)
Oregon not in Portland		-0.133
		(0.113)
Washington not in Portland		-0.197**
		(0.0984)
Self Employed	-0.0190	-0.0185
	(0.0642)	(0.0644)
Female	-0.0116	-0.00379
	(0.0564)	(0.0561)
Black	-0.199	-0.195
	(0.171)	(0.167)
Asian	-0.0732	-0.0905
	(0.0968)	(0.0973)
Other Minority	0.0838	0.0892
	(0.113)	(0.112)
Hispanic	0.0842	0.0910
	(0.130)	(0.128)
Constant	1.742***	1.880***
	(0.342)	(0.345)
Age and Education	YES	YES
County Income and Population	YES	YES
Year FE	YES	YES
Observations	853	853
R-squared	0.047	0.053

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

The Portland-Vancouver-Hillsboro MSA has the largest coefficient estimate for state to state comparison at 0.114, however the estimate is still statistically insignificant. Portland is also the first of the large MSAs where the coefficient on Asian is insignificant and also has the smallest coefficient estimate on the Asian indicator out of all the MSAs in the analysis.

The Kansas City MSA should provide the best comparison as Kansas does not license massage therapist and Missouri does. However, the coefficient estimate suggests less than an 8% wage premium if we were to take it as significant. That estimate is less than half of the premium suggested by Thornton and Timmons (2013) of licensed vs unlicensed states.

Finally, the Providence-Warwick MSA suggests a potential wage premium in the wrong direction. Additionally, the sample size for massage therapists dwindles significantly.

Ultimately, there is no evidence of a wage premium in any of the MSAs included. Even in the most extreme licensing differences such as Missouri-Illinois and New York-New Jersey. At most it appears there is evidence of a MSA premium, which is consistent with economic theory. This suggests that the theoretical model acts as an upper bound for potential wage premiums due to differences in licensing, at least in the case of massage therapists. It could also be possible that the differences in licensing, even at the most strict are not a significant barrier to entry.

So far this analysis had focused on only supply side characteristics. It is possible that these areas have different demand side effect and that is what is driving price. If one area was such lacking in demand as to drive price down, then I would expect firms to exit in equilibrium. If an area was in such high demand, then I would expect firms to enter and drive down prices. Thus, only firm characteristics should be driving prices. I have no reason to suspect that any one portion of the nation has any more need for massage therapists than any other. Therefore demand should be relatively constant across the different locals.

It is also possible that the results are driven by small sample sizes. In some instances there are 30 or less observations in the pool for within the MSA or outside of the MSA for certain samples. However, in the larger sample sizes of New York City and Portland, their isn't a wage premium.

Table 2.5: Results: Kansas City

	(1)	(2)
	MO-KS Simple	MO-KS MSA
Missouri	0.0753	
	(0.144)	
Missouri in Kansas City		-0.0687
		(0.199)
Missouri not in Kansas City		0.165
		(0.197)
Kansas not in Kansas City		-0.0247
		(0.296)
Self Employed	0.000321	0.00573
	(0.143)	(0.143)
Bachelors	0.263*	0.258*
	(0.153)	(0.150)
Graduate Degree	1.069**	0.991*
	(0.509)	(0.547)
Female	-0.0506	-0.0703
	(0.148)	(0.147)
Black	-0.815*	-0.780*
	(0.426)	(0.445)
Asian	-0.274	-0.222
	(0.378)	(0.407)
Other Minority	-0.938	-0.981
	(0.821)	(0.814)
Hispanic	0.303	0.248
	(0.450)	(0.458)
Constant	1.972***	1.939***
	(0.608)	(0.661)
Age and Education	YES	YES
County Income and Population	YES	YES
Year FE	YES	YES
Observations	152	152
R-squared	0.219	0.226

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

Table 2.6: Results: Providence-Warwick

	(1)	(2)
	MA-RI Simple	MA-RI MSA
	(1)	(2)
Massachusetts	-0.218	
	(0.297)	
Massachusetts in Providence		NA
Massachusetts not in Providence		-0.403
		(0.402)
Rhode Island not in Providence		-0.241
		(0.448)
Self Employed	0.117	0.126
	(0.270)	(0.273)
Female	-0.174	-0.141
	(0.236)	(0.246)
Black	0.136	0.140
	(0.603)	(0.607)
Asian	0.0989	0.0836
	(0.467)	(0.472)
Other Minority	0.105	0.122
	(0.284)	(0.293)
Hispanic	0.184	0.177
	(0.211)	(0.217)
Constant	1.953	2.033
	(1.260)	(1.293)
Age and Education	YES	YES
County Income and Population	YES	YES
Year FE	YES	YES
Observations	104	104
R-squared	0.111	0.113

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

2.6 Conclusion

Occupational licensing affects a significant portion of the US population and licensing is often handled at the state level. This leads to a variation in licensing laws between states and potential effects in labor markets that span state borders. This article looks at the competing occupational wage premiums between licensing schemes and the competition effects of local labor markets.

In modeling local competition as a circular city with different marginal costs for firms with differing licensing schemes the theoretical model predicts that the occupational licensing premium within a market that spans a border will only be one-third of what would be the case of fully separate markets. Thus, the competition effect of being geographically close to lower price substitutes has an effect on buyer behavior.

In testing the model on the US metropolitan areas that span state borders there doesn't appear to be any occupational wage premium within the metropolitan area. In looking state-to-state there doesn't appear to be a wage premium in comparing states that border one another at all. The only premium that appears to exist is a premium for working inside of the MSA, which is consistent with economic theory. When looking at states that do not border one another, wage premiums are present. Understanding the geographic and spatial element of regulation is important.

If the competition effect outweighs the licensing effect as evidenced here then it is important for state legislators to know that licensing an occupation that provides a good or service that can be obtained out of state then there will be only losses in terms of employment in the home state. There will be no gain in terms of higher pay for those that obtain a more difficult license. This is especially important for states that have large population centers on the state border, such as Missouri, or states that have a relatively small geographic footprint such as Rhode Island.

This evidence may also be a reason that states engage in yardstick competition and that there are often geographic patterns in regulatory laws.

More specific geographic data would be necessary to understand if there is an entrepreneurial location decision being made as in Rohlin et al. (2014) and that is a logical extension of this study.

Chapter 3

The Effect of Expansion of Nurse Practitioner Scope of Practice on Early Covid-19 Deaths

3.1 Introduction

A shortage of health professionals in the United States has been a notable concern among health policy makers. According to the Bureau of Health Workforce, only 55 percent of the workforce need for primary care was met in 2017.[1]

The nationwide problem of shortage manifests itself even more under the recent pandemic. To address the health workforce shortage, a number of states temporarily expanded the scope of practice for nurse practitioners (NPs). NPs are well-trained health care personnel, typically requiring post-graduate training. According to the American Association of Nurse Practitioners (AANP), NPs with full autonomy are authorised to "evaluate patients; diagnose, order and interpret diagnostic tests; and initiate and manage treatments.[2]" Although they are well-prepared to provide

[1]The National Conference of State Legislatures provided a detailed summary on the statistics, which is retrievable at https://www.ncsl.org/research/health/health-professional-shortage-areas-2017-postcard.aspx

[2]See the official website of AANP (https://www.aanp.org/advocacy/state/state-practice-environment) for more details.

primary, acute, and specialty care, their scope of practice varies by state. According to the classification by AANP, in a state with 'restricted/reduced practice', NPs need to have a collaborative agreement with or under direct supervision of a licensed health professional, e.g. physician or dentist. The limited authority of NPs not only reduces health access in rural areas, but also significantly increased the administrative burden of the supervising personnel in an expense of time for patient care (Traczynski & Udalova, 2018a). Therefore, granting NPs independent practice authority should have a positive impact on patient outcomes.

As of the first quarter of 2020 eighteen states were classified as "restricted/reduced NP practice" by the AANP. Among those, Alabama, Arkansas, California, Indiana, Massachusetts, Missouri, North Carolina, Pennsylvania, South Carolina, Texas, and West Virginia waived select practice requirements and Kentucky, Louisiana, New Jersey, New York, Virginia, and Wisconsin temporarily suspended all scope of practice requirements. Florida, Georgia, Illinois, Mississippi, Ohio, and Utah did not implement changes. In the empirical exercise, we leverage on this quasi-experimental setting to compare daily COVID mortality in the treated states with those that did not implement any change before and after the emergency response. Although the discussion evaluates the recent emergency response under the pandemic, the finding here contributes to the ongoing debate of whether NPs should be granted independent authority.

This report estimates the effect of expanded scope of practice for NPs on COVID mortality through the Midwest and Southeast regions of the United States using a synthetic control design. In the West and Northeast region, most of the states have already granted full scope of practice prior to the outbreak of COVID. This limits out sample size to Midwest and Southern states, primarily because of the availability of never-treated states as control groups.

According to our estimates, expanding scope of practice for NPs caused a statistically significant reduction in deaths per day for non-rural counties in 4 of the 10 states analyzed. In Arkansas, we find a mean reduction in daily deaths of -4.74 over the 10-30 day term post expansion of scope of practice. Our results support granting NPs full independent authority to east the shortage of healthcare workforce, which has been a challenge facing growing health care demand.

3.2 Literature on Scope of Practice and Covid-19

There have been many studies on the impact of licensing and scope of practice laws on provider supply and health outcomes.

In terms of restriction of entry, Kleiner and Kudrle (2000) show that tougher licensing prevents entry into the dental profession. Also, Markowitz and Adams (2020) show that scope of practice restrictions on Advance Practice Registered Nurses, APRNs, does not cause them to alter many labor market supply decisions and that scope of practice matters most for whether or not APRNs are self-employed. APRNs in states with full practice authority are six to seven percentage points more likely to be self-employed (Markowitz & Adams, 2020).

Less scope of practice restrictions appear to have more significant effects in rural areas and help with access to care. Spetz, Skillman, and Andrilla (2017) show that nurse practitioners practice more in primary care in rural areas, and they more often report practicing to the full extent of their scope of practice, were more satisfied with their work, and planned to stay in their job more often when practicing in a rural area. McMichael (2018) finds that relaxing NP licensing laws increases the supply of NPs in areas with few practicing physicians by 60 percent, but the effect decreases as the supply of physicians grows.

In terms of care quality there is no evidence that expanding scope of practice results in poorer outcomes. Markowitz, Adams, Lewitt, and Dunlop (2017) investigate scope of practice restrictions on Certified Nurse Midwives, CNMs, and find that scope of practice laws do not appear to help or hurt in regard to heath outcomes of mothers or infants and states with no scope of practice barriers have lower induced labor rates and lower Cesarean section births. McMichael, Spetz, and Buerhaus (2019) find that states with nurse practitioner practice without physician oversight had lower overuse of emergency departments after an expansion of Medicaid. Specifically, while states with physician oversight had a 28% increase, states without only had a 7% increase (McMichael et al., 2019). Traczynski and Udalova (2018b) find NP independence increases frequency of routine checkups, improves care quality, and decreases emergency room use by patients with ambulatory care sensitive conditions. In addition, the indirect costs of primary care falls when the supply of

providers rise. Allowing NPs to practice and provide without oversight increases care for underserved populations (Traczynski & Udalova, 2018b).

Ultimately there is no evidence that expansion of scope of practice is correlated with "bad" outcomes, at worst it appears that scope of practice expansion has a null effect. If expanding scope of practice is causally related to higher access to health care in rural areas and other better healthcare outcomes then we would expect that expanding scope of practice in response to the Covid-19 pandemic would result in better pandemic response.

There is also much literature regarding the Covid-19 pandemic. Bayne, Norris, and Timmons (2020) give an overview on the emergency occupational licensing and scope of practice reforms that took place as a response to the Covid-19 pandemic. The National Bureau of Economic Research hosts a plethora of new working papers regarding Covid-19. Burlig, Sudarshan, and Schlauch (2021) study the effect of domestic travel bans as a response to slow the spread of Covid-19 in developing countries. The authors find that domestic travel bans are effective only when either short or very long and that mid-length bans when policy makers can't commit to long enough ones are actually associated with a larger spread. Goldhaber et al. (2021) test to see whether the different modes of public school instruction influence spread and find that while naive estimates show that in-person and hybrid modes of instruction are correlated with higher spread, the correlation does not hold when accounting for community factors like mask wearing or political preferences. The authors note that the exception to this is that in-person schooling is associated with a larger spread in areas with high pre-existing case levels (Goldhaber et al., 2021). Kuchler, Russel, and Stroebel (2020) show that Covid-19 was more likely to spread in areas more stronger social networks and that areas with higher social network conceitedness to early hot-spots had significantly more cases by March 2020. McLaren and Wang (2020) test the effects of workers moving to a work-from-home situation instead of being in the workplace on reducing mortality. McLaren and Wang (2020) find that moving away from the workplace had no no effect on mortality until mid-May 2020 but then had increasing effects beyond that point insomuch as 10% of a county's workforce moving out of the office would lower deaths by three-quarters thirty days later.

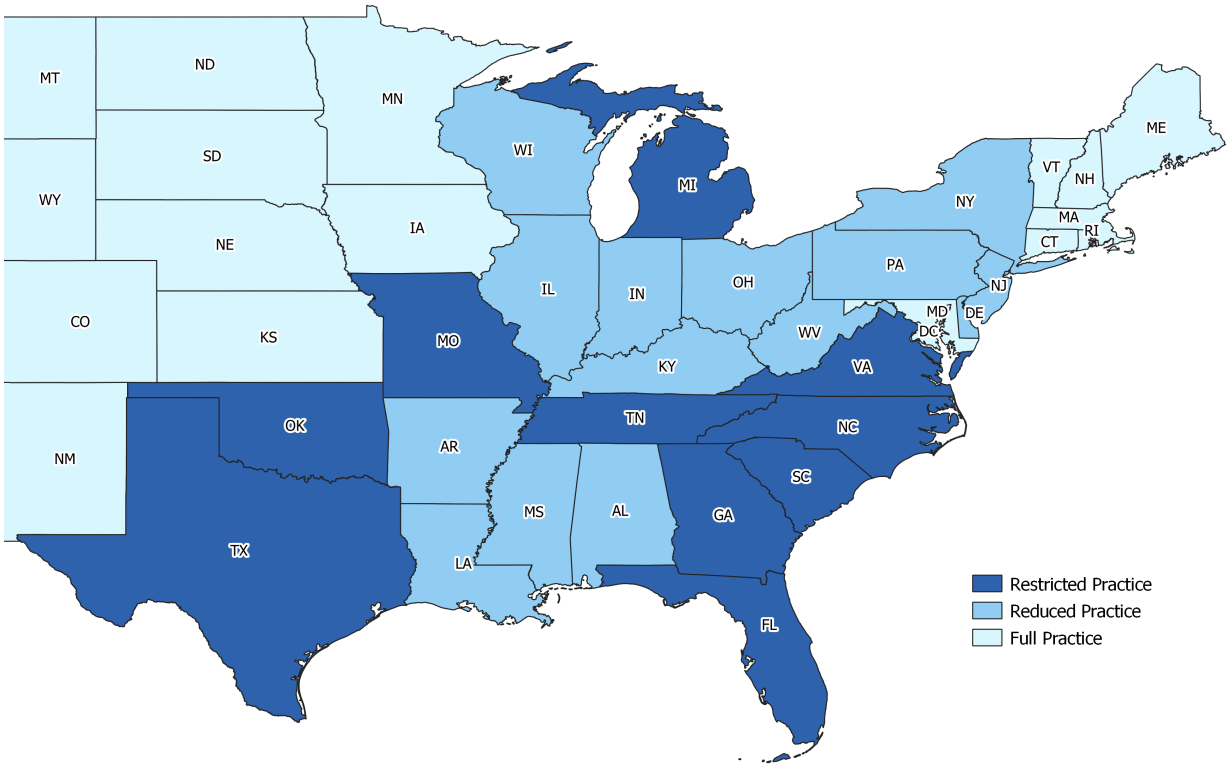
3.3 Restriction on NPs and State Emergency Response

Scope of practice for nurse practitioners varies by state. As of 2020, thirty-two states had restrictions on scope of practice. NPs in states in a "reduced practice" environment are required to have a collaborative agreement with a health professional (e.g. licensed physician), listing the types of care, treatment and procedures the NP is allowed to perform. NPs in states with "restricted practice" are required to work under direct supervision of a physician in order to provide patient care. Put simply, in states with full practice someone could see an NP for a visit at a physician's office, the NP could do the entire evaluation, diagnosis, and treatment including a prescription if necessary. In a state with reduced practice, the law reduces the ability of the NP to do at least one of those duties, and they must have a career-long collaborative agreement with another health provider such as an MD. So an individual could see a NP but a MD would be associated with them. In states with restricted practice the MD is the direct supervisor of the NP and the NP would not be able to practice without their presence. In states with restricted practice, there is a physician in the building.

According to the American Association of Nurse Practitioners (AANP), five of the Midwest states; Iowa, Minnesota, Nebraska, North Dakota, and South Dakota, allow full practice (light blue in Figure 1), meaning that NPs can work independently and are authorized for patient diagnosis and prescription. None of the states in the Southeast allow for full practice. On the West Coast (not shown in Figure 1) only California and Utah do not have full practice for NPs. California is classified as restricted practice and Utah is classified as reduced practice. The Northeast only has reduced and full practice states, no restricted practice.

Amid the pandemic, states with reduced or restricted practice authority started to expand the scope of practice for NPs since mid-March. The aim is to cope with healthcare workforce shortage (American Association of Nurse Practitioners, 2020; Spetz, 2020; Wade, 2020). Figure 2 shows states that expanded scope of practice as an emergency response. Again not shown in Figure 2 are California and Utah. California expanded scope of practice on April 14, 2020. Utah did not expand scope of practice.

Figure 3.1: State Practice Environment

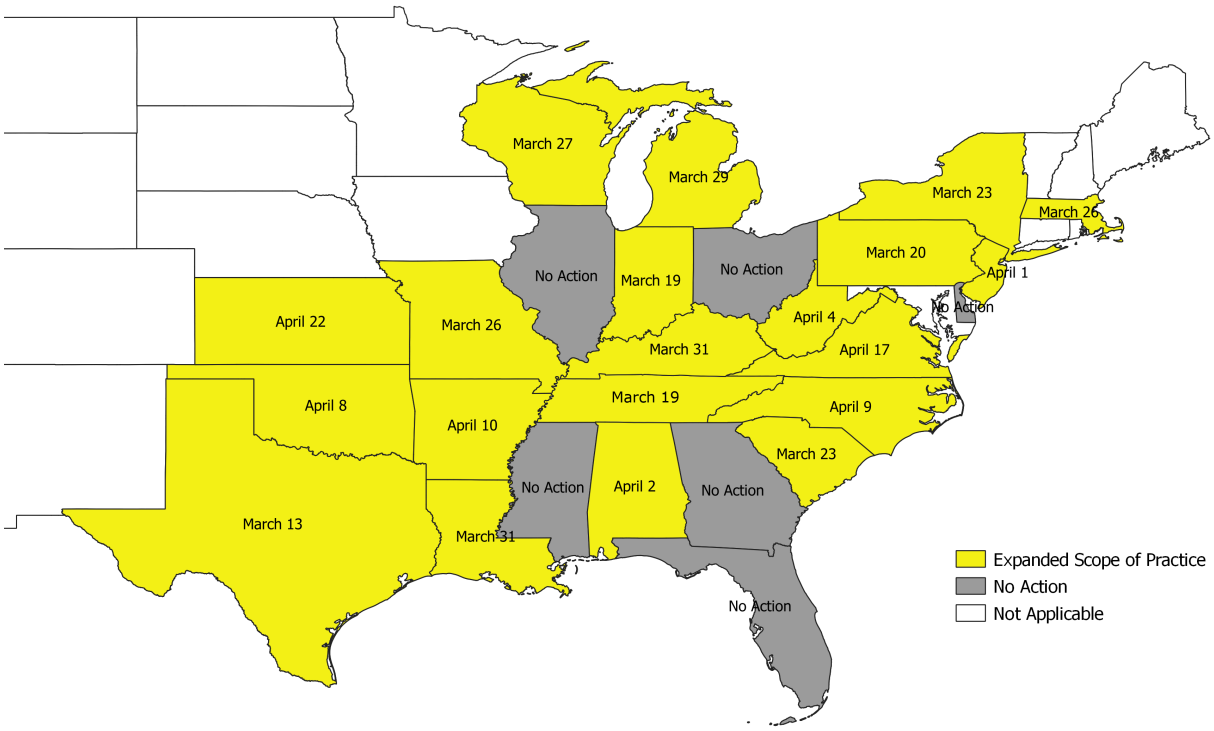


Source: American Association of Nurse Practitioners (AANP), 2020

Among the Midwest states, Indiana was the earliest to temporarily waive part of the supervision requirements on March 19th. In the South, Tennessee was the first, also on March 19th, followed by South Carolina expanding on March 23rd.

To implement a synthetic control design we have two restrictions we implement on which counties can be analyzed. First, for each treated county, a county in a state that expands scope of practice, there need to be an adequate number of untreated counties for comparison. Second, treatment needs to occur after there have been a sufficient number of pre-treatment outcomes. Thus, states that expand scope of practice before there are measurable daily deaths in their counties or in comparison counties are excluded.

Figure 3.2: Scope of Practice Expansion



Source: American Association of Nurse Practitioners (AANP), 2020

Ultimately, these two restrictions leads to treatment counties being in the Midwest and Southeast regions. Specifically, the states which contain treated counties are Alabama, Arkansas, Kansas, Kentucky, Louisiana, Michigan, North Carolina, Virginia, Wisconsin, and West Virginia.

Of the other states that expanded scope of practice, Texas, Indiana, Tennessee, Pennsylvania, New York, and South Carolina expand too early. Massachusetts is left with only donor counties in the same region from Delaware. California is left with only donor counties in the same region from Utah. Oklahoma is excluded for this reason as well. Massachusetts, California, and Oklahoma are excluded for lack of comparison counties.

3.4 Empirical Strategy

To evaluate the effectiveness of expanding scope of practice during the pandemic, we look into the impact on COVID-related mortality. Data on county level daily mortality are retrieved from New York Times (2020).[3] It is compiled from both state and local governments and health departments. It includes data down to the county level which is what we will be using. The data include both probable and confirmed deaths from Covid-19.

To estimate a cause-and-effect relationship between expanded scope of practice and mortality, this report employs the synthetic control method (Abadie, Diamond, & Hainmueller, 2010; Abadie & Gardeazabal, 2003). The essence of this statistical technique is to construct a counterfactual which mirrors the post-policy mortality that would have been observed had the policy not happened. We then obtain the daily policy effect by directly comparing the counterfactual mortality with the observed mortality.

Formally, let $y_{c,d} = y^N_{c,d} + \alpha_{c,d} SOPwaiver_{c,d}$ be the observed outcome for a treated county c in any day d. The effect of expanding SOP in each post-revision day will then be $\alpha_{c,d} = y^{Post}_{c,d} - y^N_{c,d}$. We observe $y^{Post}_{c,d}$ directly but not the counterfactual $y^N_{c,d}$. Abadie and Gardeazabal (2003) propose to estimate

$$\hat{\alpha}_{c,d} = y^{Post}_{c,d} - \sum_{j}^{J} w_j * y_{j,d} \tag{3.1}$$

where $y_{j,d}$ is the observed outcome of county j in other states in which the SOP policy remained restricted in the sampling period in day d. w_j is the weight assigned to county j to minimize the difference between a treated county and a weighted average of untreated counties. $\sum_j w_j * y_{j,d}$ is then the synthetic control to approximate the counterfactual $y^N_{c,d}$. Because the analysis involves multiple counties, we present the 'mean' treatment effect by averaging $\hat{\alpha}_{c,d}$ of each state as in Dube and Zipperer (2015).

[3]The *New York Times* hosted a database about COVID-related death and cases on GitHub. It is publicly available at http://https://github.com/nytimes/covid-19-data.

To ensure that the synthetic county is able to mirror the post-revision outcome of interest that would have been observed for a real treated county, the key rests on the choice of predictors in determining w_j. In addition to the pre-treatment outcome, we make use of several important covariates that predict COVID-related deaths. These include pre-policy daily COVID death, pre-policy daily confirmed COVID cases (also retrieved from the *New York Times* database), and county demographic characteristics (number of NPs, population size, percent of 65+ population, percent of black, number of hospital, and number of beds) obtained from the Area Health Resource Files and daily climate data on mean temperature by county from the PRISM Climate Group at Oregon State University (Health Resources & Services Administration, 2022; PRISM Climate Group, 2022).

Selecting the appropriate duration pre and post-policy period is challenging in the context of COVID. For the pre-policy period, an important property of the synthetic control technique is that the pre-policy daily COVID death has to be informative enough to produce reliable post-policy predictions. This limits the start of the sample period to late March because many counties had not yet recorded any COVID deaths until then. Most states implemented their waivers or suspension in early to mid April. However, a few states were exceptionally early movers. Therefore, we cannot produce a dependable counterfactual for the counties in Indiana, Missouri, and South Carolina even though they have adequate counterfactuals to draw from. For the post-policy period, an important property of the synthetic control method is the focus of one particular policy change at a time. The further we make predictions the more likely other policies begin to bias the results, for examples, lockdown, mask orders, and certificate of need expansions to name a few (Coyne, Duncan, & Hall, 2021; McCannon & Hall, 2021; Redford & Dills, 2021; Storr, Haeffele, Lofthouse, & Grube, 2021) To ensure the SCM predictions accurately represent the effects, we limit the predictions up to 30 days.

For statistical inference, we compare the estimated effect to a distribution of placebo effects (Abadie et al., 2010). Specifically, we perform the synthetic control method foir each county in other states, treating them as if they had gone through the same policy change. Similar to the conventional framework of permutation inference, $\hat{\alpha}_{Other_j,t}$ obtained for each placebo unit j then

belongs to the distribution under the null hypothesis. To assess if the true effect ($\hat{\alpha}_{IL,t}$) is at the extremely lower tail of the placebo distribution, for a one-tailed test at the 95% confidence level, $\hat{\alpha}_{IL_i,t}$ needs to rank below the 5^{th} percentile of the distribution.

3.5 Results

The mean treatment effect for each state can be found in Table 1. Statistical inference is calculated using the method of Dube and Zipperer (2015). Dube and Zipperer (2015) show a method for pooling multiple case studies, in this case multiple counties, using placebo tests and mean percentile rank. As percentile rank is approximately uniformly distributed it is possible to test whether the rank of the treated unit is in the tails of the distribution. It is possible that there are heterogeneous effects of expansion of scope of practice across urban and rural areas and it may be that there is a lag before the policy takes effect. For a more complete analysis we have broken the sample into rural and non-rural counties and shorter (within 10 days) and longer (beyond 10 days) time periods. For the sake of our analysis we define rural according to be a county with a population less than 50 thousand, this is similar to the definition of the Census Bureau and Office of Management and Budget (Health Resources & Services Administration, 2021).

The synthetic control results can be found in Figures 3 and 4. The solid line of each graph represents the actual daily mortality of a state (average of all counties), whereas the dotted line shows the predicted counterfactual using the synthetic control technique. The red vertical line in the middle of each graph represents the day before the policy takes place. For example, in the top-left corner of Figure 3, the solid line shows that Kansas counties recorded an increasing number of COVID-related death with a modest decline in magnitude, compared to the counterfactual, since April 22 when Kansas started to authorize temporary independent practice for NPs. In the following we analyze the effects by region beginning with the Midwest.

Table 3.1: Mean Effects and Mean Ranks

	Shorter term (within 10 days)		Longer term (beyond 10 days)	
	Mean effect	Mean rank	Mean effect	Mean rank
Alabama				
rural (N=22)	-0.015	0.457	-0.335	0.447
non-rural (N=26)	0.038	0.471	-1.596**	0.357
Arkansas				
rural (N=30)	-0.180	0.471	-0.669	0.433
non-rural (N=13)	-1.321**	0.299	-4.735***	0.201
Kansas				
rural (N=32)	-0.192	0.492	-0.183	0.505
non-rural (N=10)	-1.347	0.406	-5.542*	0.350
Kentucky				
rural (N=6)	-0.957	0.414	-2.944	0.343
non-rural (N=2)	5.223	0.927	8.691	0.568
Louisiana				
rural (N=3)	6.005	0.879	12.7	0.890
urban (N=10)	-3.456	0.784	-72.580	0.736
Michigan				
rural (N=8)	0.213	0.644	0.502	0.662
non-rural (N=21)	-0.612	0.417	-0.407	0.504
North Carolina				
rural (N=35)	0.004	0.447	0.15	0.498
non-rural (N=54)	1.109	0.563	2.108	0.608
Virginia				
rural (N=70)	0.143	0.484	0.224	0.522
urban (N=37)	2.865	0.616	6.911	0.672
Wisconsin				
rural (N=5)	-0.081	0.517	-0.146	0.513
non-rural (N=24)	-0.484***	0.379	-2.035**	0.364
West Virginia				
rural (N=11)	-0.160	0.494	-0.106	0.737
non-rural (N=11)	-0.406	0.379	-2.355	0.659

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

3.5.1 In the Midwest

For Kansas, the expansion of scope of practice has a nearly immediate effect on daily Covid mortality. The synthetic counterfactual in Figure 3 deviates from the real and the deviation grows through the end of the measured period. The results in Table 1 show that the effect seen in Table 3 are mostly due to non-rural counties in the later days of the analysis. The mean effect in both rural and non-rural counties in the shorter term while negative are statistically insignificant. The statistical insignificance continues for rural counties through the longer term. In non-rural counties through the longer term we see a definite mean effect. Through the longer term, non-rural counties in Kansas experience a 5.542 reduction in daily Covid related deaths. This effect is significant at the 10% level.

The expansion of scope of practice has a similar effect in Wisconsin as in Kansas although is slightly slower developing with the most drastic deviation of counterfactual and real occurring in the last few days of the period. Again, according to Table 1 the significant effect occurring in Wisconsin is happening in non-rural counties. Within 10 days the mean effect for non-rural counties is a reduction of just under half a death per day, significant at the %1 level. Beyond 10 days non-rural Wisconsin counties experiences a mean decrease in deaths by 2.035, significant at the 5% level.

Michigan has a more interesting effect over time. While there is immediate deviation of counterfactual and real there is also convergence at the very end of the period. This convergence appears to be due to a drastic uptick in real deaths per day in the latter third of the period. None of the mean effects in Michigan are statistically significant. So we cannot say that expanding scope of practice led to a measurable and significant decrease in deaths in Michigan.

3.5.2 In the Southeast

In the Southeast we find some similar results as the in the Midwest but also three states in which the results are counter-intuitive. The results of the Southeast Region can be seen in Figure 4. We will begin with the discussion of Alabama, Louisiana, Arkansas, and West Virginia as they all behave

Figure 3.3: Midwest States

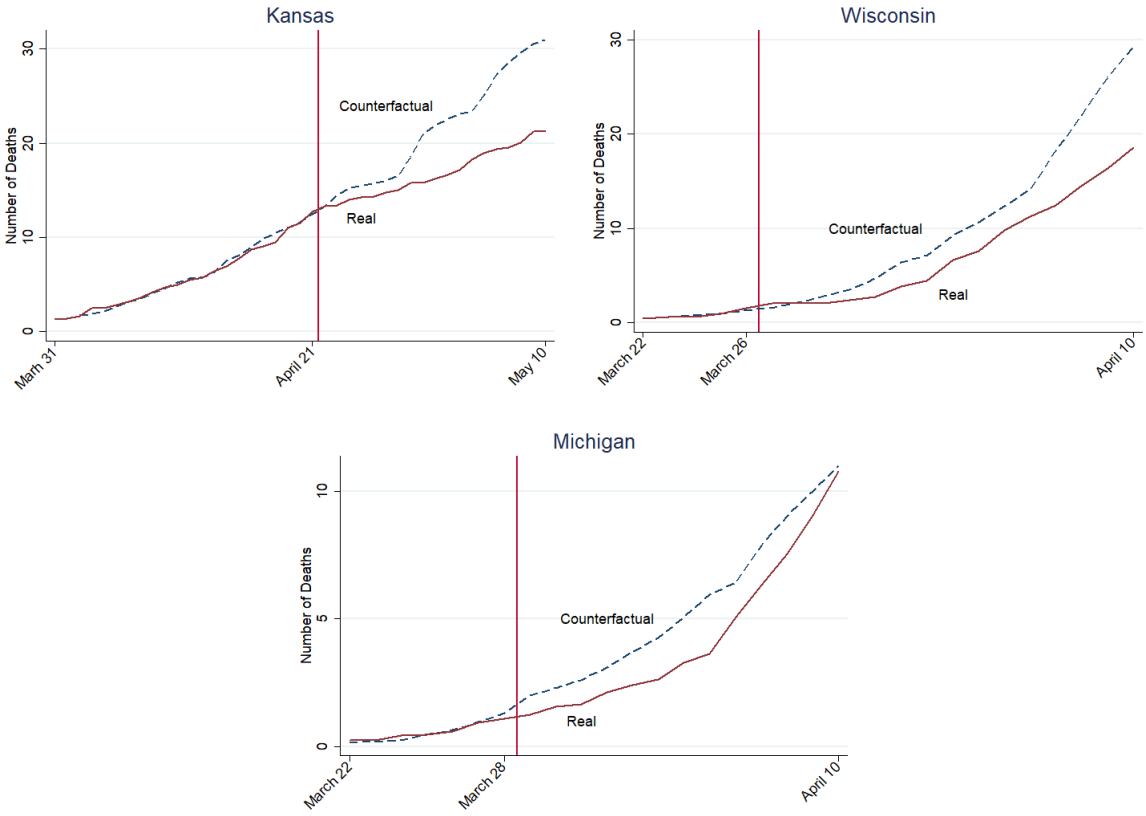
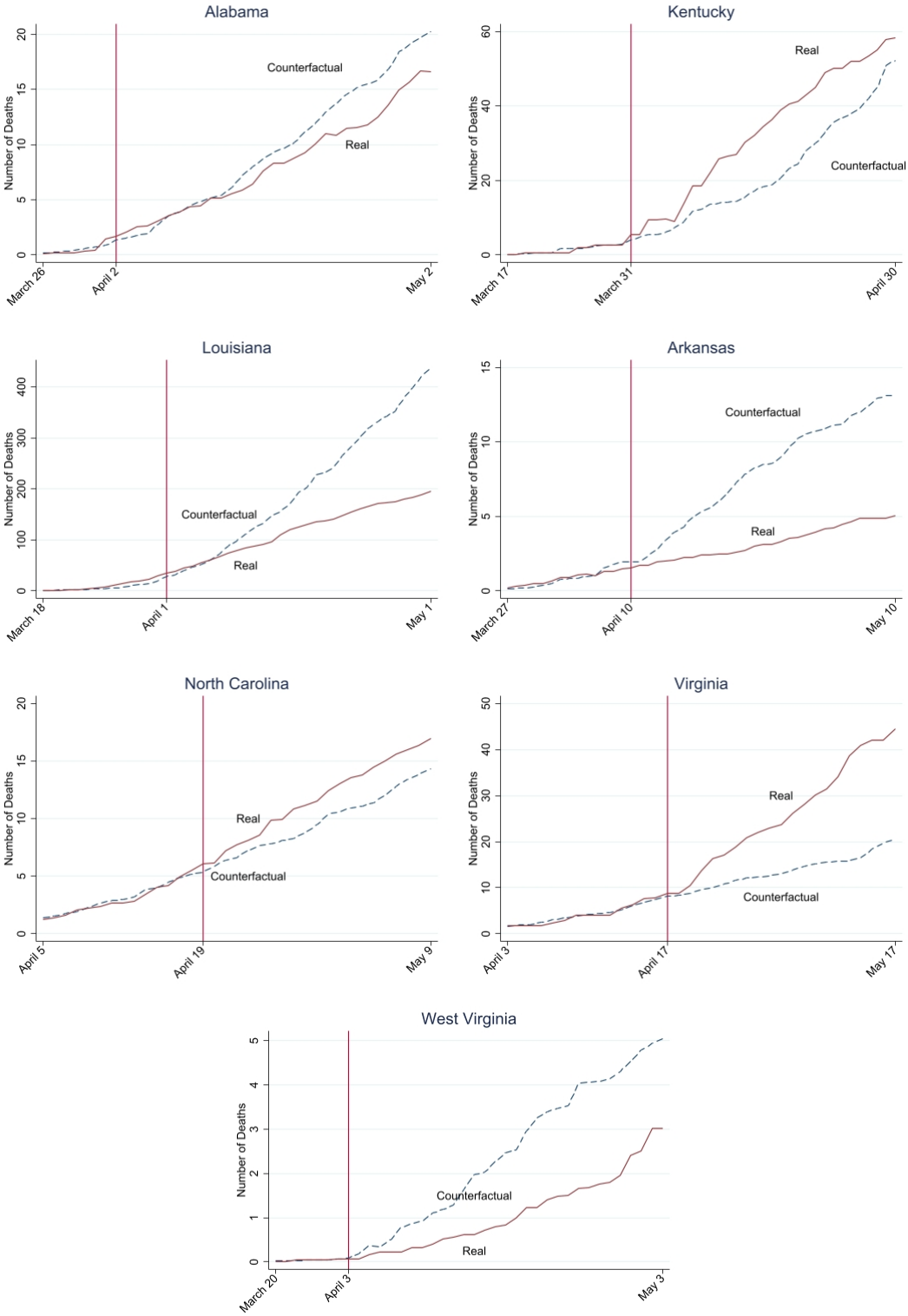


Figure 3.4: Southeast States



similar to Midwest and according to theory. Then we will discuss Kentucky, North Carolina, and Virginia.

In Alabama there is a small deviation of the counterfactual from the real that grows over the longer period, more than 10 days. From the results in Table 1 we can see the average effect in Alabama is driven by the non-rural counties and significantly in the longer term. The mean effect beyond 10 days after the expansion of scope of practice for non-rural counties in Alabama was a reduction of 1.54 deaths per day.

Louisiana has the largest effects out of all the states we analyze. In the short term there is small deviation of the counterfactual from the real and the deviation increases drastically through the entire period. Of note, there are very few counties in Louisiana for which we have observations at the start of the synthetic, only thirteen. The low number of counties observed leads to a insignificant result across all facets we analyze despite Louisiana having the largest mean effect measured, a reduction of 72.6 deaths for non-rural counties.

Arkansas has the most statistically significant effects out of each state we look at and they again appear to due to non-rural counties. In the short term non-rural Arkansas counties experienced a mean reduction in deaths per day of 1.32, significant at the 5% level. In the longer term, that mean reduction in deaths per day increased to -4.74, significant at the 1% level. This is the largest significant result we find. This mean reduction would amount to approximately 94 fewer deaths over the 20 days after the policy was implemented. Adding in the 13 fewer deaths that would have happened over the shorter term, Arkansas potentially reduced Covid related deaths by over a hundred by expanding scope of practice.

West Virginia also experiences an insignificant reduction over the entire time period. We are hesitant to discuss the results in West Virginia as there are very few if any deaths in the pre-treatment period. On April 3, 2020 when the policy was introduced to West Virginia the total number of positive cases reported in the entire state was only 237.[4] The first confirmed death

[4]see https://dhhr.wv.gov/News/2020/Pages/COVID-19-Daily-Update—4-3-2020.aspx

in the state was March 29th (West Virginia Department of Health & Human Resources, 2020a, 2020b).

Kentucky, North Carolina, and Virginia all behave counter-intuitively. For Kentucky it is possible this is because we only observe 9 counties. Kentucky also exists in the perfect geographic center of all of the counterfactual states, so while it is classified as a state in the Southeast it borders the two Midwest counterfactual states. The Kentucky result is also not robust to altering the possible counterfactual counties. For example, if treating Kentucky as a Midwest state and including donor counties from only Ohio and Illinois we find a more intuitive result with the counterfactual growing faster than the real. Thus, we feel Kentucky is best interpreted as a null result.

North Carolina and Virginia both have large growth in real deaths as opposed to the counterfactual. We believe this can possibly be explained by how late both states expanded scope of practice as an emergency measure. Compared to the other states in our analysis, which also implemented measures such as suspension of Certificates of Need laws (CON laws) and lockdown orders. While most states implemented these in a fairly local time frame, if they implemented them at all, North Carolina and Virginia had more of a roll out of policies over time. North Carolina issued suspension of CON laws on March 12, lockdown orders on March 30, and then finally removal of scope of practice restrictions on April 19. Virginia similarly suspended CON laws on March 12, issued lockdown orders on March 30, and then removed scope of practice restrictions on April 17. Thus the attempt to increase supply of healthcare workers may have been a little too late into the spread to have major affects.

Our results suggest that the increase in scope of practice was most effective in non-rural areas. This result is potentially due to the large numbers of NPs practicing in non-rural areas in states with limited scope of practice. By increasing that scope, those NPs in non-rural areas were able to take on more of the burden of the early pandemic and our results suggest that led to a prevention of hundreds of deaths compared to the synthetic counterfactual, if not more.

3.6 Concurrent Policies

There is a concern that the above analysis may mix up the effect of other concurrent policies. In other words, the reduction in death toll is indeed caused by some other policies but we may falsely attributes the improvement to the expanded scope of practice. This concern is particularly valid given there are now policy changes all around the place in response to the nationwide health risk

To check the robustness of our prediction, we test to see if the social distancing policy or lockdown policy, major attempts by states in response to the pandemic, has the same improvement on death toll. For Kansas, Wisconsin, and Michigan, social distancing measures were implemented in late March. For Alabama, Kentucky, Louisiana, North Carolina, Virginia, and West Virginia lockdown either occurred in late March or very early April. Arkansas never officially had a lockdown. We therefore implement the same estimation procedure using the synthetic control method but moving the treatment date in each state corresponding to the start date of lock down. As shown in Figures 5 and 6, in all the three states, the actual death toll continues to grow at a higher rate than the predicted counterfactual, except for North Carolina. We take this to mean that lockdown orders did not have the same effect as scope of practice expansion.

Over the same period some states chose to suspend certificate of need (CON) laws. The states that chose to do so and also expanded scope of practice were Alabama, North Carolina, Virginia, and Michigan. The difference in time periods in which states chose to implement both policies varies from 2 days apart, in Alabama, and 39 days apart in North Carolina and Virginia. Additionally, some of the states that chose to implement reduction in scope of practice choose to not suspend certificate of need laws and some of the states that choose to not expand scope of practice did suspend certificate of need laws. Thus, to compare CON laws suspension would be an entirely different analysis worthy of its own paper.

Figure 3.5: Lockdowns in the Midwest

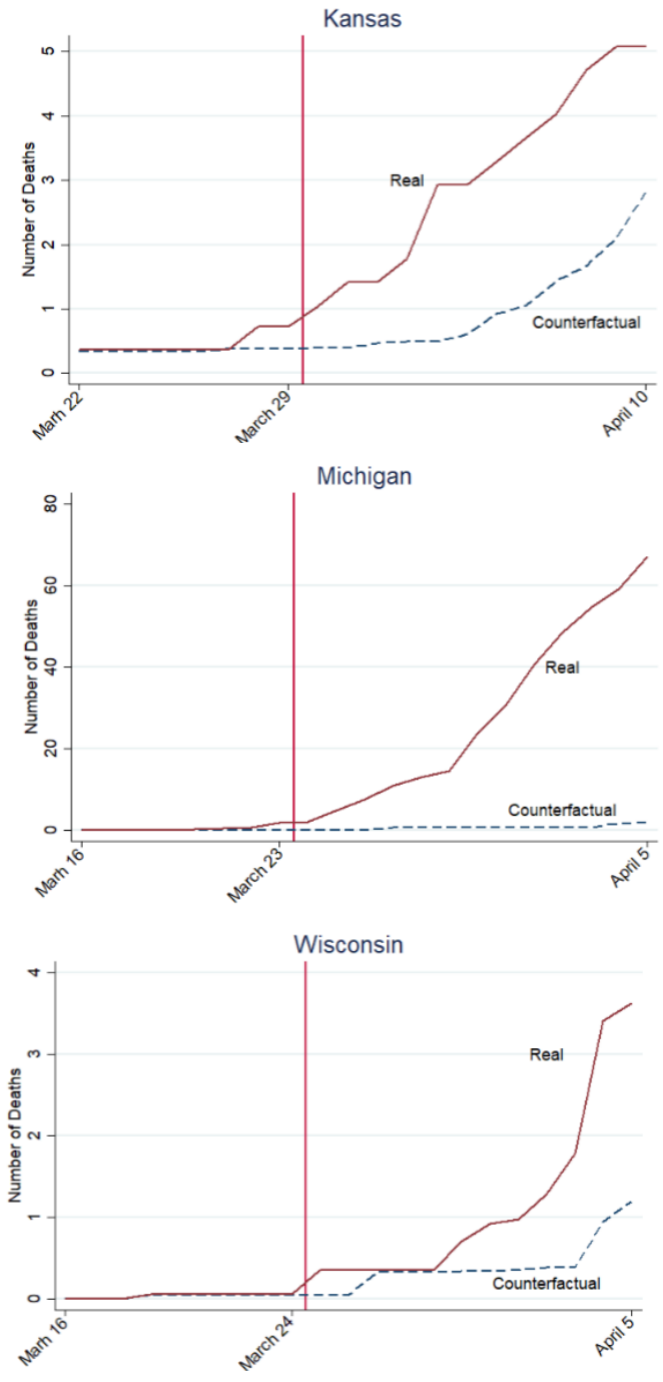
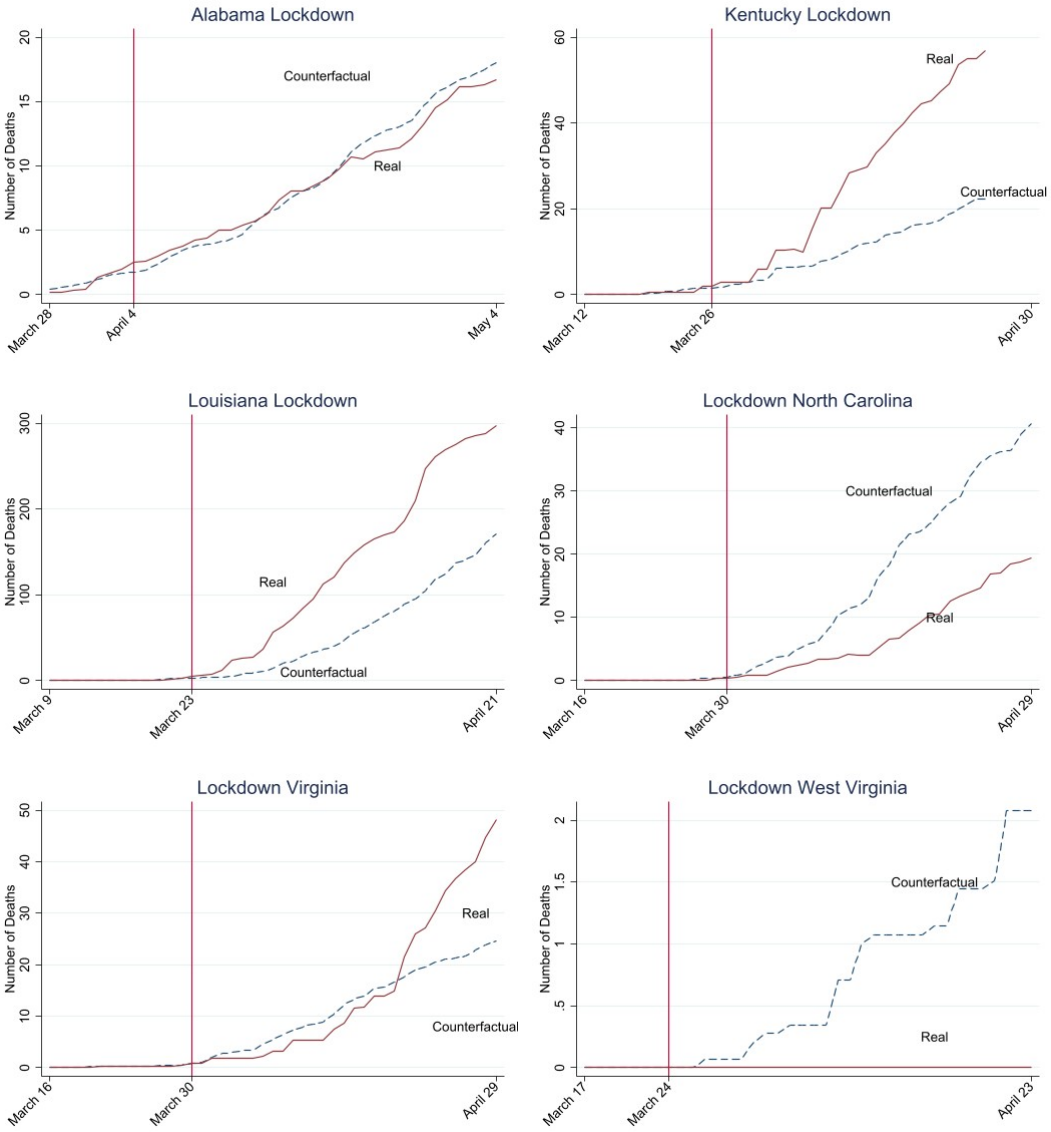


Figure 3.6: Lockdowns in the Southeast



Conclusion and Policy Implication

Amid the unprecedented crisis, many regulators have realized the cost of stringent worker regulation. It is surprising that a handful of states remain silent despite of the apparent fact on health workforce shortage.

Using county level data on Covid-19 deaths we construct synthetic controls for states that implemented emergency scope of practice expansions for nurse practitioners. The analysis we have done here shows that allowing NPs to have a broader scope of practice is one way states were able to mitigate COVID-related deaths. In both the Midwest and the Southeast the expansion of scope of practice for nurse practitioners was associated with a reduction in daily COVID deaths. This reduction appears to most prominent in non-rural areas and the largest effects took place between 10 and 30 days after the expansion of scope of practice.

The very argument of regulating workers in the form of occupational licensing is consumer protection. In the case of reducing scope of practice for NPs, regulators often worry about the quality of service offered by NPs. This report provides empirical evidence that granting NPs independent authority does more good than harm. The case studies we have here, can be good references for states refusing to grant NPs full scope of practise to start taking a similar initiative to fully utilize our already available talents in the health sector.

As we move away from the early months of the covid-19 pandemic some states are choosing to allow permanent increases in the scope of practice for NPs. We suggest future research for this topic would be to examine how the choice of maintaining a broader scope of practice for NPs affects access to care in states that choose to keep the expansion permanently.

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3

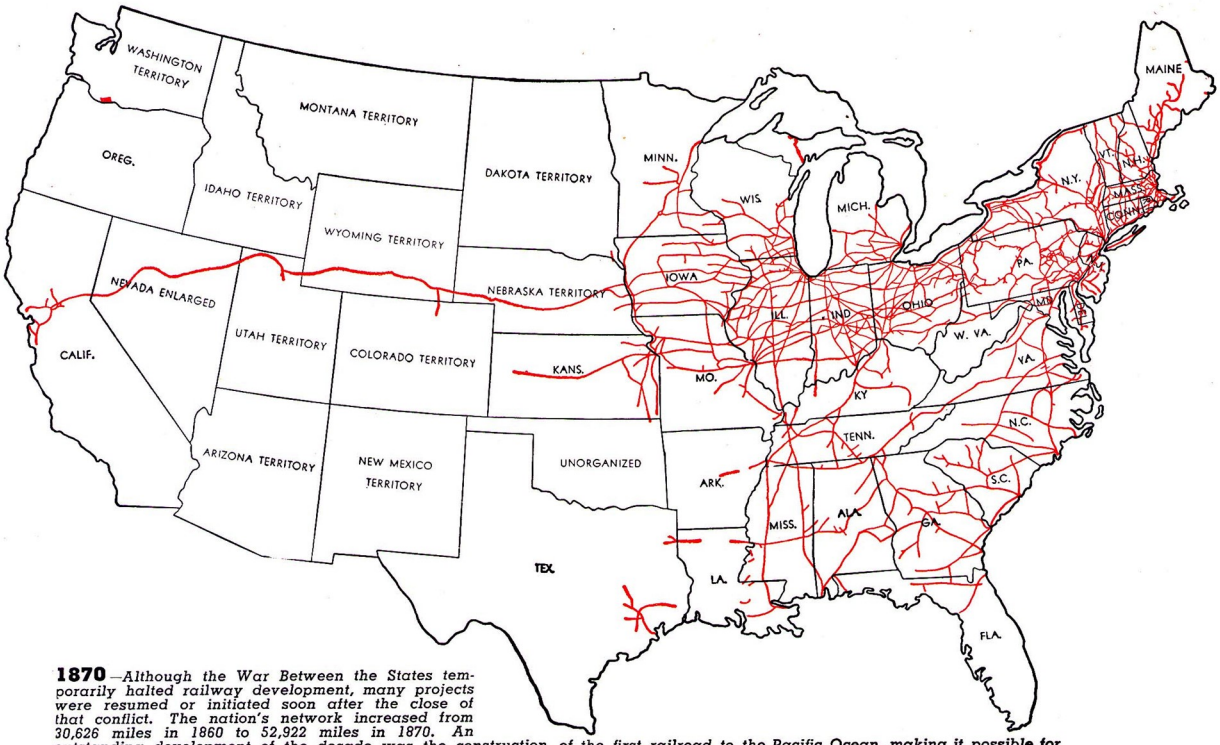
G Georeferencing Appendix for Chapter 1

Georeferencing is the technique of linking a map or image to a geographic coordinate system. This can be done via GIS software. By using a photo editing software and GIS one can extract elements of a map and place them into a GIS program for analysis. Tutorials for this using the open source photo editing software GIMP and open source QGIS are available at:

$$https://docs.qgis.org/2.18/en/docs/training_manual/forestry/stands_digitazing.html$$

For this analysis, maps from the Association of American Railroads publication American Railroads, Their Growth and Development (1948) were used.

1870 Railroad Map

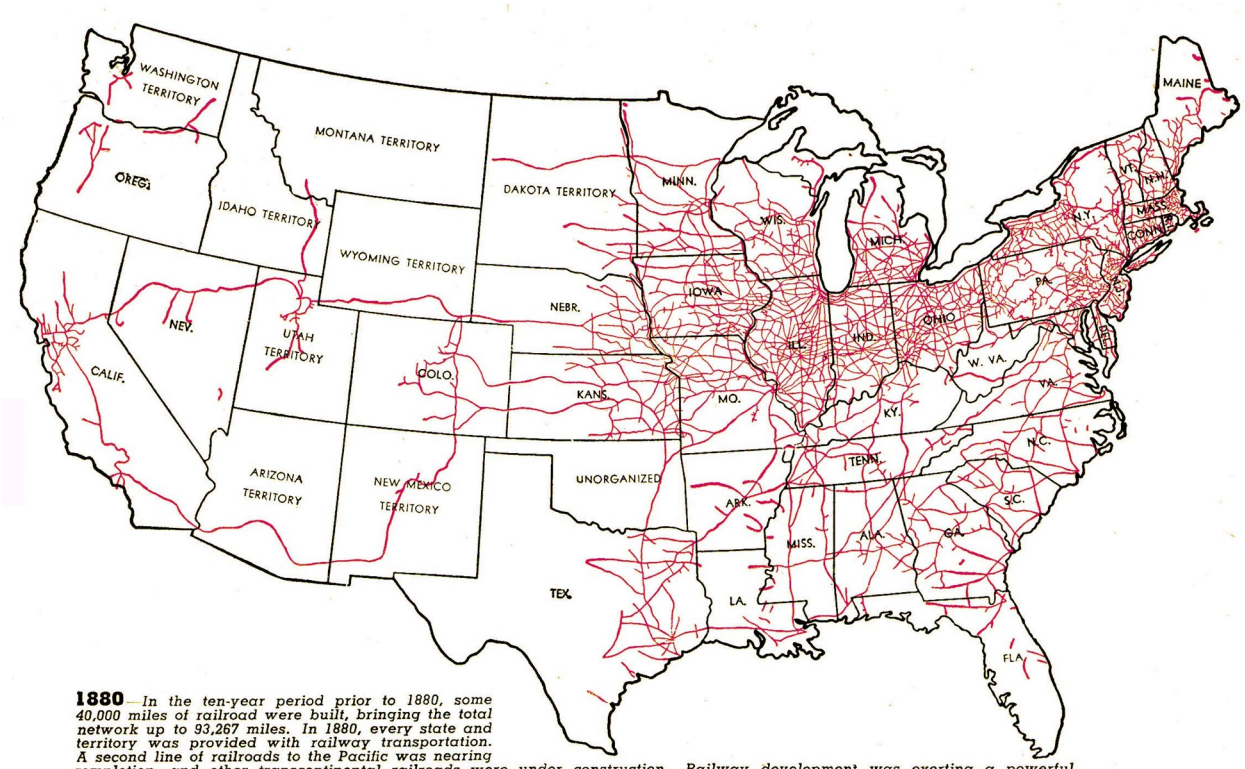


1870—*Although the War Between the States temporarily halted railway development, many projects were resumed or initiated soon after the close of that conflict. The nation's network increased from 30,626 miles in 1860 to 52,922 miles in 1870. An outstanding development of the decade was the construction of the first railroad to the Pacific Ocean, making it possible for the first time to travel all the way across the country by rail. Railway development in the Mississippi and Missouri valleys was especially notable during this period.*

The railroads were extracted from the image and georeferenced onto the U.S. Census Bureau's state and county shapefiles. The railroads were then converted to a GIS vector and the distance from each county's central position was calculated. See the 1870 example below.

Railroad distances (in miles) for the 1870 and 1880 years are available from the author upon request.

1880 Railroad Map



1880—*In the ten-year period prior to 1880, some 40,000 miles of railroad were built, bringing the total network up to 93,267 miles. In 1880, every state and territory was provided with railway transportation. A second line of railroads to the Pacific was nearing completion, and other transcontinental railroads were under construction. Railway development was exerting a powerful influence upon immigration and agricultural and industrial growth throughout the country.*

1870 Map Georeferenced