An Empirical Assessment of Occupational Licensing Reforms in the Healthcare Sector

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INTRODUCTION

Occupational licensing regulations are becoming increasingly common throughout the United States, and between 25 and 30 percent of jobs require workers to have a license. Despite their prevalence in the economy, however, evidence from the economics literature suggests that these regulations influence wages, prices, and employment in ways that impose significant costs on society without providing corresponding improvements in safety or quality for consumers. Consequently, several states and the federal government have reformed occupational licensing regulations across the country. This dissertation studies how occupational licensing reforms in the healthcare sector have affected labor market for nurses and health outcomes for consumers.

Chapter one analyzes the effect of the Nurse Licensure Compact ("Compact") on labor market, geographic, and educational outcomes for nurses. The Compact automatically provides registered nurses, licensed practical nurses, and licensed vocational nurses with a multistate license to work in any Compact state. Between 2000 and 2015, twenty-five States entered the Nurse Licensure Compact. Using a rich individual-level dataset spanning 1992 to 2018, I find that the Compact decreased employment and wages for nurses, and increased geographic mobility and entry into higher education programs.

Chapter two studies the effect of the Nurse Licensure Compact on labor market outcomes for military-spouse nurses. Individuals married to military personnel experience materially worse labor-market outcomes than their similarly situated peers married to civilians. Accordingly, the Compact may uniquely benefit this group of nurses. Using data from the American Community Survey, I find that the NLC improves employment outcomes for military-spouse nurses.

Finally, chapter three investigates the effect of independent practice for advanced practice registered nurses ("APRNs") on patient health outcomes during the COVID-19 pandemic.

During March and April, 2020, fourteen states eliminated or reduced scope-of-practice restrictions for APRNs. I estimate the effect of these changes on state- and county level COVID-19 testing, case, and fatality rates, as well as non-COVID-19 fatality and all-cause fatality rates. I find consistent evidence that broadening APRN scope of practice reduced non-COVID-19 fatalities by approximately three percent between January 26 and September 26, 2020. I also find some evidence that greater APRN independence reduced COVID-19 fatalities and case rates.

Chapter 1: Examining Multistate Licensure and Labor Market Outcomes under the Nurse Licensure Compact

I. Introduction

Occupational licensing requirements are becoming increasingly common throughout the United States, and between 25 and 30 percent of jobs require workers to have a license (Kleiner and Krueger 2013). An occupation license is a form of government regulation, often imposed at the state level, which requires an individual to hold a license to perform a particular occupation. In this chapter, I study the effect of a recent reform to occupational licenses for registered nurses, licensed practical nurses, and licensed vocational nurses called the Nurse Licensure Compact (Evans 2015). Understanding the effect of the Nurse Licensure Compact has important implications both for the literature as well as policymakers designing reforms for other healthcare occupations (Evans 2015; Steinbrook 2014).

Licensing regulations are a growing subject of academic focus. Evidence from economics studies suggests that these regulations influence wages, prices, and employment in ways that impose significant costs on society without providing corresponding improvements in safety or quality for consumers (Kleiner 2015). Similarly, empirical research in the legal literature shows that state-sanctioned licensing boards are "foxes guarding the henhouse"—that is, licensing boards are controlled by members of the licensed occupation and inflate wages or prices to the detriment the consumers (Allensworth 2017). But some evidence shows that licensing has distinct benefits for minority groups in the form of higher wages or better representation in the labor force (Law and Marks 2009; Blair and Chung 2018) and Akerlof (1970) theorizes that licensing protects consumers by reducing uncertainty in the market for professional services. On balance, however, the adverse effects of licensing have caused many academics to advocate for reform, particularly given the restrictions that licensing poses on labor mobility between states (Sanderson 2014; Kleiner 2015; Johnson and Kleiner 2017; Nunn 2019). Similarly, the White House recently noted that while licensing improves service quality, it can impede worker mobility, reduce employment opportunities for excluded workers, and harm consumers (U.S. Department of the Treasury Office of Economic Policy, Council of Economic Advisers, and the Department of Labor 2015).

It was with these anticompetitive interests in mind that the National Council of State Boards of Nursing ("NCSBN") lobbied for the creation of the Nurse Licensure Compact in the year 2000, which thirty-four states have adopted since its inception (Evans 2015). The Nurse Licensure Compact ("NLC" or "Compact") is an interstate agreement that automatically endows individuals in Compact states with a multistate license that grants them the privilege to work as a nurse in any other Compact state. In this way, the Nurse Licensure Compact theoretically relaxes the restrictions on labor mobility for which single-state licensure is often criticized (Federal Trade Commission, 2018). In this chapter, I exploit the staggered adoption of the NLC to study how this reform has affected labor-market, migration, and human-capital outcomes for registered nurses.

Despite nursing being the second largest licensed occupation in the country (Bureau of Labor Statistics, 2019), the Nurse Licensure Compact has received relatively little attention in the literature. One exception is DePasquale and Stange (2016), which finds that the NLC had little effect on labor market outcomes—such as likelihood of employment, wages, or commuting to a different state for work—or geographic mobility between states. In contrast, Ghani (2019) finds that the NLC boosted cross-state, job-related mobility. The tension between these two studies does not create a clear path forward for policymakers seeking to reform occupational licensing in other professions. To that end, this chapter seeks to provide additional evidence of the effect of the NLC on labor market and geographic outcomes for nurses in order to better assess the efficacy of the Compact. The results provided in this chapter may also guide policymakers seeking to establish other multistate licensure agreements, such as the NSCBN's efforts to create the Advanced Practice Registered Nurse Compact ("APRNC"). The APRNC would function similarly to the Nurse Licensure Compact by allowing an advanced practice registered nurse to hold one multistate license with the privilege to practice in other APRN Compact states (National Council of State Boards of Nursing 2020).

In this chapter, I provide evidence the Nurse Licensure Compact has adverse impacts on labor market outcomes for registered nurses in the form of lower probability of employment, wages, and longer periods of unemployment. I also find some evidence that the Compact improves mobility between states. Finally, I show that the Nurse Licensure Compact increases the likelihood that an individual is enrolled in a higher education program, and that this effect is driven by enrollment in master's programs or higher. To my knowledge, exploring the effect of licensure on human capital outcomes is sparsely studied in the literature, and a distinct contribution of this chapter.

I derive my results using data from the National Sample Survey of Registered Nurses ("NSSRN"), a nationally representative dataset that collects demographic, labor market, and geographic information from tens of thousands of nurses every four years. I describe this dataset in Part V, where I also describe my empirical methodology. Part VI provides my results and Part VII discusses several robustness checks of these results. Finally, Part VIII discusses possible mechanisms driving my findings. In this Part, I theorize that multistate licensure under the Nurse Licensure Compact relaxes barriers to entry from out of state competition. As a result, wages and employment decrease while weeks unemployed increase. In addition, increased competition may explain why individuals choose to enter master's programs or higher. Masters-level degrees are needed for nurses to work as advanced practice registered nurses ("APRNS"). APRNS are licensed separately from registered nurses and are therefore in a separate labor market not subject to increased competition under the NLC (Evans 2013). Thus, as employment outcomes in registered nursing worsen, transitioning to work as an APRN may become more attractive.

II. Background on Nurse Licensure and the Nurse Licensure Compact

This Part first details the several theoretical justifications for occupational licensing in Section A before providing background information on the regulation of the nursing occupation in Section B. This Part concludes by providing a history of the Nurse Licensure Compact in Section C.

a. Justifications of Licensure in the Healthcare Sector

Occupational licensing regulations have been justified on multiple grounds, and many of these justifications support the licensing of nurses. One prevalent justification for nurse licensure is the information asymmetry between consumers of professional services and providers of professional services (Leland 1979). Under this reasoning, licensing is necessary since consumers lack the time or expertise to distinguish between high-quality professionals and low-quality "quacks." Similarly, Arrow (1963) noted that the uncertainty created by information asymmetries in the healthcare sector justified licensure requirements for healthcare professionals. In particular, licensing requirements reduce the uncertainty consumers may feel over the prospects of their medical treatment. Finally, consumers may be risk averse, and licensing requirements may reduce their fear that they will be dissatisfied with their healthcare provider, making them more likely to receive healthcare (Larkin 2018).

There are several other justifications for licensing applicable to the healthcare sector. One is paternalism: society's collective knowledge is greater than any one individual, and so society should set professional standards for professions such as nursing (Larkin 2018). Licensing may also encourage service-providers to invest in human capital because they will not fear being confused with "quacks" or other less qualified rivals. Finally, the exclusion of "quacks" or "charlatans" from the profession enhances its appearance of respectability, which may lead to the entry of higher-quality members into the trade over time.

To be sure, there are numerous criticisms of licensing regimes. Among the most notable criticisms is licensing's rent-seeking effects in which licensing requirements enable incumbent professionals to earn supra-competitive profits that result from limiting entry into the profession. Stigler (1971) wrote that the thrust of occupational licensing is to decrease competition, bar entry into the licensed profession, and attach legal consequences to what would otherwise be private economic actions. Another criticism levied against licensing boards is that licensed professionals protect their own members to the detriment of the consumers that they serve. Because licensing boards are staffed by members of the profession holding a license issued by the board itself, they sometimes fail to discipline members of their profession who harm consumers (Allensworth 2020). Occupational licensing has also historically been used to bar minorities and women from entering the licensed profession (Allensworth 2017).

The rationale and criticisms of licensure are applicable to registered nurses. As would be the case with other healthcare professions, asymmetric information between consumers and nurses is one justification for licensing nurses. About sixty percent of nurses work in hospitals, and, absent a licensing regime, patients at these hospitals lack the time or expertise to distinguish between trained nurses and untrained ones (2018 National Sample Survey of Registered Nurses).

To be sure, however, hospitals have strong incentives to hire qualified nurses, and because human resources departments would have the expertise to distinguish between high-quality and low-quality nurses, there is little cause to worry that asymmetric information would lead to hospitals hiring unqualified nurses even absent a licensing regime. But because registered nurses also provide home care and work in other non-hospital settings, there are many settings in which asymmetric information between patients and nurses justifies licensing requirements. Plus, if consumers are risk averse or highly uncertain about undertaking surgery or other risky medical procedures, the knowledge that their nurse passed state licensing requirements may offer them peace of mind. Finally, some of the other justifications for licensing are also applicable to nurses. For instance, licensing regulations ensure that nurses who seek additional education or training will not be confused with "quacks" in the labor market. In addition, licensure imparts respectability.

Yet it may also be the case that nurse licensure has evolved less to protect consumers and instead to earn supra-competitive rents for licensed members. Stigler's critiques of licensing in general could apply to nursing boards. Moreover, licensing boards sometimes protect bad actors within the profession rather than the public. For example, the Minnesota Board of Nursing often permitted nurses that admitted to misconduct several times to keep their license (Stahl, 2013). Thus, even though there are theoretical justifications for nurse licensure, licensing requirements may still have anticompetitive effects in the labor market for nurses that will be explored in this chapter.

b. History of Nurse Licensure

Nurse licensure in the United States began in the nineteenth century. In the 1800s, nursing students learned their trade in a variety of educational settings that ranged from 6-week to 3-year programs located in hospitals, schools, or through correspondence schools (Benefiel 2011). It was this inconsistency in education that lead to the advent of regulation in the nursing industry during the early 1900s. North Carolina was the first state to pass a bill allowing nurses to register voluntarily with the state in 1903. From there, more states began to adopt voluntary registration programs in which individuals could register as a nurse after completing approved school curriculum and passing a board examination. Once registered, an individual was granted a permissive license and could use the title "registered nurse." Those without a nursing license could not use this title, but could still practice as nurses.

In 1938, New York became the first state to pass mandatory nurse licensure legislation, although this was not enacted until 1947 due to shortages of nurses during World War II (Benefiel 2011). The New York licensing regulations defined two types of nursing (registered and practical), provided the first definition of scope of nursing practice, and made it against the law to practice nursing without a license.

Today, registered nurses are licensed at the state level (Russell 2017), and until the NLC, were required to receive a new license in each state in which they sought employment. The process to acquire a license does not vary substantially across states, regardless of NLC status. Each state requires prospective nurses to pass the National Council Licensure Examination ("NCLEX") before receiving a license. The NCLEX exam is a uniform, standardized exam and passage of the NCLEX exam is a prerequisite to licensure in each state. Many states also require nurses to pass background checks and pay fees before receiving a license.

c. History of the Nurse Licensure Compact

The single-state approach to nurse licensure dominated the country through most of the twentieth century. Criticism of this approach grew in the 1990s (Finocchio et. al. 1995) and, in part to facilitate telemedicine, the National Council of State Nursing Boards began exploring multistate licensure for registered nurses (Evans 2013). In 1999, the Nurse Licensure Compact was released, and in 2000 the first states joined the NLC. In effect, The Nurse Licensure Compact ("NLC" or "Compact") is an interstate agreement that synchronizes licensing requirements for registered nurses between member states, which in practice makes it easier for registered nurses, licensed vocational nurses, and licensed practical nurses to work and move between NLC states. The Nurse Licensure Compact does not meaningfully change state-level licensing requirements. Instead, the Compact automatically grants nurses in member states a multistate license that permits them to work in other Compact states without acquiring a new license for that state. In this way, the NLC facilitates cross-state commuting or migration between Compact states for nurses with active, multistate licenses. The NLC began in 2000, and 25 states joined the NLC by 2015.[1]

Absent the Nurse Licensure Compact, licensing restrictions across states may inhibit labor mobility. If a licensed nurse wants to work in or move to a new state, she must apply for licensure by endorsement in the new state. This process often requires the prospective nurse to pay fees, submit background checks, and complete paperwork in order to receive a new license. For instance, in California—a non-Compact state—applicants must pay $49 for a fingerprint card and a $350 application fee—plus a $100 fee if the applicant applies for a temporary license—along with completing a lengthy application packet, sending fingerprint scans, filing a

[1] The NLC was replaced by the Enhanced Nurse Licensure Compact ("e-NLC") on January 19, 2018 with 29 member states. The e-NLC is substantively similar to the NLC, but did increase some background check requirements for multistate licensure. The primary dataset that I use for Chapter 1 of my dissertation includes data from the National Sample Survey of Registered Nurses that was conducted in early 2018. However, the 2018 NSSRN asked respondents to record their responses as of December 31, 2017, so the adoption of the e-NLC does not meaningfully affect my analysis.

request for school transcripts form, and verifying active licensure in another state (California Board of Nursing 2020). The California Nursing Board takes between ten and twelve weeks to process each application.

There are several reasons why states may adopt the Nurse Licensure Compact. DePasquale and Stange (2016) theorize that states could be joining the Compact in response to growing demand for nurses or due to declining supply. By entering the Compact, member states gain access to a pool of registered nurses from all other states that belong to the NLC. Alternatively, some states may enter the Compact to improve patient care. Detailing the history of Utah's entry into the NLC, Poe (1999) explains that Utah joined the Compact to (1) make it easier for nurses who worked with out-of-state patients in Utah hospitals to continue providing care to those patients when they returned to their home states; (2) bolster the nascent telemedicine industry in Utah by reducing the number of licenses that Utah nurses needed to hold in order to practice telemedicine in other states; and (3) ease administrative burdens for Utah's licensing board so that it does not have to re-evaluate the credentials of registered nurses in good standing in other states. Similarly, when Montana joined the Nurse Licensure Compact in 2015, representatives from the state's Board of Nursing stated that joining the NLC promoted public safety and would help nurses give cross-border care in a highly rural part of the country (Gustafson 2015).

On the other hand, states that decline to enter the NLC purportedly do so out of concerns for patient welfare. For instance, the Washington State Nurses Association ("WSNA") cited concerns over patient safety as a driving factor behind not joining the Compact (Huntington 2016). Namely, the WSNA worried that nurses licensed in other states would not adhere to Washington's safety standards if they were permitted to practice in that state under a multistate license. The WSNA also feared that it would seed sovereignty to an interstate commission of Compact administrators that were not accountable to any state or government. California and Rhode Island have similar concerns over patient safety (Gorman 2018).

In addition, some states oppose entry into the Compact because of lobbying efforts by nursing unions in their state. Rhode Island declined to join the enhanced Compact at least in part because a prominent state nursing union argued that the Compact would permit out-of-state nurses to take jobs from Rhode Island's nurses (Bogdan, 2017). Similarly, a Massachusetts union has publicly opposed the Compact, which likely explains Massachusetts' refusal to enter the NLC (Massachusetts Nurses Association 2017), and the public employee nurse's union in California helped defeat a bill that would have granted California's entry into the Compact (Grimes 2020).

Regardless of the reason for adoption or non-adoption of the Compact, from a theoretical standpoint the Nurse Licensure Compact should ease labor-market frictions that arise from licensing in several ways. First, nurses that live in states that belong to the NLC benefit from a significantly easier process when they seek work in a different Compact state (DePasquale and Stange 2016). The NLC streamlines this process in several ways. First, the NLC permits nurses that live in one Compact state to work in a different Compact state without relicensing. Second, the NLC gives nurses a grace period when moving to a new Compact state. Whenever any nurse permanently relocates to another state, she must apply for a license in that state. However, under the NLC, nurses have a 30-90 day grace period before they must acquire the new license after moving. Nurses that relocate to non-Compact states do not benefit from this grace period and must obtain a license in their new state before they can begin working there. Third, Compact nurses only need to apply for licensure by endorsement when they permanently relocate—moves that are temporary, perhaps because the nurse is a travel nurse or is married to military personnel—do not require licensure by endorsement. These nurses can work immediately upon arriving in their new state. Thus, the NLC eases frictions associated with moving between Compact states, but does not offer benefits when nurses move to non-Compact states.

III. Related Literature

Approximately one in four workers in the United States is licensed, which is a greater portion of workers subject to minimum wage or that belong to unions (Kleiner 2000). Compared to its prevalence in the economy, however, occupational licensing has received relatively little attention in the literature, and occupational licensing reforms such as the one examined in this paper are almost entirely unstudied.

The growing occupational licensing literature largely examines the effect of occupational regulations on labor market outcomes, prices, and consumer welfare. This literature has a long history. Adam Smith discussed how tradesmen's guilds lengthened apprenticeship programs and limited the number of apprentices per master to decrease the supply of skilled labor and increase wages in his *Wealth of Nations* (Smith 1776). Milton Friedman and Simon Kuznets discussed the restrictive nature of occupational licensing regulations in a work published by the National Bureau of Economic Research (Friedman and Kuznetz 1945).

This Chapter, and much of the licensing literature generally, addresses the effect of occupational licensing regulations on labor market outcomes, such as wages or labor supply. Two competing theories of licensing are relevant to explain the effect that licensing may have on these outcomes. First, under a barrier-to-entry theory, licensing laws restrict labor supply and decrease competition, which theoretically increases wages (Stigler 1971). Applying this theory to the Nurse Licensure Compact, states that enter the Compact make it easier for out-of-state nurses to work in their state. This effect reduces entry barriers and may accordingly lower wages due to an increase in the nursing labor force. A second theory of licensing is derived from the signaling literature, and posits that the market for professional services is an adverse selection problem (Leland 1979). Under this theory, licensing is most valuable when consumers cannot differentiate between low-quality or high-quality professionals. Licensure solves this problem by setting minimum quality standards that serve as a signal of quality. Because the substantive requirements for nurse licensing do not meaningfully differ across states, the signaling theory of licensing does not provide any clear implications for the Nurse Licensure Compact. Even so, multistate licenses may send a noisier signal of quality than single state licenses, which may adversely affect nurses in Compact states.

When assessing the effect of licensing on labor-market outcomes, the empirical literature supports both theoretical explanations of the effect of licensing outlined above. For example, Kleiner and Krueger (2010) use data from a household telephone survey and find that workers that have an occupational license receive approximately fifteen percent higher wages. Similarly, Kleiner and Krueger (2013) shows that licensing is associated with an eighteen percent increase in wages. Gittlemen, Klee, and Kleiner (2018) use the Survey of Income and Program Participation to find that a government-issued, mandatory license raises hourly wages by approximately 7.5 percent. In addition, there is some evidence that licensing serves a signaling function as well. Blair and Chung (2018) find that licensure functions as a signaling device that reduces the wage gap for licensed minorities and women relative to their unlicensed peers.

If licensure acts as a barrier to entry, one mechanism through which licensure may restrict labor supply is by limiting labor mobility. Johnson and Kleiner (2020) examine interstate migration for 22 licensed occupations and find that occupational licensing restrictions reduce movement between states. Similarly, Bloomfield, Bruggemann, Christensen, and Leuz (2015) show that the harmonization of accounting standards across European Union countries increased cross-country mobility of accountants in the EU. Turning toward the impact of licensing on migration from other countries, Peterson, Pandya, and Leblang (2014) show that between 1973 and 2010, states with greater physician control over licensure requirements imposed more stringent requirements for immigrant-physician licensure, and, as a result, received fewer immigrant-physicians. Federman, Harrington, and Krynski (2006) find that states with stricter English-language requirements for licensure as a manicurist had fewer Vietnamese immigrants working as manicurists. Finally, looking at the effect of scope-of-practice restrictions for advanced practice nurses, Markowitz and Adams (2020) use the National Sample Survey of Registered Nurses data to show that the level of scope-of-practice restrictions are not strong determinants of labor market outcomes, such as wages, employment, or migration.

The empirical literature addressing licensing in the healthcare sector also finds that licensure requirements increase wages—often without evidence that licensure improves patient outcomes. Kugler and Sauer (2005) show high returns to acquiring a license among immigrant physicians in Israel. Kleiner and Park (2010) find that occupational licensing increases wages for dental hygienists. There is also little evidence that more stringent licensure requirements in healthcare occupations benefit consumers. Kleiner and Kudrle (2000) find that more restrictive licensing requirements for dentists do not improve dental outcomes for new Air Force recruits. Similarly, Kleiner et. al. (2016) find that expanding nurse practitioner's scope of practice—that is, giving them more independence—decreases physician wages without worsening healthcare outcomes, as measured by infant mortality rates or malpractice insurance prices. Finally, Markowitz et. al. (2017) find that barriers to practice for nurse midwives neither improves nor harms infant or maternal health outcomes.

Although registered nurses are the second largest licensed occupation, they have received relatively little attention in the licensing literature. Law and Marks (2017) estimate the effect of state adoption of licensure requirements for registered and practical nurses and find that licensure raised wages by between five and ten percent without reducing overall labor-force participation. DePasquale and Stange (2016) also study the effects of relaxing occupational licensing regulations for nurses using evidence from the Nurse Licensure Compact. Their data come from the 1990 and 2000 Census and the 2001-2012 American Community Survey, and DePasquale and Stange find that the NLC had no effect on any of the labor market outcomes tested in their paper, such as wages, hours worked, interstate commuting, or interstate mobility. Conversely, Ghani (2019) use the Census Bureau's job-to-job flows and find that adoption of the Nurse Licensure Compact boosted job-related migration between states.

The tension between DePasquale and Stange and Ghani creates an opening to further explore the effect of the Nurse Licensure Compact on labor market outcomes and mobility. The NSSRN is particularly well-suited to study the effect of the NLC on interstate mobility since this dataset includes geographic information regarding which state and county a nurse resided in for both the sample year and the previous year. I can therefore test interstate mobility between years at the individual level. In addition, the NSSRN includes information on the duration of unemployment and whether the individual belongs to an education program. Both of these outcomes are unexplored in the literature, and are a distinct contribution provided by this chapter.

IV. Conceptual Model

I extend an existing model from DePasquale and Stange (2016) to create a static model of an individual's joint labor force participation and migration decisions given the constraints imposed by occupational licensure requirements. I also expand this model to consider dynamic effects of the NLC on migration, labor market outcomes, and human capital attainment.

I begin with the theoretical effect of the NLC on labor force participation and migration. Under the NLC, individuals that reside in one Compact state can more easily work in a different Compact state. Accordingly, the relevant labor market is characterized by three geographic areas: the individual's current home state (H), a workplace in a nearby state that the individual can commute to without moving (N), and a labor market in another distant state that would require moving there to work (D). The wage offered by each of these three markets for any period is given by {$W_{i,H}$; $W_{i,N}$; $W_{i,D}$}. Workers also receive a random utility draw from living in their home state or the alternative distant state. The random utility draw is affected by family choices, such as a spouse receiving a job offer in a different state. Denote this random draw as {$e_{i,H}$, and $e_{i,D}$}.

Moving to a distant state imposes moving costs of D_D, and commuting to a nearby state imposes costs of D_N. Absent the NLC Agreement, moving or commuting also imposes licensure costs of L since the individual will have to spend time qualifying for a new license and pay fees to the state licensing board. Individuals currently working in their home state have already paid licensing costs associated with working in that state, and will not bear licensure costs of L. Because the NLC permits nurses to work across state lines or obtain licensure in a Compact state quite easily, L $\rightarrow$ 0 in Compact states. I hypothesize that the NLC will increase migration, have an ambiguous effect on wages and similar outcomes, and increase the likelihood that an individual joins a higher education program.

Migration

The NLC should increase migration. Workers will move when $\max\{W_{i,D} - D_D - L, 0\} + e_{i,D} > \max\{W_{i,H}, W_{i,N} - D_N - L, 0\} + e_{i,H}$. Thus, moving can be desirable if the wage in the distant state, $W_{i,D}$, or if the random utility draw, $e_{i,D}$, is sufficiently high. The NLC eliminates L, which should increase cross-state migration. But $L \rightarrow 0$ also makes it more attractive to commute—given by $W_{i,N} - D_N - L$—suggesting that workers will not need to move to work in a different nearby state. Thus, the effect of the NLC on cross-state migration could be lessened by the fact that the NLC makes commuting to neighboring states easier.

Labor Market Outcomes

The effect of the Nurse Licensure Compact on employment, wages, and hours is theoretically ambiguous. Consider the first stage effects of the NLC on employment. Here, workers that choose to remain in their home state will enter the labor market if $\max\{W_{i,H}, W_{i,N} - D_N - L\} > 0$ while workers that move to a distant state will work if $W_{i,D} - L > 0$. Relaxing licensing requirements—$L \rightarrow 0$—increases the likelihood that workers will either commute to a neighboring state or work after moving to a distant state. Thus, the NLC should increase employment. Furthermore, since individuals have access to a broader labor market under the NLC, they can also select jobs with higher wages or hours. Thus, under a "broader market" theory of the NLC, workers are more likely to find employment or accept jobs with higher wages or better hours.

But increased labor mobility may have secondary effects that the static model does not fully account for. As $L \rightarrow 0$, the effects of licensing as a barrier to entry disappear (Stigler 1971).

Accordingly, individuals in Compact states may face competition from out-of-state workers, which may lead to decreased employment, higher unemployment, lower wages, and lower hours. Call this effect the "competition" effect. Because it is unclear ex ante whether the "broader market" effect or the "competition" effect dominates, the effect of the NLC on labor market outcomes is theoretically ambiguous.

Human Capital Attainment

Reducing licensure requirements may cause individuals to seek additional education if the reduction causes a net decrease in wages. In the context of the Nurse Licensure Compact, registered nurses that experience a reduction in wages may seek additional education (i.e. a master's degree or higher) to become an advanced practice registered nurse ("APRN"), which is licensed occupation that is not subject to the NLC. Since education is costly, an individual utility maximizer will continue working as a nurse under her current education level, E_0, when doing so exceeds the utility from acquiring more education, E_1, to work as an APRN. For simplicity, let the utility from work represent the net present value of all future earnings, and let the present value of earnings depend on education (E) and licensing requirements (L) since previous research shows that licensing can act as a barrier to entry that affects wages (Kleiner and Krueger 2013; Stigler 1971). Formally, let $U\ (Work) = \sum \frac{W}{(1+r)^t} = \sum \frac{L+E}{(1+r)^t}$. Individuals will work at education level E_0 rather than E_1 when $U\left(Work_{E_0}\right) > U\left(Work_{E_1}\right)$, or $\sum \frac{L+E_0}{(1+r)^t} > \sum \frac{L+E_1}{(1+r)^t}$. As $L \rightarrow 0$ under the NLC, U $\left(Work_{E_0}\right) \rightarrow \sum \frac{E_0}{(1+r)^t}$. Thus, the utility from working as a nurse at E_0 becomes less attractive relative to acquiring education E_1 to work as a nurse practitioner. It follows that the NLC should therefore cause more individuals to enter higher education programs.

V. Data and Methodology

In this Part, I first describe the NSSRN dataset before outlining my empirical methodology, which consists of exploiting the staggered adoption of the NLC to estimate difference-in-difference and triple difference models.

a. Data Description

The National Council of State Boards of Nursing ("NCSBN") provides adoption dates for when each state joined the Nurse Licensure Compact. The NCSBN is a not-for-profit organization whose membership consists of nursing boards from each state, the District of Columbia, and four U.S. territories. I verify the information from the NCSBN using Westlaw. Table 1 lists NLC adoption dates for each state. In addition, Figure 1 illustrates the staggered adoption of the NLC over time. Eight states—Utah, Iowa, Arkansas, Texas, North Carolina, Maryland, Wisconsin, and Delaware—adopted the NLC by July 1, 2000. A total of eighteen states joined the NLC by January 1, 2005. Finally, by October 1, 2015, twenty-five states had adopted the NLC.

I use data from the National Sample Survey of Registered Nurses ("NSSRN") to test the effect of the NLC on labor-market, migration, and human-capital outcomes for nurses. The NSSRN is a nationally representative survey of nurses with active licenses. The NSSRN began in 1977 and sampled nurses every four years between 1980 and 2008 and again in 2018. The NSSRN includes information on an individual's experience, income, work and educational attainment before acquiring a nursing credential, race, gender, age, marital status, employment status, and geographic location. State or county of residence, along with the state in which the nurse works, is available in the survey year and in the previous year. The NSSRN also includes whether the individual is currently enrolled in a higher education program. I restrict the sample to 1992 to 2018 since information on whether the individual is an advanced practice registered nurse, and therefore not eligible for the NLC, is only available from 1992 on. APRNs are nurse practitioners, nurse midwives, nurse anesthesiologists, and clinical nurse specialists.

I use the NSSRN data to construct the outcome and control variables used in this chapter. The outcomes of interest in this chapter can be classified into three categories: labor market outcomes, geographic outcomes, and educational outcomes. The labor market outcomes I study are (1) whether an individual is employed; (2) annual wages, (3) weeks unemployed, (4) weekly hours, (5) whether the individual works in a different state than he or she lives in, and (6) whether the individual works in a different Compact state. I also use the two years of state-level geographic information per survey wave to examine whether the NLC affected two geographic outcomes: (1) whether the individual moved to any different state and (2) whether the individual moved to a Compact state. Finally, I use information on whether an individual is enrolled in a higher education program to construct three education outcomes: (1) whether an individual is enrolled in any education program, (2) whether an individual is enrolled in a bachelor's degree program, and (3) whether an individual is enrolled in a master's program or higher. Because work as an advanced practice registered nurse requires a master's degree or higher, examining if the NLC affects whether an individual is enrolled in a master's program is one way to proxy for whether the NLC affects an individual's decision to pursue other licensed healthcare occupations.

I also use the NSSRN to construct control variables. I create two classes of education variables. The first is a set of binary variables for each individual's non-nursing degree—i.e. no degree, associate degree, bachelor's degree, or a master's degree or higher. The second set of education variables are binary variables for the type of nursing degree an individual has—i.e. an associate degree, bachelor's degree, master's degree or higher, or other. I also construct binary variables for an individual's race and gender. To capture the effect of household characteristics, I construct binary variables for whether there are children under 6 in the home, under 18 in the home, and whether the individual is married. I also create a continuous variable for years worked in nursing and two binary variables for whether the individual is employed as a licensed practical nurse or had any experience as a nurse's aide or similar occupations prior to becoming a nurse. Finally, the NSSRN also includes information on whether an individual is a registered nurse or an APRN, which I use to construct binary variables for each occupation category.

I report summary statistics for my baseline sample in Table 2. In particular, I report summary statistics for the NSSRN sample for (1) a partial sample of only nurses and (2) the full sample of nurses and APRNs, which forms the sample for my triple difference models. Statistics are shown separately by whether the individual lives in a Compact state. Annual wages for nurses are approximately $41,870, measured in 2000 dollars. Nurses in Compact states earn more than those in non-Compact states and work more hours each week. Nurses in Compact states are also slightly more likely to be male, black, or married. Turning to the full sample of nurses and APRNs shows the following. First, this sample includes 161,203 observations and the average annual wages are approximately $46,580. Nurses and APRNs in Compact states earn more and work more hours than their counterparts in non-Compact states. They are also slightly older, more likely to be black, and more experienced than non-Compact nurses and APRNs.

b. Empirical Specification

In this Section I begin by describing my difference-in-difference methodology. However, because these estimates may be biased due to state-specific trends in the nursing labor market, I also detail a triple-difference methodology designed to correct for this source of bias.

i. Difference-in-Difference Methodology

My baseline empirical model is a difference-in-difference regression equation of the following form:

(Equation 1) $$Y_{ist} = B_0 + B_1 Treatment_{st} + B_2 X_{ist} + \zeta_s + \gamma_t + \epsilon_{ist}$$

where Y_{ist} represents my outcomes of interest for individual *i* in state *s* in year *t*. X_{ist} represents a vector of worker-specific control variables such as age, marital status, gender, race, and educational attainment. State-specific fixed effects are represented by ζ_s, and γ_t represents time fixed effects. B_1 is my coefficient of interest.

In addition, there may be a lag between exposure to treatment under the Nurse Licensure Compact and the decision to move to a new state or enroll in an education program. For this reason, I define treatment in the models estimating the effect of the NLC on geographic and education outcomes as whether the individual was treated by the Nurse Licensure Compact in the prior year. In other words, treatment is measured at time *t-1* while outcomes are measured at time *t*. I define treatment this way in each of the "geographic" and "education" models estimated in this chapter.

ii. Triple Difference Methodology

The baseline difference-in-difference specification assumes that the parallel trend assumption is satisfied; that is, that the outcomes of interest would trend in the same direction in the treatment and control states in the absence of the Nurse Licensure Compact (Angrist and Pishke, 2009). However, this assumption could be violated if some states adopted the NLC in response to growing demand for nurses or due to declining supply (DePasquale and Stange 2016). To overcome this source of bias, I also estimate triple difference models in addition to the baseline difference-in-difference models outlined in the prior section. The NSSRN dataset includes information regarding whether the individual surveyed is an advanced practice registered nurse, such as a nurse practitioner. Advanced practice nurses are not affected by the NLC, and so can serve as a control group for the triple difference models. I estimate a model of the following form:

(Equation 2)

$$Y_{ist} = B_0 + B_1 Treatment_{st} + \mathrm{B}_2 Nurse_{ist}$$
$$+ \; B_3 \, Treatment * \; Nurse_{ist} + B_3 X_{ist} + \zeta_s + \gamma_t + \gamma_t * t + \epsilon_{ist}$$

X_{ist} represents a vector of worker-specific control variables such as age, marital status, gender, race, and educational attainment. State-specific fixed effects are represented by ζ_s, and γ_t represents time fixed effects. In addition, I include state-specific linear time trends, $\gamma_t * t$. The coefficient of interest is B_3, which captures the differential impact of the NLC on nurses.

VI. Results

This Part presents my empirical estimates of the effect of the Nurse Licensure Compact on labor market, geographic, and educational outcomes for nurses. I first report results from the baseline difference-in-difference model before providing results for the triple-difference models. In general, both models show that the Nurse Licensure Compact adversely affected labor market outcomes for nurses; a conclusion that is particularly demonstrated in the triple-difference models. I also find some evidence that the Nurse Licensure Compact increased cross-state mobility in Compact states as well as the likelihood that individuals were enrolled in an education program, particularly a master's program or higher. I next create a continuous treatment variable that captures the "compounding" benefits of the Nurse Licensure Compact and find similar results as in the triple-difference models. Finally, I divide my sample into white and non-white groups to test for heterogenous effects of the Nurse Licensure Compact by an individual's race. Unlike other literature in this area (Blair and Chung, 2018), I do not find that the NLC had heterogenous effects by race.

a. Baseline Model

Table 3 reports results from the baseline difference-in-difference model. Each model includes state fixed effects, year fixed effects, and a full set of demographic controls, such as age, gender, race, marital status, education, and prior experience. I find that the Nurse Licensure Compact decreased whether an individual was employed by 1.2 percent, which is statistically significant at the ten-percent level. In line with this result, I also find that the NLC modestly decreased weeks unemployed, annual wages, hours worked, and whether the individual worked in a different state. However, none of these results are statistically significant at conventional levels. I also find that the NLC significantly increased whether an individual works in or moved to a Compact states by 2 and 3.3 percent, respectively. Finally, Table 3 reports the effect of the NLC on the education outcomes-of-interest. I find that the NLC has a positive effect on whether an individual is enrolled in any education program or in a bachelor's program, but neither result is statistically significant at conventional levels. I also find a statistically significant and positive effect of the NLC on whether an individual is enrolled in a master's program or higher.

The baseline difference-in-difference models could be biased if the nursing occupation is subject to policies or labor-market shocks that coincide with adoption of the Compact. For example, states may join the Compact due to declining supply of nurses or increased demand for health services, which have been on the rise for decades. If this is the case, the underlying presumption of a difference-in-difference model that non-Compact states can serve as a control for Compact states could be violated. To address this source of bias, in the next section I estimate triple-difference models that include an additional control group, advanced practice registered nurses, that are unaffected by NLC adoption.

b. Triple Difference Models

Table 4 reports results from the triple-difference models. The coefficient of interest is the interaction term between Nurse and NLC, and can be interpreted relative to the control group of APRNs. The triple difference point estimates indicate that the Nurse Licensure Compact adversely affected several labor-market outcomes. First, the Nurse Licensure Compact decreased the likelihood of employment by 1.8 percent (relative to 86.7 percent employment), increased weeks unemployed by 0.047 weeks (or 7.9 hours relative to mean of 18.5 hours unemployed), and decreased wages by 9.8 percent (relative to mean wages of $41,200 in year 2000 dollars). This decrease in wages is similar in magnitude to the seven to fifteen percent increase in wages associated with occupation licensing requirements found in other studies (Kleiner and Krueger 2010; Kleiner and Krueger 2013; Gittlemen, Klee, and Kleiner 2018). I also find modest effects of the NLC on weekly hours, whether the individual worked in a different state, and whether the individual commuted to a different NLC state for work. However, none of these results were statistically significant at conventional levels.

The triple-difference estimates indicate that the geographic outcomes measured in this chapter were unaffected by the NLC. Table 4 shows that the NLC did not have a statistically significant effect on whether the individual moved states or moved to a new Compact state. However, I find strong statistically significant effects of the effect on the NLC on education outcomes: the NLC increased whether an individual was enrolled in any education program by 1.7 percent and whether an individual was enrolled in a master’s program or higher by 1.5

percent. Though I find a positive effect of the NLC on whether an individual was enrolled in a bachelor's program, this effect is not statistically significant.

c. Continuous Treatment Model

In this Section, I model the compounding nature of the Nurse Licensure Compact. Individuals working within the Compact may benefit from being able to work in more places as more states enter the Compact. For example, those living in Utah benefit more from the Compact in 2004 when seventeen states belonged to the Compact than in 2000 when only eight states had joined. I follow Depasquale and Stange (2016) and create a "continuous treatment" variable which captures the fraction of other states that are part of the Compact weighted by the share of workers that move into that state from all other Compact states. Formally,

$$Continous\ Treatment_{st} = Compact_{st} * \sum_{k=1}^{k} Compact_{kt} * Weight_{ks}$$

I use the continuous treatment variable to re-estimate my triple difference model. Estimates from this model are reported in Table 5. The coefficient signs for Nurse * NLC generally align with those in the baseline difference-in-difference and the triple-difference models, though, as in DePasquale and Stange (2016), the magnitudes are two to three times as large. Notably, the signs for Nurse * Continuous Treatment are both statistically significant and negative in the "Employed" and "Log(Wages)" models. Finally, I find similar results for the geographic outcomes as well as the education outcomes as in prior models: the NLC has a positive and statistically significant effect on whether an individual enrolled in Any Education Program or a Masters/Ph.D. program.

d. Heterogenous Effects by Race

Blair and Chung (2018) find that licensure functions as a signaling device that reduces the wage gap for licensed minorities and women relative to their unlicensed peers. Given this finding, the value of a multistate license may have heterogenous effects according to an individual's race. To understand whether this is the case for licenses issued under the Nurse Licensure Compact, I divide my sample into white and non-white individuals before re-estimating each model on these two sample groups. I report the effect of the Nurse Licensure Compact on each outcome for the white and non-white samples in Tables 6 and 7.

As with previous models, the coefficient of interest is that for NLC * Nurse. The only model in which this coefficient is statistically significant in both Table 6 and Table 7 is that for whether an individual is enrolled in a master's program or higher. At first blush, this indicates that non-white individuals are more likely to enroll in master's or Ph.D. programs than white individuals. However, a t-test shows that the two coefficients are not statistically different from one another. Accordingly, I hesitate to draw any strong conclusions about heterogenous effects of the NLC based on race.

VII. Robustness Checks

This Part presents several robustness checks for the primary results outlined above. First, because state-specific time trends may be biasing any results I identify above, I incorporate state-by-year fixed effects into the baseline triple-difference model and report the corresponding results in Table 8. Next, to provide evidence that the parallel trends assumption is satisfied I conduct event studies for each of my outcome variables in Section VII.B. Finally, Section VII.B also reports event studies for the triple-difference models.

a. Inclusion of State by Year Fixed Effects

In this Section I re-estimate my models using state-by-year fixed effects. These results are reported in Table 8. I find that my primary results are robust to the inclusion of state-by-year fixed effects.

Looking first at labor-market outcomes, Table 8 aligns with prior results showing that the Nurse Licensure Compact adversely affects many labor-market outcomes for nurses. In particular, the NLC decreased whether the individual was employed by 1.7 percent, increased the number of weeks unemployed by 0.046 weeks (7.72 hours), and decreased wages by just over 10 percent. Thus, including state-by-year fixed effects did little to affect the estimates as the magnitudes of the Nurse * NLC coefficients are approximately the same in Table 4 and 8.

Table 8 also lists results for my geographic outcomes of interest. Again, I find that the Nurse Licensure Compact had no effect on whether an individual moved states or moved Compact states. The effect sizes are approximately the same as Table 4. Finally, Table 8 reports results for education outcomes, and I again find that these results are robust to inclusion of state-by-time fixed effects. The NLC increased whether an individual entered an education program by approximately 1.9 percent, and increased whether an individual entered a masters or Ph.D. program by 1.8 percent. I also find that the NLC had no statistically significant effect on whether an individual entered a bachelor's program. The magnitude of these results closely matched the magnitude of the coefficients estimated in Table 4 that did not include state-by-year fixed effects.

b. Event Study Analysis

i. Difference-in-Difference Models

Difference-in-difference models rely on the parallel trend assumption, which requires that any relevant trends between treatment and control states would slope in a similar direction absent treatment (Angrist and Pischke 2009). The parallel trends assumption is unverifiable, but evidence for whether this assumption is satisfied can be provided by conducting event studies.

To that end, I conduct event studies for each outcome variable in Figures 2 through 12. Because the data from the 2018 survey is so far removed from the next closest survey wave in 2008, I limit the event study to states that joined the Nurse Licensure Compact at any point between 1992 and 2008.[2] I also follow Markowitz and Adams (2020) and define event time in terms of survey waves (rather than years) before and after a law change. For example, for states that joined the Nurse Licensure Compact in the year 2000, data from survey years 1992 and 1996 are coded as -2 and -1 while data from 2004 and 2008 are coded as +1 and +2.

I conduct event studies for each of my outcomes of interest. I include a full set of control variables and state and year fixed effects. These results are reported in Figures 2 through 12. The reference period is -1, or one survey wave prior to treatment. Each event study shows that the coefficient for each pre-treatment period is statistically insignificant from zero. This result provides some evidence that the parallel trends assumption is satisfied, though, again, this assumption cannot be proven with certainty.

Figures 7 and 9 are worth highlighting, as both figures indicate that the NLC improved cross-state mobility for nurses. Figure 7 shows that the Nurse Licensure Compact increased the probability that an individual worked in a different Compact state by 0.6% in the first year of adoption, 1.4% in the first wave, and 2.2% in the second wave after adoption. Similarly, Figure 9 shows that the NLC resulted in a 1.9% and 2.5% percent increase in whether an individual moved to an NLC state during the first and second waves after adoption.

[2] I conduct event studies using data from the 2018 survey wave and find no meaningful changes to my results.

ii. Triple-Difference Models

I also estimate event studies for the triple-difference models. To implement this approach, I interact each event-wave dummy variable with a binary variable for whether an individual is employed as a nurse. Except for this change, I conduct the "triple difference" event studies in the same way as the "difference-in-difference" event studies in the prior section. Notably, I limit the "triple-difference" event study to states that joined the Compact between 1992 and 2008.

I plot the event studies in Figures 13 through 23. With one exception, each event study shows that each pre-treatment periods are statistically indistinguishable from zero, which indicates that the baseline triple difference findings in this paper are not driven by "pre-treatment" trends. The only pre-period statistically distinguishable from zero is in the event study for the "Weeks Unemployed" outcome, which shows that the third pre-treatment period is positive and statistically significant. However, because each period represents the four years between each survey wave, this period is twelve years prior to Compact adoption, and therefore poses little threat to identification. Of note, Figure 15 shows that wages decrease after the NLC is adopted, and that this decrease is statistically significant in the first and second post-adoption waves. In addition, Figure 16 shows that the Compact caused a statistically significant reduction in weekly hours worked in the initial post-treatment period.

VIII. Discussion

The results developed in the previous Parts indicate that the Nurse Licensure Compact has had a largely adverse impact on labor-market outcomes for nurses. Across each model, I find that the NLC reduced the likelihood that an individual was employed by between 1.2 percent and 1.8 percent. In addition, I find strong evidence that the Nurse Licensure Compact decreased wages—by between nine and eleven percent in the baseline models—and some evidence that the

Compact increased the number of weeks unemployed. Overall, then, these results paint a consistent story that multistate licensure has adversely affected registered nurses. In addition, the size of the decrease in wages aligns with other results in the literature finding that licensure increases wages by between seven and fifteen percent (Kleiner and Krueger, 2010; Kleiner and Krueger, 2013; Gittlemen, Klee, and Kleiner, 2018).

One mechanism through which licensing is theorized to restrict entry is through creating geographic barriers to entry of labor from other states (Johnson and Kleiner, 2017). I find some indications that the Nurse Licensure Compact makes it easier for individuals to work in other Compact states, suggesting that the NLC reduces barriers-to-entry inhibiting cross-state mobility for nurses. Though the triple-difference models report null effects of the NLC on geographic outcomes, the baseline difference-in-difference models do show that the NLC had a statistically significant effect on whether an individual worked in a different NLC state or moved NLC states, and the event studies conducted in Figures 7 and 9 corroborate this finding. Figure 7 shows that the NLC is associated with between a 0.6 and 2.2 percent increase in whether an individual worked in a different state, and Figure 9 shows that the NLC caused approximately a 1.9% to 2.5% increase in whether an individual moved to an NLC state. One possible conclusion from these results is that the NLC increased competition in the labor market for nurses within Compact states, which would explain why likelihood of employment and wages suffer as a result of Compact adoption. More intuitively, as labor supply from other states increases, more workers seek out roughly the same number of jobs, and accordingly the probability that any individual is employed decreases. Similarly, the increase in labor supply decreases wages (Stigler 1971).

The adverse labor-market effects that result from NLC adoption may also explain the results pertaining to the effect of the Compact on educational decisions. Across each model, I find that the Nurse Licensure Compact increased whether an individual belonged to any educational program. In addition, I find that this effect is only statistically significant for individuals enrolled in masters or Ph.D. level programs but not for individuals enrolled in bachelor's programs. One explanation for these results is that individuals are leaving the nursing profession to change careers. Advanced practice registered nurses, which require advanced degrees but are separately licensed than registered nurses, are not subject to the Nurse Licensure Compact and may therefore be an attractive career transition for registered nurses adversely affected by increased out-of-state competition.

IX. Conclusion

The Nurse Licensure Compact is an agreement between thirty-four states that automatically grants registered nurses in member states the ability to work in any other member state without applying for a separate license. In this chapter, I exploit the staggered adoption of the Compact to study the effect that multistate licensure had on labor-market, geographic, and educational outcomes for registered nurses. I show that the Compact decreased wages and the probability of employment while increasing the time that registered nurses spent unemployed. I also find some evidence that the NLC increased whether an individual worked in a different Compact state or moved to a different Compact state, but I find no effect on whether an individual worked in any different state or moved to any state. Finally, I find robust evidence that the Nurse Licensure Compact increased the likelihood that an individual was enrolled in any education program, and that this effect was driven by increased enrollment in master's programs or higher. The estimates pertaining to education are, to my knowledge, among the first evidence of how licensing regulations can impact human capital decisions.

I theorize that as the NLC increases competition within Compact states and wages or employment decline, individuals in Compact states decide to enter education programs. In particular, this effect is driven by individuals entering master's or higher programs because they seek to change their career trajectory to work as an advanced practice registered nurse. APRN's are separately licensed than registered nurses and not affected by the Nurse Licensure Compact. Thus, those working as an APRN can likely avoid the adverse labor market outcomes caused by the NLC.

Of course, the analysis presented in this chapter fails to address every potential benefit of the Compact since I only focus on one aspect of the Nurse Licensure Compact: its effect on labor market outcomes for the regulated occupation. The NLC may have benefits not captured in this paper. For instance, the NLC reduces barriers to providing telehealth across state lines, which may benefit patients who live in one Compact state but commute to another state for their primary care. In addition, the NLC may ease labor market frictions in the short term in ways that I cannot capture in my dataset. For example, the Compact may have unique benefits to military-spouses, which I explore in chapter 2. In addition, perhaps it is the case that, after natural disasters or other emergencies, Compact states can better respond to nursing shortages than non-Compact states. To that end, I explore the effect of the NLC on COVID-19 case and fatality rates in more detail in chapter 3 of my dissertation.

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Tables

Table 1: Adoption of NLC over Time

State	Adoption
Maryland	1999
Arkansas	2000
Delaware	2000
Iowa	2000
North Carolina	2000
Texas	2000
Utah	2000
Wisconsin	2000
Idaho	2001
Maine	2001
Mississippi	2001
Nebraska	2001
South Dakota	2001
Arizona	2002
Tennessee	2003
New Mexico	2004
North Dakota	2004
Virginia	2005
New Hampshire	2006
South Carolina	2006
Colorado	2007
Kentucky	2007
Rhode Island	2008
Missouri	2010
Montana	2015

Notes: Table 1 lists the year that each state adopted the Nurse Licensure Compact.

Table 2: Summary Statistics, NSSRN

	Nurses Only			Nurses & APRNs		
Variable	**All States**	**Compact States**	**Non-Compact States**	**All States**	**Compact States**	**Non-Compact States**
Weeks Unemployed	0.11	0.15**	0.09	0.10	0.12*	0.10
Annual Wages ($2000)	41.87	43.00**	41.39	46.58	50.20**	45.34
Weekly Hours	36.23	37.45**	35.87	36.30	37.20**	36.00
Work in Different State	0.06	0.08**	0.05	0.06	0.08**	0.05
Moved States	0.04	0.09**	0.08	0.09	0.10**	0.08
Pursuing Any Education	0.09	0.09	0.09	0.09	0.08	0.09
Pursuing Bachelors	0.04	0.04**	0.05	0.04	0.03**	0.04
Pursuing Masters	0.04	0.04**	0.04	0.04	0.04**	0.04
Pre-RN Associates	0.08	0.10**	0.07	0.08	0.10**	0.07
Pre-RN Bachelors	0.08	0.11**	0.08	0.10	0.14**	0.09
Pre-RN Masters	0.01	0.01	0.01	0.01	0.01**	0.01
Pre-RN None	0.84	0.79**	0.85	0.81	0.77**	0.83
RN - Bachelors	0.30	0.33**	0.29	0.35	0.40**	0.34
RN - Associates	0.46	0.50**	0.44	0.42	0.44**	0.41
RN - Masters/PhD	0.00	0.00**	0.00	0.01	0.01**	0.01
RN - Other	0.24	0.17**	0.26	0.22	0.15**	0.25
Age	44.14	45.93**	43.63	45.02	46.84**	44.37
Male	0.06	0.07**	0.06	0.07	0.08**	0.06
White	0.89	0.88**	0.90	0.89	0.88**	0.89
Black	0.05	0.07**	0.04	0.05	0.06**	0.04
Asian	0.04	0.03**	0.04	0.04	0.03**	0.04
Native American	0.01	0.01**	0.01	0.01	0.01**	0.01
Other Race	0.01	0.01	0.01	0.01	0.01	0.01
Kids under 6	0.20	0.18**	0.20	0.19	0.19**	0.20
Kids under 18	0.29	0.23**	0.31	0.28	0.22**	0.30
Married	0.73	0.74**	0.72	0.73	0.75**	0.72
No Prior Experience	0.49	0.36**	0.53	0.15	0.18**	0.14
Prior LPN Experience	0.16	0.20**	0.15	0.47	0.35**	0.51
Experience (Years)	17.00	17.94**	16.74	18.18	19.46**	17.77
Observations	127,420	28,783	98,637	161,203	41,152	121,057

Notes: T-tests were conducted on each variable to test where the means between Compact and non-Compact states were equal. ** p<0.01, * p<0.05, + p<0.1.

Table 3: Baseline difference-in-difference estimates for the effect of the Nurse Licensure Compact

	Labor Market Outcomes						Geographic Outcomes		Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Education Program	BA Program	MA+ Program
NLC	-0.012	-0.002	-0.003	-0.001	-0.003	0.020	-0.005	0.033	0.006	0.003	0.005
	(0.006)+	(0.019)	(0.014)	(0.006)	(0.004)	(0.003)**	(0.006)	(0.004)**	(0.004)	(0.003)	(0.003)+
Observations	157,571	127,420	127,420	127,420	127,420	127,420	127,420	127,420	127,420	127,420	127,420
R-squared	0.156	0.014	0.122	0.092	0.049	0.021	0.110	0.144	0.030	0.039	0.019

Notes: All specifications include state and year fixed effects along with full controls. Controls include indicators for education, race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (years).
Robust, clustered standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 4: Baseline triple difference estimates for the effect of the Nurse Licensure Compact

	Labor Market Outcomes						Geographic Outcomes		Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Any Education Program	Ba Program	MA+ Program
NLC	0.010	-0.070	0.111	0.021	-0.013	0.018	-0.015	-0.000	0.005	0.018	-0.009
	(0.010)	(0.050)	(0.023)**	(0.009)*	(0.007)+	(0.004)**	(0.003)**	(0.001)	(0.002)*	(0.002)**	(0.002)**
Nurse	-0.072	-0.055	-0.292	-0.052	-0.016	-0.001	-0.011	0.030	-0.013	-0.001	-0.012
	(0.004)**	(0.015)**	(0.017)**	(0.006)**	(0.003)**	(0.000)*	(0.010)	(0.004)**	(0.007)+	(0.004)	(0.005)*
NLC * Nurse	-0.018	0.047	-0.098	-0.012	0.004	-0.004	-0.004	-0.004	0.017	0.004	0.015
	(0.007)**	(0.019)*	(0.022)**	(0.007)	(0.005)	(0.003)	(0.007)	(0.003)	(0.004)**	(0.003)	(0.003)**
Observations	195,847	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203
R-squared	0.146	0.016	0.200	0.088	0.050	0.020	0.117	0.160	0.027	0.040	0.017

Notes: All specifications include state and year fixed effects along with full controls and state-specific linear time trends. Controls include indicators for education, race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (years). Robust, clustered standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 5: Continuous treatment, triple-difference estimates for the effect of the Nurse Licensure Compact

	Labor Market Outcomes						Geographic Outcomes		Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Any Education Program	BA Program	MA+ Program
NLC	0.001	-0.072	0.142	0.007	-0.007	-0.005	-0.015	0.000	0.005	0.018	-0.009
	(0.017)	(0.030)*	(0.040)**	(0.025)	(0.014)	(0.012)	(0.003)**	(0.002)	(0.002)*	(0.002)**	(0.002)**
Nurse	-0.071	-0.051	-0.294	-0.054	-0.015	-0.005	-0.002	-0.019	-0.011	-0.001	-0.013
	(0.004)**	(0.014)**	(0.016)**	(0.006)**	(0.003)**	(0.002)**	(0.019)	(0.030)	(0.011)	(0.006)	(0.009)
Nurse * NLC	-0.042	0.074	-0.195	-0.009	0.005	-0.003	-0.006	-0.012	0.034	0.011	0.031
	(0.014)**	(0.032)*	(0.041)**	(0.016)	(0.012)	(0.009)	(0.016)	(0.009)	(0.008)**	(0.006)+	(0.007)**
Observations	195,847	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203
R-squared	0.146	0.016	0.200	0.088	0.050	0.020	0.117	0.160	0.027	0.040	0.017

Notes: All specifications include state and year fixed effects along with full controls. Controls include indicators for education, race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (years). Robust, clustered standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.11.

Table 6: Triple-difference estimates for the effect of the Nurse Licensure Compact (white sample)

	Labor Market Outcomes						Geographic Outcomes		Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Any Education Program	BA Program	MA+ Program
NLC	-0.002	-0.062	0.119	0.027	-0.004	0.018	-0.016	-0.000	0.005	0.017	-0.008
	(0.008)	(0.051)	(0.022)**	(0.008)**	(0.006)	(0.005)**	(0.004)**	(0.001)	(0.002)*	(0.001)**	(0.002)**
Nurse	-0.074	-0.061	-0.304	-0.057	-0.016	-0.001	-0.011	0.030	-0.013	-0.003	-0.012
	(0.005)**	(0.016)**	(0.017)**	(0.006)**	(0.003)**	(0.000)*	(0.011)	(0.005)**	(0.007)+	(0.004)	(0.005)*
NLC * Nurse	-0.020	0.046	-0.105	-0.015	-0.002	-0.004	-0.006	-0.005	0.016	0.005	0.014
	(0.007)**	(0.020)*	(0.021)**	(0.007)*	(0.007)	(0.003)	(0.008)	(0.003)*	(0.004)**	(0.003)	(0.004)**
Observations	174,845	143,282	143,282	143,282	143,282	143,282	143,282	143,282	143,282	143,282	143,282
R-squared	0.148	0.015	0.197	0.090	0.033	0.021	0.088	0.103	0.025	0.038	0.014

Notes: All specifications include state and year fixed effects, along with full controls and state-specific linear time trends. Controls include indicators for race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (year). Robust, clustered standard errors are in parenthesis. ** p<0.01, * p<0.05, + p<0.1.

Table 7: Triple-difference estimates for the effect of the Nurse Licensure Compact (non-white sample)

	Labor Market Outcomes				Geographic Outcomes				Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Any Education Program	BA Program	MA+ Program
NLC	-0.001	-0.112	0.029	0.005	0.044	-0.034	-0.022	-0.026	0.007	-0.002	0.005
	-0.010	(0.060)+	-0.030	-0.018	-0.035	(0.013)**	-0.014	-0.035	-0.010	-0.005	-0.010
Nurse	-0.057	-0.028	-0.204	-0.014	-0.006	0.001	-0.005	0.013	0.008	0.025	-0.014
	(0.007)**	-0.057	(0.022)**	-0.011	-0.006	-0.003	-0.006	-0.008	-0.006	(0.004)**	(0.006)*
NLC * Nurse	0.005	0.075	-0.038	0.011	-0.001	-0.003	0.010	0.009	0.015	0.001	0.020
	-0.010	-0.067	-0.034	-0.016	-0.013	-0.005	-0.013	-0.010	-0.014	-0.007	(0.010)*
Observations	21,002	17,921	17,921	17,921	17,921	17,921	17,921	17,921	17,921	17,921	17,921
R-squared	0.141	0.027	0.186	0.066	0.369	0.043	0.315	0.053	0.042	0.061	0.033

Notes: All specifications include state and year fixed effects, along with full controls and state-specific linear time trends. Controls include indicators for race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (year). Robust, clustered standard errors are in parenthesis. ** p<0.01, * p<0.05, + p<0.1.

Table 8: Triple-difference estimates for the effect of the Nurse Licensure Compact with state-by-year fixed effects

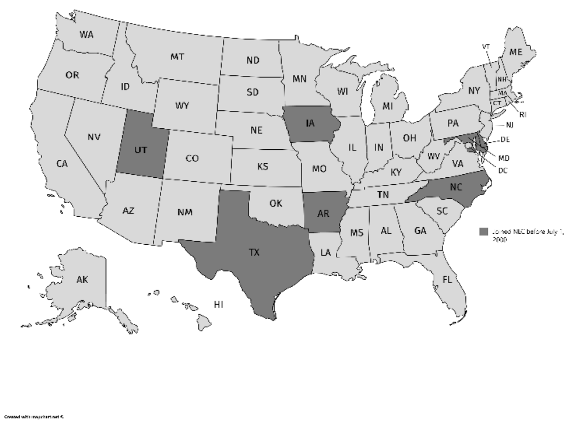
	Labor Market Outcomes						Geographic Outcomes		Education Outcomes		
VARIABLES	Employed	Weeks Unemployed	Log(Annual Wages)	Log(Weekly Hours)	Work in Different State	Work in Different NLC State	Moved States	Moved NLC States	Enrolled in Any Education Program	BA Program	MA/Ph.D. Program
Nurse * NLC	-0.017	0.046	-0.106	-0.010	0.005	-0.004	0.003	-0.002	0.019	0.004	0.018
	(0.007)*	(0.020)*	(0.023)**	(0.007)	(0.005)	(0.003)	(0.006)	(0.003)	(0.004)**	(0.004)	(0.003)**
Observations	195,847	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203	161,203
R-squared	0.149	0.018	0.204	0.091	0.053	0.132	0.132	0.223	0.029	0.042	0.019

Notes: All specifications include state and year fixed effects, along with full controls. Controls include indicators for race, gender, whether the individual has prior experience in healthcare, children under age 6, children under age 18, and marital status. I also control for age, age squared, and experience (year). Robust, clustered standard errors are in parenthesis. . ** p<0.01, * p<0.05, + p<0.1.

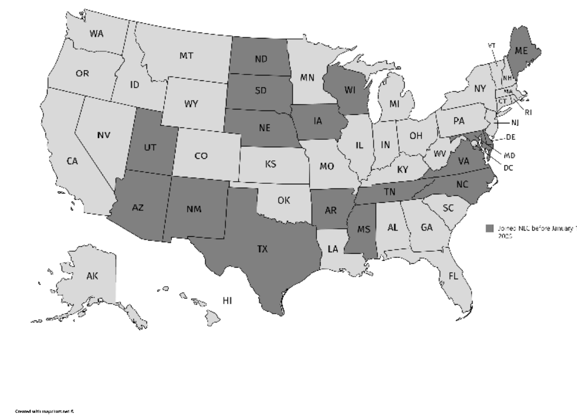
Figures

Figure 1: Adoption of NLC over Time

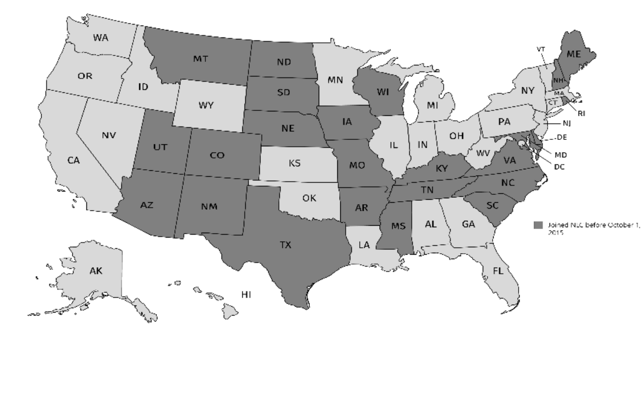
2000



2005



2015



Note: Highlighted states have entered the Nurse Licensure Compact by the end of the specified year.

Figure 2: Event study for the effect of the NLC on probability of employment

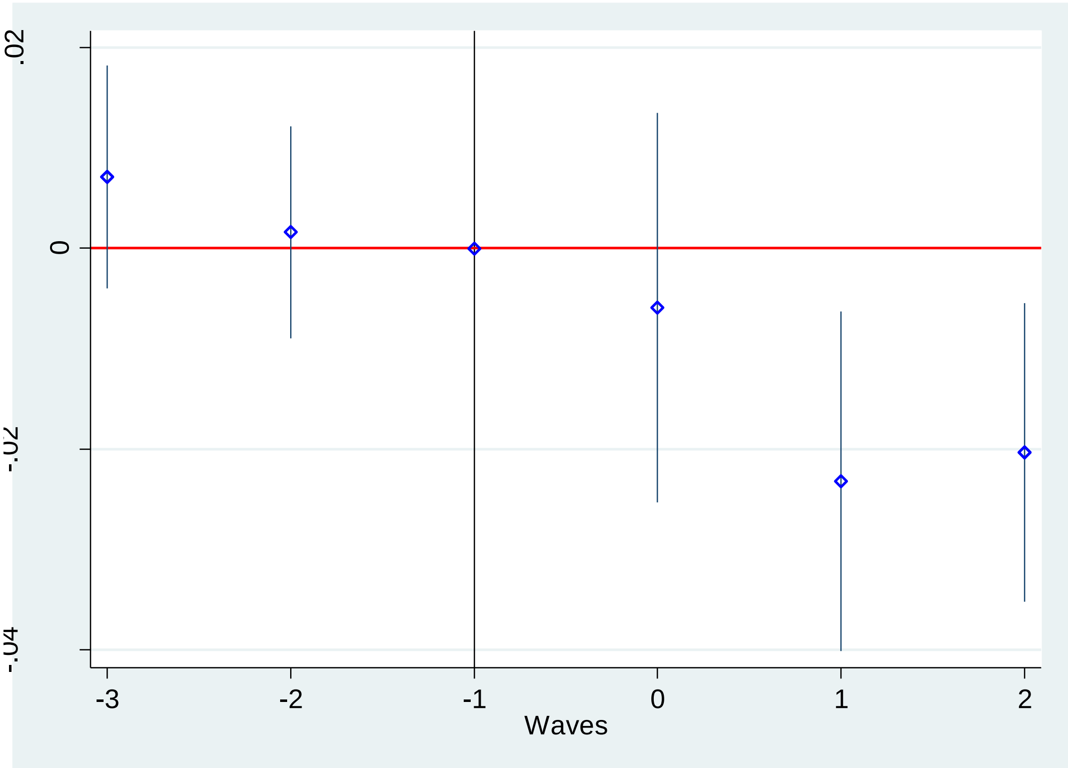


Figure Notes: Figure 2 plots an event study for the effect of the NLC on probability of employment. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 3: Event study for the effect of the NLC on weeks unemployed

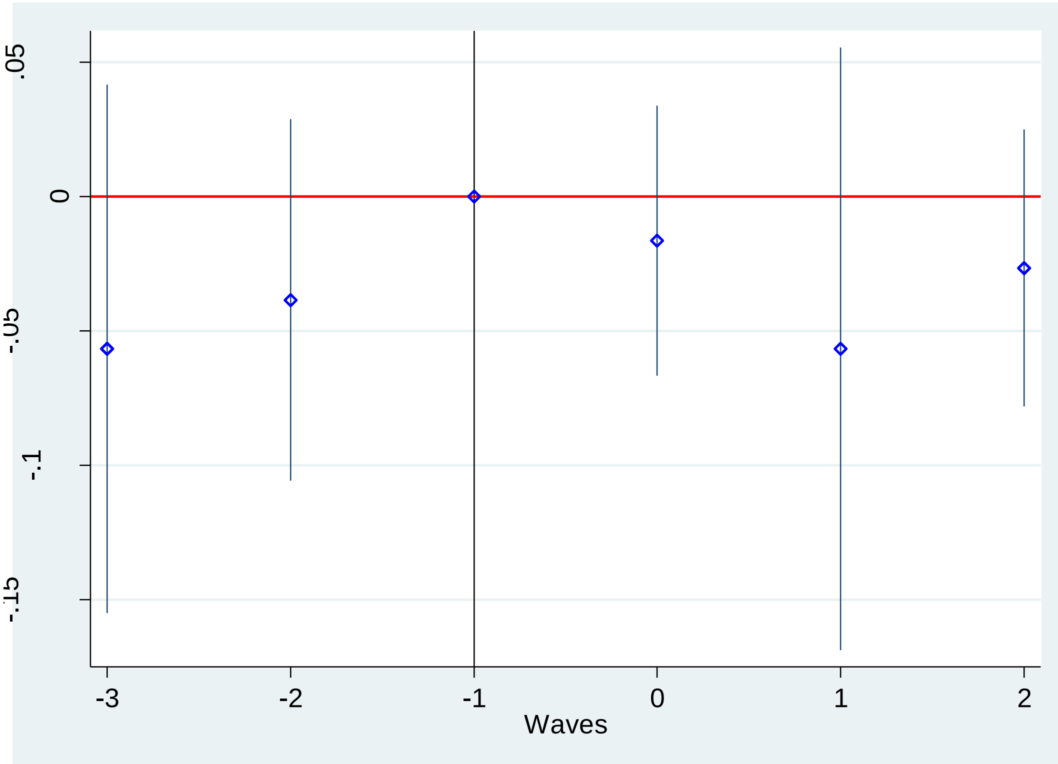


Figure Notes: Figure 3 plots an event study for the effect of the NLC on weeks unemployed. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 4: Event study for the effect of the NLC on log(wages)

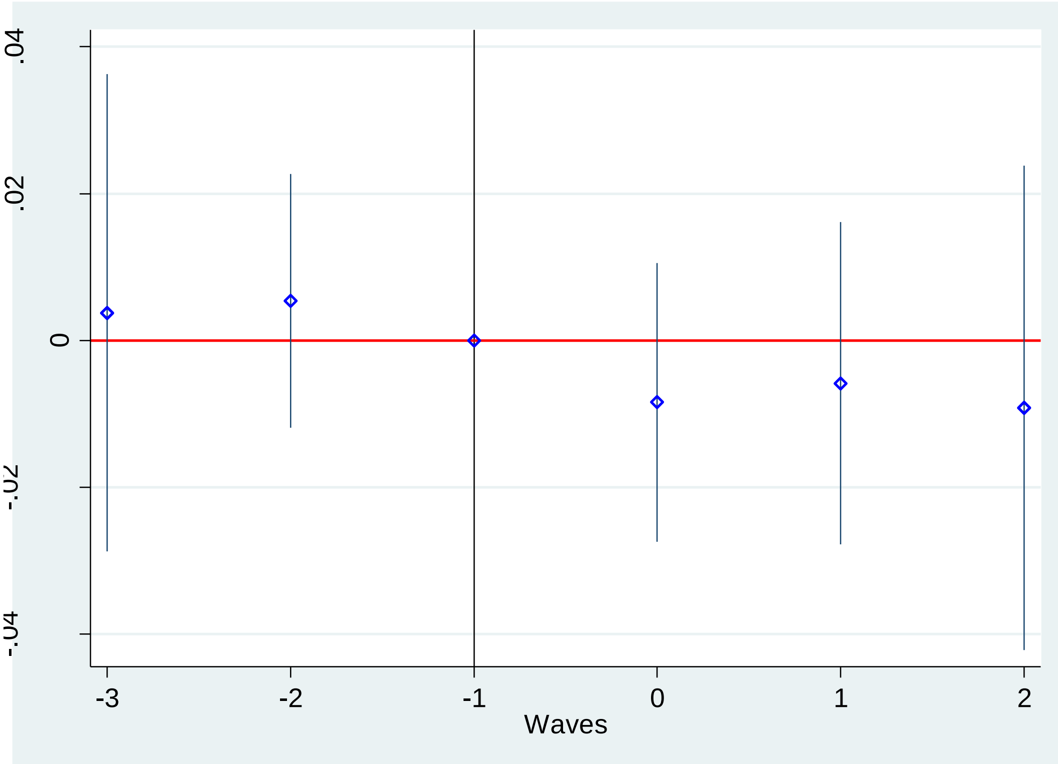


Figure Notes: Figure 4 plots an event study for the effect of the NLC on log(wages). The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 5: Event study for the effect of the NLC on log(weekly hours)

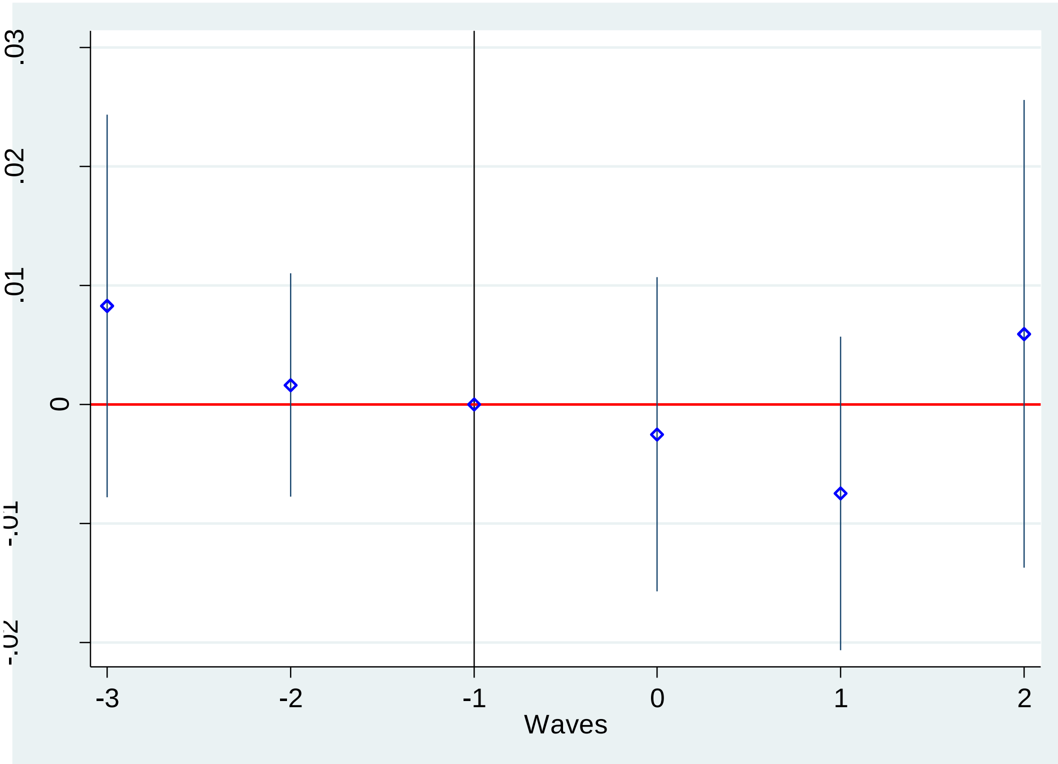


Figure Notes: Figure 5 plots an event study for the effect of the NLC on log(weekly hours). The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 6: Event study for the effect of the NLC on working in a different state

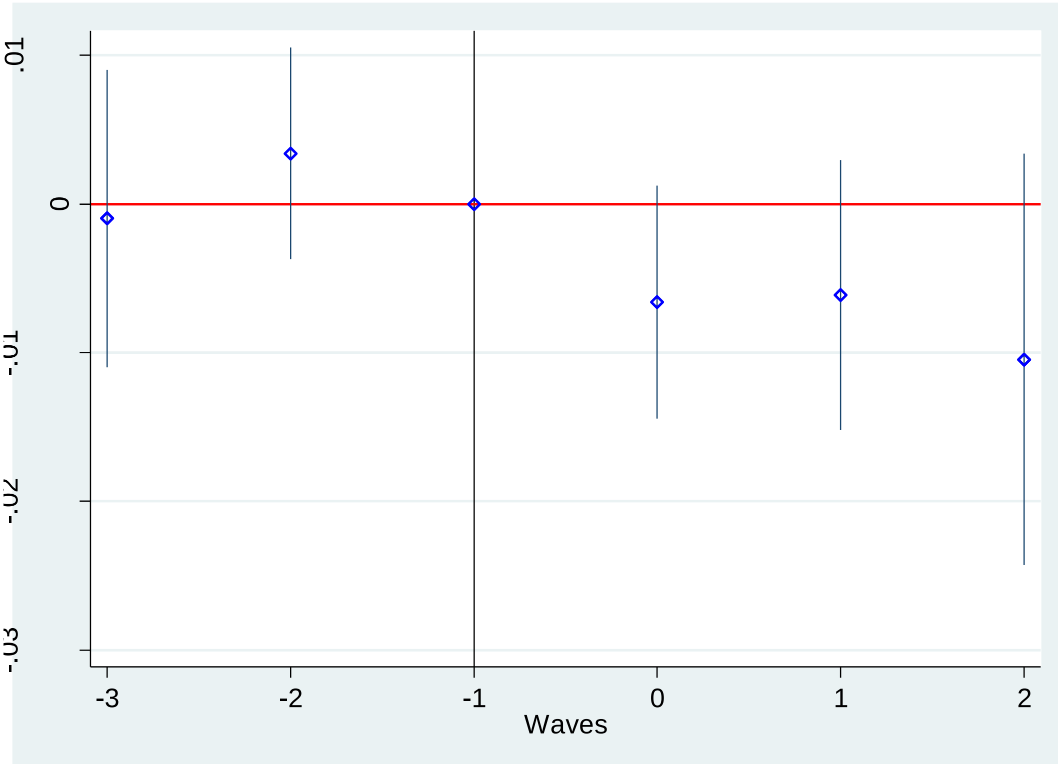


Figure Notes: Figure 6 plots an event study for the effect of the NLC on probability of working in a different state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 7: Event study for the effect of the NLC on working in a different NLC state

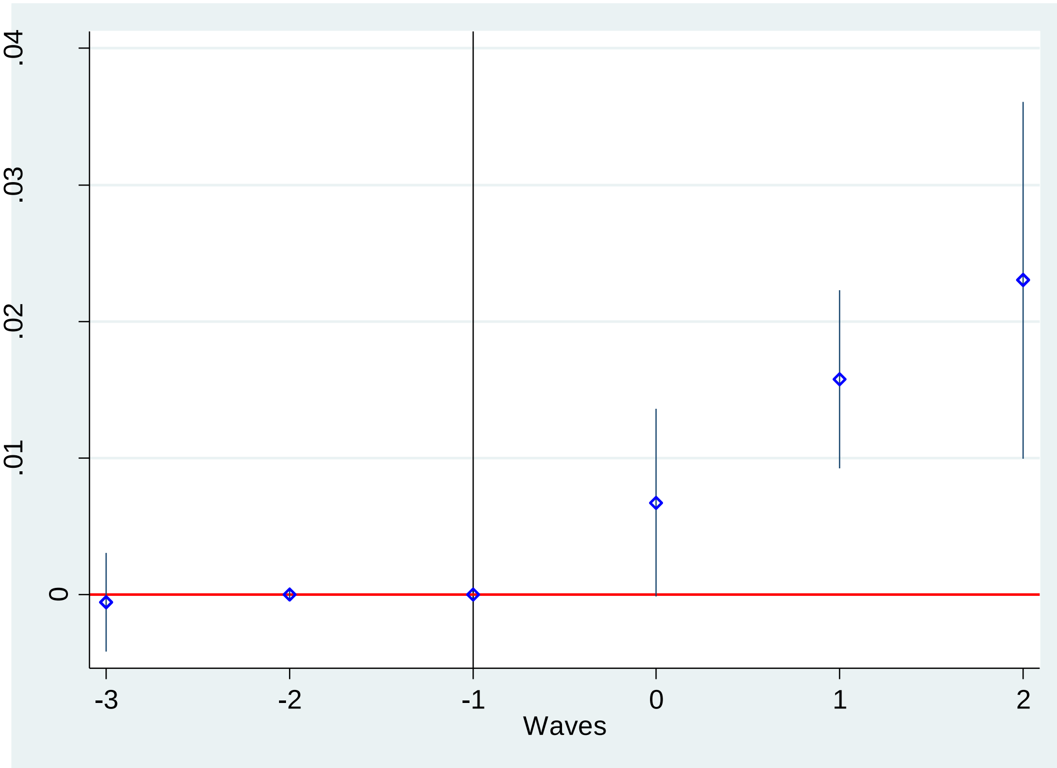


Figure Notes: Figure 7 plots an event study for the effect of the NLC on probability of working in a different Compact state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 8: Event study for the effect of the NLC on moving to a new state

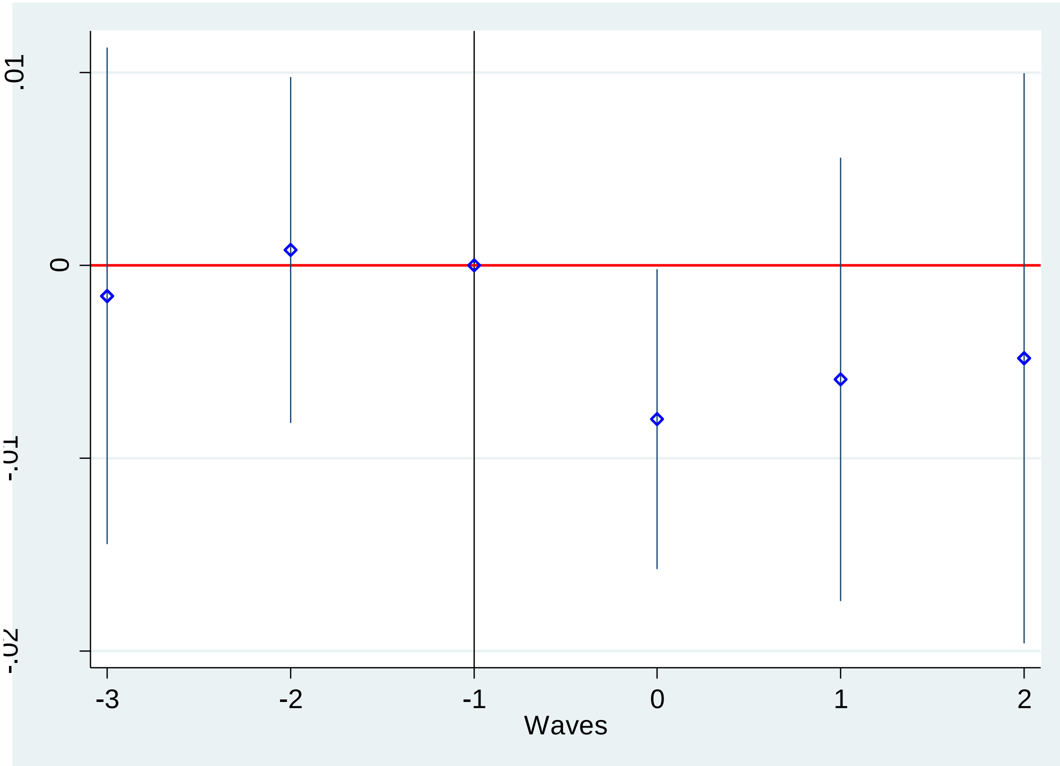


Figure Notes: Figure 8 plots an event study for the effect of the NLC on probability of moving to a new state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 9: Event study for the effect of the NLC on moving to a different NLC State

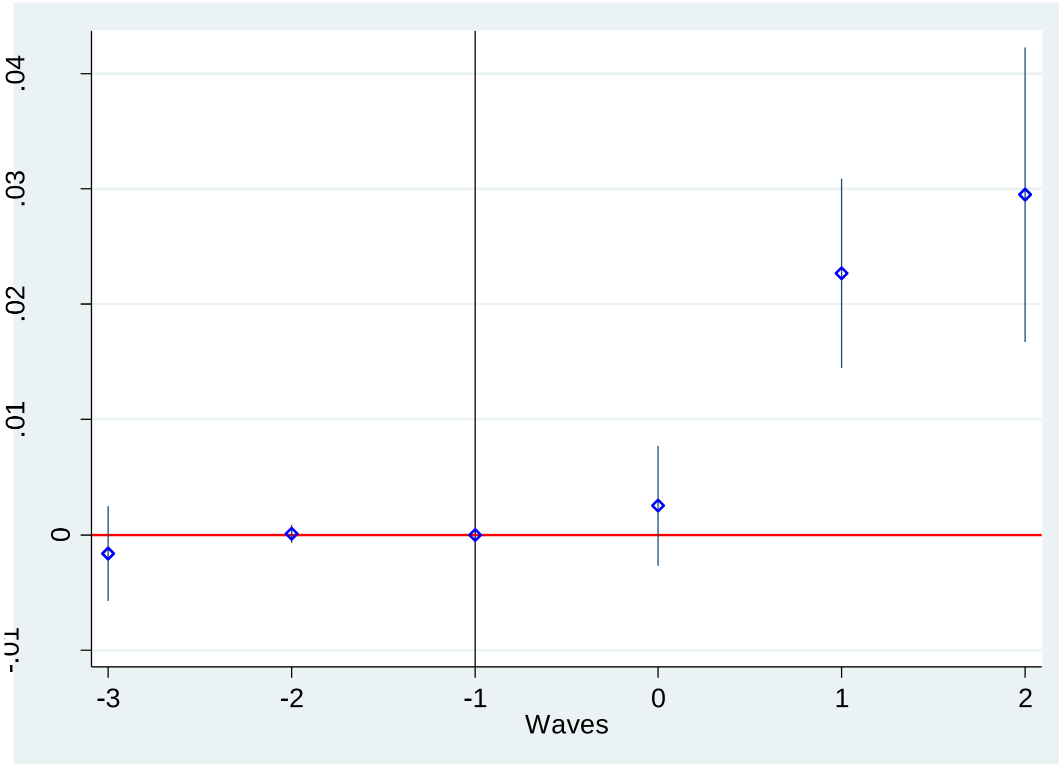


Figure Notes: Figure 9 plots an event study for the effect of the NLC on probability of moving to a new Compact state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 10: Event study for the effect of the NLC on enrolling in any education program

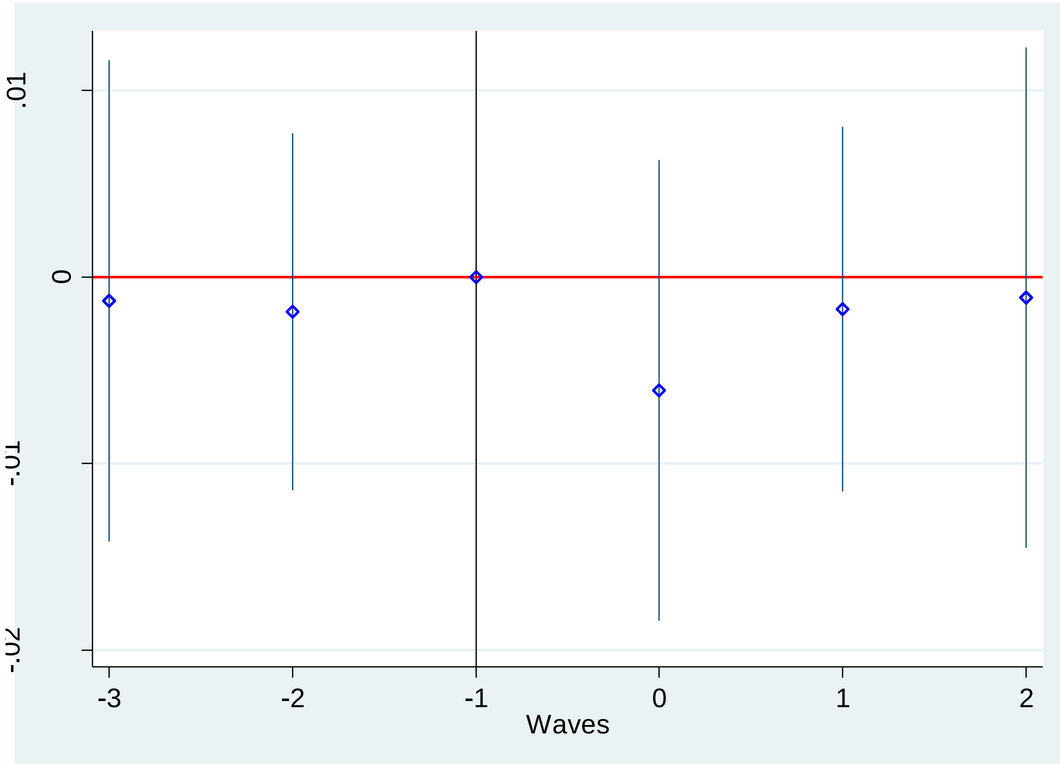


Figure Notes: Figure 10 plots an event study for the effect of the NLC on probability of enrolling in any education program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 11: Event study for the effect of the NLC on enrolling in a BA program

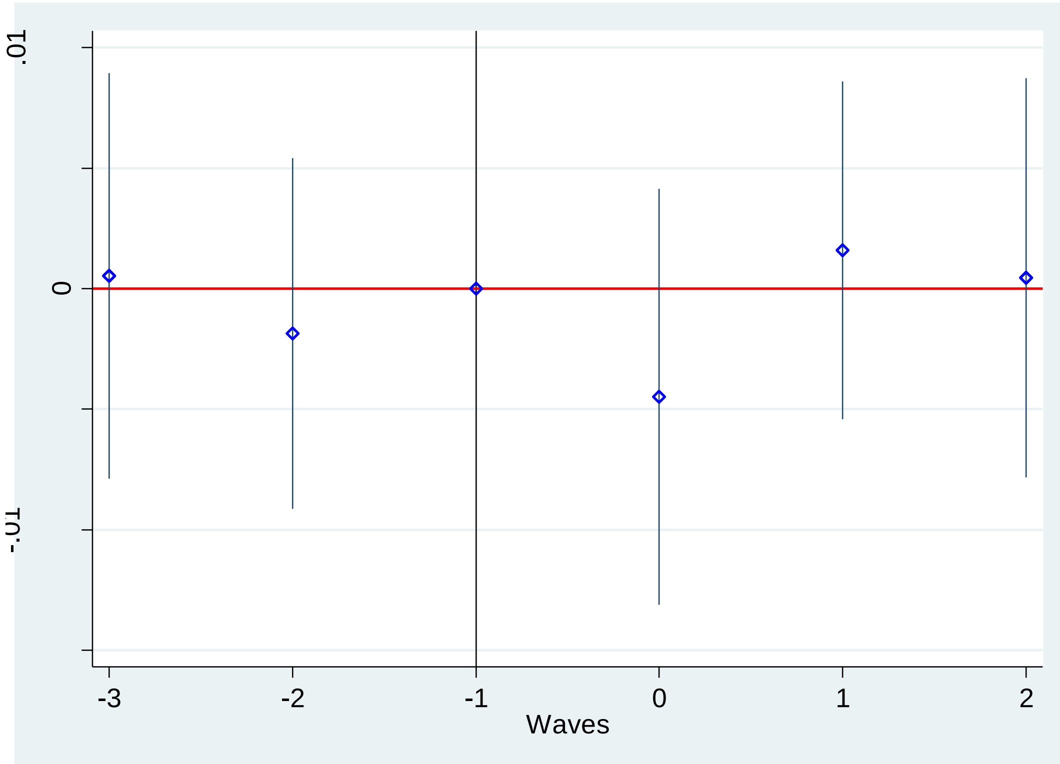


Figure Notes: Figure 11 plots an event study for the effect of the NLC on probability of enrolling in a BA program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 12: Event study for the effect of the NLC on enrolling in an MA program

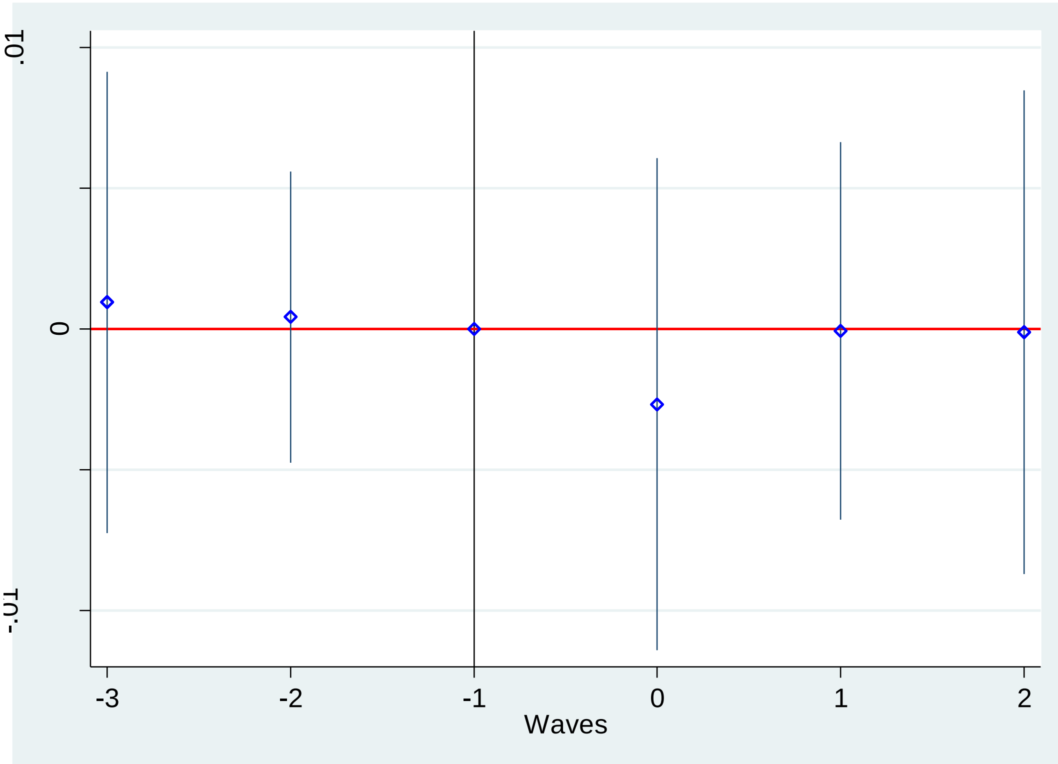


Figure Notes: Figure 12 plots an event study for the effect of the NLC on probability of enrolling in an MA program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 13: Triple difference event study for the effect of the NLC on employment

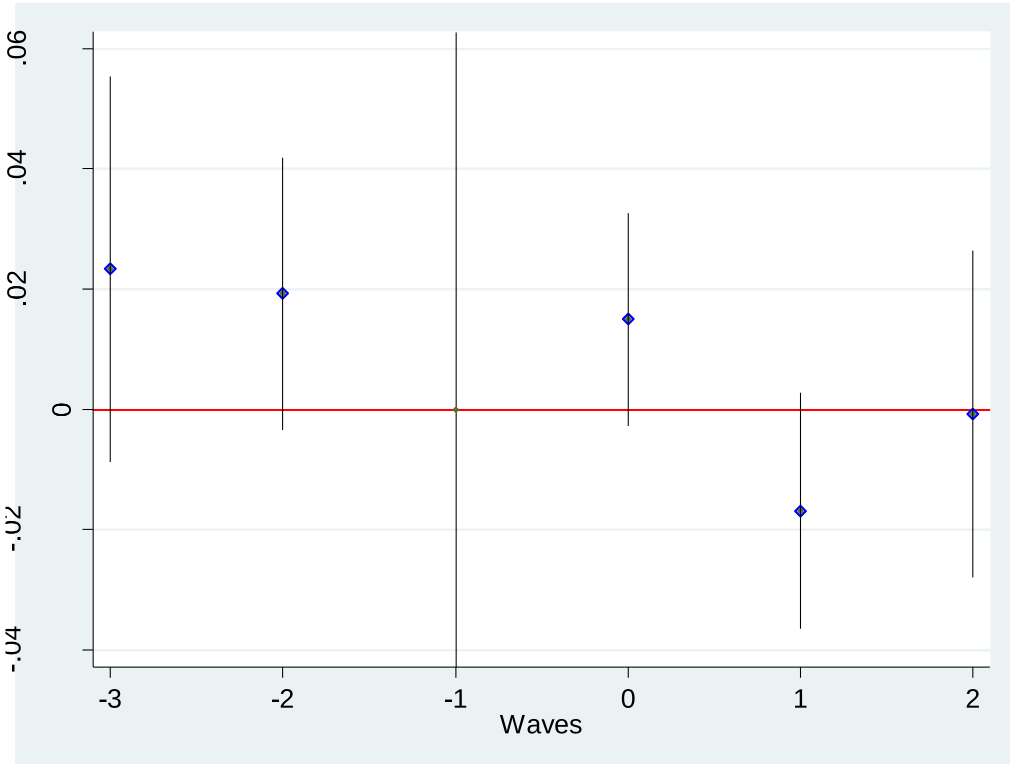


Figure Notes: Figure 13 plots a triple-difference event study for the effect of the NLC on probability of employment. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 14: Triple difference event study for the effect of the NLC on weeks unemployed

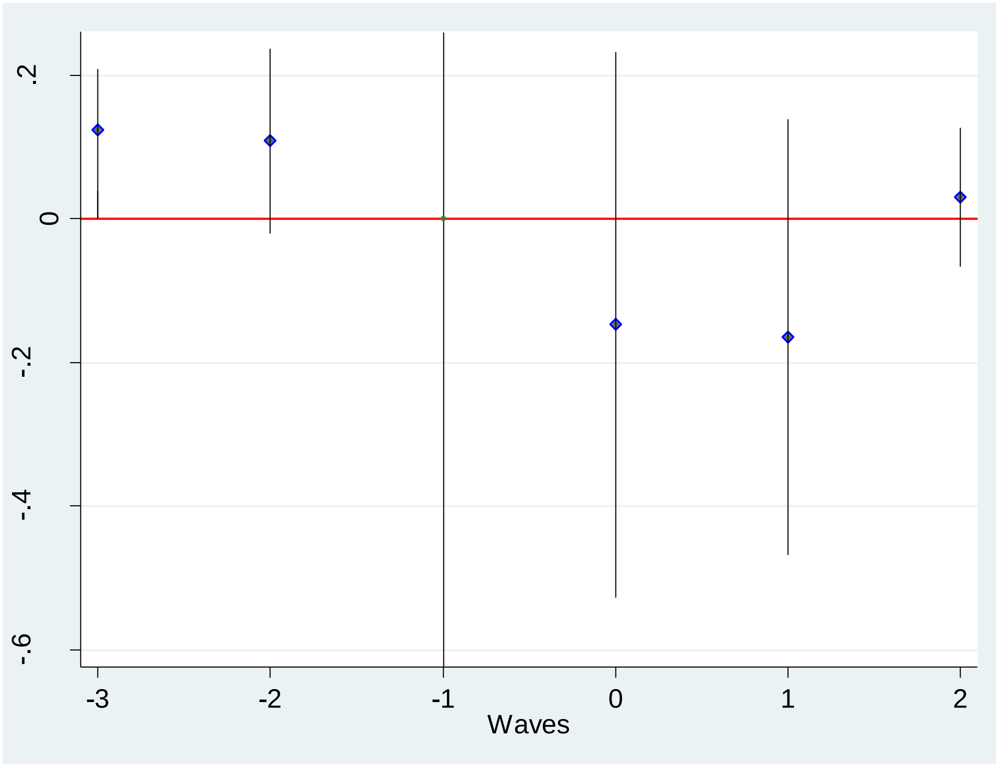


Figure Notes: Figure 14 plots a triple-difference event study for the effect of the NLC on weeks unemployed. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 15: Triple difference event study for the effect of the NLC on log(wages)

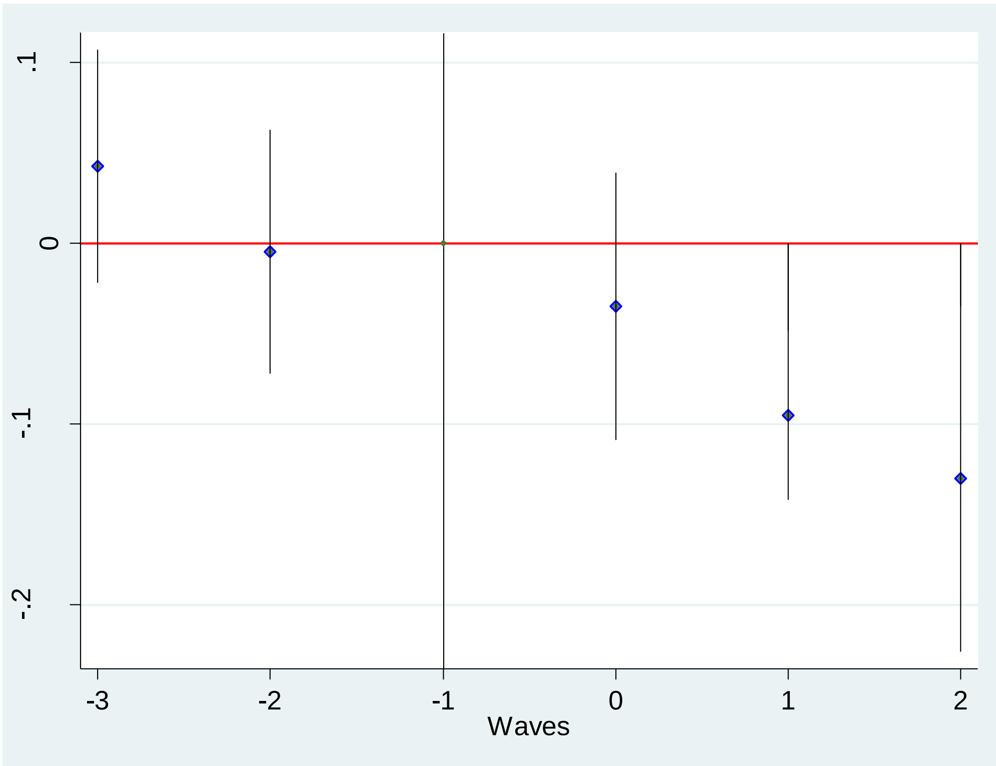


Figure Notes: Figure 15 plots a triple-difference event study for the effect of the NLC on log(wages). The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 16: Triple difference event study for the effect of the NLC on log(weekly hours)

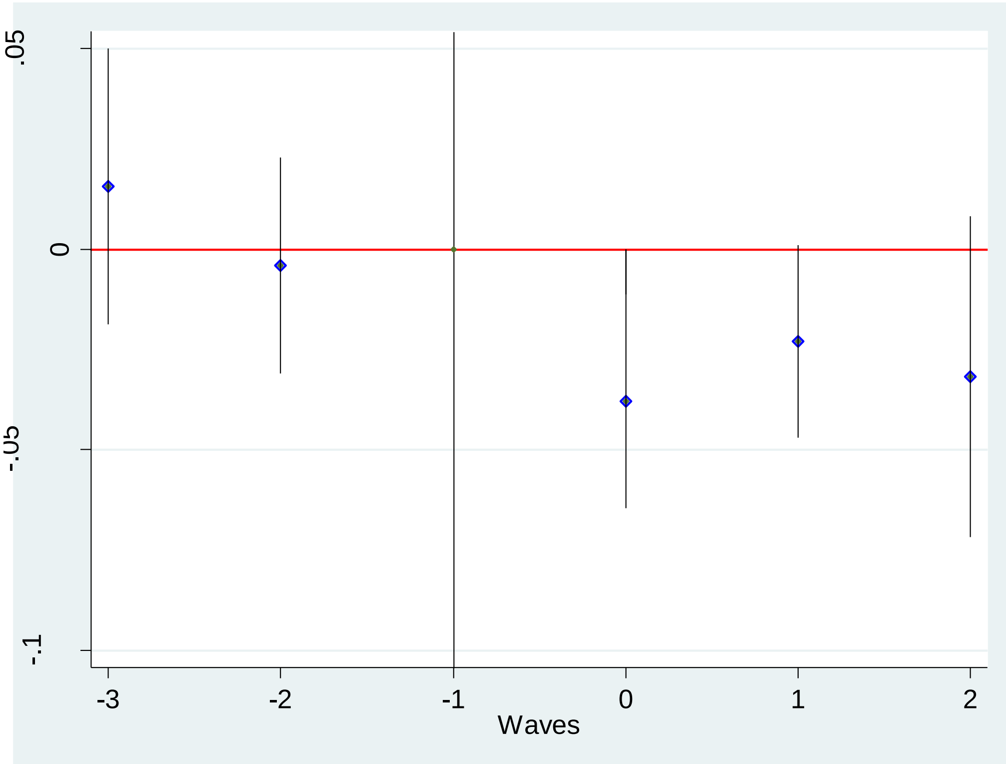


Figure Notes: Figure 16 plots a triple-difference event study for the effect of the NLC on log(weekly hours). The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 17: Triple difference event study for the effect of the NLC on working in a different state

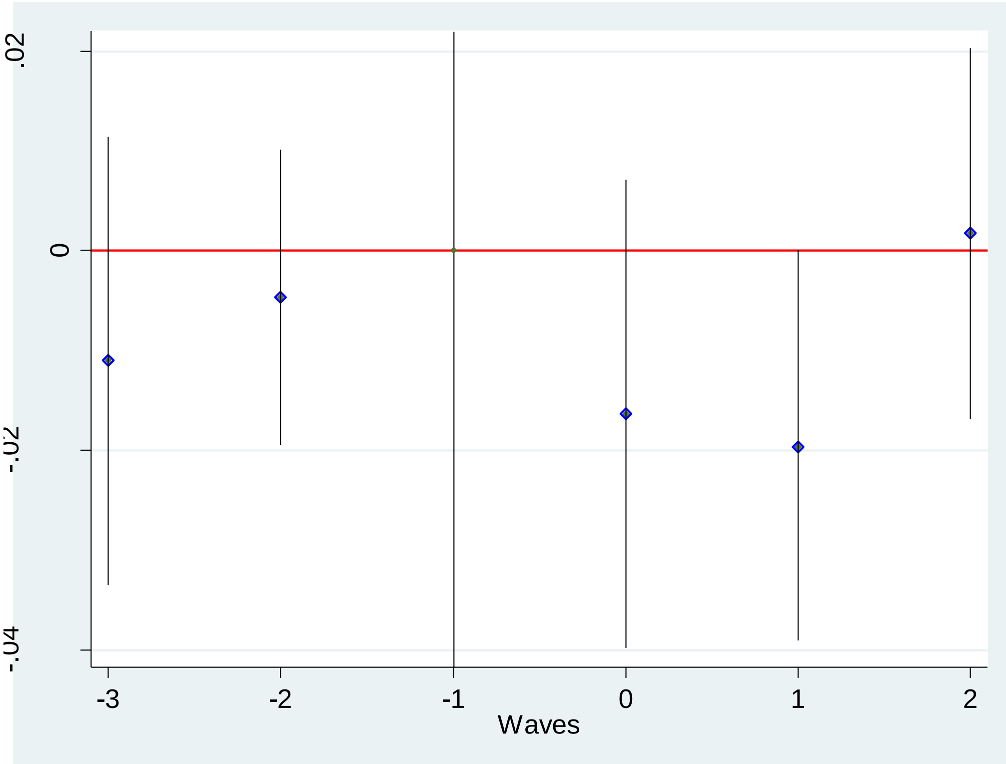


Figure Notes: Figure 17 plots a triple-difference event study for the effect of the NLC on probability of working in a different state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 18: Triple difference event study for the effect of the NLC on working in a different NLC state

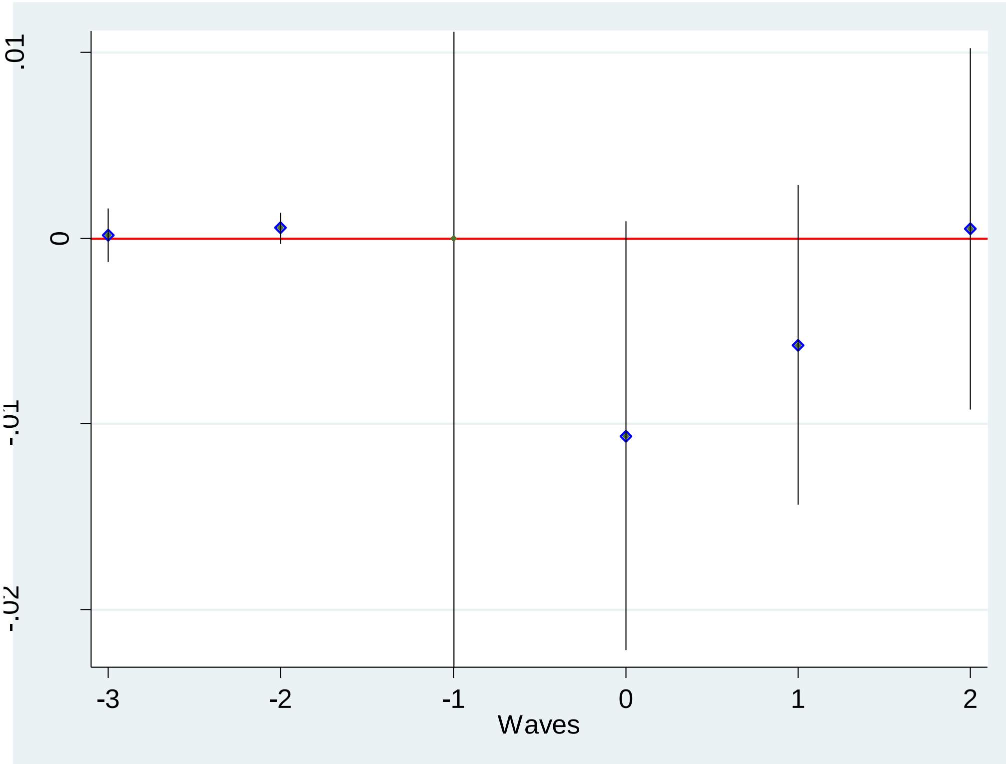


Figure Notes: Figure 18 plots a triple-difference event study for the effect of the NLC on probability of working in a different NLC state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 19: Triple difference event study for the effect of the NLC on moving to a different state

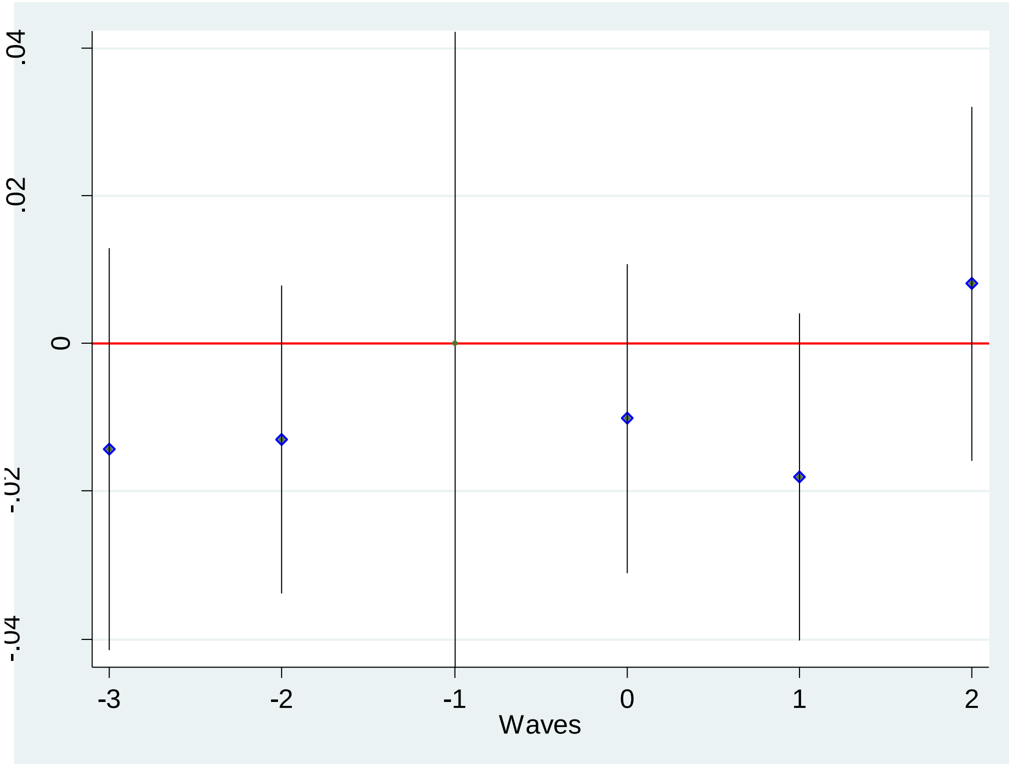


Figure Notes: Figure 19 plots a triple-difference event study for the effect of the NLC on probability of moving to a new state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 20 Triple difference event study for the effect of the NLC on moving to a different NLC state

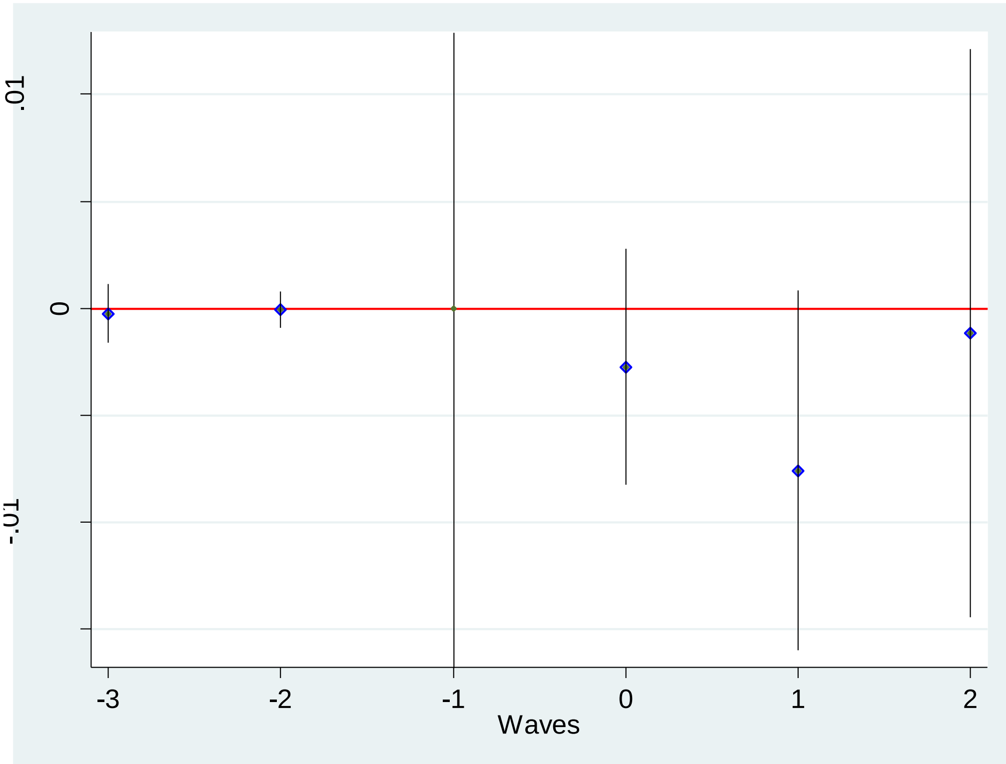


Figure Notes: Figure 20 plots a triple-difference event study for the effect of the NLC on probability of moving to a new NLC state. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 21: Triple difference event study for the effect of the NLC on enrolling in any education program

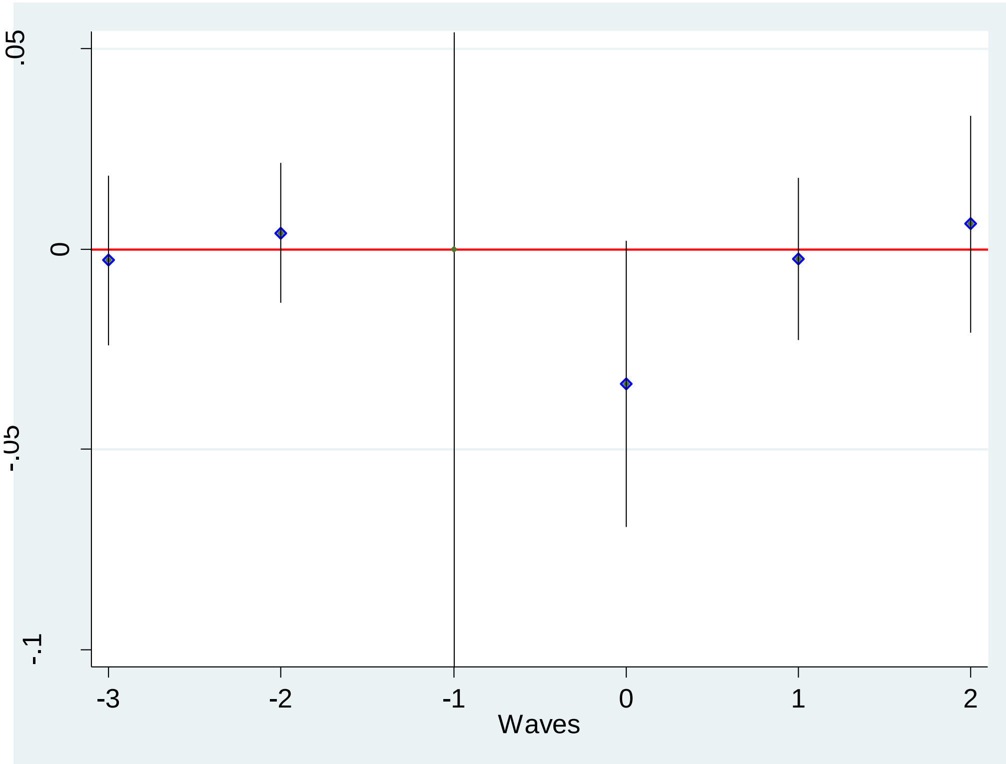


Figure Notes: Figure 21 plots a triple-difference event study for the effect of the NLC on probability of enrolling in any education program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 22: Triple difference event study for the effect of the NLC on enrolling in a BA program

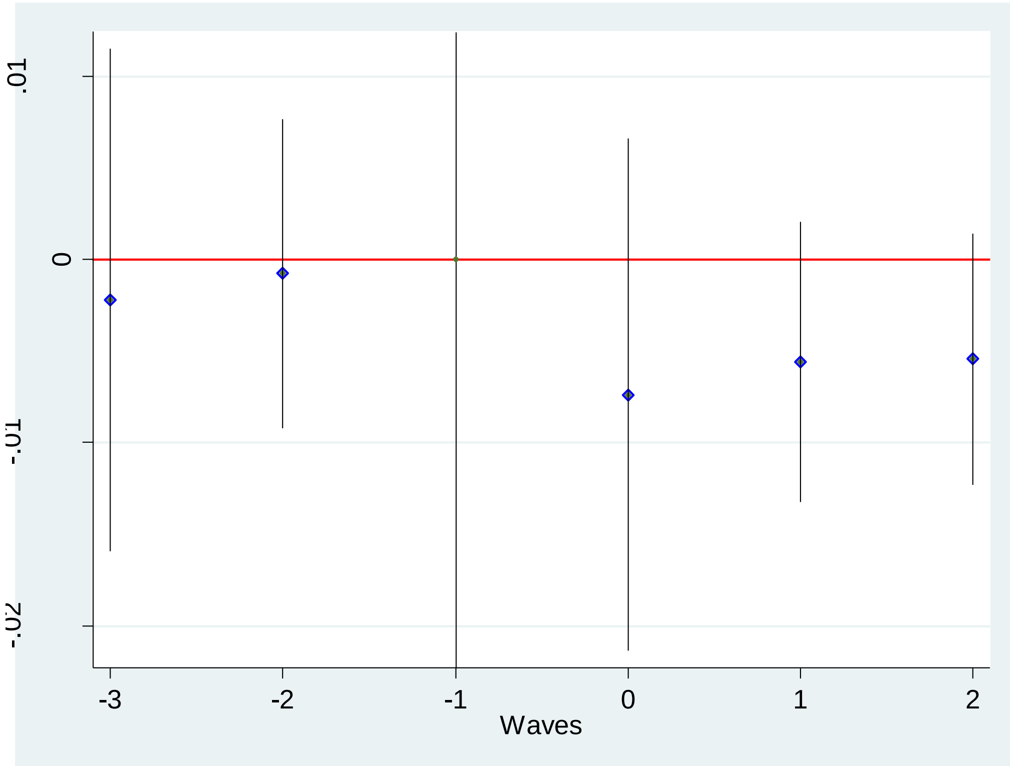


Figure Notes: Figure 22 plots a triple-difference event study for the effect of the NLC on probability of enrolling in a BA program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Figure 23: Triple difference event study for the effect of the NLC on enrolling in an MA program

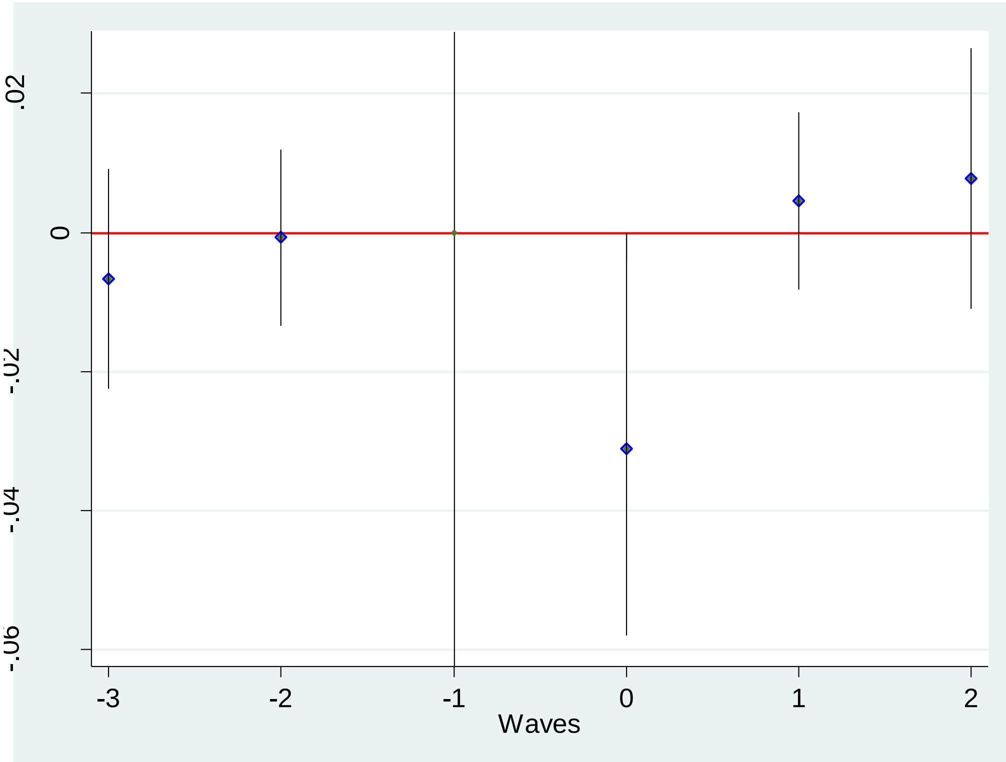


Figure Notes: Figure 23 plots a triple-difference event study for the effect of the NLC on probability of enrolling in an MA program. The analysis is limited to each survey wave between 1992 and 2008 and includes full controls, state fixed effects, and time fixed effects. Following Markowitz and Adams (2020), the sample is defined in terms of survey waves rather than years. The omitted reference wave is -1.

Chapter 2: The Rolling Stone Gathers No Jobs: Does the Nurse Licensure Compact Benefit Military Spouses?

I. Introduction

Nearly seven hundred thousand people are married to active-duty military personnel in the United States (Council of Economic Advisors 2018). Substantial evidence from the economics literature indicates that this group, which I refer to as military spouses, experience worse labor market outcomes than their similarly situated peers married to civilians (Burke and Miller 2016; Lim et. al. 2007; Kinskern and Segal 2011). This phenomenon is driven in part by two interrelated factors. First, military families move across state lines approximately seven times more often than civilian families, and second, military spouses are more likely to work in licensed occupations than civilian spouses (Council of Economic Advisors 2018). Because job-specific tenure is correlated with long-run employment and earnings, regular cross-state migration may impede military spouses from developing or maintaining their careers, particularly for those employed in licensed occupations (Burke and Miller 2016). In fact, the federal government and the military have expressed concerns that occupational licensing requirements adversely affect military families. A recent White House report from the Council of Economic Advisors reported that “military spouses are more likely than other workers to be caught up in the country’s patchwork of occupational licensing laws, both because they are more likely to move across State lines and because they are disproportionately employed in occupations that require a license” (Council of Economic Advisors 2018).

In this chapter, I examine how the Nurse Licensure Compact affects labor-force participation, employment, wages, and similar labor-market outcomes for nurses married to military personnel (“military-spouse nurses”). The Nurse Licensure Compact (“NLC” or

“Compact”) is an interstate agreement that automatically endows registered nurses, licensed practical nurses, and licensed vocational nurses in Compact states with a multistate license to work in any other Compact state (Evans 2015). Because the Compact functionally eliminates many of the licensing requirements that burden military spouses employed in nursing, military-spouse nurses residing in NLC states may fare better in the labor market than those in non-NLC states. Indeed, easing licensing restrictions for this group of nurses is cited as an important benefit of the Nurse Licensure Compact. One advocacy group states that the NLC permits “military spouse nurses to seamlessly continue working without having to obtain a new license each time they relocate” (NurseCompact.com 2019).

This chapter empirically examines whether the NLC has improved labor market outcomes for nurses married to military personnel. I hypothesize that the NLC will be particularly beneficial for this group of nurses due to their unique position in the labor market. To be sure, chapter one of my dissertation demonstrated that the Nurse Licensure Compact reduced employment and wages for the nursing occupation as a whole. However, these findings may not apply to military spouses because this group differs in meaningful ways from the sample examined in chapter one. For example, military spouses relocate to different states seven times as frequently as civilian spouses (Council of Economic Advisors 2018). Moreover, the timing of these moves is unpredictable since they are determined by the needs of the military rather than a desire by military families to take advantage of employment opportunities in other states (Burke and Miller 2016). Thus, despite finding that the NLC adversely affects labor-market outcomes for nurses in chapter one, it is likely that the Compact may still benefit military spouses due to this group’s unique characteristics.

To estimate the effect of the NLC on labor market outcomes for military-spouse nurses, I create a sample of individuals married to military personnel using data from the 1990 Census as well as the 2000 to 2018 American Community Surveys ("ACS"). These data sources include detailed demographic and labor market information in an ongoing, nationwide annual survey. In addition, the ACS data harmonized by IPUMS-USA permits me to attach spousal information for married individuals in the ACS sample, including the spouse's occupation (Rapino and Beckhusen, 2013). In this way, I can identify whether an individual is married to a military service member with certainty. With this approach, I construct a sample consisting only of registered nurses, licensed vocational nurses, and licensed practical nurses with a spouse in the armed forces.

Using these data, I estimate a difference-in-difference model in which I leverage geographic and temporal variation in the roll out of the Nurse Licensure Compact to assess how the Compact affected labor-market outcomes for military spouses employed in nursing. In addition, I exploit the unique nature of cross-state moves in military families as an additional source of causal identification. Moves in the military are exogenously imposed on military families since they are determined by the needs of the military rather than by the family's desire explore labor market opportunities in other states (Burke and Miller, 2016; Carter and Swisher, 2020). I leverage this additional source of exogeneity to causally identify the effect of the NLC on labor-market outcomes for military-spouse nurses.

Overall, I find consistent evidence that the NLC improved employment outcomes for nurses married to military personnel. In particular, I show that the NLC increased labor force participation by five percent, the probability of employment by eight percent, and the probability of working in the last week by seven percent. However, I find that the Compact has no statistically significant effect on weekly hours, wages, whether an individual is looking for work, or whether an individual works in a different state or a different NLC state. My results are largely robust to the inclusion of state-specific time trends and removing individuals with master's degrees or higher who may be working as advanced practice registered nurses ("APRNs") from the sample.

The findings in this chapter are the first empirical work to assess how occupational licensing reforms affect labor-market outcomes for military spouses. The unique problems posed by heterogenous occupational licensing requirements across states for military spouses has been largely overlooked in the occupational licensing literature. However, these problems have been recognized in the public policy arena, and Arizona and Utah have recently adopted legislation to automatically recognize out-of-state licenses held by military spouses that relocate from other states. In addition, Republican Senator Mike Lee from Utah has introduced similar legislation into Congress to address the problems posed by licensing as well (Military Spouse Licensing Relief Act of 2020). Because the mechanism underlying these state and federal laws is similar to that of the Nurse Licensure Compact—that is, both sets of laws rely on automatic and universal recognition of out-of-state occupational licenses—results pertaining to the effect of the NLC also speak to how Utah, Arizona, and pending federal legislation may affect labor-market outcomes for military spouses in licensed occupations other than nursing.

This chapter proceeds as follows. Part I provides background information and broad characteristics of military spouses as well as an overview of federal and state policies to assist military spouses in the labor market. Next, Part II ties this chapter to the economics literature on family migration and labor-market outcomes for military spouses. In Part III, I describe the data and empirical methodology used in this chapter, as well as a theoretical model that predicts how the Nurse Licensure Compact can improve military spouse's employment outcomes by eliminating licensing requirements that may discourage them from entering the labor market in their new state. Finally, Part V outlines my finding that the NLC improves employment outcomes for military-spouse nurses.

II. Background

This Part begins with a description of the demographic characteristics of military spouses to highlight this group's unique characteristics. Then, I detail federal programs designed to help military spouses adjust after moving to a new state as well as recent proposals to modify licensing requirements for military spouses under state and federal law. Finally, I explain how the Nurse Licensure Compact may help military spouses in the nursing profession re-enter the labor market after they move.

a. Characteristics of Military Spouses

To situate my results for nurses within a broader context, this Part provides demographic and background characteristics of military spouses in the United States. The Council of Economic Advisors estimates that there are roughly 690,000 military spouses in the United States (Council of Economic Advisors 2018). The average age of a working military spouse is 33, and approximately 92% of military spouses are female. Military spouses are generally of higher educational attainment than civilian spouses, with 40% having obtained a four-year degree relative to 30% of civilian spouses. Estimates of the unemployment rate for spouses of active-duty military members range from 10 to 24 percent (Council of Economic Advisors 2018; Dorvil 2017). In addition, using data from the American Community Survey, the Council of Economic Advisors estimates that military spouses earn roughly twenty-six percent, or 12,000 dollars, less each year than their similarly situated civilian counterparts. Thirty-three percent of active-duty spouses reported moving in the last year. Finally, roughly thirty-five percent of employed spouses work in an occupation that requires an occupational license.

Spouses of military members experience higher unemployment and earn lower wages than civilian spouses for two reasons. First, military spouses are more likely to be licensed than their civilian counterparts (Council of Economic Advisors 2018). Second, they move approximately once every two to three years, or seven times the rate of civilian spouses (Burke and Miller 2016; Cooney, De Angelis, and Segal 2011; Council of Economic Advisors 2018). Occupational licensing regulations place especially high burdens on spouses of military personnel since regular movement between states forces military spouses to frequently seek re-licensure in each new state. The costs of re-licensure may discourage military spouses from re-entering the labor market or induce them to find work in a different non-licensed occupation. Moreover, the timing and location of moves for military families are often unpredictable since relocations are based on the needs of the military rather than by the family's desire to seek opportunities in another state (Burke and Miller, 2016; Carter and Wozniak, 2018; Carter and Swisher, 2020). As a result, military spouses oftentimes cannot proactively apply for licenses in their new state, and nearly one in four military spouses report that it took over ten months for them to acquire an occupational license to work in their family's new location (Tang et. al., 2018). Overall, moves to new states impose significant impediments to long-run employment or earnings for military spouses.

b. State and Federal Policies to Aide Military Spouses

The federal government and numerous states have enacted policies designed to help individuals married to military personnel adjust to labor-market frictions imposed by regularly relocating to new states. At the federal level, the Department of Defense offers several programs to help military spouses advance their career or education. (Burke and Miller, 2016). For example, the DoD offers a scholarship worth up to $4,000 for military spouse to help them develop more portable careers (Burke and Miller, 2016). In addition, the DoD has recognized that occupational licensing restrictions especially burden military spouses since obtaining licensure in a new state is both time consuming and expensive. For example, in California, applicants for a registered nursing license must pay nearly $500 in fees and wait up to four months for the application to process (California Board of Nursing, 2020). Furthermore, military spouses often cannot prepare in advance for the requirements of a state's licensing regime given both the unpredictable nature of military moves and inadequate lead-time to prepare for a move. In 2017, one in three military service members surveyed by the Defense Manpower Data Center reported that the amount of time to prepare for their most recent move was a moderate, large, or very large problem (Tang, 2018). To remedy this, the Department of Defense has worked with state licensing boards to expedite the acceptance of out-of-state licenses for military spouses (Department of Defense, 2019). In addition, at the congressional level, Senator Mike Lee introduced the Military Spouse Licensing Relief Act of 2020 into the Senate in September, 2020 to also address the problems posed by licensing (Military Spouse Licensing Relief Act of 2020). If passed, this bill would provide that any professional licenses held by a military service member or their spouse in good standing shall be considered valid in the individual's new jurisdiction for the duration of the military order.

Several states have also passed laws to recognize out-of-state licenses for military spouses. Arkansas passed HB 1184 in 2017 which requires state boards and commissions to promulgate regulations for temporary licensure for spouses of active-duty service members (AR H.B. 1184). Similarly, Texas allows military spouses working as teachers to temporarily work in

Texas while pursing licensure (H.B. 1934). Two other states have taken more expansive measures. In 2018, Utah exempted all active-duty service members and their spouses from obtaining a distinct Utah license as long as their license is in good standing in another state (S.B. 227). One year later, Arizona passed similar legislation to Utah (H.B. 2569).

Interstate agreements like the Nurse Licensure Compact can also assist military spouses that move across state lines due to military orders. In fact, the Department of Defense cited the NLC as the gold standard of reform and noted that easing licensing restrictions for military spouses is an important benefit of the Compact (Department of Defense, 2019). Under the NLC, any occupational license held by a registered nurse, licensed vocational nurse, or licensed practical nurse in a Compact state is automatically recognized by every other Compact state. As a result, in the words of one advocacy group, the NLC permits "military spouse nurses to seamlessly continue working without having to obtain a new license each time they relocate." (NurseCompact.com 2019). To illustrate, consider an example (NCSBN.org/NLC 2019):

> Mary Smith is a military spouse with primary state of legal residency in Florida (an NLC state). Mary holds a Florida multistate license. The Smith family is a military family and has been stationed in Virginia (an NLC state) for 2 years. Mary is able to practice in Virginia under the Florida multistate license as long as she maintains legal residency in Florida during her time in Virginia. Therefore, Mary does not need to apply for a Virginia license. After living in Virginia, Mary's family is ordered to a base in Texas (an NLC state) for several years. Similarly, Mary is able to practice in Texas under the Florida multistate license while maintaining Florida as her state of legal residency. If Mary's family were to be stationed in a non-compact state, then she would need to hold a license issued by that state or apply for license by endorsement with that state.

III. Literature Review

This chapter contributes to several strands of the extant literature while also adding to work I conducted in chapter one of my dissertation. Here, I examine how the Nurse Licensure Compact may have unique benefits for military spouses that differ from the effects of the NLC on the nursing occupation overall. In addition to building on chapter one, I also contribute to the literature on labor-market outcomes for military families as well as the literature studying family migration.

A growing body of work studies the labor-market outcomes for military members or their spouses. Burke and Miller (2016) examine the effect of a permanent change of station ("PCS") moves on spousal earnings and employment. Burke and Miller find that PCS moves reduce spousal earnings by between $2,100 and $3,700 in the year of the move relative to spouses married to service members that did not move, and that PCS moves across state lines result in larger reductions in earnings of approximately $4,200. Similarly, Lim et. al. (2007) find that military spouses earn less, on average, and are less likely to be employed than their civilian counterparts. Lim and Schulker (2010) find that military spouses are more likely to be under-employed, and Kinskern and Segal (2011) show that military spouses earn approximately twenty-five percent less than civilian spouses. Cooke and Spiers (2005) find that migration is associated with a ten percent decrease in employment for wives of military personnel. Finally, Rapino and Beckhuson (2013) use the American Community Survey between 2007 and 2011 to conclude that within military couples, wives tend to be unemployed and more educated than husbands relative to civilian couples. In addition, they show that military spouses move close to military installments, and civilian spouses move to metropolitan areas with large populations.

This chapter also contributes to the literature on family migration since the results developed herein demonstrate that occupational licensing reforms can improve employment outcomes for licensed "tied-migrants" after a cross-state move. In the economics literature, a tied migrant is a spouse who moves across state lines at the expense of their own career opportunities in order to advance their partner's career (Burke and, 2016; Mincer, 1978). Women are more likely to be a tied spouse than men (Taylor, 2007). Moreover, status as a tied migrant leads to worse outcomes in the labor market, particularly for women. In Sandell (1977), migrant husbands have relative wage growth while migrant wives experience relative wage declines compared to non-migrants. Boyle et. al. (1999) finds that wives in higher-status occupations than their husbands experience negative effects of migration on their earnings. Similarly, Cooke (2004) shows that family migration improves husbands' income but leaves wives' income unchanged.

Mincer (1978) explains that being a tied migrant lowers employment outcomes due to both fewer employment opportunities for spouses at the new locations and because spouses voluntarily leave the labor market to engage in nonmarket activity related to establishing a new household. These nonmarket interruptions reduce experience and wage growth. Moreover, Burke and Miller (2016) recognizes that these interruptions may be exacerbated by state-level occupational licensing regulations, as these regulations create barriers to entering the labor market in an individual's new state. In this chapter, I contribute to this vein of the literature by empirically assessing how the Nurse Licensure Compact—which permits nurses licensed in one Compact state to immediately work in another Compact state—affects "tied" spouses who move across state lines with their spouse in the armed forces.

IV. Data Sources, Empirical Strategy, and Predictions

a. Data Sources

This section describes the data sources used in this chapter. First, I collect state-by-state enactment dates for the Nurse Licensure Compact from the National Council of State Boards of Nursing ("NCSBN"). The NCSBN is a not-for-profit organization whose membership consists of nursing boards from each state, the District of Columbia, and four U.S. territories. I verify the information from the NCSBN using Westlaw. Table 1 lists NLC adoption dates for each state. In addition, Figures 1 illustrates the staggered adoption of the NLC over time. Eight states—Utah, Iowa, Arkansas, Texas, North Carolina, Maryland, Wisconsin, and Delaware—adopted the NLC by July 1, 2000. A total of eighteen states joined the NLC by January 1, 2005. Finally, by October 1, 2015, twenty-five states had adopted the NLC. These twenty-five states form the basis for my analysis, though nine states have adopted the Compact since 2018.

In addition to collecting NLC enactment dates, I also construct a sample of nurses who are married to a military-service member. To do this, I use the 1990 Census along with the 2000 to 2018 American Community Survey ("ACS"). The ACS collects detailed demographic and labor market information in an ongoing annual survey in the years between the decennial census. In particular, these data include information about age, sex, race, income, occupation, education, usual hours worked, employment, and where one lives and works. The four-digit occupation codes in the ACS identify registered nurses, licensed practical nurses, and licensed vocational nurses, each of which is subject to the Nurse Licensure Compact (Evans, 2015). In addition, the ACS identifies whether an individual belongs to the armed forces, and I drop these individuals from the sample.

In order to study how the Nurse Licensure Compact affects military spouses, I must be able to identify whether someone is married to a military service member. The ACS data, as harmonized by IPUMS-USA, permits me to attach spousal information to each observation, including the spouse's occupation. Occupation is recorded as four-digit census occupation-classification scheme with separate codes for occupations within the armed forces. Thus, I can identify whether an individual's spouse is enlisted in the military with certainty, and can therefore generate a sample consisting solely of nurses married to military personnel. The ACS is a common data source to assess labor market outcomes for military spouses (Rapino and Beckhusen, 2013; Council of Military Advisors, 2018).

I construct several outcome and control variables using the ACS data. The outcomes of interest in this chapter are wages and weekly hours worked, as well as indicator variables for whether the individual is in the labor market, employed, is looking for work, worked last week, works in a different state, or works in a different Compact state. I also use the ACS data to control for age, race, gender, educational attainment, citizenship status, and the number of children in the household.

Summary statistics for military-spouse nurses are reported in Table 2. The baseline sample includes 1,829 observations. Approximately 90 percent of the sample are in the labor force and eighty-seven percent are employed. Military-spouse nurses work approximately 36 hours each week and earn nearly $33,895 each year. About three percent of individuals in the sample work in a different state and about one percent work in a different NLC state. Though not reported in Table 2, descriptive statistics from the ACS show that nurses married to civilians earn approximately $3,000 more each year than their counterparts with spouses serving in the military.

b. Empirical Methodology

To test the effect of the NLC on military-spouse nurses, I leverage the geographic and temporal variation in the roll-out of the NLC to construct a difference-in-difference model. The formal specification is:

$$Y_{ist} = B_0 + B_1 Treatment_{st} + B_2 X_{ist} + \zeta_s + \gamma_t + \epsilon_{ist} \qquad \text{(Equation 1)}$$

The outcomes of interest are reflected in Y_{ist}, and include: labor force participation, employment, weekly hours worked, wages, whether the individual is looking for work, worked last week, works in a different state, or works in a different Compact state. $Treatment_{st}$ is a binary variable equal to one if the individual currently resides in a Compact state and zero otherwise. X_{ist} represents a vector of control variables consisting of individual and demographic characteristics such as age, education, and race. ζ_s are state fixed effects and γ_t are time fixed effects. The coefficient of interest in Equation 1 is B_1.

I estimate Equation 1 using the sample of military-spouse nurses constructed from the ACS. Conditional on satisfaction of the parallel trends assumption, difference-in-difference estimation strategies can achieve causal identification (Angrist and Pishke, 2009). But using a sample of military-spouses permits me to exploit an addition source of exogeneity to identify the effect of the Nurse Licensure Compact. Cross-state moves by military spouses are based on military needs rather than spousal career opportunities, and so these moves are plausibly exogenous to labor market opportunities for either partner (Burke and Miller, 2016). Thus, in addition to leveraging geographic and temporal variation in the difference-in-difference model for identification, I can also leverage the exogenous nature of military moves to causally identify the effect of the NLC on labor-market outcomes for nurses married to service members.

c. Theoretical Model and Predictions

To frame my analysis, I develop a static model of an individual's labor market decisions after their spouse's military orders relocate them to a new state. Additionally, I use this theoretic model to generate predictions about the effect of the NLC on labor force participation, employment, wages, and similar outcomes for military spouses. To be sure, military-spouse nurses may be adversely affected by the NLC just as all nurses are, as evidenced by the analysis developed in chapter one of this dissertation. That is, military-spouse nurses that move to

Compact states may experience stronger competition in the labor market, and so the NLC may reduce their labor force participation, employment, wages, or hours worked.

However, the NLC may benefit military-spouse nurses because this group of nurses fundamentally differ from the broader nursing population in meaningful ways. Indeed, as discussed above, military spouses not only move more frequently than civilian spouses, but also move based on military orders rather than employment opportunities (Burke and Miller, 2016). Accordingly, this group of nurses may be more heavily burdened by the patchwork of state licensing regulations than nurses married to civilians. Thus, the reduced licensing restrictions under the Nurse Licensure Compact may have unique benefits for military spouses employed in nursing that are not enjoyed by other nurses.

To illustrate the potential benefits of the Nurse Licensure Compact for nurses married to military service members, consider the following model. Let W_N be wages from nursing, L be the licensure costs, and W_O be wages from other non-nursing positions. After moving, an individual will enter the labor market as a nurse in their new state when

$$W_N - L > W_O$$

Under the Nurse Licensure Compact, $L \rightarrow 0$. Thus, military-spouse nurses that move between Compact states will be more likely to enter the nursing labor market than those who do not move between Compact states. It also follows that military-spouse nurses in Compact states will also benefit on the intensive margins in terms of higher wages, longer hours worked, and a higher probability of having worked in the last week.

V. Results and Robustness Checks

This Part provides the primary results of this chapter. To summarize the baseline specification, I find that the Nurse Licensure Compact caused a five percent increase in labor force participation, eight percent increase in probability of employment, and a seven percent increase in the likelihood of working in the last week. In addition, my results indicate that the NLC has no statistically significant effect on hours worked, wages, whether an individual is looking for work, or whether the individual works in a different state or a different Compact state. I also explore whether multistate licensure benefited nurses that moved within the last year, and I find little evidence that the NLC improved labor-market outcomes for recent movers. Finally, I show that my primary results are largely robust to the inclusion of time trends as well as limiting the sample to nurses without master's degrees or higher that may be working as APRNs.

a. Primary Results

Table 3 reports estimates from the baseline difference-in-difference model. I find that the Nurse Licensure Compact has distinct benefits for military spouses. In particular, the NLC significantly increased labor force participation by five percent, the probability of employment by eight percent, and the probability of working in the last week by seven percent. However, the Compact had no statistically significant effect on weekly hours, wages, whether an individual is looking for work, or whether an individual works in a different state or a different NLC state. Overall, these results indicate that automatic multistate licensure through the Compact eased some labor-market frictions for nurses married to military personnel. Because this class of nurses moves across state lines seven times more often than their civilian counterparts, the patchwork of state regulated licensing regimes places particularly high burdens on this group of nurses. Plus, military spouses cannot plan for cross-state moves as easily as civilian spouses, further worsening the problems posed by state licensing requirements (Tang, 2018). Thus, the findings from this chapter indicate that the automatic recognition of out-of-state licenses under the NLC can increase employment outcomes for military-spouse nurses. More broadly, this finding suggests that Arizona and Utah's laws that recognize out-of-state licenses for military spouses will improve employment outcomes for licensed military spouses that move to those two states.

In addition to my primary findings, I also examine the effect of the NLC on military-spouse nurses that moved within the last year. To do so, I re-estimate the baseline specification but restrict the sample to individuals that moved across state lines within the last year. Because military families move across state lines substantially more frequently than civilians, the NLC may have stronger benefits for this sample of the nursing population. The results from Table 4, however, show that the NLC does not have a statistically significant impact on any of the outcomes measured in this chapter except that the Compact caused a twenty-eight percent reduction in whether an individual works in a different state.

b. Robustness Checks

I conduct two robustness checks in this chapter. First, I remove individuals with master's degrees or higher since these individuals may be working as advanced practice registered nurses rather than registered nurses. Because APRNs are not subject to the NLC (Evans, 2015), including them in the sample may bias my results. Until 2010, the ACS did not differentiate between registered nurses and advanced practice nurses in its four-digit occupation codes. The results presented in Table 3 may therefore be biased due to systemic measurement error. To overcome this problem, I remove 134 individuals with a master's degree or higher from the sample. Table 5 reports results from re-estimating the baseline difference-in-difference model on the revised sample of military-spouse nurses. I find that the main result pertaining to probability of employment is robust to the exclusion of APRNs from the sample—the NLC increased whether an individual was employed by seven percent. However, I no longer find that the NLC increased labor force participation or the likelihood of working in the last week at conventional levels of statistical significance.

I also re-estimate the baseline model with state-specific time trends. Results are presented in Table 6. I find that the Nurse Licensure Compact increased the likelihood of employment and whether the individual worked last week by fifteen percent and sixteen percent, respectively. These magnitudes align with those found in the baseline specification. However, I do not find that the NLC had a statistically significant effect on labor force participation when state specific time trends are included, though the coefficient is positive. Finally, under this specification, I find that that the NLC has no effect on weekly hours worked, wages, whether an individual is looking for work, or whether the individual works in a different state or a different Compact state.

VI. Conclusion

There are nearly seven hundred thousand military spouses in the United States. Evidence from the economics literature along with a growing body of government reports indicates that this group of individuals are more likely to be unemployed and earn less than their civilian counterparts (Burke and Miller, 2016; Council of Economic Advisors, 2018). One explanation for this phenomenon is that military spouses are more likely to be caught up by the patchwork of occupational licensing laws than civilian spouses. This is both because military families move across state lines seven times more often than civilian families and because military spouses are more likely to be employed in occupations that require a license (Council of Economic Advisors, 2018).

In this chapter, I assess how the Nurse Licensure Compact, which automatically grants military spouses employed as nurses with a multistate license to work in any other Compact state

(Evans 2015), affects labor market outcomes for military spouses employed as nurses. To do so, I construct difference-in-difference models that leverage geographic and temporal variation in the state-by-state adoption of the Nurse Licensure Compact. I estimate this model using a unique dataset created from the American Community Survey. The ACS, as harmonized by IPUMS-USA, permits me to attach spousal occupations to each observation. These characteristics include four-digit occupation codes that identify whether the spouse serves in the armed forces. With this methodology, I generate a sample of registered nurses and licensed practical nurses who are married to a military service person.

Using these data, I find that the Nurse Licensure Compact improves employment outcomes for military spouses. In particular, the baseline difference-in-difference results show that the NLC causes statistically significant increases in labor force participation by five percent, the probability of employment by eight percent, and the likelihood that an individual worked in the last week by seven percent. I find no effect of the NLC on weekly hours worked, wages, whether an individual is looking for work, or whether the individual works in a different state or a different Compact state at conventional levels of significance.

Economists and policymakers theorize that the regulatory costs associated with licensure requirements cause military spouses to leave the labor market or experience longer periods of unemployment than civilian spouses (Burke and Miller 2016; Council of Economic Advisors 2018). The results developed in this chapter suggest that automatically recognizing out-of-state licenses can improve labor force participation and the probability of employment for military spouses. Several states have passed legislation to automatically recognize out-of-state licenses for military spouses, and my findings suggest that this legislation will likely improve labor market outcomes for military spouses employed in licensed occupations when they move to their new state.

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Tables

Table 1: Adoption Years of the Nurse Licensure Compact

State	Adoption
Maryland	1999
Arkansas	2000
Delaware	2000
Iowa	2000
North Carolina	2000
Texas	2000
Utah	2000
Wisconsin	2000
Idaho	2001
Maine	2001
Mississippi	2001
Nebraska	2001
South Dakota	2001
Arizona	2002
Tennessee	2003
New Mexico	2004
North Dakota	2004
Virginia	2005
New Hampshire	2006
South Carolina	2006
Colorado	2007
Kentucky	2007
Rhode Island	2008
Missouri	2010
Montana	2015

Notes: Table 1 lists the year that each state adopted the Nurse Licensure Compact.

Table 2: Summary Statistics for ACS Sample

	Mean	Std. Dev.
Outcome Variables		
Labor Force Participation	0.90	0.30
Employed	0.87	0.34
Weekly Hours	36.43	9.71
Annual Wage	33,895.02	25,611.49
Looked for Work Last Week	0.55	0.50
Worked Last Week	0.84	0.36
Work in Different State	0.03	0.18
Work in Different NLC State	0.01	0.11
Control Variables		
Licensed Practical Nurse	0.13	0.34
Male	0.01	0.11
White	0.81	0.39
Black	0.08	0.28
Asian	0.06	0.24
Native	0.01	0.07
Other Race	0.04	0.20
High School Education	0.10	0.30
Some College Education	0.46	0.50
BA Degree	0.36	0.48
MA/Ph.D.	0.07	0.26
Age	34.81	8.47
Naturalized Citizen	0.06	0.24
Not a Citizen	0.04	0.19
No Children	0.31	0.46
One Child	0.24	0.43
Two Children	0.32	0.46
Three to Four Children	0.12	0.33
Five or More Children	0.01	0.09
No Children Under Age 5	0.69	0.46

Table Notes: This Table reports summary statistics for the main sample from the American Community Survey. There are 1,829 observations.

Table 3: Difference-in-difference estimates for the effect of the Nurse Licensure Compact on military-spouse nurses

VARIABLES	(1) Labor Force Participation	(2) Employed	(3) Weekly Hours	(4) Log(Wage)	(5) Looking for Work	(6) Worked Last Week	(7) Work in Different State	(8) Work in Different NLC State
NLC	0.05	0.08	-0.67	-0.09	0.03	0.07	-0.03	0.00
	(0.03)+	(0.03)**	(0.93)	(0.08)	(0.04)	(0.04)*	(0.03)	(0.01)
Observations	1,829	1,829	1,829	1,829	1,829	1,829	1,829	1,829
R-squared	0.10	0.11	0.09	0.35	0.50	0.11	0.11	0.11

Table notes: Table 3 reports results from a difference-in-difference model estimating the effect of the NLC on labor market outcomes for military-spouse nurses. Each specification includes state and year fixed effects, along with full controls. Controls include indicators for race, gender, educational attainment, children under age 5, citizenship status, and number of children. I also control for age and age squared. Robust, clustered standard errors are in parenthesis. ** $p<0.01$, * $p<0.05$, + $p<0.1$.

Table 4: Difference-in-difference estimates for the effect of the Nurse Licensure Compact on recent movers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Labor Force Participation	Employed	Weekly Hours	Log(Wage)	Looking for Work	Worked Last Week	Work in Different State	Work in Different NLC State
NLC	0.11	0.26	-4.88	0.10	0.23	0.27	-0.28	-0.20
	(0.27)	(0.22)	(6.47)	(0.35)	(0.22)	(0.24)	(0.12)*	(0.14)
Observations	205	205	205	205	205	205	205	205
R-squared	0.46	0.51	0.40	0.48	0.41	0.48	0.39	0.46

Table notes: Table 4 reports results from a difference-in-difference model estimating the effect of the NLC on labor market outcomes for military-spouse nurses that moved states within the last year. Each specification includes state and year fixed effects, along with full controls. Controls include indicators for race, gender, educational attainment, children under age 5, citizenship status, and number of children. I also control for age and age squared. Robust, clustered standard errors are in parenthesis. ** $p<0.01$, * $p<0.05$, + $p<0.1$.

Table 5: Difference-in-difference estimates for the effect of the Nurse Licensure Compact on military-spouse nurses without master's degrees

VARIABLES	(1) Labor Force Participation	(2) Employed	(3) Weekly Hours	(4) Log (Wage)	(5) Looking for Work	(6) Worked Last Week	(7) Work in Different State	(8) Work in Different NLC State
NLC	0.05	0.07	-0.92	-0.10	0.03	0.06	-0.03	0.00
	(0.03)	(0.03)*	(1.05)	(0.08)	(0.04)	(0.04)	(0.03)	(0.01)
Observations	1,695	1,695	1,695	1,695	1,695	1,695	1,695	1,695
R-squared	0.10	0.11	0.10	0.36	0.52	0.11	0.12	0.12

Table notes: Table 5 reports results from a difference-in-difference model estimating the effect of the NLC on labor market outcomes for military spouses without master's degrees or higher since this group may be working as APRNs. Each specification includes state and year fixed effects, along with full controls. Controls include indicators for race, gender, educational attainment, children under age 5, citizenship status, and number of children. I also control for age and age squared. Robust, clustered standard errors are in parenthesis. ** p<0.01, * p<0.05, + p<0.1.

Table 6: Difference-in-difference estimates for the effect of Nurse Licensure Compact on military-spouse nurses (including time trends)

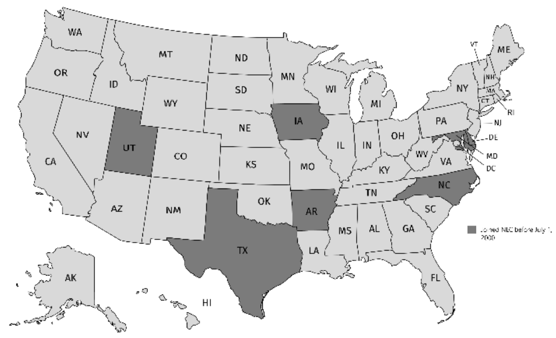
VARIABLES	(1) Labor Force Participation	(2) Employed	(3) Weekly Hours	(4) Log (Wage)	(5) Looking for Work	(6) Worked Last Week	(7) Work in Different State	(8) Work in Different NLC State
NLC	0.08	0.15	-2.75	-0.04	0.03	0.16	-0.00	-0.00
	(0.06)	(0.05)*	(2.28)	(0.15)	(0.10)	(0.06)*	(0.02)	(0.02)
Observations	1,829	1,829	1,829	1,829	1,829	1,829	1,829	1,829
R-squared	0.11	0.13	0.12	0.37	0.51	0.13	0.14	0.15

Table notes: Table 6 reports results from a difference-in-difference model estimating the effect of the NLC on labor market outcomes for military spouses. Each specification includes state and year fixed effects, along with state-specific time trends and full controls. Controls include indicators for race, gender, educational attainment, children under age 5, citizenship status, and number of children. I also control for age and age squared. Robust, clustered standard errors are in parenthesis. ** p<0.01, * p<0.05, + p<0.1.

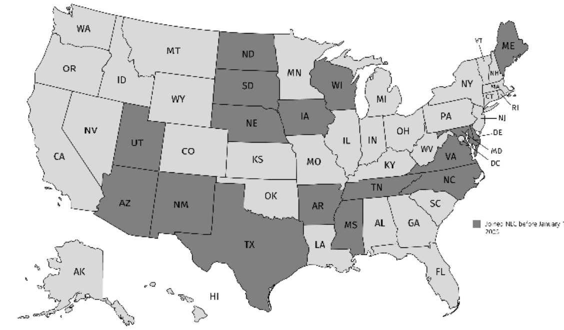
Figures

Figure 1: Adoption of NLC over Time

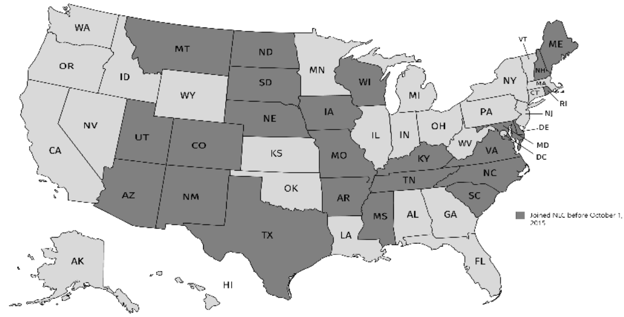
2000



2005



2015



Note: Highlighted states have entered the Nurse Licensure Compact by the end of the specified year.

Chapter 3: Scope Creep During COVID-19: Public Good or Public Harm?

I. Introduction

The COVID-19 pandemic has posed unique challenges to the healthcare sector and spurred numerous federal, state, and local government policy responses to combat the disease. In addition to well-publicized lockdown orders or emergency health declarations, many states also eliminated some or all scope-of-practice ("SOP") restrictions for advanced practice registered nurses ("APRNs") in early 2020 to help the healthcare sector respond to the pandemic.[3] In this chapter, I study the effect of these changes on weekly COVID-19 testing rates, case rates, fatality rates, non-COVID-19 fatality rates, and all-cause fatality rates.[4] I also estimate the effect of membership in the Nurse Licensure Compact ("NLC" or "Compact"), which automatically grants registered nurses in member states a multistate license to practice in any other member state, on each of the five outcomes listed above. Given the flexibility the NLC provides to recruit out-of-state nurses during emergencies, Compact states may respond better to the COVID-19 pandemic than non-Compact states (NLC Story 2020).

This chapter primarily analyzes changes to scope-of-practice restrictions for APRNs. Scope-of-practice laws are a subset of state occupational licensing laws that govern APRNs and determine which services they may provide and the conditions under which they may provide them (McMichael, 2018). Two classes of SOP laws are particularly important for APRNs. First, physician supervision laws require APRNs to be supervised by physicians as they perform their work. Second, prescriptive authority laws limit the medications that APRNs may prescribe. Many states permit APRNs to practice with full independence; that is, they can practice without

[3] The American Medical Association refers to expansion of APRNs' scope of practice as "scope creep." (Robeznieks, 2020).
[4] All-cause fatalities are fatalities from all causes of deaths.

any physician supervision or without any restrictions on the medications they may prescribe. As of January 2020, twenty-nine states granted APRNs full practice authority. During March and April 2020, fourteen of the remaining twenty-one states that restricted APRN practice removed or meaningfully reduced their SOP restrictions in response to the healthcare crisis posed by the pandemic.

Anecdotal evidence suggests that relaxing scope-of-practice restrictions helped “flatten the curve” and reduce COVID-19 cases and fatalities. For example, in May 2020, the Associate Chief Nursing Officer for Advanced Practice Nursing for Vanderbilt University Medical Center wrote in the *Tennessean* that, as a result of expanded SOP in Tennessee, ARPNs in that state have been able to build temporary assessment cites for COVID-19 patients and reduce hospital and emergency room visits by treating patients through telemedicine (Kapu 2020).

This chapter builds on this anecdotal evidence to empirically estimate the effect of APRN independence on COVID-19 testing rates, case rates, fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates. In theory, having fewer SOP restrictions should permit APRNs to perform more COVID-19 tests and to treat more patients, which should reduce COVID-19 case and fatality rates. Moreover, as APRNs play a broader role in the healthcare system, all-cause fatalities and non-COVID-19 fatalities could theoretically decrease as well. To test these theories, I use data from the Center for Disease Control and Prevention, the COVID-19 Tracking Project, and the *New York Times*, which has compiled case and fatality information from state and local health agencies. I leverage geographic and temporal variation to construct difference-in-difference models that identify the effect of changes to APRN scope of practice on the per-week testing, case, and mortality outcomes measured in this chapter between January 26 and September 26, 2020.

Overall, my results show that APRN independence has helped states respond more effectively to the pandemic. In particular, I find consistent evidence that APRN independence caused a reduction of approximately 0.63 weekly non-COVID-19 fatalities per 100,000 state residents during the time frame I studied. Relative to the mean, this amounts to a 3.4 percent reduction in the mortality rate from all non-COVID-19 deaths. I also find some evidence that APRN independence reduced COVID-19 fatalities and all-cause fatalities. Although the baseline difference-in-difference models report null effects of APRN independence on each of these three outcomes, difference-in-difference models that separate treatment timing into three-week intervals between January and September suggests that APRN independence reduced COVID-19 cases, fatalities, and all-cause fatalities. Furthermore, a cross-sectional model that compares states with full ARPN independence in January 2020 to states with restricted SOP also shows that states with APRN independence performed better than restricted states, as measured by all-cause fatality rates and non-COVID-19 fatality rates. Finally, I find no evidence that APRN independence affected testing rates.

Though this chapter focuses primarily on examining the effects of ARPN SOP laws, I also estimate the effect of states' membership in the Nurse Licensure Compact on the same five health outcomes listed above. This chapter thus builds on the work in chapter one, which studies how the NLC affects labor market outcomes, to assess the effect of the NLC for healthcare consumers. As of January 2020, thirty-two states had joined the NLC. For this section of the chapter, I estimate a cross-sectional model in which I compare states that had adopted the NLC prior to the start of the pandemic against states that had not adopted the Compact.[5] In theory, states that belong to the NLC should be able to better respond to nursing shortages during the

[5] Ohio is the only state to adopt the NLC during the pandemic. Ohio joined the Compact in July, 2020. For the purposes of this analysis, however, I consider Ohio an untreated state.

pandemic than states that do not belong to the NLC. However, I find that the NLC had no statistically significant effect on any of the five outcomes measured in this chapter.

This chapter contributes to two branches of the literature. First, I add to the developing occupational licensing literature assessing scope-of-practice laws in the healthcare sector. Like several articles in this literature (e.g. Markowitz et al. 2017; McMichael 2020), I find that more expansive SOP for advanced practice registered nurses improves patient outcomes. And, while I cannot say that the NLC is associated with improved consumer outcomes, I do find evidence that the NLC at least did not worsen outcomes. In addition, this chapter augments the growing body of knowledge regarding federal, state, and local policy interventions to combat COVID-19. Like the studies showing the efficacy of shelter-in-place orders (Dave et al. 2020) or mask mandates (Lyu and Wehby 2020), I find that expanding APRN scope of practice can be an additional tool for policymakers to combat the pandemic.

The chapter proceeds as follows. In Part II, I provide background information about scope-of-practice laws for APRNs before summarizing the relevant literature in Part III. Part IV describes my data along with my empirical strategy. Finally, Parts V and VI provide my main results along with several robustness checks, which largely corroborate my primary finding that APRN independence improved COVID-19 related outcomes during the pandemic.

II. Background: APRN Scope of Practice and Recent Changes

Advanced practice registered nurses ("APRNs") are registered nurses that have completed both a bachelor of science in nursing and a master of science degree, or higher, in nursing (Adams and Markowitz, 2018). There are four types of APRNs: nurse practitioners ("NPs"), clinical nurse specialists ("CNSs"), certified nurse midwives ("CNMs"), and certified registered nurse anesthetists ("CRNAs"). Nurse practitioners are the largest of these four groups, and there were approximately 180,000 NPs in the United States in 2018 according the Bureau of Labor Statistics. In addition, there were 6,300 CNMs, as well as 44,000 CRNAs.[6] Generally, APRNs receive less formal training than physicians, as most masters of science programs for APRN range between eighteen months and three years (McMichael 2020). Even so, APRNs play an important role in the healthcare system and perform the same functions as physicians in a variety of settings. There is in fact a strong consensus in the clinical literature that healthcare outcomes for APRNs exceed those of physicians when APRNs provide healthcare within the scope of their practice, education, and training (Laurant, et al. 2018). Relative to physicians, nurse practitioners are more likely to practice in primary care, to help underserved populations, and to work in rural areas (McMichael, 2020).

Although nurse practitioners and other APRNs often perform many of the same functions as physicians, especially in primary care or family practice medicine, they are subject to stricter occupational licensing regulations. The most important subset of these laws is scope-of-practice laws that govern which services they may provide and the conditions under which they may provide them (McMichael 2018). Two classes of SOP laws are particularly important for APRNs. First, physician supervision laws require APRNs to be supervised by physicians as they perform their jobs. Second, prescriptive authority laws limit the medications that APRNs may prescribe. As of January 2020, twenty-nine states permitted APRNs to practice with full independence; that is, they can practice without any physician supervision and without any restrictions on the medications they may prescribe (Phillips 2020; McMichael 2020).

During the COVID-19 pandemic, fourteen states that had restricted APRN scope of practice as of the start of 2020 relaxed their restrictions. This permitted APRNs to act with more

[6] Certified nurse specialists were not tracked by the Bureau of Labor Statistics.

independence and to serve patients without as much supervision from physicians or restrictions on the types of procedures they could perform. For example, in Michigan, Governor Whitmer issued Executive Order 2020-30, which permitted NPs to "provide medical services appropriate to the professional's education, training, and experience, without physician supervision and without criminal, civil, or administrative penalty to a lack of such supervision." In practice, this order suspended "restrictions in the scope of practice, supervision, and delegation to nurse practitioners" who are professionally employed and who are responding to the COVID-19 pandemic in their facility (Renke et al. 2020). This change in scope-of-practice restrictions permitted NPs in Michigan to perform functions in the healthcare sector that they could not perform previously. In an academic hospital near Detroit, Michigan, for example, a group of pediatric nurse practitioners with experience managing acute and critically ill patients were deployed to the COVID-19 ward to assist physicians with frontline care to critically ill patients there (Renke et al. 2020).

A second example from Tennessee also illustrates the ways in which changes to scope of practice helped the medical system combat the pandemic. Tennessee issued multiple executive orders that, collectively, waived chart-review requirements for APRNs, suspended the requirement of physician collaboration agreements needed to write prescriptions, and waived the requirement that an APRN's physician-supervisor conduct a site visit every thirty days (Tennessee Executive Orders 15 and 28). To understand the practical effect of these changes, consider the views of the Associated Chief Nursing Officer for Vanderbilt University Medical Center:

> "APRNs [in Tennessee] have been able to devote time typically used for administrative work to provide more immediate care and build temporary assessment sites for COVID-19 patients – tasks that have been extremely effective as patient volumes increased when the first wave of infections hit. They have also been able to keep hospital and Emergency

> Room visits down by treating patients at primary care clinics, in their private offices, in patient homes and through telemedicine – all of which were not possible prior to these [Executive Orders]." (Kapu 2020).

Section IV.b and Table 2 detail changes made by other states during the pandemic. Overall, the Michigan and Tennessee examples combined with evidence from the economics and clinical literatures finding that APRNs provide high-quality patient care suggest that expanding APRN scope of practice may theoretically increase COVID-19 testing rates while also decreasing cases, COVID-19 fatalities, and non-COVID-19 fatalities. There are several potential mechanisms driving these theoretical predictions. First, as evidenced by the example from a Michigan hospital, APRNs can provide additional care to COVID-19 patients, which may reduce COVID-19 fatalities. In addition, expanding APRN SOP permits nurses to assist with other areas of healthcare, such as treating patients with heart problems through telemedicine, which could reduce non-COVID-19 fatalities as well as COVID-19 case and fatality rates. Moreover, states with independent practice for APRNs have a more flexible healthcare system than states that restrict APRN independence. For example, in states with independent practice, physicians can assist with COVID-19 patients while APRNs help patients with other ailments, such as heart attacks or strokes. In this way, full-practice authority states may see lower all-cause mortalities during the COVID-19 pandemic. Finally, removing restrictions on APRNs may permit them to conduct more COVID-19 tests, which could ultimately reduce case rates if infected patients remain home rather than spread the disease to others.

III. Literature Review

This chapter contributes to two strands of the literature. First, I add to the occupational licensing literature that assesses the effect of scope-of-practice regulations on public health outcomes. Second, I contribute to the growing number of studies analyzing the effect of public policy interventions on the COVID-19 pandemic. Overall, my results align with other findings in the literature that APRN independence benefits public health, although I find no evidence that multistate licensure under the NLC improved any of the outcomes measured in this chapter. These findings suggest that broader scope of practice for APRNs can help states combat public health crises like the coronavirus.

This chapter relates to the literature examining the effect of scope-of-practice laws for APRNs or physician's assistants ("PAs") on labor market and healthcare outcomes. Looking first at the labor market literature, studies typically find that independent practice improve wages, hours, mobility, and labor supply for APRNs. For example, Kleiner et al. (2016) find that NP earnings increase as APRN independence increases. Conversely, Markowitz and Adams (2020) use the National Sample Survey of Registered Nurses data to show that SOP restrictions are not strong determinants of many labor market decisions, such as employment, wages, migration, or self-employment, although hours worked and self-employment increase when NPs practice in less restricted regulatory environments. McMichael (2018) shows that NP independence increases the supply of NPs, particularly in areas with health provider shortages, but that PA independence has no effect on labor supply. Perry (2012) shows that NPs are less likely to move from states that grant them prescriptive authority, and Reagan and Salberry (2013) show that restricting SOP for NPs reduces labor supply.

In addition, several studies examine the effect of NP and PA scope of practice on healthcare outcomes. Markowitz et al. (2017) find that SOP laws for certified nurse midwives have no effect on maternal or infant health outcomes, but that states with no barriers to practice have higher probabilities of CNM-attended births. McMichael (2020), however, shows that APRN independence significantly reduces the rates of several labor and delivery procedures, such as C-sections and inductions, by between one and two percentage points. He also finds similar results for PA scope of practice. Similarly, Yang et al. (2016) find, among other utilization outcomes, that women in states with autonomous practice for nurse midwives were 13 percent less likely to have a pre-term birth and 11 percent less likely to have babies with a low birth weight. Kleiner et al. (2016) show that independence in prescription writing for NPs has no effect on infant mortality rates. Looking at the healthcare system more broadly, Traczynski and Udalova (2018) demonstrate that granting NPs autonomy increases access to care, reduces emergency department visits for primary care, reduces healthcare costs by up to 1.3 percent, and increases healthcare utilization in underserved populations.

Other research in this literature focuses on the effect of APRN independent practice on prescription drug prescribing. McMichael (2018) shows that NP independence increases NP opioid prescribing rates. Similarly, Stange (2014) finds that NP prescriptive authority is associated with modest increases in office-based visits, and Spetz et al. (2013) find that independent NP prescribing is associated with a higher probability of prescriptions being filled.

The second literature that this chapter contributes to is the growing efforts to analyze the effect of state-level policy changes on the COVID-19 pandemic. Two notable articles examine shelter-in-place orders. First, Dave et al. (2020) find that shelter-in-place orders ("SIPOs") decreased cumulative COVID-19 cases by over 50 percent three weeks after the orders were adopted. Second, Friendson et al. (2020) show that California's statewide SIPO implemented on March 19 decreased cases by between 125 and 220 cases per 100,000 people by April 20. Assessing the effect of mask mandates for fifteen states and Washington, D.C., Lyu and Wehby (2020) demonstrate that these mandates reduced COVID-19 growth rates by between 1 and 2 percent, as measured in five-day increments after their adoption, between March 31 and May 22.

In absolute terms, the authors estimate that more than 200,000 cases were averted due to these mandates. Finally, Courtemanche et al. (2020) examine the effects of (1) banning large social gatherings; (2) closing schools; (3) closing entertainment venues, gyms, bars, and restaurants; and (4) enacting shelter-in-place orders on COVID-19 case rates. They find that, between March 1 and April 27, shelter-in-place orders prevented approximately ten million COVID-19 cases. Moreover, the authors find that without any of the four measures examined in their paper, there would have been thirty-five million more cases over the period studied.

I contribute to both literatures outlined above. This paper is the first to explore the effect of scope-of-practice laws on health outcomes from the COVID-19 pandemic, and, like most of the literature, I find that less restrictive SOP laws have beneficial health outcomes. I also contribute to the growing COVID-19 literature by assessing the effect of an unstudied state policy intervention—namely, broader SOP laws—on COVID-19 outcomes. Evidence from this chapter indicates that independent practice for APRNs is an effective tool to combat the pandemic.

IV. Description of Data and Empirical Strategy

In this section, I first detail the sources of outcome, treatment, and control data used in this chapter. Next, I describe the empirical specifications used to produce my results. In order to assess the effect of SOP changes in March and April, I leverage cross-state, cross-time variation in changes to state's SOP laws to estimate difference-in-difference models for each testing, case, and fatality rate outcome. This section also describes the cross-sectional approach used to study how states that belong to the NLC compared to states that did not adopt the Compact.

a. Outcome Data

The outcome data for this project are drawn from several publicly available sources. First, I collect mortality data from the Center for Disease Control and Prevention ("ll-cause data"). This dataset consists of provisional counts of death by week, state, and underlying cause of death. I use 2020 data spanning January 26 through September 26, a time frame that closely aligns with other data I collect from the *New York Times*' COVID-19 data repository for case rates. The CDC mortality data include weekly fatality counts from all causes, natural causes, COVID-19, and several other diseases, such as diabetes or Alzheimer's disease. I use these data to construct three outcome variables: the all-cause fatality rate, COVID-19 fatality rate, and the non-COVID-19 fatality rate. I construct the non-COVID-19 fatality rate by subtracting the number of COVID-19 fatalities from the number of "all-cause" fatalities. To normalize these three outcomes across states, I convert each raw fatality measure into fatality rates per 100,000 state residents per week.

I also collect data on county-level COVID-19 case counts and state-level COVID-19 testing rates. COVID-19 cases are drawn from state and local health agency data made public in the *New York Times*' COVID-19 data repository. The *New York Times* data records cumulative daily infections and fatality counts for every county in the United States. To normalize the data across counties, I convert the *New York Times* data counts to weekly infection rates per 100,000 county residents. The sample currently spans January 26, 2020 to September 26, 2020. Data on COVID-19 testing rates are provided by the *Atlantic*'s COVID-19 tracking project. This data tracks the total number of COVID-19 tests conducted during the pandemic at the state level. Similar to the other outcomes in this chapter, I convert this data to weekly test rates per 100,000 state residents.

In addition, I collect state- and county-level information related to other COVID-19 policy interventions as well as economic, demographic, or other characteristics that are correlated with COVID-19 test, case, or fatality rates. In particular, I control for shelter-in-place orders, mask mandates, non-essential business closure orders, and emergency orders at the state level using sources compiled by researchers at Boston University (Raifman et al., 2020). In the models estimating the effect of the NLC, I include a host of control variables at the county level, such as population, population density, median income, educational attainment, physicians per capita, nurse practitioners per capita, physician's assistants per capita, unemployment rate, percent of the county that is white, percent male, and the state governor's party affiliation. Summary statistics, along with a full list of control variables, are provided in Table 1. The county characteristics data were collected by researchers from Johns Hopkins University (Killeen et al., 2020).

Table 1 reports summary statistics for the outcomes of interest, treatment variables, and control variables. The sample includes 112,234 observations at the county-week level. There were approximately 754 tests per 100,000 state residents conducted during the sample period, along with 1.3 COVID-19 fatalities, 19.1 all-cause fatalities, and 17.8 non-COVID-19 fatalities per 100,000 state residents. In addition, there were 51.8 COVID-19 cases per 100,000 county residents. Roughly 58.5 percent of the counties were in states that granted APRNs full independence or states that greatly expanded APRN SOP during the pandemic, and 74.4 percent of counties were in Compact states. Finally, 19.8 percent of the sample is covered by a shelter-in-place order, 33.4 percent has a mask mandate, 15.6 closed non-essential businesses, and 81.2 percent had an emergency order.

In addition, Figure 3 provides a time-series plot for each of the five outcome variables measured in this chapter. Each outcome is measured at the weekly level. Figure 3 shows that COVID-19 case rates increased throughout the first twenty-six weeks of the pandemic, before declining slightly over the next ten weeks. COVID-19 fatalities and all-cause fatalities sharply increased in late March, approximately ten to twelve weeks into the pandemic. Non-COVID-19 fatalities increased slightly around this time as well before steadily decreasing through the week of September 19, 2020. Finally, COVID-19 testing rates largely increased during the sample period, although testing rates do flatten at approximately twenty-five weeks into the data.

b. Source of Treatment Data

I combine information from several sources to construct the treatment variables used in this chapter. First, I collect information on the "pre-COVID-19" scope-of-practice landscape from McMichael and Markowitz (2020), which uses statutory and regulatory language to code nurse practitioner scope-of-practice laws for each state over the last twenty-three years. Though the coding scheme from McMichael and Markowitz (2020) focuses on nurse practitioners, relying on this coding scheme is appropriate for this project since nurse practitioners are the largest group of APRNs and because NP scope-of-practice correlates with other APRN groups (see e.g. McMichael 2020). In line with the best practices developed in this paper, I code each state as having either independent practice for APRNs or restricted practice for APRNs. I define states as granting independent practice when APRNs can practice without any physician supervision or without any restrictions on the medications they may prescribe. Pre-pandemic full-independence states are shaded in dark grey in Figure 1.

Next, I collect information on whether and how states with restrictive scope-of-practice laws for APRNs expanded those restrictions during the first several months of the pandemic.

Changes to APRNs' scope of practice are tracked by the American Association of Nurse Practitioners ("AANP"). I also verify these changes by cross-referencing the AANP's list with the executive or administrative order that is the primary source for the change. Figure 1 shows states that have not changed their restrictive scope-of-practice laws during the pandemic in white along with states that have meaningfully expanded their restricted scope-of-practice laws in medium grey.

States varied in the ways in which they expanded APRN scope of practice. New Jersey, Louisiana, Kentucky, and Wisconsin eliminated physician-supervision practice agreement requirements for all APRNs such that these four states in effect granted APRNs full, unsupervised practice. Other states, such as Massachusetts, suspended practice agreement requirements for all APRNs with over two years' experience. Still other jurisdictions, such as Pennsylvania, Tennessee, and South Carolina, relaxed restrictions on written prescription guidelines. In addition, several states—including Michigan, North Carolina, Alabama, and Indiana—made it easier for APRNs to work in new practice areas by, among other things, waiving requirements that APRNs update supervisory agreements before being transferred to practice in a new facility. Tennessee and Missouri waived red-tape administrative requirements that limited APRNs' independence, such as chart review, site visits by supervising physicians, and requirements that APRNs practice within 75 miles of their supervising physician. Although these latter changes were less extreme than those in, say, New Jersey, anecdotal evidence suggests that even these smaller changes to supervisory regulations played a large role in combatting COVID-19 in the early weeks of the pandemic (Kapu, 2020). Finally, some states, like California and Oklahoma, only eliminated restrictions on the number of APRNs each physician could supervise. Because some healthcare experts opine that eliminating these restrictions has little practical benefit, I code these states as untreated for the purposes of my analysis. (Bluth, 2020). Table 2 summarizes the timing and key elements of each state's change to its SOP laws for APRNs.

Finally, data on states that belong to the NLC is provided by the National Council of State Boards of Nursing. As of January 2020, thirty-three states belonged to the Nurse Licensure Compact. NLC states are highlighted in grey in Figure 2.

c. Empirical Methodology

To estimate the effect of APRN independence during the COVID-19 pandemic, I leverage geographic and temporal variation in the adoption of APRN independence to construct a difference-in-difference model that identifies the effect of the APRN scope-of-practice changes on COVID-19 test rates, case rates, fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates. Formally, I estimate:

$$Y_t = \mathrm{B}_0 + \mathrm{B}_1 APRN\ Independence_{st} + \mathrm{B}_2 X_{ct} + \delta_t + \zeta_c + \epsilon_{ct} \quad (1)$$

where $APRN\ Independence$ is a binary variable equal to one if state *s* had independent practice as of January 2020 or meaningfully expanded its scope of practice during the pandemic, and zero otherwise. As discussed in the prior section, restricted states used a spectrum of approaches when relaxing SOP for APRNs in their state. However, because anecdotal evidence from Michigan (Renke et al., 2020) and Tennessee (Kapu, 2020) suggest that even partially removing restrictions on APRN SOP had meaningful impacts on patient outcomes, I define any states with significant changes to their SOP regime as treated (or equal to one). The outcome variables, Y_t, are COVID-19 testing rates, case rates, fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates. Case rates are measured at the county-week level while the other four outcome variables are measured at the state-week level. I also include county fixed effects, given by ζ_c, and time fixed effects, given by δ_t. Finally, I include a vector of control variables, X_{ct}, which includes average temperature,[7] whether the state was under a shelter-in-place order, mask mandate, partial business closure, or emergency order.

Next, to estimate the effect of the NLC on the outcomes of interest measured in this chapter, I use a cross-sectional model that exploits cross-state variation in whether states have adopted the Nurse Licensure Compact. Formally, I estimate the following equation:

$$Y_t = \mathrm{B}_0 + \mathrm{B}_1 NLC_s + \mathrm{B}_2 X_{ct} + \delta_t + \epsilon_{ct} \qquad (2)$$

where Y_t represents my outcomes of interest. *NLC* represents a binary variable equal to one if the state belonged to the Nurse Licensure Compact as of January 2020, and zero otherwise. I also include time fixed effects, reflected in δ_t. Equation 2 includes a vector of control variables, X_{ct}, for state-level shelter-in-place orders, mask mandates, or emergency orders, as well as many county-level demographic and economic characteristics, such as unemployment rate, median income, percent white, and percent in poverty. I also control for average temperature each month at the county level since temperature affects COVID-19 transmissibility (Sajedi et al., 2020).

V. Results

This section details my findings for the effect of APRN independence on the five health outcomes measured in this chapter: COVID-19 test rates, case rates, fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates. In general, I find consistent evidence across several models that greater APRN independence reduces non-COVID-19 fatality rates by roughly 3 percent. In addition, I find some evidence that fewer SOP restrictions reduced COVID-19 cases, fatalities, and all-cause fatalities over the course of the pandemic. However, I find that

[7] Research shows that temperature affects COVID-19 transmission rates. (Sajedi et al., 2020).

APRN independence had no effect on testing rates. Finally, I find no effect of the NLC on the outcomes measured in this chapter.

a. Baseline Results

Table 3 reports results from the baseline difference-in-difference estimation of the effect of APRN independence on the health outcomes measured in this chapter: the COVID-19 test rate, case rate, fatality rate, all-cause fatality rate, and non-COVID-19 fatality rate. I find that scope-of-practice changes had no statistically significant effect on the testing rate, case rate, COVID-19 fatality rate, or the all-cause fatality rate. However, the baseline difference-in-difference model shows that APRN scope-of-practice expansions caused a statistically significant reduction of roughly 0.63 weekly non-COVID-19 fatalities per 100,000 state residents. This amounts to a 3.3 percent decrease relative to the mean fatality rate of 17.8. This result aligns with anecdotal evidence that laws APRN independence permits APRNs to provide care for all types of patients and is also consistent with the literature showing that expansive scope of practice for APRNs benefits public health (e.g. Markowitz et al.; McMichael, 2020). Indeed, half of APRNs surveyed by the American Association of Nurse Practitioners in August reported that expanding scope of practice was beneficial in meeting patient needs during the pandemic, and the findings from the baseline difference-in-difference model corroborate this story (Heath, 2020).

Interestingly, Table 3 also shows that expanding APRN scope of practice increased COVID-19 fatalities by 0.18 deaths per 100,000 state residents—though this result is not statistically significant at conventional levels. Even so, the positive coefficient in this specification may result from endogeneity in the timing of when states expanded their scope of practice for APRNs; that is, many states changed their SOP laws just prior to a large increase in

COVID-19 fatalities. This source of endogeneity is discussed in more detail below, and I use several different approaches to address this problem.

b. Heterogenous Effect of SOP Laws Over Time

The reallocation of advanced practice nurses to COVID-19 wards or delays in implementing systems to treat new types of patients may result in a lag between when states reduced SOP restrictions and when those changes become effective. Indeed, the event study plotted in Figure 11 and the corresponding discussion in Section VI.A suggest that reducing SOP restrictions had a delayed effect on COVID-19 fatality rates. Therefore, to further explore the heterogenous effect of SOP laws over time, I adopt a model described in Dave et al. (2020), in which the authors estimate the effect of shelter-in-place orders on COVID-19 cases in five-day increments following the adoption of the order. I adapt this model to estimate the effect of scope-of-practice laws on COVID-19 outcomes in three-week increments. Formally, I estimate the following equation:

$$
\begin{aligned}
Y_t = \mathrm{B}_0 + \ & \beta_1 APRN\ Independence_{Week0to2} + \beta_2 APRN\ Independence_{Week3to5} \\
& + \mathrm{B}_3 APRN\ Independence_{Week6to8} + \cdots + \beta_9 APRN\ Independence_{Week21to24} \\
& + B_{10} APRN\ Independence_{Week25plus} + X_{ct} + \delta_t + \zeta_c + \epsilon_{ct}
\end{aligned}
$$

Here, $APRN\ Independence_{Week0to2}$ is an indicator variable equal to one for the first three weeks after an SOP law is enacted and 0 otherwise, $APRN\ Independence_{Week3to5}$ is an indicator variable equal to one for the next three weeks after enactment and zero otherwise, and so on. I also include the same county- and state-level control variables as in the base model as well as state and time fixed effects. I estimate this model for each outcome of interest in this chapter and plot the coefficients for each of the treatment variables in Figures 4 through 8. That is, Figure 4 reports the estimated coefficients for the variables $APRN\ Independence_{Week0to2}$, $APRN\ Independence_{Week3to5}$, etc., but not coefficients for the control variables such as whether the state has a mask mandate in place.

Figures 4 through 8 plot coefficients for the effect of SOP laws on test rates, case rates, COVID-19 fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates, respectively. Figure 4 shows that APRN independence had little effect on testing rates. However, Figure 5 shows that, over time, APRN independence decreased case rates, as the coefficients for both "21 to 24 weeks" and "25-plus weeks" are statistically significant and negative. In addition, I find consistently negative coefficients for all periods after "12 to 14 weeks" in the model estimating the effect of SOP changes on COVID-19 fatality rates, as illustrated in Figure 6. Here, after an initial spike in COVID-19 fatalities, the plotted coefficients turn negative, and, for weeks 25-plus, are different from zero at the 10 percent level. This trend provides some evidence that expanding APRN scope of practice reduced COVID-19 fatalities over time. Moreover, Figure 7 shows that APRN independence causes a significant and negative effect of APRN independence on the all cause fatality rate. Finally, Figure 8 plots the effect of APRN independence on non-COVID-19 fatality rates. From the "3 to 5 weeks" period on, I find that broader SOP laws significantly reduced non-COVID-19 fatality rates by between 0.7 and 1.7 fatalities per 100,000 state residents. Overall, Figures 4 through 8 indicate that reducing scope-of-practice restrictions for APRNs helped combat the coronavirus pandemic over time, as reflected by the downward trend in case and fatality rates for COVID-19, and helped the healthcare system more broadly by reducing fatalities from non-COVID-19 causes.

c. Cross-Sectional Analysis

The event study for the COVID-19 fatality rate outcome in Figure 11 indicates that states that removed their APRN scope-of-practice restrictions did so just prior to a large increase in fatalities from COVID-19. Indeed, this trend aligns with the practical reality of the early pandemic, as many states changed their scope-of-practice laws in late March and early April just before the first wave of COVID-19 cases. Accordingly, it may be the case that the anticipated spike in COVID-19 cases and fatalities caused states to reduce scope-of-practice restrictions for APRNs. Moreover, this phenomenon may also explain the fact that APRN independence had a positive, though statistically insignificant, effect on COVID-19 fatalities in the baseline model. To overcome this potential source of endogeneity, in this section I estimate a cross-sectional model in which I compare states with full independence for APRNs at the start of the pandemic with states that restricted APRN scope of practice. In this model, I treat the COVID-19 pandemic as an exogenous shock to the healthcare sector. This assumption is plausible given that each state had established their scope-of-practice regime in January 2020 without any anticipation of a worldwide pandemic.

Formally, I estimate the following model:

$$Y_{ct} = \mathrm{B}_0 + \mathrm{B}_1 PreCOVID\ Independence_{\mathrm{s}} + \mathrm{B}_2 X_{c,t} + \delta_t + \epsilon_{ct}$$

where Y_{ct} represents my outcomes of interest, $PreCOVID\ Independence$ is a binary variable equal to one if state s granted APRNs full independence in January 2020 and zero otherwise; $X_{c,t}$ represents a vector of county- and state-level control variables, such as education, income, healthcare professionals per capita, mask mandates, shelter-in-place orders, and population; and δ_t represents time fixed effects. The cross-sectional nature of this model does not permit me to include state fixed effects. However, the rich set of control variables and time fixed effects, as well as the fact that each state's scope-of-practice laws were adopted prior to the pandemic, offer a plausible means of identifying the effect of APRN independence on healthcare outcomes.

Table 4 reports estimates from the cross-sectional model. Consistent with the baseline results, I find that full independence for APRNs has no statistically significant impact on COVID-19 testing rates, case rates, or fatality rates. However, I do find that independent practice is associated with a statistically significant decrease of 1.6 all-cause fatalities per 100,000 people and 1.3 non-COVID-19 fatalities per 100,000 people. Together, these results suggest that reducing barriers to practice for nurse practitioners helped states better absorb shocks to the healthcare sector.

d. Nurse Practitioner Supply

In this section, I extend the baseline model to examine whether the effects of APRN independence vary with the number of APRNs per capita. To do so, I re-estimate the baseline difference-in-difference model with an interaction term for the number of NPs per 100,000 state residents. I report estimates from this model in Table 5. In general, I find that the interaction term APRN Independence * NPs per 100k is not statistically indistinguishable from zero at conventional levels. But, I do find that this interaction term is negative and statistically significant at the 10 percent level in the model for county-level COVID-19 case rates. Here, each additional nurse practitioner per 100,000 residents in a treated state is associated with 0.2 fewer COVID-19 cases per 100,000 people.

e. Nurse Licensure Compact

Table 6 reports results from the cross-sectional model estimating the effect of the Nurse Licensure Compact on the five healthcare outcomes measured in this chapter. This model includes time fixed effects and a host of economic, educational, demographic, and similar control variables. Overall, I find that the NLC has no statistically significant effect on any outcome measured in this chapter. In particular, Table 6 shows that the NLC is associated with a decrease in the COVID-19 fatality rate by 0.06 fatalities per 100,000 people; a reduction in all-cause fatality by 0.35 deaths per 100,000; and a decrease in non-COVID-19 fatalities of 0.29 deaths per 100,000 people. However, none of these findings were statistically significant at conventional levels. Curiously, Table 6 also shows that the NLC is associated with reduced testing by 39.8 fewer tests per 100,000 and increased cases of 9.8 per 100,000, although neither of these results is statistically significant at conventional levels. In short, the evidence that NLC states performed better during the pandemic is scant, suggesting that advocates' claims that the NLC permits states to better respond to healthcare crises may be overstated (NLC Story, 2020). However, I do not find any strong evidence that these states performed *worse* than others during the pandemic according to the measures employed in this chapter.

VI. Robustness Checks

In this section, I perform several robustness checks for the validity of my primary models. First, I conduct event studies to (1) test whether the parallel trends assumption is satisfied (Angrist and Pishke, 2009), and (2) to explore the heterogenous effects of changes to APRN scope of practice over the course of the pandemic for each of the five outcome variables measured in this chapter. Next, I divide my sample into urban/suburban and rural counties to further explore possible endogeneity between changes to APRN scope of practice during the pandemic and COVID-19 case and fatality rates. Finally, I re-estimate the baseline model after dropping several "marginally treated" states to confirm that these states are not driving my results. I find that my primary findings are robust to each of these changes.

a. Event Study Analysis

Given the rapidly changing environment of the COVID-19 pandemic, it is especially important to understand how the pandemic was affecting states prior to the adoption of any particular policy. In addition, satisfying the parallel trends assumption is integral to properly identifying the effect of a policy change in a difference-in-difference model (Angrist and Pishke, 2009). Given these two interests, I conduct event studies for each outcome variable measured in this chapter to visually illustrate the effect of APRN scope-of-practice changes in the weeks prior to and following the law change. I graph these results in Figures 9 through 13.

First, I find strong evidence in each event study that the parallel trends assumption is satisfied. In particular, these studies show that each of the periods prior to the expansion of APRN SOP during the pandemic are not statistically distinguishable from zero, with one exception. Figure 13 shows one statistically significant and negative coefficient for the non-COVID-19 fatality rate outcome two weeks prior to the adoption of scope-of-practice changes. However, given that each of the other pre-periods trend in a flat line and only one of thirty-five pre-periods is different from zero across all five event studies, the single pre-period that is different from zero presents substantially less cause for concern. In addition, each of the other four event studies show no evidence of pre-trends that may bias my results, further reducing the possibility that the parallel trends assumption is systemically violated.

In addition, the event study for COVID-19 fatalities plotted in Figure 11 is illustrative of the heterogenous effects of relaxing SOP laws over time. After an initial sharp spike in COVID-19 fatalities, Figure 11 shows COVID-19 fatalities decreasing over time. This pattern likely explains the positive effect of APRN independence on fatality rates in the baseline difference-in-difference model. Difference-in-difference models estimate average effects across all periods (Goodman-Bacon, 2018), and the spike in COVID-19 fatalities immediately after SOP restrictions were relaxed in March and April outweighed the eventual decline in fatalities associated with relaxing these restrictions. In short, the baseline model misrepresents the full picture: after the initial spike in fatality rates at the start of the pandemic, fatalities trended downward, suggesting that reducing SOP restrictions may have had some effect on COVID-19 fatalities not captured in the baseline model. To be sure, none of the post periods are statistically different from zero, but even so this general pattern offers some evidence that APRN independence decreased COVID-19 fatalities.

b. Effect of APRN Independence in Rural Counties

In this section, I create two subsamples of urban/suburban counties and rural counties in order to address the potential endogeneity of states expanding APRN scope of practice just prior to a sharp increase in cases and fatalities during the first wave of the pandemic in March and April. Given the increased spread of COVID-19 in densely populated areas, urban and suburban counties were the focal point of the pandemic in its early stages while rural counties were temporarily spared. Thus, states that relaxed their SOP laws arguably did so because of the pandemic within urban counties, and APRN independence can therefore be perceived as orthogonal to the pandemic's progression in rural counties in spring 2020.

Before presenting my estimates, note that due to limitations in the CDC data, I can only estimate the effect of APRN independence on the two outcomes for which I have county-level data: COVID-19 cases and COVID-19 fatalities. The county-level data is derived from the *New York Times*, which provides the source for the COVID-19 case rate outcome used in my primary analysis. Like it does for case data, the *New York Times* provides county-level cumulative fatalities for COVID-19, and I convert the cumulative fatality counts into weekly fatality rates per 100,000 county residents. I define urban, suburban, and rural counties in accordance with the

National Council for Health Statistics ("NCHS") Urban-Rural Classification scheme for this analysis.[8]

Table 7 reports difference-in-difference results for each sample divided into two panels: Panel A reports results for the urban and suburban sample; Panel B reports results from the rural sample. The results presented in this table indicate that APRN independence has a heterogenous effect by level of urbanization. First, I find positive, though statistically insignificant, effects of ARPN independence on COVID-19 case and fatality rates in urban and suburban counties. Conversely, I find that APRN independence has a negative and statistically insignificant effect on COVID-19 case and fatality rates in rural counties. In conjunction, these findings suggest that states granted APRN's more independence in response to growing case and fatality rates in urban or suburban counties. Thus, the finding in the baseline model that APRN independence increased fatalities may be driven by the explosion of the pandemic in urban counties.

Figures 14 and 15 plot event studies for the two county-level COVID-19 case and fatality rate outcomes used in this section using the rural counties sample. Figure 14 shows little connection between APRN independence and COVID-19 cases in rural counties. However, Figure 15 shows that, from the sixteenth "post-passage week" onward, APRN independence decreases COVID-19 fatalities, although this decrease is not statistically significant at conventional levels.

c. Remove Five States with Highest Case Rates

As discussed in previous sections, states that anticipate being overwhelmed by the pandemic may relax their SOP laws prior to sharp increases in COVID-19 cases or fatalities. As

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[8] I classify a county as urban if the NCHS classification of a county is "Large central metro" or "Large fringe metro." I classify a county as suburban if the NCHS classification of a county is "Medium metro" or "Small metro." Finally, I classify a county as rural if the NCHS classification is "Micropolitan" or "Noncore" (U.S. Department of Health and Human Services 2014).

a result, positive coefficients on the ARPN independence variable may lead to the erroneous conclusion that removing restrictions on APRNs increased COVID-19 cases or fatalities, when in reality this relationship is driven by the timing of changes to SOP restriction relative to COVID-19 growth rates in the population. To address this potential endogeneity, I remove the five states with the highest COVID-19 case rates as of March 24, which represents the beginning of week 10 in my data and corresponds to approximately the start of the first wave of the pandemic. These states are New Jersey, Massachusetts, New York, Louisiana, and Connecticut. Louisiana and New Jersey both had restricted APRN scope of practice prior to the pandemic, but removed these barriers completely on March 31 and April 1, respectively.

Table 8 reports results from difference-in-difference models estimating the effect of relaxing SOP laws on the five outcomes measured in this chapter using a sample that does not include New York, Massachusetts, Louisiana, Connecticut, and New Jersey. I find that ARPN independence caused a decrease of 0.57 non-COVID-19 fatalities per 100,000 state residents, relative to a mean rate of 17.8 fatalities per 100,000. I do not find that APRN independence had a statistically significant effect on any of the other outcomes measured in this chapter at conventional levels. Overall, these findings align with my baseline difference-in-difference results showing that expanding APRN independence reduced non-COVID-19 fatalities during the pandemic but had little effect on the other outcomes measured in this chapter.

d. Remove Marginally Treated States

As described in Section IV.b, some states expanded APRN scope of practice more broadly than others. In particular, the changes in Missouri and Alabama related primarily to supervisory obligations rather than relaxing SOP restrictions. To be sure, anecdotal evidence indicates that even changes like these still had a meaningful effect on patient care and reduced

COVID-19 case and fatality rates (Kapu, 2020). But even so, a cleaner approach may be to limit the analysis strictly to states that expanded scope of practice while coding states that only changed supervisory obligations as untreated. Thus, I re-estimate the baseline difference-in-difference models and define the SOP variable for Missouri and Alabama as zero.

Table 8 reports results from this model and shows that my primary results are robust to dropping the two "marginally treated" states from the analysis. Again, I find that APRN independence had no statistically significant effect on test rates, case rates, COVID-19 fatality rates, or all-cause fatality rates. However, I again find that the APRN independence decreased the non-COVID-19 fatality rate by roughly 0.62 deaths per 100,000 people. This finding is statistically significant at the 10 percent level.

VII. Conclusion

The COVID-19 pandemic has strained the healthcare sector and prompted numerous policy responses from federal, state, and local governments. In this chapter, I examine one yet to be studied policy change: the expansion of APRN scope of practice by fourteen states that restricted their SOP prior to the pandemic. Using a difference-in-difference model in which I exploit geographic and temporal variation in the adoption of SOP changes, I find that expanding SOP for APRNs during the pandemic reduced non-COVID-19 fatalities by approximately 0.63 fatalities per 100,000 state residents. In addition, I find some evidence that less restrictive SOP laws reduced COVID-19 cases and fatalities in models that estimated the effect of these laws in three-week intervals following their initial passage date. However, I find little indication that less restrictive scope-of-practice laws affected COVID-19 testing rates.

In addition to studying scope-of-practice changes amid the COVID-19 pandemic, this chapter also examines whether the Nurse Licensure Compact has helped states reduce COVID-

19 cases, increase tests, or reduce fatalities. In doing so, this chapter further develops the picture from chapter one, which examines the effect of the NLC on labor market outcomes for nurses. Here, I find that states that belonged to the NLC as of January 2020 fared no differently than states that were not part of the NLC in terms of COVID-19 testing rates, case rates, COVID-19 fatality rates, all-cause fatality rates, and non-COVID-19 fatality rates.

Overall, the findings in this chapter are consistent with other results in the literature showing that APRN independence improves patient outcomes. In addition, my results indicate that removing restrictions on SOP for advanced practice nurses in states with restrictive scope-of-practice laws could help those states better combat the pandemic and improve patient outcomes.

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Tables

Table 1: Summary Statistics

	Mean	Std. Dev.
Outcome Variables		
Test Rate per 100,000	754.0	711.3
Case Rate per 100,000	51.8	120.3
COVID-19 Fatality Rate per 100,000	1.3	2.2
All-Cause Fatality Rate per 100,000	19.1	3.9
Non-COVID-19 Fatality Rate per 100,000	17.8	3.1
Treatment Variables		
SOP Law	58.5	49.9
Nurse Licensure Compact	74.4	43.6
Control Variables (All Models)		
Shelter-in-Place Order	19.8	39.8
Mask Mandate	33.4	47.2
Non-Essential Business Closure	15.6	36.3
Emergency Order	81.2	39.1
Average Temperature	59.8	18.2
Control Variables (Cross-Sectional Models)		
Percent with Less than HS Degree	13.4	6.3
Percent with HS Degree	34.3	7.2
Percent with Some College	30.7	5.2
Percent with College Degree or More	21.6	9.4
Median Income (2018)	52,786.6	13,856.3
Density per Square Mile	265.5	1,791.0
Percent in Poverty	14.5	5.5
Percent Male	50.1	2.3
Percent Ages 0 to 17	22.1	3.4
Percent Ages 16 to 64	58.7	3.8
Percent Ages 64 and over	19.2	4.7
Percent White	84.6	16.2
MDs per 100,000	254.7	41.1
NPs per 100,000	51.4	11.9
PAs per 100,000	26.6	10.8
ICU Beds per 100,000	13.4	54.3
Unemployment Rate	4.1	1.5
Employment to Population Ratio	0.5	0.1
Democratic Governor	42.4	49.4
County Population	106,849.1	365,824.2

Table notes: Table 1 provides summary statistics for the sample. There are 112,234 observations. Testing, case, and fatality rates are measured per 100,000 people per week.

Table 2: Summary of SOP Changes during the COVID-19 Pandemic

State	Date Enacted	Date Ended	Details
Alabama	April 2		Increased APRN-to-physician ratio; expand prescription authority and provide additional services
Indiana	April 1		Permit APRNs to maintain multiple practice agreements; suspend documentation requirements
Kansas	April 22	May 31	Suspend practice agreements
Kentucky	March 31		Suspend practice agreements
Louisiana	March 31		Suspend practice agreements
Massachusetts	April 26		Suspend supervision requirements for APRNs with 2+ years' experience
Michigan	March 30	July 13	Revoke restrictions preventing APRNs from practicing to the full extent of their training
Missouri	April 1		Suspend some documentation and supervision requirements
New Jersey	April 1		Suspend practice agreements
North Carolina	April 9		Permit APRNs to be reassigned to other practice areas without new practice agreements; waive several administrative requirements
Pennsylvania	March 20		Expand prescription authority; remove restrictions requiring NPs to practice within a specific clinical specialty; remove administrative requirements
South Carolina	March 23		Permit APRNs to be reassigned to other practice areas without new practice agreements; expand prescriptive authority
Tennessee	March 19	May 18	Suspend some documentation and supervision requirements
Wisconsin	March 27		Suspend practice agreement requirements

Table Notes: This table lists the states that expanded APRN SOP during the pandemic, the dates these changes were implemented, and details of the SOP changes.

Table 3: Difference-in-difference estimates of the effect of APRN independence

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
APRN Law	-70.23	1.59	0.18	-0.45	-0.63
	(86.78)	(6.51)	(0.40)	(0.60)	(0.29)*
Average Temperature	-4.43	-2.57	-0.05	-0.07	-0.02
	(4.29)	(0.56)**	(0.01)**	(0.02)**	(0.01)
Shelter in Place Order	52.07	8.37	1.01	1.38	0.37
	(63.14)	(5.51)	(0.29)**	(0.39)**	(0.17)*
Mask Mandate	-26.11	-25.60	0.26	-0.20	-0.46
	(86.68)	(9.25)**	(0.41)	(0.61)	(0.34)
Business Closure	-73.31	9.62	0.82	0.90	0.08
	(47.61)	(6.04)	(0.26)**	(0.32)**	(0.14)
Emergency Order	-52.76	12.99	0.08	-0.34	-0.42
	(81.30)	(5.88)*	(0.26)	(0.45)	(0.28)
Observations	112,234	112,234	112,234	112,234	112,234
R-squared	0.786	0.254	0.366	0.547	0.757

Table notes: This table reports estimates from the baseline difference-in-difference model for the effect of APRN scope-of-practice changes on each outcome reported in this chapter. Each specification includes full controls and time and county fixed effects. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 4: Cross-sectional estimates of the effect of APRN independence

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
Independent Practice	127.69	-1.37	-0.30	-1.60	-1.30
	(104.78)	(7.03)	(0.34)	(0.74)*	(0.56)*
Percent Less than HS	1.53	1.96	0.02	-0.09	-0.10
	(3.63)	(0.44)**	(0.01)+	(0.03)**	(0.03)**
Percent HS	6.71	-0.52	-0.00	0.06	0.06
	(2.34)**	(0.22)*	(0.01)	(0.02)**	(0.02)**
Percent Some College	2.37	-0.47	-0.00	-0.09	-0.09
	(3.64)	(0.22)*	(0.01)	(0.03)**	(0.02)**
Median Household Income	0.00	-0.00	0.00	-0.00	-0.00
	(0.00)*	(0.00)	(0.00)	(0.00)*	(0.00)**
Population Density	0.01	-0.00	0.00	0.00	-0.00
	(0.00)**	(0.00)**	(0.00)**	(0.00)	(0.00)
Poverty Rate	135.96	-11.95	-0.69	0.48	1.16
	(335.63)	(54.64)	(1.21)	(3.21)	(2.58)
Percent Male	-718.46	266.08	-0.84	-12.07	-11.23
	(591.24)	(99.57)*	(2.16)	(5.83)*	(5.85)+
Percent Ages 0 to 17	86.20	109.40	-1.59	-4.92	-3.33
	(445.33)	(42.87)*	(1.40)	(4.44)	(3.92)
Percent Ages 16 to 64	898.40	31.21	-0.96	-1.32	-0.36
	(485.13)+	(39.73)	(1.33)	(3.34)	(3.28)
MDs per 100k	1.03	-0.10	0.01	-0.00	-0.01
	(0.96)	(0.08)	(0.00)+	(0.01)	(0.01)+
NPs per 100k	3.10	0.21	0.00	0.09	0.09
	(3.30)	(0.18)	(0.01)	(0.02)**	(0.02)**
PAs per 100k	-1.65	-0.35	-0.01	-0.05	-0.04
	(3.84)	(0.22)	(0.01)	(0.03)+	(0.02)
ICU Beds per 100k	-0.12	-0.00	-0.00	-0.00	-0.00
	(0.05)*	(0.01)	(0.00)	(0.00)	(0.00)

Unemployment Rate	30.78	-1.36	-0.03	-0.12	-0.09
	(18.46)	(1.89)	(0.06)	(0.17)	(0.15)
Emp. to Pop. Ratio	-316.85	25.96	-0.52	-3.47	-2.95
	(269.97)	(27.40)	(1.08)	(2.04)+	(1.64)+
Democratic Gov.	74.48	-4.12	-0.33	-0.44	-0.11
	(76.58)	(4.75)	(0.20)	(0.49)	(0.47)
Average Temperature	-0.87	-0.69	-0.00	-0.03	-0.03
	(2.67)	(0.23)**	(0.01)	(0.02)	(0.02)
Percent White	-162.91	-80.17	-1.34	-0.79	0.55
	(140.33)	(13.45)**	(0.52)*	(1.35)	(1.23)
Total Population	0.00	0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)+	(0.00)	(0.00)	(0.00)
APRN Law	-112.79	-4.37	-0.02	0.43	0.44
	(93.63)	(7.25)	(0.34)	(0.60)	(0.48)
Shelter in Place Order	67.97	-6.16	0.42	0.46	0.04
	(79.10)	(5.75)	(0.25)	(0.38)	(0.32)
Mask Mandate	5.09	-17.87	0.34	0.13	-0.21
	(73.80)	(7.47)*	(0.30)	(0.46)	(0.43)
Business Closure	-76.68	19.42	1.16	1.81	0.65
	(51.57)	(5.81)**	(0.29)**	(0.34)**	(0.26)*
Emergency Order	-102.12	9.48	-0.19	-0.21	-0.02
	(57.99)+	(4.94)+	(0.18)	(0.57)	(0.52)
NLC	-34.50	10.89	-0.00	-0.20	-0.20
	(95.82)	(6.34)+	(0.27)	(0.59)	(0.59)
Observations	112,234	112,234	112,234	112,234	112,234
R-squared	0.702	0.185	0.252	0.366	0.488

Table Notes: This Table reports estimates from a cross-sectional model comparing states with full APRN scope-of-practice laws against states with restrictive scope-of-practice laws. I include time fixed effects in each model. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 5: Difference-in-difference estimates of the effect of APRN independence and nurse practitioner supply

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
APRN Independence	59.96	18.43	0.80	-0.39	-1.18
	(196.20)	(12.82)	(0.79)	(1.39)	(0.75)
APRN Independence * NPs per 100k	-1.68	-0.22	-0.00	-0.00	0.01
	(1.96)	(0.13)+	(0.01)	(0.01)	(0.01)
Average Temperature	-4.30	-2.55	-0.05	-0.07	-0.02
	(4.14)	(0.56)**	(0.01)**	(0.02)**	(0.01)+
Shelter in Place Order	50.33	8.15	1.00	1.38	0.38
	(63.46)	(5.53)	(0.28)**	(0.39)**	(0.17)*
Mask Mandate	-20.77	-24.91	0.28	-0.20	-0.48
	(84.52)	(9.30)*	(0.41)	(0.62)	(0.35)
Business Closure	-74.78	9.43	0.81	0.89	0.09
	(46.98)	(6.07)	(0.26)**	(0.32)**	(0.14)
Emergency Order	-54.80	12.73	0.07	-0.34	-0.41
	(82.38)	(6.07)*	(0.25)	(0.44)	(0.28)
Observations	112,234	112, 234	112, 234	112, 234	112, 234
R-squared	0.787	0.254	0.368	0.548	0.757

Table Notes: This Table reports estimates from a difference-in-difference model that interacts the treatment variable, SOP Law, with the number of nurse practitioners per 100,000 state residents. Each specification includes full controls and county and time fixed effects. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 6: Cross-sectional estimates of the effect of the Nurse Licensure Compact

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
NLC	-39.72	9.71	-0.06	-0.35	-0.30
	(103.93)	(6.47)	(0.29)	(0.69)	(0.65)
Shelter in Place Order	51.54	-5.75	0.47	0.71	0.24
	(81.42)	(6.00)	(0.25)+	(0.38)+	(0.34)
Mask Mandate	14.01	-18.56	0.30	-0.10	-0.40
	(76.86)	(7.67)*	(0.30)	(0.47)	(0.42)
Business Closure	-76.19	18.27	1.11	1.60	0.49
	(49.22)	(6.00)**	(0.29)**	(0.42)**	(0.33)
Emergency Order	-96.60	9.43	-0.20	-0.28	-0.08
	(57.52)+	(4.84)+	(0.17)	(0.59)	(0.53)
Percent Less than HS	1.88	1.97	0.02	-0.09	-0.11
	(3.72)	(0.42)**	(0.01)+	(0.03)**	(0.03)**
Percent HS	5.28	-0.62	-0.00	0.06	0.06
	(2.63)+	(0.21)**	(0.01)	(0.02)**	(0.02)**
Percent Some College	3.74	-0.55	-0.01	-0.12	-0.12
	(4.78)	(0.24)*	(0.01)	(0.03)**	(0.02)**
Median Household Income	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)*	(0.00)	(0.00)	(0.00)**	(0.00)**
Population Density	0.01	-0.00	0.00	0.00	-0.00
	(0.00)**	(0.00)**	(0.00)**	(0.00)	(0.00)
Poverty Rate	198.70	-20.64	-1.00	-1.80	-0.80
	(361.70)	(53.29)	(1.15)	(3.24)	(2.64)
Percent Male	-473.38	280.48	-1.57	-11.85	-10.28
	(501.68)	(99.36)**	(1.96)	(5.59)*	(5.54)+
Percent Ages 0 to 17	65.15	128.72	-1.70	-0.97	0.73
	(480.89)	(45.24)**	(1.36)	(4.66)	(4.07)
Percent Ages 16 to 64	784.24	50.91	-0.78	3.67	4.46
	(500.71)	(42.94)	(1.31)	(3.36)	(3.28)
MDs per 100k	1.07	-0.11	0.01	-0.01	-0.01

	(1.11)	(0.07)	(0.00)+	(0.01)	(0.01)*
NPs per 100k	2.93	0.19	0.00	0.09	0.09
	(3.37)	(0.18)	(0.01)	(0.02)**	(0.02)**
PAs per 100k	-0.89	-0.36	-0.01	-0.06	-0.05
	(3.88)	(0.23)	(0.01)	(0.02)*	(0.02)+
ICU Beds per 100k	-0.13	-0.00	-0.00	-0.00	-0.00
	(0.06)*	(0.01)	(0.00)	(0.00)	(0.00)
Unemployment Rate	33.84	-1.40	-0.04	-0.15	-0.11
	(18.59)+	(1.93)	(0.06)	(0.17)	(0.15)
Emp. to Pop. Ratio	-269.35	25.73	-0.88	-3.95	-3.07
	(282.45)	(29.08)	(1.02)	(2.10)+	(1.80)+
Democratic Gov.	53.49	-4.81	-0.32	-0.34	-0.03
	(77.53)	(4.94)	(0.23)	(0.52)	(0.49)
Average Temperature	-0.92	-0.59	0.00	-0.01	-0.01
	(2.54)	(0.20)**	(0.01)	(0.02)	(0.02)
Percent White	-160.93	-78.85	-1.33	-0.56	0.77
	(139.80)	(13.89)**	(0.55)*	(1.37)	(1.20)
Total Population	0.00	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)+	(0.00)	(0.00)	(0.00)
Observations	112,234	112,234	112,234	112,234	112,234
R-squared	0.700	0.185	0.250	0.352	0.476

Table Notes: This Table reports estimates from a cross-sectional model comparing states that belong to the NLC with states that do not. I include time fixed effects in addition to a host of county- and state-level control variables, such as education, average income, density, temperature, and healthcare professionals per capita. I also include shelter-in-place orders, mask orders, business closures, and emergency orders. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 7: Difference-in-difference estimates of the effect of APRN independence (rural and non-rural counties)

	Panel A: Urban Counties		Panel B: Non-Urban Counties	
	COVID-19	COVID-19	COVID-19	COVID-19
VARIABLES	Case Rate	Fatality Rate	Case Rate	Fatality Rate
APRN Independence	6.55	0.41	-0.88	-0.15
	(6.21)	(0.34)	(7.83)	(0.27)
Average Temperature	-2.62	-0.06	-2.72	-0.08
	(0.59)**	(0.02)**	(0.62)**	(0.02)**
Shelter in Place Order	11.30	0.65	4.08	0.14
	(5.14)*	(0.22)**	(6.52)	(0.24)
Mask Mandate	-15.98	0.34	-31.21	-0.06
	(8.36)+	(0.45)	(10.85)**	(0.38)
Business Closure	15.26	0.56	5.93	0.18
	(6.89)*	(0.28)+	(6.80)	(0.34)
Emergency Order	16.38	0.45	9.93	0.42
	(4.34)**	(0.22)*	(8.24)	(0.20)*
Observations	41,612	41,612	70,622	70,622
R-squared	0.272	0.214	0.251	0.143

Table notes: This table provides results from estimating the baseline difference-in-difference model on a sample of urban/suburban and rural counties. Each specification includes full controls in addition to county and time fixed effects. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

Table 8: Difference-in-difference estimates of the effect of APRN independence without five most affected states

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
APRN Independence	-112.00	0.76	0.26	-0.30	-0.57
	(71.75)	(6.77)	(0.43)	(0.68)	(0.33)+
Average Temperature	-3.22	-2.38	-0.04**	-0.06**	-0.02
	(3.51)	(0.55)**	(0.01)	(0.02)	(0.01)
Shelter in Place Order	60.32	4.70	0.71**	1.06**	0.35
	(69.38)	(5.12)	(0.23)	(0.33)	(0.17)+
Mask Mandate	-28.33	-26.55	-0.05	-0.48	-0.43
	(86.20)	(9.57)**	(0.39)	(0.62)	(0.34)
Business Closure	-80.21	5.73	0.61	0.69	0.09
	(52.58)	(6.37)	(0.26)*	(0.36)+	(0.15)
Emergency Order	-33.32	10.45	0.11	-0.27	-0.38
	(69.21)	(6.07)+	(0.20)	(0.38)	(0.28)
Observations	106,468	106,468	106,468	106,268	106,268
R-squared	0.77	0.25	0.51	0.69	0.78

Table notes: This table provides results from estimating the baseline difference-in-difference model without the five hardest hit states in mid-March: New York, New Jersey, Massachusetts, Louisiana, and Connecticut. Each specification includes full controls in addition to county and time fixed effects. Standard errors are clustered at the state level and reported in parentheses. ** p<0.01, * p<0.05, + p<0.1.

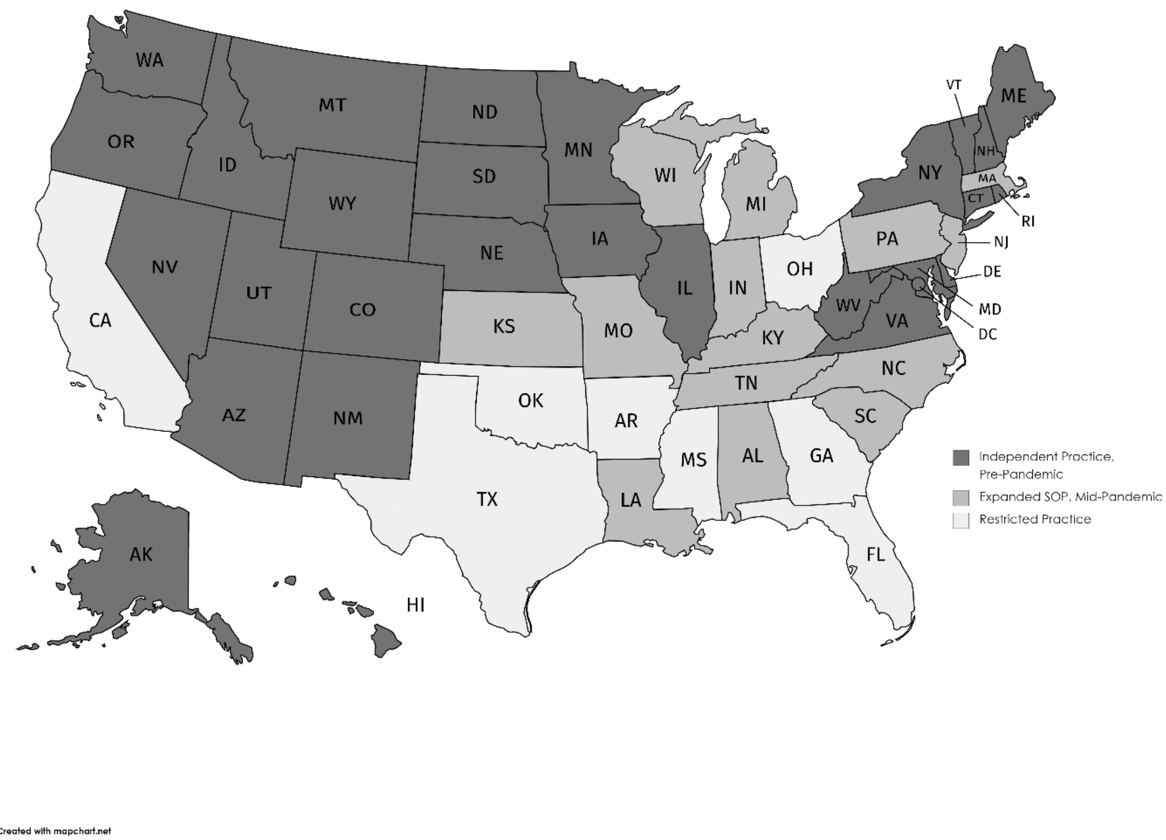
Table 9: Difference-in-difference estimates of the effect of APRN independence without marginally treated states

VARIABLES	(1) COVID-19 Test Rate	(2) COVID-19 Case Rate	(3) COVID-19 Fatality Rate	(4) All-Cause Fatality Rate	(5) Non-COVID-19 Fatality Rate
APRN Independence	-23.20	-0.12	0.18	-0.45	-0.63
	(95.94)	(6.61)	(0.46)	(0.70)	(0.33)+
Average Temperature	-4.28	-2.58	-0.05	-0.07	-0.02
	(4.24)	(0.56)**	(0.01)**	(0.02)**	(0.01)
Shelter in Place Order	51.66	8.38	1.01	1.37	0.37
	(64.06)	(5.53)	(0.29)**	(0.39)**	(0.17)*
Mask Mandate	-26.77	-25.54	0.25	-0.19	-0.44
	(87.97)	(9.25)**	(0.41)	(0.62)	(0.34)
Business Closure	-76.23	9.74	0.82	0.90	0.08
	(47.96)	(6.05)	(0.26)**	(0.32)**	(0.14)
Emergency Order	-51.61	12.91	0.09	-0.35	-0.44
	(81.33)	(5.81)*	(0.26)	(0.46)	(0.28)
Observations	112,234	112,234	112,234	112,234	112,234
R-squared	0.786	0.254	0.367	0.547	0.756

Table notes: I re-code AL and MO as untreated and report estimates from the corresponding difference-in-difference model for the effect of APRN scope-of-practice changes on each outcome reported in this chapter. Each specification includes time, county fixed effects, and full controls. Standard errors are clustered at the state level and reported in parentheses. . ** p<0.01, * p<0.05, + p<0.1.

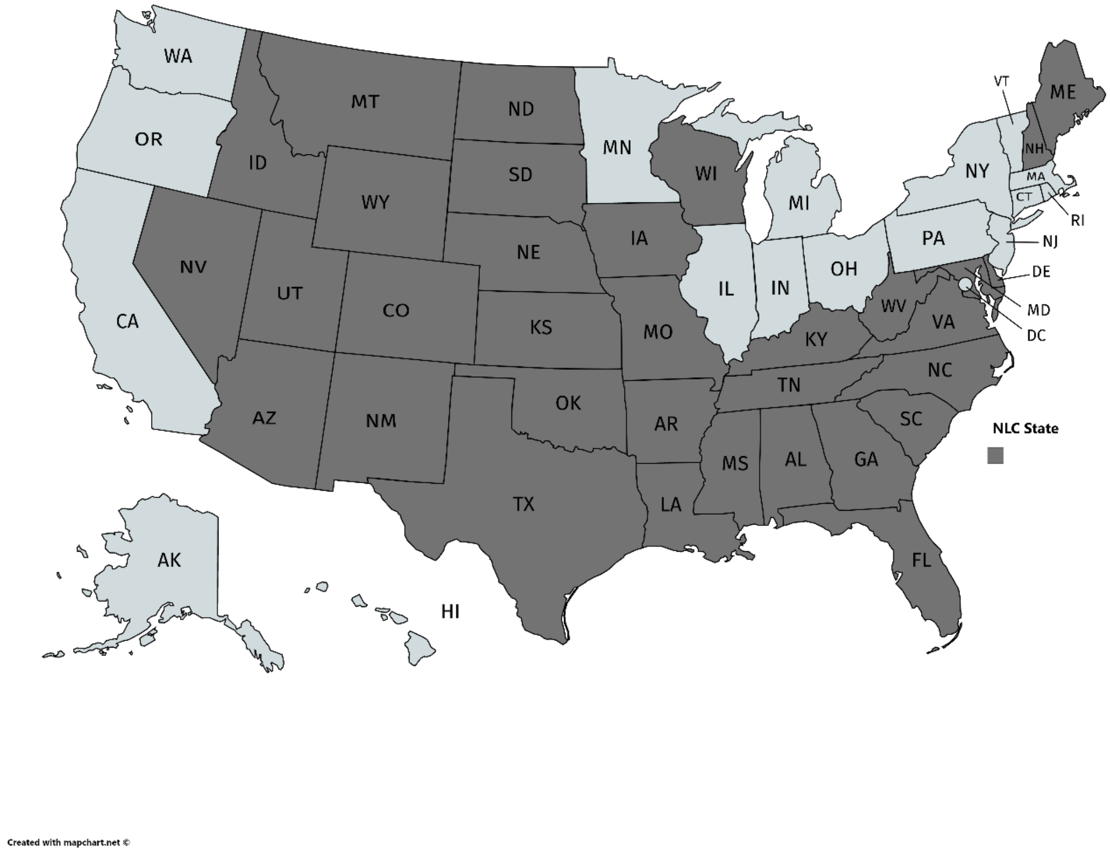
Figures

Figure 1: APRN Scope-of-Practice Law Changes



Notes: Figure 1 illustrates the changes that states have made to scope-of-practice laws for APRNs. States highlighted in medium grey have restrictive SOP laws that were relaxed during the pandemic (e.g. Tennessee); states highlighted in dark grey already gave APRNs unrestricted SOP authority (e.g. Washington); states in light grey had restrictive SOP laws that did not change during the pandemic (e.g. Georgia).

Figure 2: Nurse Licensure Compact States



Notes: This Figure illustrates states that belonged to the Nurse Licensure Compact as of January 2020 in dark grey.

Figure 3: Time Series Plot of Outcome Variables

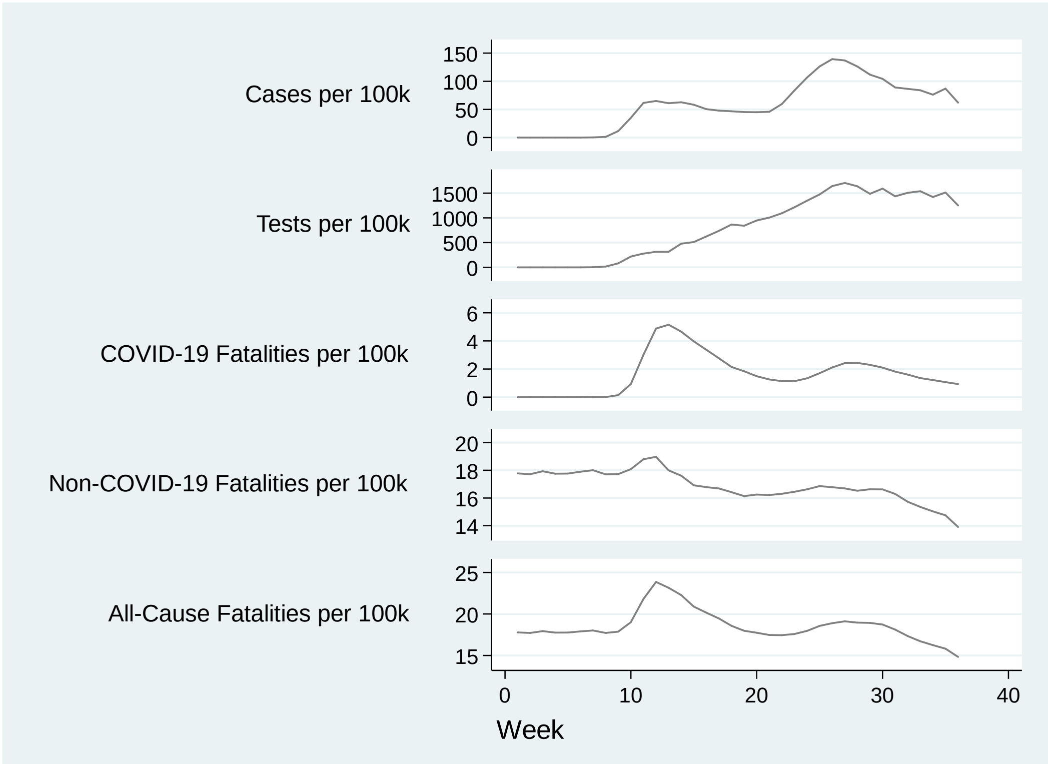


Figure notes: Figure 3 plots each outcome variable weekly. This figure spans January 26 to September 26, 2020.

Figure 4: Difference-in-difference estimate of APRN independence on COVID-19 test rates (three-week intervals)

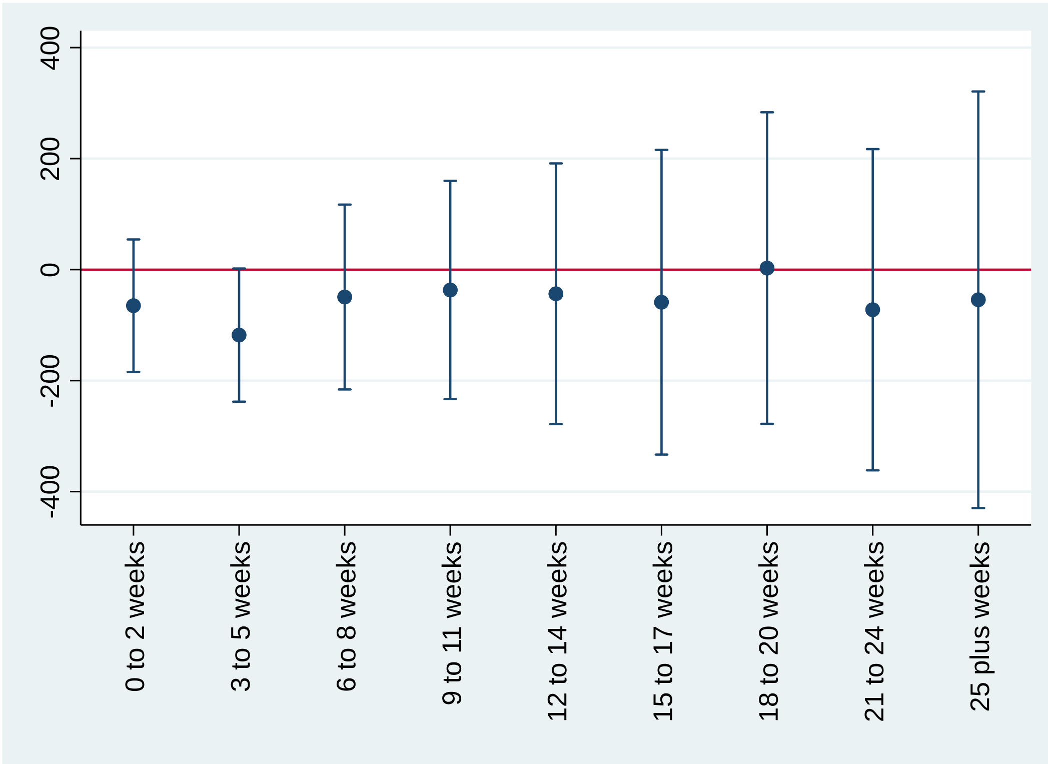


Figure Notes: Figures 4 plots point estimates from a difference-in-difference model estimating the effect of laws expanding APRN independence on the COVID-19 testing rate in the first 3 weeks after adoption, weeks 3 to 5 after adoption, weeks 6 to 8, and so on. The model includes full controls and fixed effects.

Figure 5: Difference-in-difference estimate of APRN independence on COVID-19 case rates (three-week intervals)

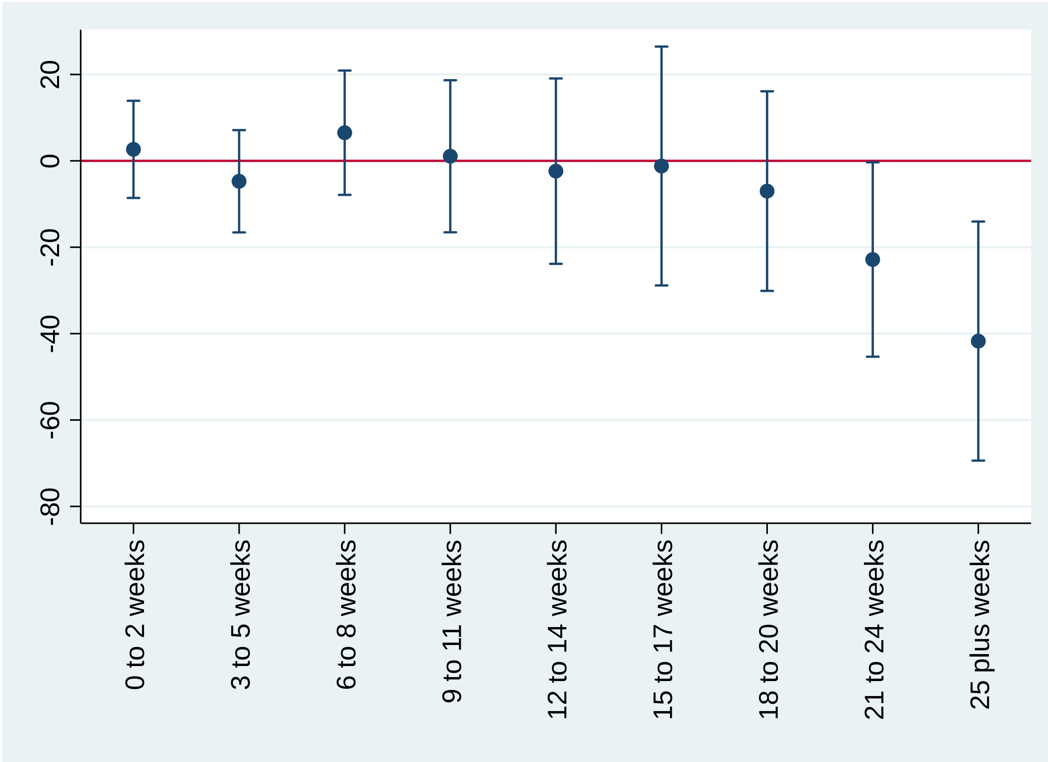


Figure Notes: Figures 5 plots point estimates from a difference-in-difference model estimating the effect of laws expanding APRN independence on the COVID-19 case rate in the first 3 weeks after adoption, weeks 3 to 5 after adoption, weeks 6 to 8, and so on. The model includes full controls and fixed effects.

Figure 6: Difference-in-difference estimate of APRN independence on COVID-19 fatality rates (three-week intervals)

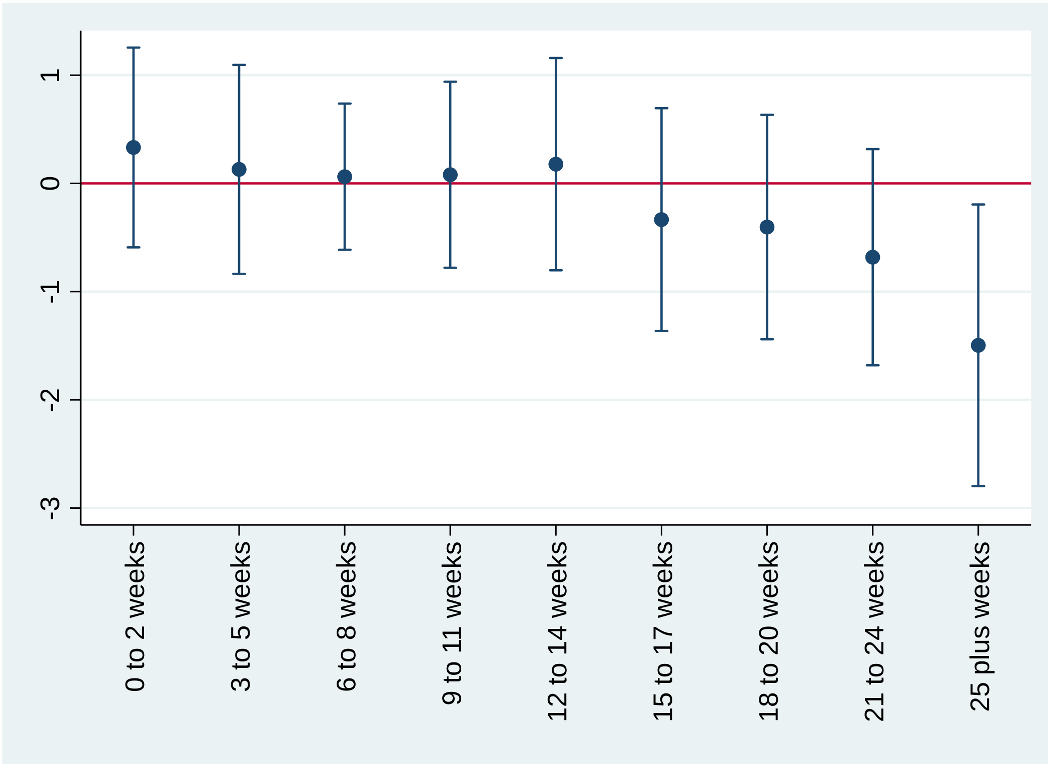


Figure Notes: Figures 6 plots point estimates from a difference-in-difference model estimating the effect of laws expanding APRN independence on the COVID-19 fatality rate in the first 3 weeks after adoption, weeks 3 to 5 after adoption, weeks 6 to 8, and so on. The model includes full controls and fixed effects.

Figure 7: Difference-in-difference estimate of APRN independence on all-cause fatality rates (three-week intervals)

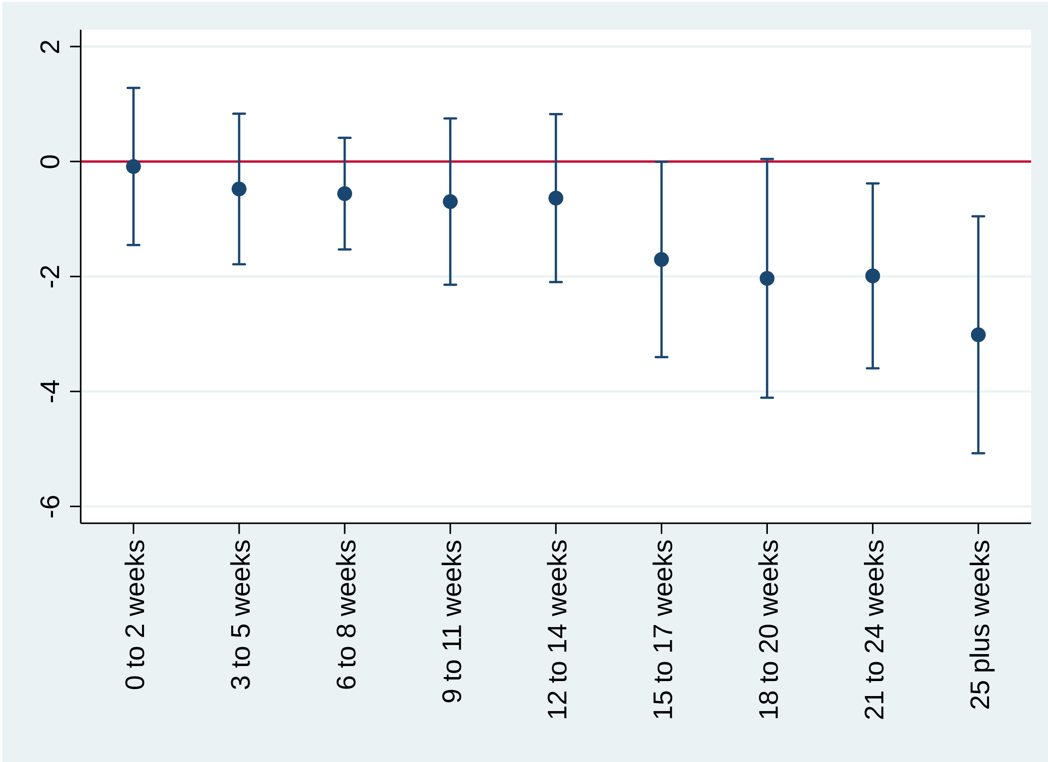


Figure Notes: Figures 7 plots point estimates from a difference-in-difference model estimating the effect of laws expanding APRN independence on the all-cause fatality rate in the first 3 weeks after adoption, weeks 3 to 5 after adoption, weeks 6 to 8, and so on. The model includes full controls and fixed effects.

Figure 8: Difference-in-difference estimate of APRN independence on Non-COVID-19 fatality rates (three-week intervals)

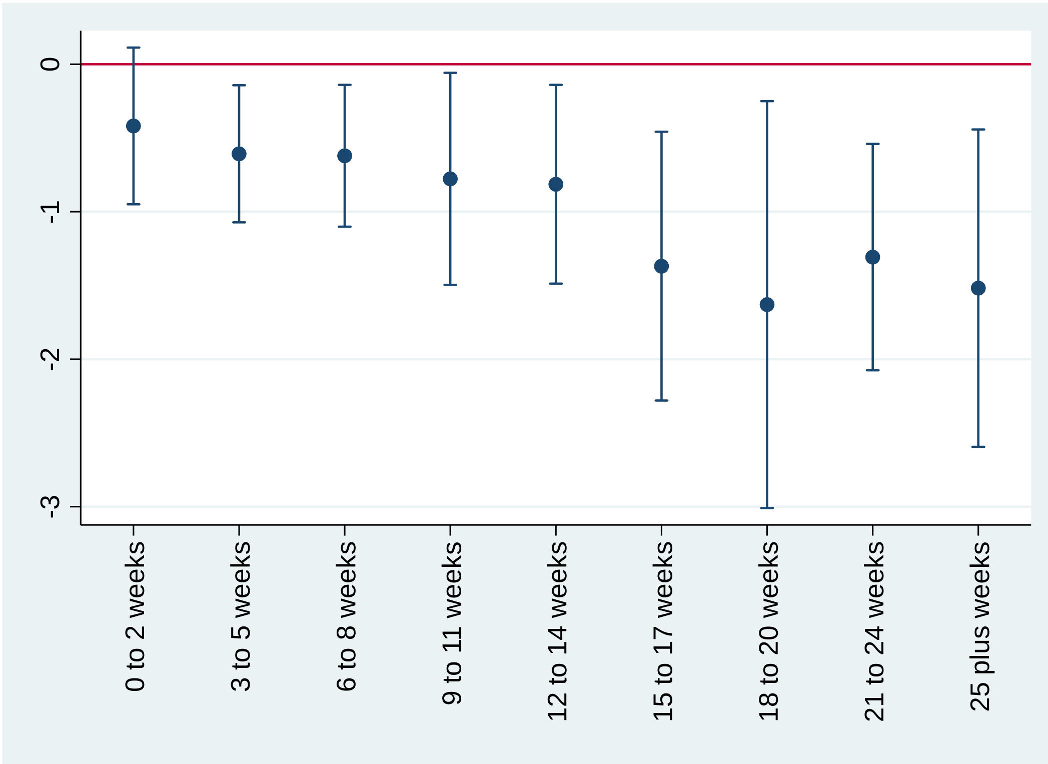


Figure Notes: Figures 8 plots point estimates from a difference-in-difference model estimating the effect of laws expanding APRN independence on the non-COVID-19 fatality rate in the first 3 weeks after adoption, weeks 3 to 5 after adoption, weeks 6 to 8, and so on. The model includes full controls and fixed effects.

Figure 9: Event study of APRN independence on the COVID-19 test rate

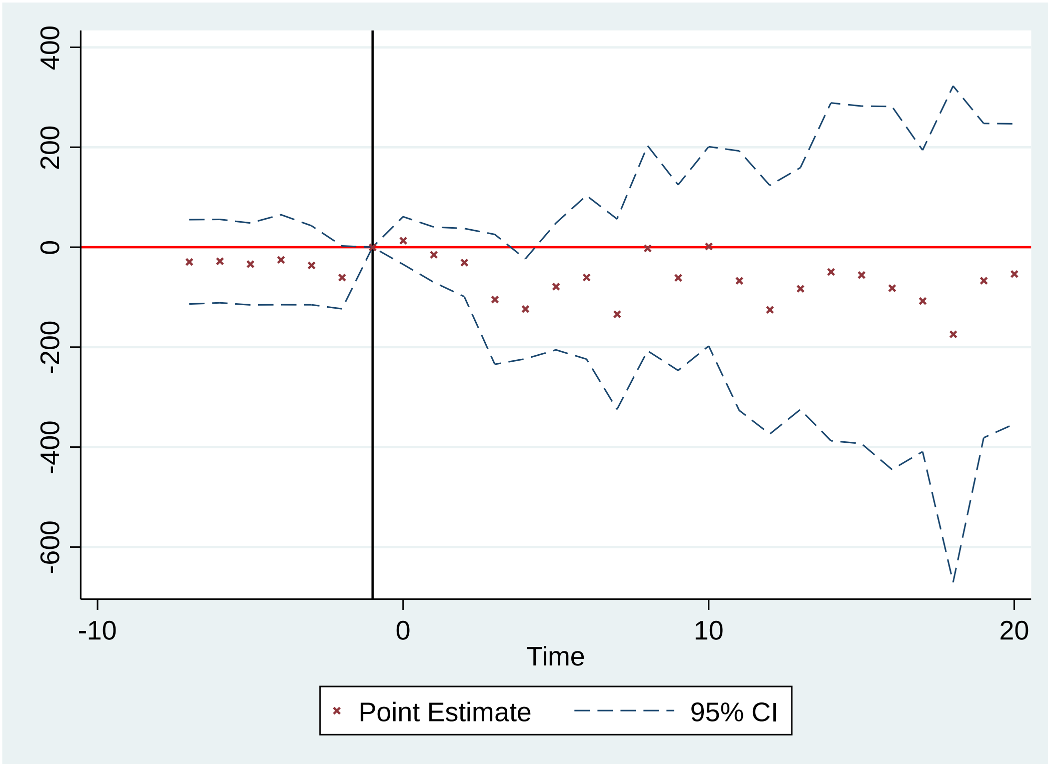


Figure Notes: Figure 9 plots an event studies for the effect of APRN independence on the COVID-19 testing rate. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 10: Event study of APRN independence on the COVID-19 case rate

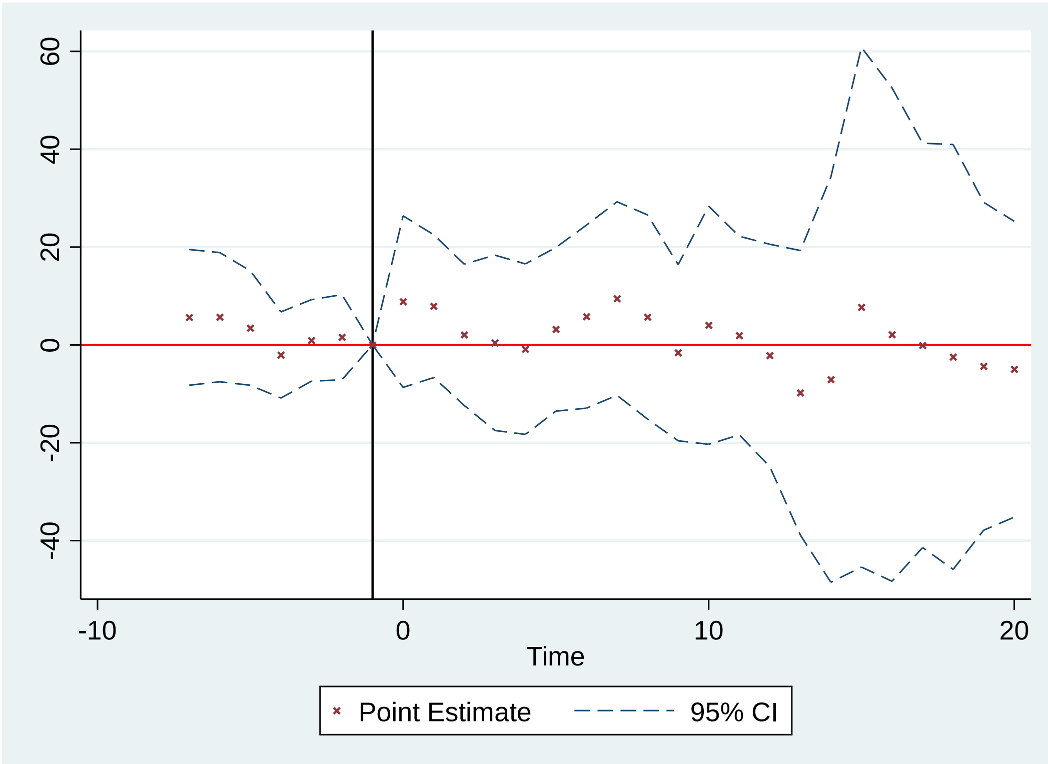


Figure Notes: Figure 10 plots an event studies for the effect of APRN independence on the COVID-19 case rate. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 11: Event study of APRN independence on the COVID-19 fatality rate

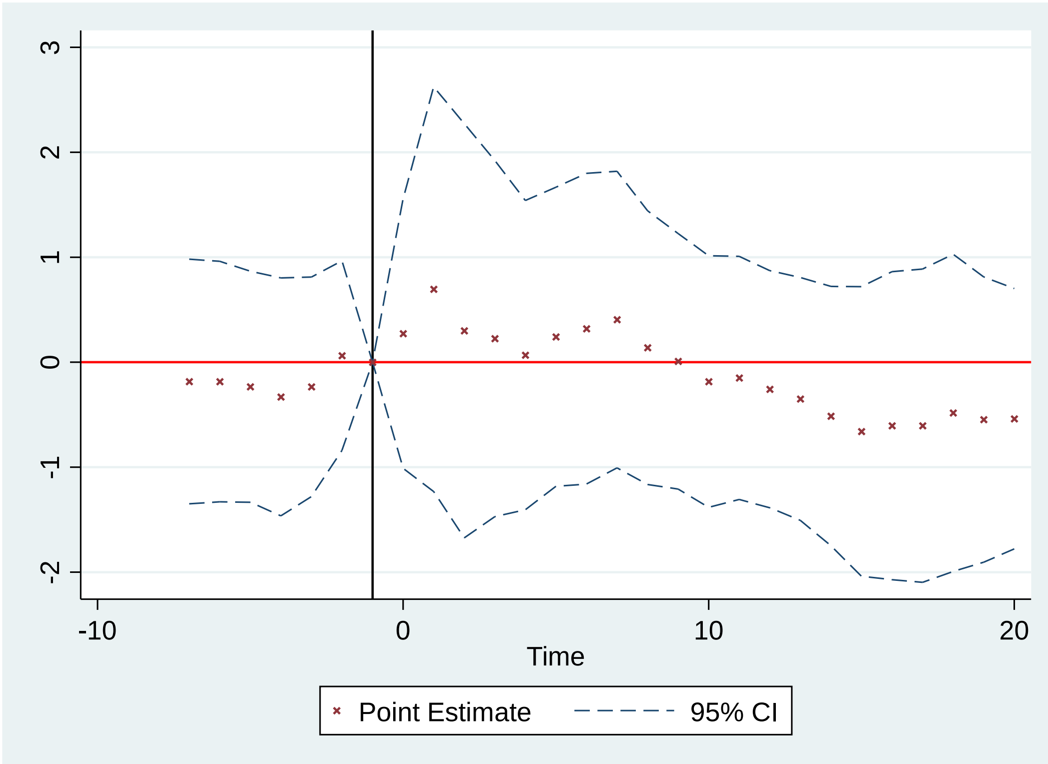


Figure Notes: Figure 11 plots an event studies for the effect of APRN independence on the COVID-19 fatality rate. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 12: Event study of APRN independence on the all-cause fatality rate

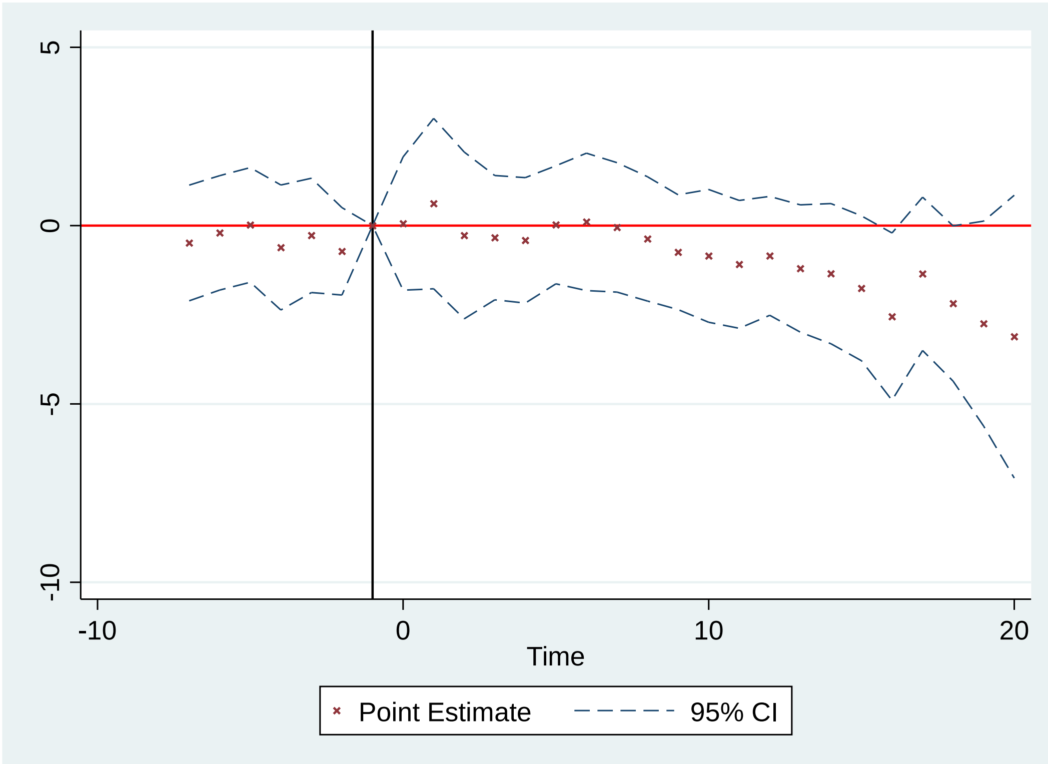


Figure Notes: Figure 12 plots an event studies for the effect of APRN independence on the all-cause fatality rate. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 13: Event study of APRN independence on the non-COVID-19 fatality rate

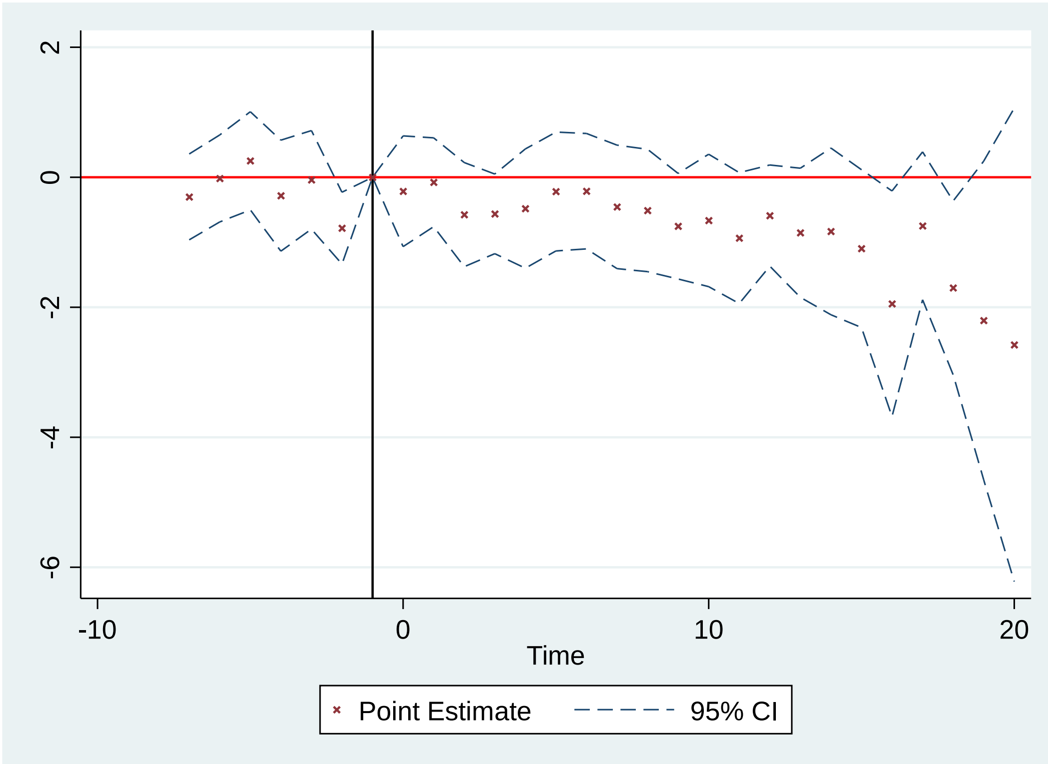


Figure Notes: Figure 13 plots an event studies for the effect of APRN independence on the non-COVID-19 fatality rate. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 14: Event study of APRN independence on COVID-19 case rate (rural counties)

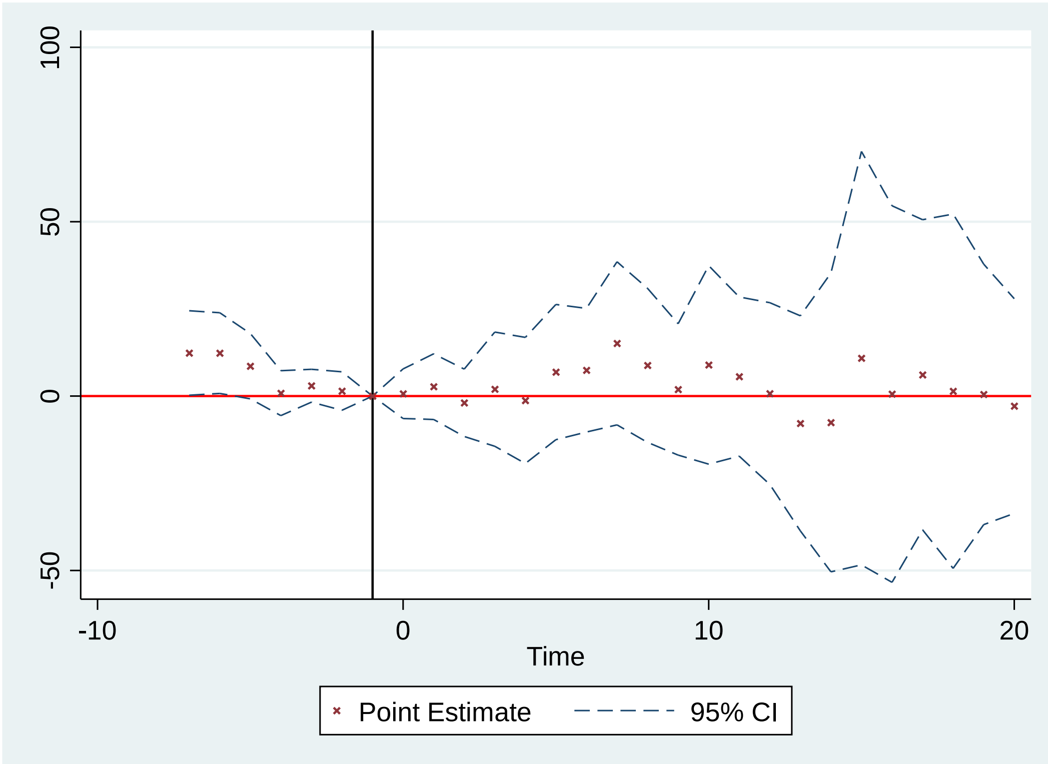


Figure Notes: Figure 14 plots an event studies for the effect of APRN independence on the COVID-19 case rate in rural counties. The event study includes time and county fixed effects in addition to a full set of control variables.

Figure 15: Event study of APRN independence on COVID-19 fatality rate (rural counties)

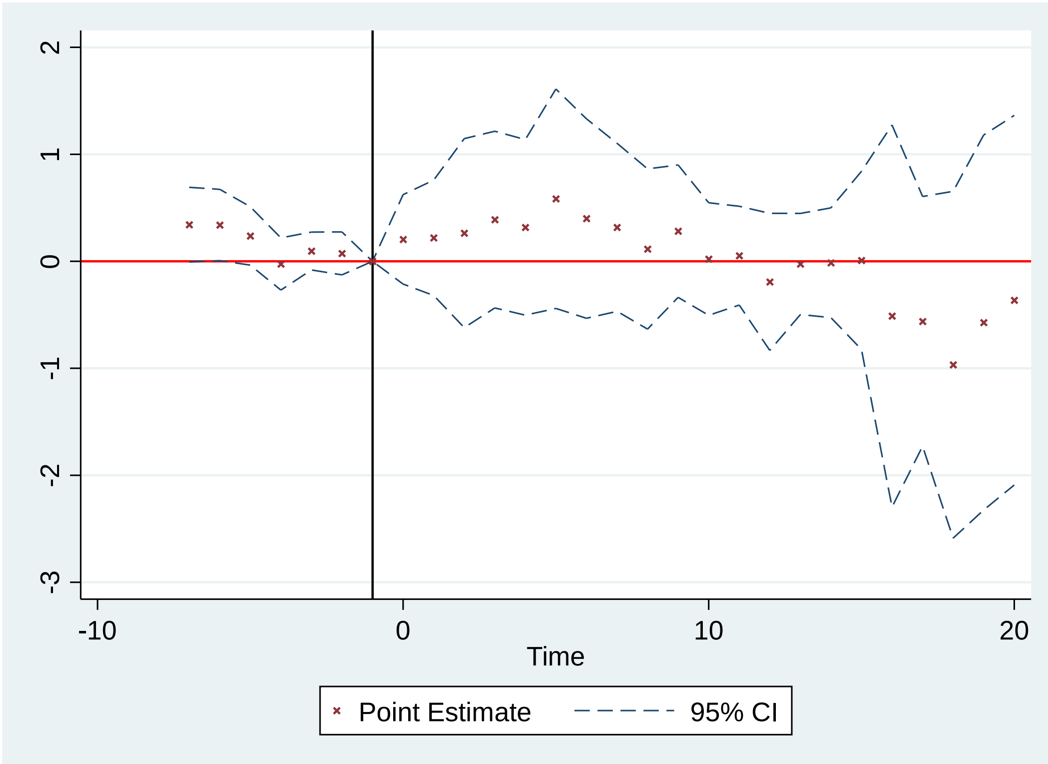


Figure Notes: Figure 15 plots an event studies for the effect of APRN independence on the COVID-19 fatality rate in rural counties. The event study includes time and county fixed effects in addition to a full set of control variables.