Occupational Licensing and Labor Market Mobility: Evidence from the Legal Profession

Adam Chilton Jacob Goldin Kyle Rozema Sarath Sanga

August 23, 2024

We study how state occupational licensing requirements shape labor mobility across U.S. legal markets. Drawing on newly collected data, we link variation in state bar exam waiver policies to lawyers' license acquisitions, professional disciplinary records, and educational histories. We find that bar exam waivers increase the number of experienced lawyers obtaining a new license by 38 percent, but that the additional lawyers are subject to more professional discipline and tend to have graduated from less selective law schools. Our results suggest that state-level occupational licensing regimes can create a trade-off between the supply and quality of professionals in an industry.

*Chilton: University of Chicago Law School, e-mail: adamchilton@uchicago.edu. Goldin: University of Chicago Law School, American Bar Foundation, and NBER, email: jsgoldin@uchicago.edu. Rozema: Northwestern University Pritzker School of Law, email: kyle.rozema@law.northwestern.edu. Sanga: Yale Law School, email: sarath.sanga@yale.edu.

1 Introduction

The freedom of people to work in new jurisdictions is critical to the efficient functioning of labor markets (Friedman, 1962). However, a large share of the U.S. workforce cannot freely work in new jurisdictions because states impose their own occupational licensing requirements. Despite these requirements serving as a barrier to labor market mobility, occupational licensing requirements are often supported because of their potential to improve the quality of services offered to consumers.

We investigate this trade-off empirically in the context of U.S. lawyers and the legal markets in which they are licensed to practice. We do so by exploiting rich policy variation in state bar exam requirements. Lawyers are licensed at the state level, and nearly all new law school graduates must pass the bar exam to obtain their first license. However, states have adopted different policies on whether to waive the bar exam for experienced lawyers applying for an additional license to move or expand their practice. One set of states requires all lawyers to pass the state's bar exam. A second set of states waives the bar exam for all experienced lawyers licensed in other states. A third set of states have reciprocity policies where they waive the bar exam only for experienced lawyers from states that also waive the bar exam. Changes in these waiver policies over time provide plausibly exogenous variation in the barriers to obtaining a license to practice law in a new state.

To study whether bar exam waivers affect the quantity and quality of lawyers obtaining licenses in a state, we assembled three novel datasets. First, to measure the policy variation, we built a dataset of historical state bar exam waiver policies from 1983 to 2019. Second, to measure labor market mobility, we built a dataset of the history of license acquisitions for 1.7 million lawyers using professional profiles from a national directory of lawyers. Third, to measure one proxy of lawyer quality, we built a dataset of the public disciplinary actions taken between 1990 and 2020 by separately collecting information for a subset of states for which this data is available.

Before studying the effect of bar exam waivers, we first document the barriers to labor

market mobility in the legal profession over time. Due to some states having reciprocity requirements, the availability of a bar exam waiver depends on the specific combination of origin and destination state. For concreteness, we refer to each pair of origin and destination states as a corridor, and we refer to a corridor as open if the destination state waives the bar exam for experienced lawyers from the origin state and as closed if it does not. Altogether, there were 54 state-level policy changes during our sample period, which produced 2,396 corridor-level changes in openness. These changes have tended to remove barriers to labor market mobility. For example, the share of corridors that were open increased from 54 percent in 1983 to 76 percent in 2019.

After documenting these new descriptive statistics, our analysis of the causal effect of bar exam waivers proceeds in two parts. In the first part of the analysis, we investigate the effect of bar exam waivers on lawyer mobility. Using a stacked event study design, we find that a bar exam waiver increases the number of experienced lawyers obtaining a license in the state by 38 percent. We find larger effects in corridors that are most commonly traversed and in corridors with destination states that have difficult bar exams. We find no evidence that the results are driven by lawyers substituting one destination state for another or waiting to gain experience before obtaining another license so as to benefit from a waiver. Finally, by exploiting the fact that some state-level policy changes alter the openness of some of the state's corridors but not others, we investigate whether our results are driven by confounding state-level events like contemporaneous changes in other policies or labor market conditions and find evidence that they are not.

In the second part of the analysis, we investigate how bar exam waivers affect the quality of experienced lawyers obtaining additional licenses. In addition to using our dataset of professional disciplinary actions as a proxy for quality, we also use data on the selectivity of the law school lawyers attended, which we observe from lawyers' professional profiles. Each measure of quality offers advantages and disadvantages: whereas professional discipline is a strong but sparse signal of quality, law school selectivity is a broadly available but much

weaker signal of quality. We find that bar exam waivers decrease lawyer quality using both measures. We specifically find that lawyers induced by a bar exam waiver to obtain an additional license are subjected to professional discipline at more than double the rates of lawyers who obtain an additional license even without a bar exam waiver (3.6 percent compared to 1.7 percent). Similarly, we find that the share of lawyers who graduated from a law school ranked outside the top-100 is 12 percent higher for lawyers induced by a bar exam waiver to obtain an additional license than for lawyers who obtain an additional license even without a bar exam waiver (54 percent compared to 48 percent). Taken together, our results suggest that licensing requirements generate a quantity-quality trade-off in the legal profession: they constitute a significant barrier to labor market mobility but also increase the quality of lawyers in a state.

Although prior research has theorized that heterogeneous state occupational requirements may generate quantity-quality trade-offs, empirical work has largely studied these two margins in isolation.[1] With respect to the quantity margin, the potential for heterogeneous state-level occupational licensing requirements to depress interstate labor market mobility has long been recognized (Roback, 1943). But a lack of comprehensive state-level data has hampered efforts to study it empirically. For example, Johnson and Kleiner (2020, at 348) observe that there is a "lack of available information on changes in licensing requirements that could be used in a traditional causal framework." There has, however, recently been a recent resurgence of interest in the topic (e.g., Criscuolo and Garicano, 2010; Kleiner and Krueger, 2010, 2013; Chaudhry et al., 2015; Mulholland and Young, 2016).[2] In particular, several recent papers have found innovative ways to study the question. DePasquale and

[1] An exception is Kleiner and Soltas (2023), who employ a structural model to study how upfront training time required to meet occupational licensing requirements affects the quantity and quality of workers who choose to enter into a profession. In contrast, we rely on a different research design based on plausibly exogenous policy variation, and our aim is to identify the trade-off that arises when states adopt different occupational licensing requirements for workers who have already entered into a profession.

[2] An older, primarily descriptive, literature laid the foundation for much of this more recent work, including contributions by Holen (1965), Pashigan (1979), Gay et al. (1982), and, more recently Peterson et al. (2014).

Stange (2016) and Shakya et al. (2022) studied the effect of state licensing requirements on the labor market mobility of registered nurses, using variation in multi-state compacts that allowed nurses registered in one state to practice in some other states without obtaining an additional license. Along similar lines, Johnson and Kleiner (2020) studied the effect of state licensing requirements on residential mobility by comparing occupations licensed at the state-level with occupations licensed at the national-level. We build on this research by compiling data on state-level licensing requirements and analyzing the interactions of these policies across pairs of states. Moreover, the bar exam is perhaps the biggest barrier to entry among any single licensing requirement in any profession.[3] By studying these granular and high-stakes changes, we are able to provide some of the most direct evidence to date on the effects of both liberalizing and tightening state occupational licensing requirements.

With respect to the quality margin, much of the empirical work has focused on the effects of the requirements that entering members of the regulated occupation must meet. This includes in fields ranging from education (Kleiner and Petree, 1988; Angrist and Guryan, 2008; Kane et al., 2008; Larsen et al., 2020), residential home and daycare services (Farronato et al., 2020; Currie and Hotz, 2004), and medical services (Kleiner and Kudrle, 2000; Kugler and Sauer, 2005; Timmons and Mills, 2018; Anderson et al., 2020). In contrast, our focus is on how barriers to legal practice for existing members of the profession shape the quality of lawyers in the state. Such state-level barriers to the mobility of experienced professionals are common across occupations in the U.S. and may affect quality differently than barriers to new entrants.

In addition to studying the quantity-quality trade-off in the same setting, our results also contribute to a literature studying the regulation of the legal profession. A number of prior studies examine the effect of occupational licensing requirements on the quantity

[3] For example, over the time period we study, the bar exam has excluded more than 200,000 people from practicing law in a state because they were unable to pass the bar exam, and the bar exam has delayed the entry of more than 300,000 other lawyers because they failed the bar exam at least once before ultimately passing it (Rozema, 2023).

and quality of new lawyers entering the legal profession (Ramseyer and Rasmusen, 2015; Anderson and Muller, 2019; Rozema, 2021, 2023, 2024a,b). We build on this literature by studying the effect of state regulations that constrain the ability of already-licensed lawyers to move or expand their legal practice into new legal markets.

The remainder of the paper proceeds as follows. Section 2 describes bar exam waiver policies. Section 3 describes the data and reports descriptive statistics. Section 4 investigates the effect of bar exam waivers on labor market mobility. Section 5 investigates how bar exam waivers affect the quality of lawyers obtaining additional licenses. Section 6 concludes.

2 Background

To obtain a license to practice law in a given state, an individual must either pass the state bar exam or meet the criteria for a bar exam waiver.[4]

2.1 The Bar Exam

Historically, each state independently designed and administered its own bar exam. Over time, however, states have increasingly coordinated the contents of the exam through an association called the National Conference of Bar Examiners. The first major coordination effort was the 1972 introduction of a 200-point multiple choice exam called the Multistate Bar Exam (MBE). Within 3 years, 44 states adopted the MBE, and by 2000, 49 states had adopted it. For most states, the MBE constitutes half of the bar exam. For some states, such as California, it constitutes a smaller part of the bar exam.

The second major coordination effort was the 2011 introduction of the Uniform Bar Exam (UBE). The UBE consists of three components: the MBE, the Multistate Essay Exam, and the Multistate Performance Exam. The UBE is a complete exam in that a state may

[4] We focus on obtaining a license, which allows one to permanently practice in a state. States also permit temporary admission to the bar for lawyers working on a specific case. This procedure is known as pro hac vice. To our knowledge, no state imposes a pro hac vice rule that depends on the origin state of the lawyer seeking temporary admission. Because the policy variation we are exploiting is at the corridor level – and thus varies based on the destination state only – the existence of the pro hac vice procedure does not pose a threat to our identification strategy.

adopt it in its entirety and have no state-specific component. This means that if multiple states adopt the UBE in its entirety, then someone only has to take the bar exam once, and they can obtain licenses in each of the states if they achieve the minimum score in each state. Alternatively, a state may adopt part of the UBE and supplement it with a state-specific component. Within 3 years of its introduction, 11 states adopted the UBE in whole or in part. By 2020, 34 states had adopted the UBE, of which 21 had adopted it in whole.

2.2 Bar Exam Waivers

To obtain a license to practice in a state, new law school graduates without a law license must pass the state's bar exam, either by taking it in its entirety, by taking the UBE in another state, or by transferring the MBE from another state and taking the state-specific component.[5] By contrast, to obtain a license to practice in a state, practicing lawyers who hold an active license in another state may be eligible for a waiver from taking the bar exam. Whether an experienced lawyer can obtain a bar exam waiver depends on the policies of the origin and destination states (i.e., the state in which the lawyer is currently licensed and the state in which the lawyer is seeking to obtain a license, respectively) and the lawyer's recent work experience. We explain each of these policies in turn.

First, some states allow for a bar exam waiver through so-called "Admission on Motion" and "Reciprocity" policies. These are the policies that lead to variation in bar exam waivers that we use in our empirical analysis. An Admission on Motion policy allows out-of-state lawyers with sufficient work experience to skip the exam and instead gain admission to the state bar "on motion." In practice, this means filing out some forms and paying a fee.[6] A Reciprocity policy limits the benefits of an Admission on Motion policy to lawyers coming from states that reciprocally allow for Admission on Motion. For example, if Michigan has a Reciprocity policy, then it would waive the bar exam for, say, Texas lawyers, if and only if

[5] All states have sunset policies that limit the time frame for transferring scores (typically 2 to 5 years). However, Wisconsin offers a unique exception to these rules. It grants graduates of its two public schools a "diploma privilege," which permits graduates to practice without taking the bar exam.

[6] For example, Connecticut's fee as of 2023 is $1,800 (Connecticut Bar Examining Committee, 2023).

Texas waives its bar exam for Michigan lawyers.

Second, to qualify for a state's Admission on Motion policy, a lawyer must have the requisite recent work experience. These requirements vary by state, but nearly all require the lawyer to have practiced for at least 5 out of either the last 7 or 10 years. A few states require 3 of the last 5 years. Because of data limitations, the empirical analysis counts a lawyer as "experienced" – and thus potentially eligible for a bar exam waiver – starting 5 years after obtaining their first license.[7] States typically have additional requirements for Admission on Motion, such as being in good standing in the state in which they practice and not having recently failed another state's bar exam.

The result of these policies is a set of contingent qualifications for a bar exam waiver.[8] In the empirical section, we will code a "corridor" between origin state i and destination state j as "open" if a lawyer from state i meeting certain qualifications, including sufficient recent work experience, does not have to take any exam to obtain a license in state j; otherwise, we code the corridor as "closed."

3 Data and Descriptive Statistics

To study the effects of bar exam waivers, we assembled three novel datasets. These datasets cover: (1) policies determining bar exam waivers, (2) license acquisitions of 1.7 million lawyers, and (3) disciplinary actions against lawyers in 37 states. This section introduces each dataset and presents descriptive statistics.

3.1 Data on State-Level Licensing Policies

One reason there has been limited research on the effect of occupational licensing on labor market mobility is that "it is difficult to determine the current requirements for licensure reciprocity, much less what they were in previous years—information required for

[7] For purposes of determining eligibility for an exam waiver, we assume that a lawyer practiced every year since they obtained a license. We do this because our data only allow us to determine when the lawyer obtained each license, not whether for any given year the lawyer was in fact practicing.

[8] Some states offer an alternative "attorney's exam" for experienced lawyers. These exams are an abbreviated or other form of the regular bar exam.

a traditional causal analysis of the effect of occupational licensing on interstate migration" (Johnson and Kleiner, 2020, at 348). We were able to collect exactly this information for the legal profession from annual editions of the Comprehensive Guide for Bar Admission Requirements.

We used this information to code each state's and D.C.'s Admission on Motion and Reciprocity policies each year from 1983 through 2019.[9] This resulted in 51 jurisdictions $\times$ 37 years $= 1,887$ jurisdiction-year observations. Across these observations, there is substantial variation in bar exam waiver policies (Figure 1). State-years between 1983 and 2019 are about equally split among waiving the exam for no one, waiving the exam reciprocally, and waiving the exam for all. Notably, over our sample period, a few states never waived the exam – including California, Delaware, and Florida – and a few states always waived the exam – including Iowa, Michigan, and Texas. Most states, however, changed their policy during this period. In total, states changed their policies on 54 occasions. Admission on Motion and Reciprocity policies have both trended upward. From 1983 to 2015, the percent of states with some form of Admission on Motion policy increased from 61 to 80 percent; at the same time, Reciprocity policies increased from 29 to 61 percent.

We use these state-level policies to determine whether an experienced lawyer can obtain a license without taking the bar exam for each origin-destination pair in each year. This resulted in 51 origins $\times$ 50 destinations $\times$ 37 years $= 94,350$ corridor-year observations. Like state bar exam waiver policies, there is substantial corridor-level variation in openness (Figure 2). Because all states do not adopt reciprocity policies, it is common for the ij corridor to be open even if the ji corridor is closed. For example, over the entire sample period, Arkansas has never had access to California, but California has sometimes had access to Arkansas. In total, out of the 2,550 corridors, 835 (33 percent) were always closed, 368 (14 percent) were always open, and 1,347 (53 percent) changed from closed to open (or vice

[9] The 1984 edition, which compiles 1983 policies, is the first year for which the Guide is available.

versa) one or more times.[10] The net result of the policy changes is that barriers to labor market mobility have tended to decrease over time (Figure 3). For example, the share of corridors that were open increased from 54 percent in 1983 to 76 percent in 2019.

3.2 Data on Lawyer Labor Market Mobility

In our setting, "labor market mobility" refers to a licensed lawyer acquiring an additional license to move or to expand their legal practice to a new state. To measure the history of where lawyers are licensed, we use lawyer profiles from the Martindale-Hubbell (MH) directory. MH has continuously published lawyer profiles since 1931. It currently draws from state bar directories, law firm listings, and other publicly available sources to create what is considered the largest and most comprehensive directory of American lawyers (Whisner, 2014). For each lawyer in the directory, MH creates a publicly available online profile that includes the law school the lawyer attended, the year they graduated, and the state and year in which they obtained their license(s).

We scraped these profiles to assemble the dataset on lawyers' license acquisitions. The dataset includes information on 1.7 million lawyers who received their first license through 2019.[11] Our focus is on experienced lawyers who obtained their initial license at least 5 years prior to obtaining a new license. During our sample period, an average of 5,383 experienced lawyers received a new license each year (Figure 4, dotted line). For comparison, an average of 35,189 new lawyers received their first legal license each year (solid line), and an average of 8,115 licensed inexperienced lawyers who do not have five years of experience received a new license each year (dashed line).[12]

---

[10] There is more variation in states' openness to other states than in states' access to other states (Appendix Figure A1). This is because even if a state waives for no one, it still has access to states that waive for all; but, even if a state waives for all, it still does not have access to states that waive for no one.

[11] We limit our sample period to the end of 2019 for two reasons. First, it appears that it takes a few years for MH to put up profiles for new lawyers. Second, many states temporarily changed their licensing requirement during the COVID-19 pandemic to avoid having to administer in-person bar exams.

[12] Since many recent law graduates obtain two licenses at the same time shortly after graduation, we define mobility as a subsequent license more than a year after obtaining an initial license. For example, New York and New Jersey coordinate to allow bar exam takers to sit for both exams.

There is substantial variation in licensing patterns across lawyers, experience levels, cohorts, law school rank, and corridors (Appendix Figure A2). Roughly 1 in 4 lawyers obtain multiple licenses over their career, and roughly 1 in 20 obtain more than two licenses (Panel A). Of lawyers who obtain multiple licenses, about 10 percent obtain multiple licenses in the same year as their law school graduation, about 40 percent obtain them within 3 years of their initial licensing, and about 80 percent obtain them within 10 years of their initial licensing (Panel B). Multi-licensing has become more prevalent for recent cohorts of lawyers. Among lawyers who started practicing law in the 1950s, only about 10 percent obtained a second license within 10 years of initial licensing (Panel C). By the 1990s, this figure was 30 percent. However, license acquisition rates given at least ten years of practice experience have remained fairly constant. The rise in multi-licensing for recent cohorts is thus concentrated in the first decade of lawyers' careers. Finally, lawyers from higher-ranked law schools are more likely to acquire multiple licenses (Panel D). Within 10 years of initial licensing, about a fourth of lawyers from the top 100 ranked law schools have multiple licenses, compared to about a fifth of lawyers from law schools ranked outside of the top 100.[13]

Our primary specification focuses on flows of lawyers through corridors between states. The annual flow of lawyers through corridors differs substantially (Figure 5).[14] For example,

[13] To further summarize labor migration patterns, we also construct measures of state-level in-migration and out-migration (Appendix Figure A3). In-migration is the share of a state's licenses granted to out-of-state lawyers, while out-migration is the share of out-of-state licenses granted to a state's lawyers. For example, for Texas, the in-migration rate would be the number of Texas licenses granted to non-Texas lawyers divided by the total number of Texas licenses, while the out-migration rate would be the number of non-Texas licenses granted to Texas lawyers divided by the total number of licenses that Texas lawyers hold. In-migration and out-migration rates significantly differ across states. In-migration is relatively high in parts of New England, the Pacific Northwest, and the Interior West. Out-migration is relatively high in parts of the Mid-Atlantic and the Midwest. Some states, like California and New York, have relatively low rates of both in-migration and out-migration. Of experienced lawyers who obtain a second license, about half stay within the same region. To further explore licensing patterns, Appendix Figure A4 documents the pairs of regions where double licensing is most common; Appendix Figure A5 documents the pairs of states where double licensing is most common; and Appendix Table A1 lists the top three states where lawyers are double licensed for each state.

[14] We consider a lawyer to have moved through the ij corridor in year t if the lawyer obtains a license for state j in year t and was licensed in state i for a year prior to t. If a lawyer has multiple licenses, they can therefore be counted as moving through multiple corridors in the same year. The District of Columbia ("D.C.") is a unique jurisdiction because it allowed lawyers who pass the bar exam in any state to obtain a license there too. As a result, most law school graduates who plan to work in D.C. take the bar exam in

it is much more common for a New York licensed lawyer to obtain a new license in Massachusetts (which is an example of geographically proximate states with large legal markets) than for a Vermont licensed lawyer to obtain a new license in Idaho (which is an example of geographically distant states with small legal markets). On average, 58 percent of corridors have flows of no more than 1 lawyer each year; 29 percent have flows of between 1 and 5 lawyers each year; 8 percent have flows between 5 and 10 lawyers each year; and 7 percent have flows of more than 10 lawyers each year. The corridors that have the highest number of lawyers moving through them tend to be adjacent states (Appendix Figure A6).

3.3 Data on Lawyer Discipline

To use discipline as a measure of lawyer quality, we collected novel data on disciplinary actions imposed by state discipline bodies. Because lawyers are professionally regulated by licensing bodies at the state level and there is currently no source of individual disciplinary actions at the national level, we separately collected data by state.

For this data collection process, we only used the official source of information on public disciplinary actions against lawyers. However, although each state's Supreme Court rules require information on all public disciplinary actions to be published, the way that information is published varies across states. As a result, collecting data on lawyer discipline required collecting information from a range of sources, including public websites, state bar magazines, and newspapers. For instance, some states maintain lawyer-level webpages for licensed lawyers that include information on public disciplinary actions. For these states, we scraped information directly from these webpages. Other states publish information on disciplinary actions in their state bar journals. For these states, we obtained hundreds of issues of these journals and searched for information on all public disciplinary actions.

---

another state. In our primary specification, we account for this unique situation by treating all lawyers who are initially double licensed in D.C. and some other state as only being licensed in D.C. for the purpose of benefiting from a future bar exam waiver. We do so because the inexperienced lawyers who obtain a D.C. and an additional license would likely be counted as practicing in D.C. if they were to seek an additional license in another state. However, below we explore the sensitivity of our results to alternative approaches to defining mobility for this group of lawyers.

We then matched this to the data from the MH profiles described in Section 3.2. To ensure the reliability of this matching process, we only matched disciplinary information from states where we were able to collect information on the year when a disciplined lawyer was licensed. The result was that we were able to match data on public disciplinary actions taken between 1990 and 2020 to MH profiles for lawyers from 37 states.

This data reveals that lawyers who obtain multiple licenses are disciplined at lower rates than lawyers who only obtain one license (Figure 6). For instance, over the first 30 years of lawyers' careers, 1.8 percent of lawyers who obtain multiple licenses are subject to professional discipline, compared to 3.0 percent of lawyers who only obtain one license. Notably, among the lawyers obtaining multiple licenses, lawyers who benefited from a waiver are disciplined at higher rates than lawyers who did not. For example, over the first 30 years of lawyers' careers, 2.1 percent of lawyers who benefited from a waiver are subject to professional discipline, compared to 1.6 percent of lawyers who did not.

4 Occupational Licensing and Labor Market Mobility

We now turn to estimating the effect of bar exam waivers on labor market mobility.

4.1 Research Design

Let y_{ijt} be the number of experienced lawyers from origin state i obtaining a license in destination state j in year t. Let x_{ijt} be an indicator equal to one if an experienced lawyer licensed in state i can obtain a license for state j in year t without taking j's bar exam. If x_{ijt} switches from 0 to 1, we say that the ij corridor "opens." If it switches from 1 to 0, we say that the ij corridor "closes."

To estimate the effect of bar exam waivers on labor market mobility, we use a stacked event study difference-in-differences research design (Cengiz et al., 2019; Deshpande and Li, 2019).[15] To implement this design, we treat an event as occurring whenever a corridor opens

[15] An advantage to this specification, which has been used extensively in prior work (e.g., Frakes and Wasserman, 2023), is that it offers a simple and transparent approach for incorporating multiple changes in licensing policy staggered across time in a manner that can account for unit-varying dynamic treatment

or closes. Between 1983 and 2019, there are 2,396 such events (Figure 7). We use an event window of 5 years before and after the event, for a total of eleven years. We exclude an event if the treated corridor experienced another event within 5-years before or after the event. This restriction excludes 662 events (Appendix Figure A7). We do not observe bar exam waiver policies before 1983, so to allow for a 5-year pre-period where we can ensure no changes in bar exam waivers, we exclude the 297 events that occurred before 1988. Our data on licensing ends in 2019, so to allow for a 5-year post-period for each event, we exclude 225 events that occurred after 2014. These restrictions result in a final sample of 1,212 events.

For each event, we construct a subsample containing exactly one treated corridor and a set of control corridors. For each treated corridor, the corresponding set of controls is made up of each corridor that did not experience a change in openness in the event window. Control corridors can be in more than one subsample of control groups.

Using the stacked dataset of event subsamples, we estimate

$$y_{eijt} = \alpha + \beta x_{eijt} + \sigma_{eij} + \zeta_{et} + u_{eijt} \tag{1}$$

where e denotes the event subsample, σ_{eij} are event by corridor fixed effects, ζ_{et} are event by year fixed effects, and u_{eijt} is the error term. In our primary specification, we report standard errors clustered at the corridor level; doing so accounts for serial correlation at the level of the treated unit over time as well as for the duplication of corridor-years across events in our stacked regression (Deshpande and Li, 2019; Wing et al., 2024). However, we consider alternative approaches to statistical inference below.

The identifying assumption for this design is that, in the absence of treatment, the flow of lawyers through each treated corridor would have evolved similarly to the flow of lawyers through the set of control corridors corresponding to the same event. This parallel trends assumption could be violated, for example, if states adjust their bar exam waiver

effects (e.g., Goodman-Bacon, 2021). We consider alternative specifications below.

policies based on factors that would exert an independent effect on flows between corridors, such as a state expanding its waiver policy because it anticipates an increase in demand for legal services. When parallel pre-trends hold, we can interpret β as the causal effect of a bar exam waiver on labor market mobility.

To explore the plausibility of parallel pre-trends, we estimate an event-study specification:

$$y_{eijt} = \alpha + \sum_{s \neq -1} \beta_s d_s \Delta x_{eij} + \sigma_{eij} + \zeta_{et} + u_{eijt} \tag{2}$$

where s indexes event time, d_s is an indicator for event time s, and $\Delta x_{eij} \in \{-1, 0, 1\}$ is corridor ij's change in openness in event e, with a positive value denoting that a corridor opens and a negative value denoting that a corridor closes. Equation 2 thus differs from a standard event study specification in that it adjusts for the direction of the change in openness. β_s is the estimated difference in mobility in the treated corridor at event time s. We estimate β_s using the stacked event study dataset with a 5-year window, excluding the d_s indicator the year before the event.

4.2 Primary Results

Figure 8 investigates parallel pre-trends and explores treatment effect dynamics by estimating the event study specification from Equation 2. The figure reveals that none of the estimates in the pre-period are statistically different from zero, consistent with the parallel pre-trends assumption and suggests that states do not change their waiver policies in response to recent changes in lawyer flows. The figure also reveals initial evidence that a bar exam waiver immediately increases mobility and that mobility appears to stabilize at a higher level by the second year following a waiver.

Table 1 investigates the effect of a bar exam waiver on labor market mobility by estimating Equation 1. Column 1 reports the estimates from our primary specification. We find that, on average, allowing lawyers from state i to obtain a license in state j without taking j's bar exam induces an additional 0.62 lawyers per year from i to obtain a license in

j. The average number of lawyers flowing through treated corridors when they are closed is 1.62. The estimate thus implies that waiving the bar exam increases labor market mobility by 38 percent.

Although our estimated effect of the bar exam waiver is large in percentage terms, it is small in levels: the point estimate represents a difference of less than one lawyer per year. The explanation is that mobility in corridors varies widely and is very small in many corridors (Figure 5). Figure 9 plots the estimated effect by corridor size – i.e., by the average number of lawyers moving through a corridor per year when the corridor is closed. The estimated treatment effect in levels increases with corridor size (Panel A) but is strikingly consistent in percentage terms (Panel B).

Our primary results pool all changes in bar exam waivers, which includes corridors opening or closing. Columns 2 and 3 of Table 1 separately estimate the effects by the direction of the change. We find that opening a corridor leads to a larger change in mobility than closing one: closing a corridor decreases mobility by 23 percent, whereas opening a corridor increases mobility by 48 percent. A plausible explanation for this asymmetry is that if a corridor is closed, there would be a stock of lawyers who would have moved through the corridor but for the bar exam requirement. Thus, when a corridor opens, there would be an initial surge of lawyers as the stock of lawyers clears. In contrast, closing a corridor would not exhibit the same dynamic.[16]

As a check on the plausibility of our results, and to better understand the mechanism through which bar exam waivers increase labor market mobility, we next consider treatment effect heterogeneity with respect to the difficulty of the bar exam being waived or imposed. To do so, we construct an index of bar exam difficulty based on the minimum passing score.[17]

[16] An alternative explanation for the asymmetry could be if the announcement of an impending closure of the ij corridor causes a surge of lawyers from i to move through the corridor before the closure is effective; if those who take advantage of the waiver at the end of the year show up as new licensees during the subsequent year, this could attenuate our estimate of the policy's short-term effect. Appendix Figure A8 reports the event study estimates separately for corridor closures and openings and provides some support for this hypothesis.

[17] Data for this analysis comes from the Comprehensive Guide to Bar Examiners. We construct the index

We then interact the indicator for a bar exam waiver with an indicator for whether the bar exam is above median in terms of the minimum passing score in the year before the change in bar exam waiver. Columns 4 and 5 of Table 1 suggest that the effect of an exam waiver is substantially larger for more difficult exams: bar exam waivers increase flows by 19 percent in below-median difficulty states, compared to 67 percent in above-median difficulty states.

We next investigate the nature of labor market mobility induced by bar exam waivers. In particular, lawyers may seek to obtain a license in a new state because they wish to move to that state or because they wish to expand their practice into that state. Our primary measure of mobility combines both of these cases. To better distinguish between them, Columns 6 and 7 of Table 1 separately estimate our primary specification for corridors that involve geographically adjacent states – for which we expect more flows to be associated with an expansion of an existing practice – and corridors that involve non-geographically adjacent states – for which we expect more flows to be associated with a change in physical residence. We find that bar exam waivers increase mobility by 36 percent for non-adjacent states, compared to 45 percent for adjacent states. These results suggest that the estimates from the primary specification capture a positive effect of waivers on both residential mobility and expansions of existing legal practices into new markets.

Finally, we consider the implications of our findings for two hypothetical national-level policies: waiving the bar exam for all experienced lawyers versus waiving it for no one. We do not intend to present this as a rigorous and definitive estimate of the impact of such policies, but rather as an attempt to estimate the magnitudes involved. In absolute terms, if the difference in labor market mobility between the two national policies (all open corridors versus all closed corridors) were the same as the primary corridor-level estimate (38 percent), then on average 6,059 experienced lawyers would obtain an additional license each year under a national policy waiving the bar exam for all experienced lawyers, compared to 4,391 under a national policy waiving the bar exam for no one (a difference of 1,668 lawyers

---

separately for each year, using the panel of minimum passing scores by state between 1983 and 2019.

per year).[18] For reference, there are approximately 1.3 million lawyers in the United States. As in the primary specification, this calculation assumes that each corridor's flow depends only on a fixed base rate and whether that corridor is open. We consider the implications of relaxing this assumption below.

4.3 Robustness Checks

This subsection explores the sensitivity of our results to several research design choices and other potential concerns.

Substitution Between States. In the primary specification, we assumed that the ij flow depends only on a base rate and whether ij is open. In practice, however, it may be that the ij flow also depends on whether other corridors are open. This could be for many reasons, but perhaps the most plausible one is that lawyers may substitute one destination state for another. That is, the decisions of lawyers in state i to obtain a license in state j may depend not just on whether ij is open, but also on whether any other corridor originating in i is open. This would violate the stable unit treatment value assumption (SUTVA) because the treatment assigned to one corridor would impact the outcome for another.

We examine this by refining the set of control corridors (Appendix Table A2). We first exclude corridors with the same origin state as the treated corridor (Column 1). For example, if Michigan-Texas is the treated corridor, control corridors originating from Michigan (such as Michigan-Florida) are excluded. This approach accounts for the possibility that the treated corridor may attract lawyers from the same origin state who would have otherwise chosen a different destination. For example, if Michigan-Texas opens while Michigan-Florida

[18] These figures come from the following calculation. In our data, an average of 5,383 experienced lawyers obtained a new license each year (Figure 4). As discussed in Section 3, this data set of lawyers is derived from the Martindale-Hubbell (MH) database. Relying on a prior estimate of the MH coverage rate of 61 percent from Rozema (2024b), and assuming that selection into the MH data is as good as random, the total number of experienced U.S. lawyers obtaining a new license each year is $5,383 \,/\, 0.61 \approx 8,825$. Finally, in 2019 about 70 percent of (lawyer-weighted) corridors were open (Appendix Figure 3). Using the corridor-level estimate of the effect of the bar waiver on mobility (38 percent), this implies that if all corridors were open, the number of lawyers obtaining an additional license each year would be $\frac{(1+0.38)\times 8,825}{(1+(0.7\times 0.31))} \approx 6,059$; if all were closed, then the number would be $\frac{8,825}{(1+(0.7\times 0.38))} \approx 4,391$.

remains closed, lawyers who might have gone to Florida could now choose Texas instead. We then exclude corridors with the same destination as the treated corridor (Column 2). In this case, if Michigan-Texas opens while Florida-Texas remains closed, Florida lawyers who might have gone to Texas could now choose a different state due to the influx of Michigan lawyers. Finally, we exclude control corridors with either the same origin or destination as the treated corridor. In the Michigan-Texas example, this would exclude both Michigan-origin corridors (like Michigan-Florida) and Texas-destination corridors (like Florida-Texas). The results across all columns remain consistent with the baseline specification.

The previous analysis establishes that our primary results are robust to removing control corridors that might lead to a SUTVA violation. We next examine the magnitude of substitution effects more directly by comparing events that present varying levels of substitution opportunities. Specifically, suppose an event causes ij to change from closed to open. If multiple other corridors originating in state i also opened at the same time, lawyers from i who might have obtained a license in state j could instead choose one of the other newly opened destination states. In contrast, if ij were the only corridor originating from i that opened, such opportunities for substitution would not arise. Therefore, we can gain insight into the presence and magnitude of substitution effects by separately estimating the impact of opening few versus many corridors at the same time.

To explore this, we compare the estimated treatment effects from events driven by origin state policy changes with those driven by destination state policy changes. We would expect origin state policy changes to create more substitution opportunities. For example, if state i changes from closed to reciprocally open, lawyers in i could suddenly gain access to 20 or more states, leading to many substitution possibilities. In contrast, this is less likely for corridors where the ij corridor opens because of a change in destination state policy. If the change in j's policy leads to ij opening, and i was already reciprocally open, then ij might be the only newly-opened corridor for lawyers in i. The data support this. On average, when a given corridor ij opens because of a change in origin state policy (i.e., i's policy),

the average number of open corridors originating from i increases by 21 (from 13 to 34). In contrast, when ij opens because of a change in destination state policy (i.e., j's policy), the average number of open corridors originating from i only increases by 2 (from 25 to 27).

The results suggest moderate substitution effects (Appendix Table A3). Specifically, the impact of an exam waiver is 42 percent for events driven by destination state policy changes (Column 1), compared to only 30 percent for events driven by origin state policy changes (Column 2).

Relatedly, one might further hypothesize that the effects could vary depending not just on the number of newly opened corridors, as discussed above, but also on the number of corridors initially open. To see why, suppose a lawyer in i would go to j or k if either ij or ik were open, otherwise the lawyer stays in i. For this lawyer, j and k are substitutes. However, the lawyer only wants to move once. If ij opens first, the lawyer obtains a license in j and is no longer at risk of moving. If ik subsequently opens, the lawyer will not get a license in k. Because having corridors open in the past reduces the stock of lawyers at risk of moving, the impact of opening ik would thus be lower than it would have been had ik opened first. This example highlights why substitution effects could cause the impact of opening the n-th corridor to be larger than opening the $n+1$ corridor. More generally, the magnitude of the effect of a bar exam waiver on labor market mobility might depend on the order in which corridors open.

To explore it empirically, we separately estimate the impact of an exam waiver using two types of events: (1) events for which the treated corridor's origin state had access to relatively few other states (fewer than 25), and (2) events for which the treated corridor's origin state had access to relatively many other states (25 or more). For both cases, we restrict the sample to events involving few opportunities for substitution. Specifically, if ij is treated, then we would include ij only if the net number of open corridors originating from i changed by at most 2 (including ij). The results suggest there is little to no difference (Appendix Table A4). For events where the origin state had high initial levels of access to

other states, bar exam waivers increase mobility by 32 percent (Column 1). For events where the origin state had low initial levels of access to other states, bar exam waivers increase mobility by 28 percent (Column 2).

Substitution Over Time. Bar exam waivers are available only to experienced lawyers. Our primary specification thus focuses on mobility for this group. However, a bar exam waiver for an experienced lawyer could cause lawyers who would have otherwise obtained a license when inexperienced to instead wait until they can benefit from the waiver. In this case, our estimates would overstate the net effect of bar exam waivers on the mobility of all lawyers.

To account for this possibility, we repeat our primary specification but use as the outcome the number of all lawyers (both experienced and inexperienced) flowing through the corridor, rather than only the number of experienced lawyers (Appendix Table A5). We obtain similar results when including this group (Column 1). We similarly find no evidence that bar exam waivers affect the mobility of inexperienced lawyers (Column 2).[19] Combined, these results do not provide evidence that changes to bar exam waivers affect the behavior of inexperienced lawyers.

Confounding Events or Conditions. It is possible that there could be contemporaneous changes in other policies or labor market conditions that are not accounted for by our primary specification. For example, if a state was experiencing strong demand for legal services, it could make it easier to obtain and maintain a license by changing multiple policies at the same time, like simultaneously adopting bar exam waivers for experienced lawyers and decreasing Continuing Legal Education requirements. In this example, our primary specification could be conflating the effect of the bar exam waiver with the effect of the contemporaneous changes in other policies and with the effect of the strength of the market for legal services.

[19] Appendix Figure A9 recreates Figure 8 while using the number of licensed inexperienced lawyers who migrate as the outcome. Appendix Figure A10 reports experience-specific estimates.

We investigate potential bias from confounding policies and market conditions by selecting control corridors that would be equally affected by all other policies and market conditions in the state that triggered the event. Importantly, when a given state changes its bar exam waiver policy, it can affect the corridors involving that state differently. For instance, if a destination state changed from reciprocally waiving to waiving for all, it would impact corridors where the origin state waived for none but not impact corridors where the origin state reciprocally waived or waived for all. However, confounding policies and market conditions that affect lawyers' decisions to seek an additional license in the destination state (other than the waiver policy) would apply to lawyers from all origin states and not just the origin states where the bar exam waiver changed. As a result, the corridors involving a given state that does not experience a change in openness can be used as a control group that nets out confounding policies and market conditions affecting that state.

As we will explain, however, implementing this strategy requires one approach for events that were triggered by a destination state policy change and a different approach for events that were triggered by an origin state policy change. For events triggered by destination state changes, our control group consists of corridors with the same destination state j that did not experience a change in a bar exam waiver but experienced a change in the other policies and market conditions in the destination state. In particular, if ij opens because of a state j policy change, we only use kj corridors that did not change openness status throughout the entire event window as the control group. These kj corridors witness other potential changes in destination state j policies and market conditions but hold fixed the bar exam waiver. The only type of events that can be used for this approach involves changes to or from waiving reciprocally because in these situations there are some kj corridors that do not experience a change in their access to state j. For example, if a state responds to increased demand for legal services by switching from waiving reciprocally to waiving for all, then lawyers from origin states that waive for none will gain access to this potential destination state, whereas lawyers from origin states that waive for all would have already

had access to this potential destination state (and thus can serve as a control group). In contrast, the reason this approach cannot include events involving changes to or from waive for all to waive for none is that they impact all corridors involving the destination state.

For an event triggered by an origin state change, our control group consists of corridors with the same origin state i that did not experience a change in a bar exam waiver but experienced a change in the other policies and market conditions in the origin state. In particular, if ij opens because of a state i policy change, we only use ik corridors that did not change openness status throughout the entire event window as the control group. These ik corridors witness other potential changes in origin state i policies and market conditions but hold fixed the bar exam waiver. Unlike with events triggered by destination state policy changes, we can use all events triggered by origin state policy changes for this approach and not just those that were triggered by changes to or from waive reciprocally. This is because there are destination states that are always open or closed, such that some corridors with origin i remain open or closed regardless of the change to the origin state's policy.

By refining the control group in this way, we address potential confounding factors at the state-year level. Appendix Table A6 reports the results. When using this approach to accounting for confounding policies and market conditions, the results are consistent with the estimates from our primary specification. They suggest that waiving the bar exam increases labor market mobility by 41 percent. The estimate from our primary specification of 38 percent is thus not a product of confounding policies or market conditions.

Stabilization of Treatment Effect Trajectories Among Control Units. In our primary specification, the control group for each treated corridor includes any corridor that did not experience a change in openness in the 5 years surrounding the event. However, there could be slow-moving dynamics from changes in bar exam waivers that "contaminate" our set of controls. We investigate the robustness of our results to this concern in several ways (Appendix Table A7). We first limit the control group to corridors that were not previously exposed to a change in the bar exam waiver during our sample period (Column

1). We next limit the control group to corridors that were always open during the sample period (Column 2) or always closed during the sample period (Column 3). Alternatively, we also consider the possibility that the most comparable set of control corridors are those that experience a policy change at some point during the sample period but not during the event window for a given event (Column 4). The estimated effect of a bar exam waiver is largely comparable across these approaches.[20]

Statistical Inference. In our primary specification, we clustered standard errors at the corridor-level to account for serial correlation of treated units over time and for the duplication of corridor-years across events in our stacked event study design (Deshpande and Li, 2019; Wing et al., 2024). Appendix Table A9 reports the results while clustering at different levels and reveals consistent results.

A potential limitation of these approaches in our setting is that they do not account for the complex dependence of corridor-level openness within state policy changes; that is, a single state policy change simultaneously opens or closes multiple corridors, depending on other states' policies. To account for this dependence, we consider an alternative approach to inference based on a permutation test designed to mirror the statistical association across policies that we observe in our data. In particular, we simulate variation in bar exam waivers at the corridor-level by randomly assigning both initial state-level waiver policies and their changes based on the empirical transition matrix we observe during our sample period (see Appendix B for details). When using this approach, the actual estimated coefficient compared to the mean is larger than all coefficients compared to the simulated mean, implying a p-value for our primary estimate of less than 0.001 (Appendix Figure B2).

Sample Selection. As discussed in Section 3, our data on license acquisition comes from Martindale-Hubbell (MH) profiles. Because the MH data does not capture the full

[20] Additionally, our primary specification excludes events only if there was a previous change in the corridor's openness within the past 5 years. In Appendix Table A8, we exclude events in which there was a previous change in the corridor's openness within the previous 10 years (Column 1) or at any prior point in the sample period (Column 2). Our results are largely stable across these specifications.

universe of lawyers, our estimates need to be adjusted when extrapolating outside of our sample population. To provide a sense of how selection into our sample could affect our estimates, we consider three possibilities: (1) missingness in the MH data is as good as random with respect to the effect of bar exam waivers on lawyer mobility; (2) bar exam waivers have a disproportionately small effect for the lawyers not in MH; and (3) bar exam waivers have a disproportionately large effect for the lawyers not in MH. To assess how these possibilities would affect the interpretation of our results, we rely on a prior estimate for the MH coverage rate of approximately 61 percent from Rozema (2024b).

Appendix Table A10 reports the results. Under the first possibility that selection into the MH sample is as good as random, the estimated relative effect would extrapolate without adjustment to the full lawyer population (as discussed above), but the estimated effect in levels would need to be scaled up to account for the share of all lawyers who we observe, $\frac{1}{0.61}$, and would be 1.02 lawyers per corridor per year. In contrast, under the most extreme form of the second possibility where bar exam waivers have no effect for the lawyers not in MH, the estimated effect in levels should not be adjusted, but the relative effect should be scaled by 0.61, and would be 23 percent. Under the third possibility where bar exam waivers have a larger effect for the lawyers not in MH, both the levels effect and relative effect should be scaled up in magnitude when extrapolating to the overall lawyer population. For example, if bar exam waivers affect the mobility of lawyers not in MH by 50 percent more than it does for lawyers in MH, then the estimated effect in levels would be 0.98 lawyers per corridor per year and the relative effect would be 53 percent. Overall, our primary estimate represents a lower bound for the effect of bar exam waivers on the level of overall lawyer mobility, and our estimates imply that the percentage effect of waiver policies for all lawyers is at least 23 percent, depending on the nature of selection into the MH data set.

Alternative Empirical Specifications. Our primary specification consists of a stacked event study, but we also investigate the sensitivity of our results to a range of alternative proposed methods for estimating causal effects in panel data settings with staggered

treatment timing.[21] Appendix Table A11 and Appendix Figures A11 and A12 report results from these different estimators. Across different approaches and specifications, the results are broadly comparable to the estimates from our primary specification.

Lawyers Moving Through Multiple Corridors. When experienced lawyers with two or more licenses obtain a third or subsequent license, there is a question of how to count their origin state. For instance, if a lawyer obtains a license in state i and then state j and then state k, it is not obvious which corridors they should count as flowing through. In the primary specification, we take the most expansive approach and count such lawyers as moving through the ij, jk, and ik corridors.

But there are at least four other ways of counting these lawyers. First, we could count the lawyer as moving through the ij and ik corridors. This would assume that lawyers always use the bar exam waiver associated with their initial license when obtaining an additional license. Second, we could count the lawyer as moving through the ij and jk corridors. This would assume that lawyers always use the bar exam waiver associated with their most recently obtained license when obtaining an additional license. Third, we could count the lawyer as just moving through the ij corridor. This would exclude the movements of lawyers who have already obtained two licenses from the sample. Finally, we could (1) count the lawyer as moving through the ij corridor but then (2) use information on how lawyer obtained a license in j as well as the policies of the ik and jk corridors to classify whether the lawyer had a bar exam waiver into k from either i or j and then (3) count the lawyer in the ik and jk corridors depending on (2). This conditional approach may be the most accurate reflection of how lawyers approach applying for new licenses, but it requires making assumptions about which corridor a lawyer flows through based on different state-level rules.

Appendix Table A12 investigates the robustness of our results to using these four

[21] We specifically report results from a conventional Two Way Fixed Effects (TWFE) specification as well as results from the following estimators: Arkhangelsky et al. (2021); Borusyak et al. (2024); Callaway and Sant'Anna (2021); de Chaisemartin and D'Haultfoeuille (2024); Dube et al. (2023); Freyaldenhoven et al. (2019); Goodman-Bacon (2021); Roth and Sant'Anna (2023); Sun and Abraham (2021); Wooldridge (2023).

alternative approaches to counting lawyers who obtain a third or subsequent license. The results for all four alternative approaches are positive, statistically significant, and similar in magnitude to our primary results. More specifically, the estimated effect of bar exam waivers on mobility for these approaches ranges from 40 to 54 percent.

The District of Columbia. Our primary results include both treated and control corridors that involve D.C. However, D.C. has made it possible for lawyers who pass the bar exam in any state to obtain a license in D.C., regardless of their number of years of experience. Given the unique nature of bar exam waivers in D.C., Appendix Table A13 assesses the sensitivity of our results to excluding events involving D.C. and any control corridors involving D.C. Making the exclusion for events and corridors involving D.C. does not meaningfully alter our estimates.

5 Occupational Licensing and Lawyer Quality

The results in the previous section provide evidence that state occupational licensing requirements impede the labor market mobility of lawyers across U.S. legal markets. However, states may nonetheless prefer to limit out-of-state lawyers' mobility into the state if requiring lawyers to pass the bar exam is an effective means of improving the quality of lawyers in the state. Indeed, the standard story for why occupational licensing requirements may be welfare-enhancing is that they screen out individuals who would provide lower quality services to consumers (e.g., Kleiner and Soltas, 2023).

However, it is not obvious that a bar exam waiver would increase the mobility of lower-quality lawyers more than that of higher-quality lawyers. For example, if a primary reason the bar exam requirement is costly to experienced lawyers is the time required to prepare for and take the exam, and if higher-quality lawyers tend to have more successful legal practices than lower-quality lawyers, the lost time associated with the exam entails a higher opportunity cost for higher-quality lawyers.[22] As a result, a bar exam waiver could,

[22] Additionally, highly educated workers are relatively mobile (e.g., Malamud and Wozniak, 2012; Notowidigdo, 2020) and enjoy greater returns to job-match quality (e.g., Amoir, 2023), so they thus may be

in theory, increase the quality of lawyers who move or expand their practice into a state.

A key empirical question for assessing the welfare effects of bar exam requirements is thus whether they disproportionately screen out lower quality lawyers. In this section, we empirically assess the effect of bar exam waivers on lawyer quality by estimating how they shape the composition of migrating lawyers.

5.1 Data

We use two proxies of quality. First, we use whether a lawyer is subject to professional discipline. Professional discipline is used as a negative outcome and a measure of quality in a range of different settings (e.g., Egan et al., 2019; Dharmapala et al., 2020; Liu and Hyman, 2021). As Section 3.3 explained, this data includes information on the disciplinary actions against lawyers that occurred in 37 states between 1990 and 2020. For this analysis, we thus use a more restricted sample than for the mobility analysis reported in Section 4.

As a second measure of lawyer quality, we use the selectivity of the law school that the lawyer attended. The institution at which a professional obtained their training is a common proxy for quality across a range of occupations (e.g., Angrist and Guryan, 2008; Ramseyer and Rasmusen, 2015; Larsen et al., 2020). For this analysis, we cannot use the full sample because our data does not identify the law school from which a lawyer earned a law degree for 11 percent of experienced lawyers who obtained a second license. Using the remaining 89 percent of lawyers, our specific measure of lower quality for this analysis is whether the law school a lawyer earned a law degree is consistently ranked outside the top 100 law schools according to the U.S. News and World Report. There were roughly 200 law schools in the U.S. in our sample period, so this measure of lower quality roughly splits the law schools into above and below-median ranking.

Both of these measures of quality have benefits and drawbacks along several dimensions. First, professional discipline is directly related to the quality of legal services, but law school selectivity is only related to the potential quality of a group of lawyers. Second,

more inclined to take advantage of bar exam waivers.

professional discipline is a strong proxy for quality because it captures the quality of specific lawyers for bad outcomes, but law school selectivity is a weaker proxy because there is likely considerable variation in lawyer quality within law schools. Third, law school selectivity captures a large group of lawyers, but professional discipline is a rare outcome. Finally, we have data on law school selectivity in all states, but we only have data on professional discipline in 37 states.

5.2 Research Design

To estimate the impact of bar exam waivers on lawyer quality, our research design is the same for both measures of quality. For concreteness, we describe the research design in the context of professional discipline. Lawyers who obtain a license through a bar exam waiver include lawyers who would have obtained an additional license even if they had to pass the bar exam ("always movers") and lawyers who only obtained an additional license because of the bar exam waiver ("marginal movers"). In setting occupational licensing requirements, a relevant policy question for a state concerned about the quality of legal services is whether the marginal movers are of lower quality than the always movers.

To investigate this question, we need a research design that allows us to estimate the discipline rate of the marginal movers. We estimate this using a two-stage least-squares (2SLS) estimator. In this setting, the "instrument" is the bar exam waiver, the first stage is the effect of the bar exam waiver on overall mobility, and the reduced form is the effect of the bar exam waiver on the mobility of lawyers subjected to professional discipline for misconduct. The regression specification for the first stage is the same as the primary specification in the previous section (Equation 1), and the regression specification for the reduced form is:

$$d_{eijt} = \alpha + \beta x_{eijt} + \sigma_{eij} + \zeta_{et} + u_{eijt} \tag{3}$$

where x_{eijt} is again an indicator equal to one if an experienced lawyer licensed in state i

can obtain a license for state j in year t without taking j's bar exam, and the new variable d_{eijt} is the number of the y_{eijt} lawyers flowing through corridor ij in event e and year t who were disciplined at some point in their career. For example, if 10 lawyers flowed through a corridor in a year, and 1 of them was disciplined at some point in their career, then $y_{eijt} = 10$ and $d_{eijt} = 1$. (For the law school selectivity measure, d_{eijt} is the number of the y_{eijt} lawyers who attended a law school ranked outside of the top 100.) Note that this approach uses the same stacked event study design as in the previous section (Equation 1), including by using the same sets of fixed effects and clustering standard errors at the corridor level. Depending on the analysis, the 2SLS estimate captures the discipline rate of the marginal movers or the share of marginal movers who attended a law school outside of the top 100.

Like a conventional IV analysis, interpreting our 2SLS estimate as the quantity of interest requires an independence assumption, an exclusion restriction, and a monotonicity assumption to hold. We discuss these assumptions first in the context of professional discipline and then in the context of law school selectivity. The independence assumption in our setting is that a bar exam waiver in the ij corridor must be independent of other factors that may influence the mobility of lower-quality lawyers through the ij corridor. One potential concern is that waiver policies and discipline policies are set together. For instance, it is possible that states could simultaneously change their bar exam waiver and their policies on discipline. However, any corresponding changes in the discipline system would affect lawyers who move through the ij corridor both with and without the waiver. To see why, let year t denote the cohort of lawyers moving in that year (as before), and let year h denote the year that a lawyer is at risk of being disciplined. Because a bar exam waiver applies the year the lawyer moves but the disciplinary system applies to all active lawyers, lawyers flowing through the ij corridor in year t are subject to the disciplinary systems in $h \geq t$, and lawyers flowing through the ij corridor in year $t+1$ are subject to the disciplinary systems in years $h \geq t+1$. As a result, changes in discipline policies would not lead to violations of the independence assumption.

The exclusion restriction in our setting is that a bar exam waiver in the ij corridor influences the mobility of lower-quality lawyers through corridor ij only by increasing total mobility through the ij corridor. One aspect of this assumption is selection into discipline. For example, this restriction would be violated if the state j disciplinary body is more likely to discipline lawyers flowing through ij who qualify for a bar exam waiver than it is to discipline lawyers flowing through ij who did not qualify for a bar exam waiver. However, state disciplinary bodies apply consistent standards for imposing public discipline, and the rules of professional conduct do not take into account how a lawyer obtains a license. Apart from this potential concern, it is difficult to think of a reason why a bar exam waiver impacts the mobility of lower-quality lawyers differently than high-quality lawyers other than through mobility itself. Therefore, the exclusion restriction is likely to hold.

The monotonicity assumption in our setting is that a bar exam waiver does not cause the mobility of lower-quality lawyers in some ij corridors to increase but causes the mobility of lower-quality lawyers in other ik corridors to decrease. Although it is possible that removing barriers to practice in state j could cause lawyers in state j to be more likely to move their practice to state k, our focus is on the flow of lawyers into state j from other states, and there is no reason why removing barriers to practice in state j would decrease mobility *into* state j.

Any concerns with using discipline as a proxy for lawyer quality in our research design are largely not present when using law school selectivity as a proxy for lawyer quality. For the independence assumption, it is difficult to think of other factors that are related to a bar exam waiver that influence the mobility of lawyers only from low-ranked law schools. For the exclusion restriction, it is similarly difficult to think of reasons why a bar exam waiver in the ij corridor would be related to the mobility of lawyers from low-ranked law schools through the ij corridor other than through its impact on overall mobility. For the monotonicity assumption, there is no obvious reason why removing barriers to practice in j would decrease the mobility of lawyers from law schools outside of the top 100 into state j. Overall, when

using law school selectivity as a proxy for lawyer quality, our institutional setting suggests the identifying assumptions for interpreting the 2SLS estimates as the discipline rate of the marginal movers are likely to hold.

5.3 Results

Table 2 reports the results using professional discipline as a proxy for lawyer quality. We first estimate the reduced form regression of the number of lawyers obtaining an additional license who are disciplined at some point during their career on the bar exam waiver (Column 1). We find that a bar exam waiver increases the flow of lawyers who are disciplined at some point during their career by 0.025 lawyers per year. Although this effect is small in absolute magnitude, many corridors have few moving lawyers and most lawyers are never disciplined, so it represents a 76 percent increase relative to the mean in the treated group without a bar exam waiver.

Next, we estimate the first stage regression of the total number of lawyers obtaining an additional license on the bar exam waiver (Column 2). In this restricted sample, we find that bar exam waivers increase mobility by 36 percent (0.683 relative to the mean in the treated group without a bar exam waiver of 1.922), which is similar to the 38 percent effect using the full sample (Column 1, Table 1) ($F \approx 40$).

Finally, we estimate the IV regression (Column 3). We find that among lawyers induced to obtain a license because of the bar exam waiver, 3.6 percent are subject to discipline at some point in their careers.[23]

With the 2SLS estimate, we then compare the discipline rate of the marginal movers to the discipline rate of the always movers. Because the length of careers after obtaining a new license we observe differs for lawyers who obtained a license with a waiver than those who

[23] One concern is that there are differences in the discipline systems between states, so using corridors that involve different destination states involves using a measure of discipline that is generated from a different system. To account for differences in discipline systems between states, we restrict the control group for each event to corridors with the same destination state. We find consistent estimates when doing so (Appendix Table A14).

obtained a license without a waiver,[24] we estimate the discipline rate of the always movers from the same period as the marginal movers. Of course, we only observe the discipline rate of all lawyers obtaining a license with a waiver, and the discipline rate of always movers is not directly observed.

However, we back out this unobserved discipline rate by combining several other estimates. To do so, note that the discipline rate of all lawyers who move when a bar exam waiver is in place is a weighted average of the always and marginal movers: $r = r_A(1-p) + r_M p$, where $r_A = \frac{\sum_{i=0}^{A} d_i}{A}$ is the discipline rate of the lawyers who are always movers in the treated corridors with a bar exam waiver (where A is the number of lawyers in this group), $r_M = \frac{\sum_{i=0}^{M} d_i}{M}$ is the discipline rate of the lawyers who are marginal movers in the treated corridors with a bar exam waiver (where M is the number of lawyers in this group), and $p = \frac{M}{M+A}$ is the fraction of all lawyers who move with a bar exam waiver who are marginal movers. We back out r_A from this equation using estimates of p, r_M, and r.[25] Comparing the discipline rate of the always and marginal movers, we find that the marginal movers are disciplined at more than double the rate as always movers (3.6 percent compared to 1.7 percent, $p < 0.05$) (Columns 4-5).

Table 3 reproduces the results from Table 2 but uses law school selectivity as a proxy for lawyer quality. We find that 53.5 percent of lawyers induced to move because of the bar exam waiver graduated from a law school outside the top 100 (Column 3). We then estimate the percent of always-mover lawyers who graduated from a law school outside the top 100

[24] Relatedly, note that the discipline rate of the always and marginal movers is a censored lifetime rate through 2020, meaning we do not observe discipline over lawyers entire career if those who move in the 5 years after the events we observe are still active after 2020. The discipline rates we estimate are thus expected to be lower than the lifetime rates we documented in Section 3.3 .

[25] We estimate r_A as follows: (1) the relative effect from the first stage estimate from Column 2 of Table 2 captures $\frac{M}{A} = 0.68/1.92 = 0.36$ (how much adopting a new bar exam waiver increases the number of lawyers in percent terms). Rearranging: $M = 0.36A$. Plugging M into the formula for p: $p = \frac{M}{M+A} = \frac{0.36A}{0.36A+A} = \frac{0.36A}{1.36A} = 0.265$. (2) The 2SLS estimate from Column 3 is the discipline rate of the marginal movers in the treated corridors with a bar exam waiver ($r_M = 0.036$). (3) The discipline rate of all lawyers in the treated corridor who move with a bar exam waiver is a descriptive statistic we observe ($r = 0.022$). (4) Plugging in p, r_M, r into the formula for r and solving for r_A: $0.022 = r_A(1-0.265) + 0.036 \times 0.265 \implies r_A = 0.017$.

using the same approach as above.[26] Comparing the 2SLS estimate of the marginal movers to this share of the always movers, we find that the marginal movers are 12 percent more likely to come from law schools outside of the top-100 than always movers (53.5 percent compared to 47.7 percent, $p < 0.05$) (Columns 4-5).

6 Conclusion

In this paper, we investigated the impact of occupational licensing requirements on the labor market mobility of lawyers and on the quality of lawyers offering legal services in a state. We specifically studied the impact of bar exam waivers for experienced lawyers on their likelihood of obtaining a license to practice law in another state and on whether the lawyers induced to move by a bar exam waiver differ in quality. To do so, we assembled novel datasets on bar exam waiver policies, license acquisitions of a sample of 1.7 million lawyers, and the professional disciplinary actions imposed on lawyers in 37 states. By exploiting more than one thousand changes in bar exam waivers between pairs of origin and destination states, we found that bar exam waivers increase labor market mobility by 38 percent. However, we also found that lawyers who are induced to obtain an additional license by a bar exam waiver are of lower quality than lawyers who would have obtained an additional license without the waiver. Taken together, these results imply that, in the legal context, occupational licensing requirements create a trade-off between the supply of labor and the quality of professionals.

Given this trade-off, future research is needed to further understand the welfare implications of occupational licensing in the legal profession. Our research specifically points

[26] Of all lawyers who move with a bar exam waiver, the share who graduated from a law school outside the top 100 is a weighted average of the always and marginal movers: $l = l_A(1-q)+l_M q$, where $l_A = \frac{\sum_{i=0}^{A} l_i}{A}$ is the share of the A lawyers who are always movers who graduated from a law school outside the top 100, $l_M = \frac{\sum_{i=0}^{M} l_i}{M}$ is the share of the A lawyers who are marginal movers who graduated from a law school outside the top 100, and $q = \frac{M}{M+A}$ is the fraction of all lawyers who move with a bar exam waiver who are marginal movers. We back out l_A from this equation as follows: (1) the relative effect from the first stage estimates from Column 2 captures $\frac{M}{A} = 0.39$, which using the above approach implies $q = 0.280$. (2) The 2SLS estimate from Column 3 is the share of the marginal movers who graduated from a law school outside the top 100 ($l_M = 0.535$). (3) The share of all lawyers who move with a bar exam waiver and who graduated from a law school outside the top 100 is a descriptive statistic we observe ($l = 0.493$). (4) Plugging in q, l_M, l into the formula for l and solving for l_A: $0.493 = l_A(1 - 0.280) + 0.535 \times 0.280 \implies l_A = 0.477$.

toward two related topics that would benefit from additional investigation. First, because occupational licensing rules appear to impact both the quantity and quality of lawyers, future research should directly investigate the welfare implications that come from expanded access to legal services relative to potential costs associated with having a higher share of lower-quality lawyers. The welfare benefits of increasing the supply of lawyers may far outweigh the costs of additional lower-quality lawyers, but more research is needed to directly explore this possibility. Second, future research should explore whether bar exam waivers create lower-quality lawyers or simply redistribute them. For instance, exam waivers may produce lower-quality lawyers if they lead to experienced lawyers not learning information that could directly improve the quality of legal services they provide; alternatively, exam waivers may simply allow existing lower-quality lawyers to expand their practices to new markets without producing any new lower quality lawyers. These two possibilities have different welfare implications and suggest different strategies for trying to protect the public.

References

Amoir, M. (2023). Education and Geographical Mobility: The Role of the Job Surplus. *American Economic Journal: Economic Policy*.

Anderson, D. M., R. Brown, K. K. Charles, and D. I. Rees (2020). Occupational Licensing and Maternal Health: Evidence from Early Midwifery Laws. *Journal of Political Economy 128*(11), 4337–4383.

Anderson, R. I. and D. T. Muller (2019). The High Cost of Lowering the Bar. *Georgetown Journal of Legal Ethics 32*(3), 307–325.

Angrist, J. D. and J. Guryan (2008). Does Teacher Testing Raise Teacher Quality? Evidence from State Certification Requirements. *Economics of Education Review 27*(5), 483–503.

Arkhangelsky, D., S. Athey, D. A. Hirshberg, G. W. Imbens, and S. Wager (2021). Synthetic Difference-in-Differences. *American Economic Review 111*(12), 4088–4118.

Borusyak, K., X. Jaravel, and J. Spiess (2024). Revisiting Event Study Designs: Robust and Efficient Estimation. *Review of Economic Studies*.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics 225*(2), 200–230.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019). The Effect of Minimum Wages on Low-Wage Jobs. *Quarterly Journal of Economics 134*(3), 1405–1454.

Chaudhry, H. J., L. A. Robin, E. M. Fish, D. H. Polk, and J. D. Gifford (2015). Improving Access and Mobility – The Interstate Medical Licensure Compact. *New England Journal of Medicine 372*(17), 1581–1583.

Connecticut Bar Examining Committee (2023). Admission Without Examination.

Criscuolo, C. and L. Garicano (2010). Offshoring and Wage Inequality: Using Occupational Licensing as a Shifter of Offshoring Costs. *American Economic Review 100*(2), 439–43.

Currie, J. and V. J. Hotz (2004). Accidents Will Happen?: Unintentional Childhood Injuries and the Effects of Child Care Regulations. *Journal of Health Economics 23*(1), 25–59.

de Chaisemartin, C. and X. D'Haultfoeuille (2024). Difference-in-Differences Estimators of Intertemporal Treatment Effects. *Review of Economics and Statistics*.

DePasquale, C. and K. Stange (2016). Labor Supply Effects of Occupational Regulation: Evidence from the Nurse Licensure Compact. *NBER Working Paper # 33244*.

Deshpande, M. and Y. Li (2019). Who is Screened Out? Application Costs and the Targeting of Disability Programs. *American Economic Journal: Economic Policy 11*(4), 213–248.

Dharmapala, D., R. H. McAdams, and J. Rappaport (2020). Collective Bargaining Rights and Police Misconduct: Evidence from Florida. *Journal of Law, Economics, and Organization 38*(1), 1–41.

Dube, A., D. Girardi, O. Jordà, and A. M. Taylor (2023). A Local Projections Approach to Difference-in-Differences Event Studies. *NBER Working Paper # 31184*.

Egan, M., G. Matvos, and A. Seru (2019). The Market for Financial Adviser Misconduct. *Journal of Political Economy 127*(1), 233–295.

Farronato, C., A. Fradkin, B. Larsen, and E. Brynjolfsson (2020). Consumer Protection in an Online World: An Analysis of Occupational Licensing. *NBER Working Paper # 26601*.

Frakes, M. and M. Wasserman (2023). Investing in Ex Ante Regulation: Evidence from Pharmaceutical Patent Examination. *American Economic Journal: Economic Policy 15*(3), 151–183.

Freyaldenhoven, S., C. Hansen, and J. M. Shapiro (2019). Pre-Event Trends in the Panel Event-Study Design. *American Economic Review 109*(9), 3307–3338.

Friedman, M. (1962). *Capitalism and Freedom*. University of Chicago Press.

Gay, R., K. Greene, and M. Kleiner (1982). Barriers to Labor Migration: The Case of Occupational Licensing. *Industrial Relations 21*(3), 383–391.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics 225*(2), 254–277.

Holen, A. (1965). Effects of Professional Licensing Arrangements on Interstate Labor Mobility and Resource Allocation. *Journal of Political Economy 73*(5), 492–498.

Johnson, J. E. and M. M. Kleiner (2020). Is Occupational Licensing a Barrier to Interstate Migration? *American Economic Journal: Economic Policy 12*(3), 347–373.

Kane, T. J., J. E. Rockoff, and D. O. Staiger (2008). What Does Certification Tell Us About Teacher Effectiveness? Evidence from New York City. *Economics of Education Review 27*(6), 615–631.

Kleiner, M. M. and A. B. Krueger (2010). The Prevalence and Effects of Occupational Licensing. *British Journal of Industrial Relations 48*(4), 676–687.

Kleiner, M. M. and A. B. Krueger (2013). Analyzing the Extent and Influence of Occupational Licensing on the Labor Market. *Journal of Labor Economics 31*(2), S173–S202.

Kleiner, M. M. and R. T. Kudrle (2000). Does Regulation Affect Economic Outcomes? The Case of Dentistry. *Journal of Law and Economics 43*(2), 547–582.

Kleiner, M. M. and D. L. Petree (1988). Unionism and Licensing of Public School Teachers: Impact on Wages and Educational Output. In *When Public Sector Workers Unionize*, pp. 305–322. University of Chicago Press.

Kleiner, M. M. and E. J. Soltas (2023). A Welfare Analysis of Occupational Licensing in U.S. States. *Review of Economic Studies 90*(5), 2481–2516.

Kugler, A. D. and R. M. Sauer (2005). Doctors Without Borders? Relicensing Requirements and Negative Selection in the Market for Physicians. *Journal of Labor Economics 23*(3), 437–465.

Larsen, B., Z. Ju, A. Kapor, and C. Yu (2020). The Effect of Occupational Licensing Stringency on the Teacher Quality Distribution. *NBER Working Paper # 28158*.

Liu, J. and D. A. Hyman (2021). Physician Licensing and Discipline: Lessons From Indiana. *Journal of Empirical Legal Studies 18*(3), 629–659.

Malamud, O. and A. Wozniak (2012). The Impact of College on Migration: Evidence from the Vietnam Generation. *Journal of Human Resources 47*(4), 913–950.

Mulholland, S. E. and A. T. Young (2016). Occupational Licensing and Interstate Migration. *Cato Journal 36*(1), 17–31.

Notowidigdo, M. J. (2020). The Incidence of Local Labor Demand Shocks. *Journal of Labor Economics 38*(3), 687–725.

Pashigan, B. P. (1979). Occupational Licensing and the Interstate Mobility of Professionals. *Journal of Law and Economics 22*(1), 1–25.

Peterson, B., S. Pandya, and D. Leblang (2014). Doctors with Borders: Occupational Licensing as an Implicit Barrier to High Skill Migration. *Public Choice 160*, 45–63.

Ramseyer, J. M. and E. B. Rasmusen (2015). Lowering the Bar to Raise the Bar: Licensing Difficulty and Attorney Quality in Japan. *Journal of Japanese Studies 41*, 113–142.

Roback, H. (1943). Legal Barriers to Interstate Migration. *Cornell Law Review 28*(8), 286–312.

Roth, J. and P. H. C. Sant'Anna (2023). Efficient Estimation for Staggered Rollout Designs. *Journal of Political Economy Microeconomics 1*(4), 669–709.

Rozema, K. (2021). Does the Bar Exam Protect the Public? Evidence from the Legal Profession. *Journal of Empirical Legal Studies 18*(4), 801–848.

Rozema, K. (2023). How Much Does the Bar Exam Decrease the Size of the American Legal Profession? *Working Paper*.

Rozema, K. (2024a). Expungement of Lawyer Disciplinary Records: Evidence from Florida. *Working Paper*.

Rozema, K. (2024b). Professional Discipline and the Labor Market: Evidence from Lawyers. *Journal of Law and Economics 67*(2), 371–414.

Rozema, K. (2024c). What Occupational Licensing Requirements Protect the Public? Evidence from the Legal Profession. *Working Paper*.

Shakya, S., S. Ghosh, and C. Norris (2022). Nurse Licensure Compact and Mobility. *Journal of Labor Research 43*(2), 260–274.

Sun, L. and S. Abraham (2021). Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *Journal of Econometrics 225*(2), 175–199.

Timmons, E. J. and A. Mills (2018). Bringing the Effects of Occupational Licensing Into Focus: Optician Licensing in the United States. *Eastern Economics Journal 44*(1), 69–83.

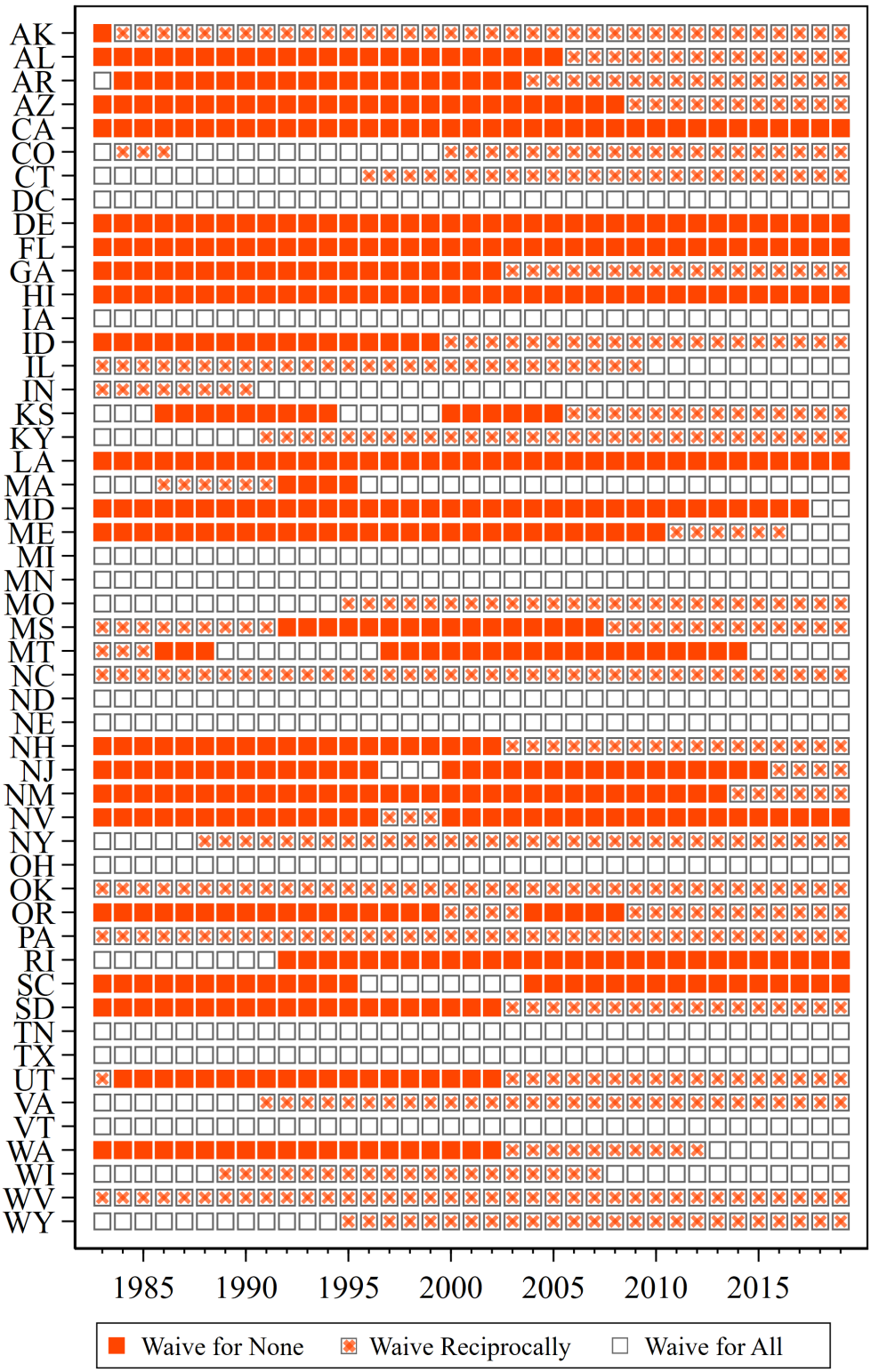
Whisner, M. (2014). The 411 on Lawyer Directories. *Law Library Journal 106*(2), 257–266.

Wing, C., S. M. Freedman, and A. Hollingsworth (2024). Stacked Difference-in-Differences. *NBER Working Paper # 32054*.

Wooldridge, J. (2023). Simple Approaches to Nonlinear Difference-in-Differences with Panel Data. *Econometrics Journal 26*(3), C31–C66.

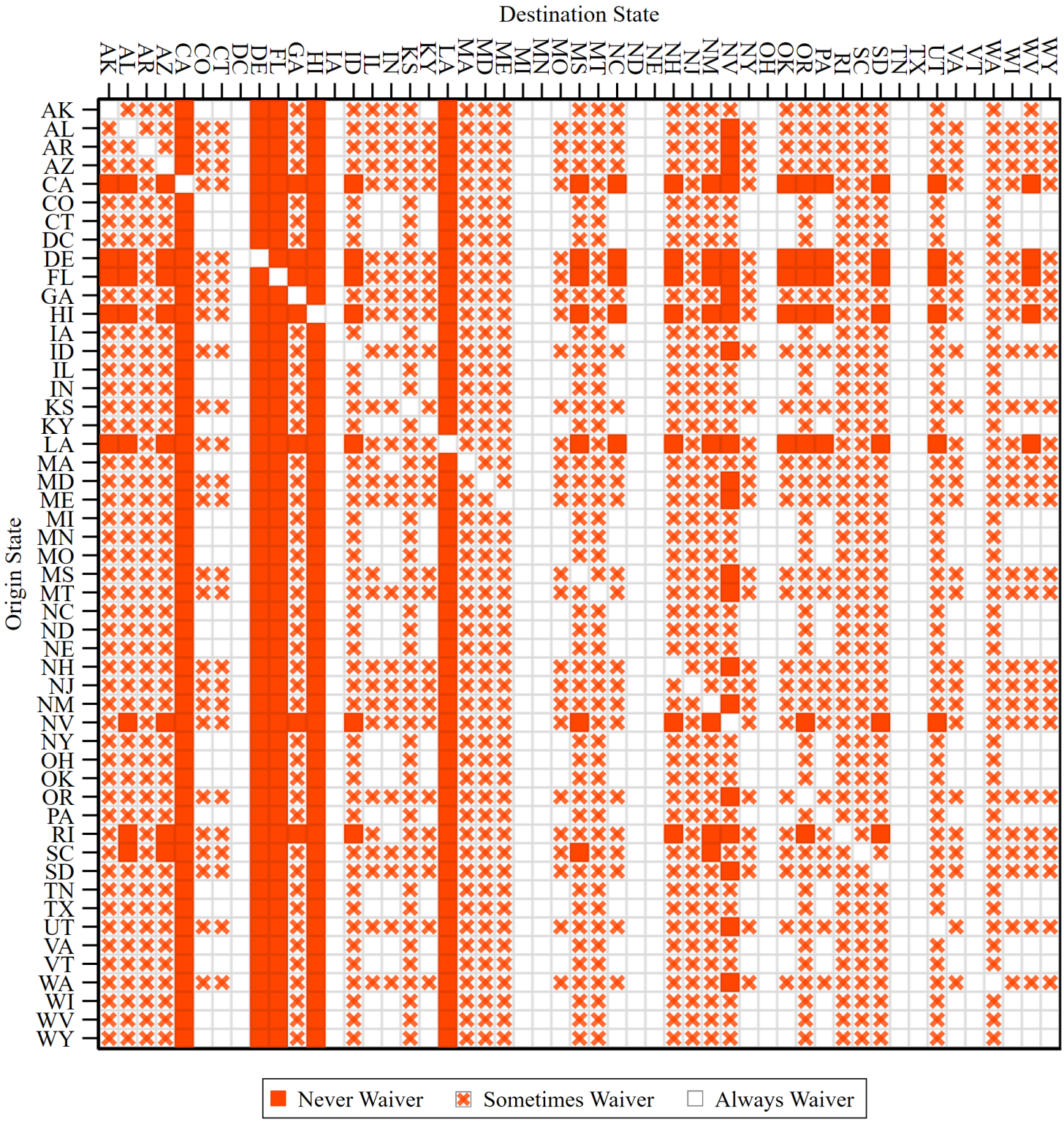
Figures and Tables

Figure 1: State Policies on Waiving the Bar Exam for Experienced Lawyers



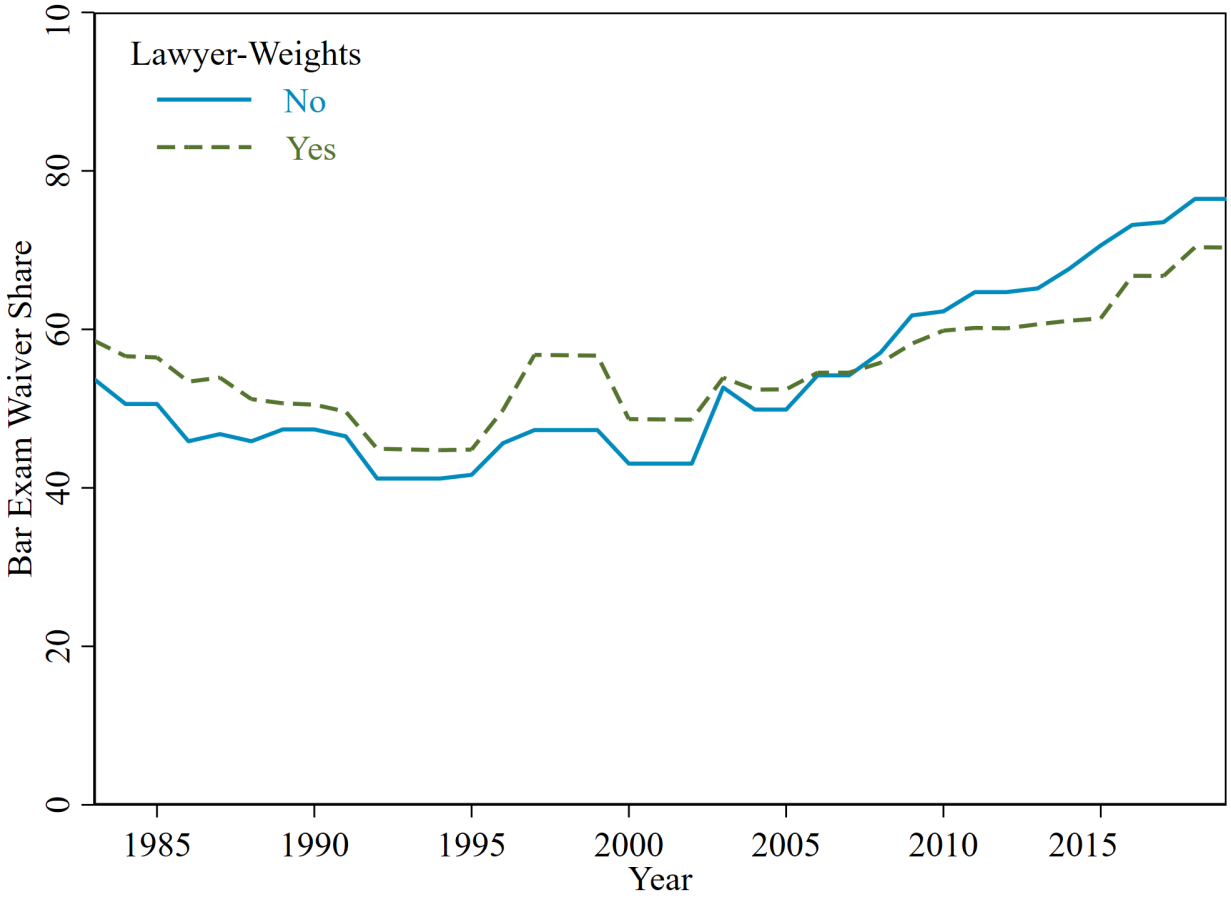
Note: This figure reports each state's policy on waiving the bar exam for out-of-state experienced lawyers. To obtain a law license, individuals must either pass the exam or qualify for an exam waiver. "Waive for None" (solid boxes) indicates that the state does not waive the exam for any out-of-state lawyers. "Waive Reciprocally" ("×" boxes) indicates that the state waives the exam only for experienced lawyers coming from states that also waive the exam. "Waive for All" (empty boxes) indicates that the state waives the bar exam for all experienced out-of-state lawyers.

Figure 2: Bar Exam Waivers for Each Pair of Origin and Destination States



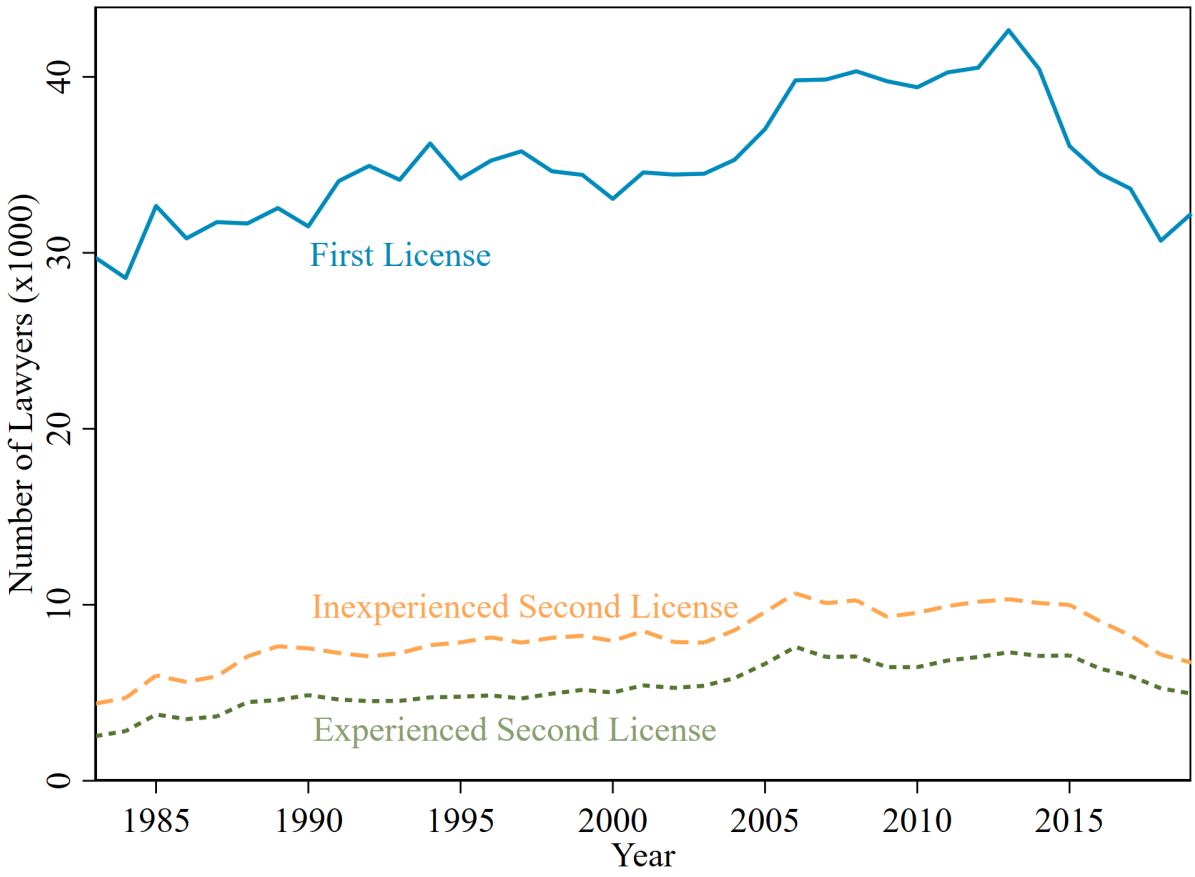
Note: This figure reports whether an experienced lawyer from one state must take the bar exam to obtain a law license in another state, for each pair of origin and destination states. With 50 states plus D.C., there are $51 \times 50 = 2550$ corridors. "Never Waiver" (solid boxes) indicates the corridor never had a bar exam waiver during the sample period (1983-2015). "Sometimes Waiver" ("×" boxed) indicates the corridor sometimes had a bar exam waiver and other times did not during the sample period. "Always Waiver" (empty boxes) indicates the corridor always had a bar exam waiver during the sample period.

Figure 3: Bar Exam Waivers Over Time



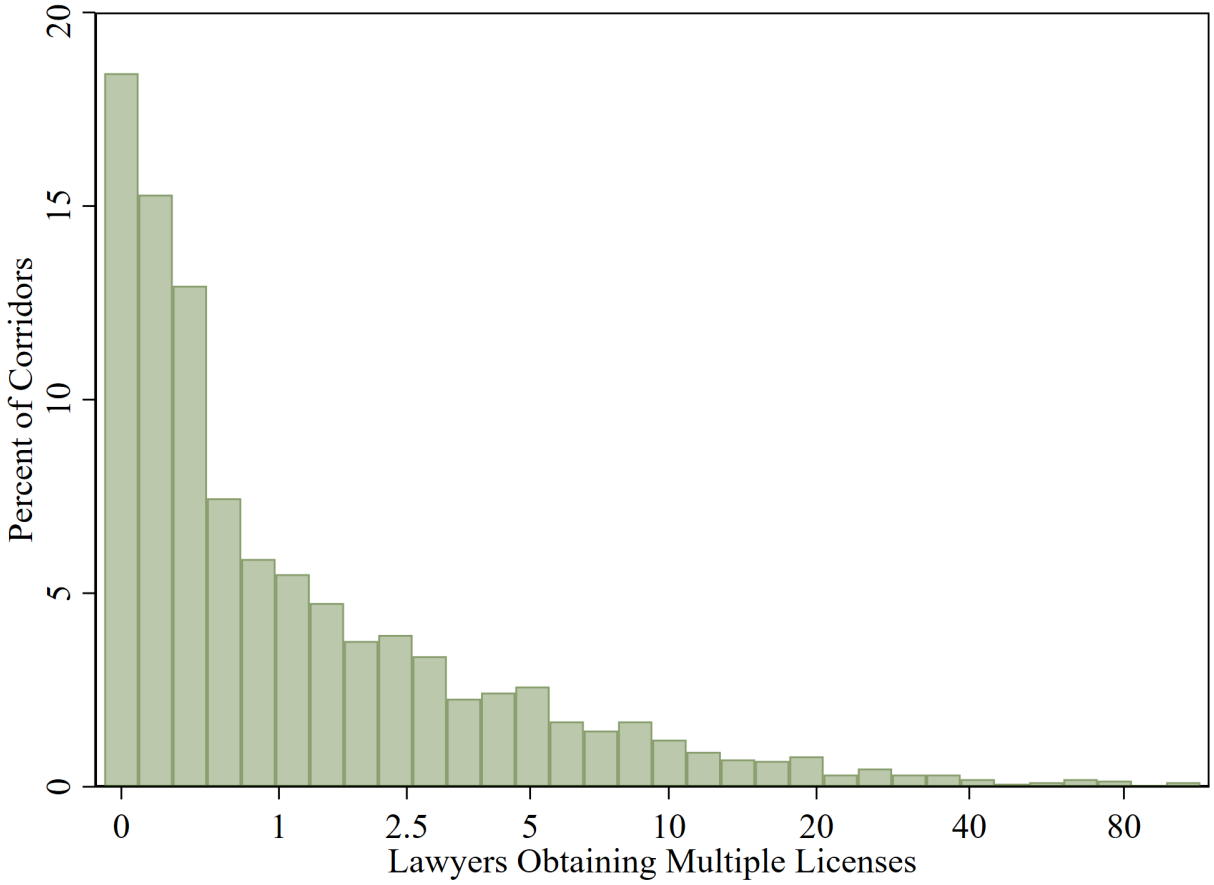
Note: This figure reports the extent to which an experienced lawyer from one state can obtain a license in another state without taking the bar exam over time. The solid line is the percent of corridors with bar exam waivers. The dashed line accounts for the number of lawyers in the origin and destination states by weighting corridors by the product of the number of active law licenses in the origin and destination states. Data on the number of active law licenses is from Rozema (2023).

Figure 4: Number of Lawyers Receiving Licenses Over Time



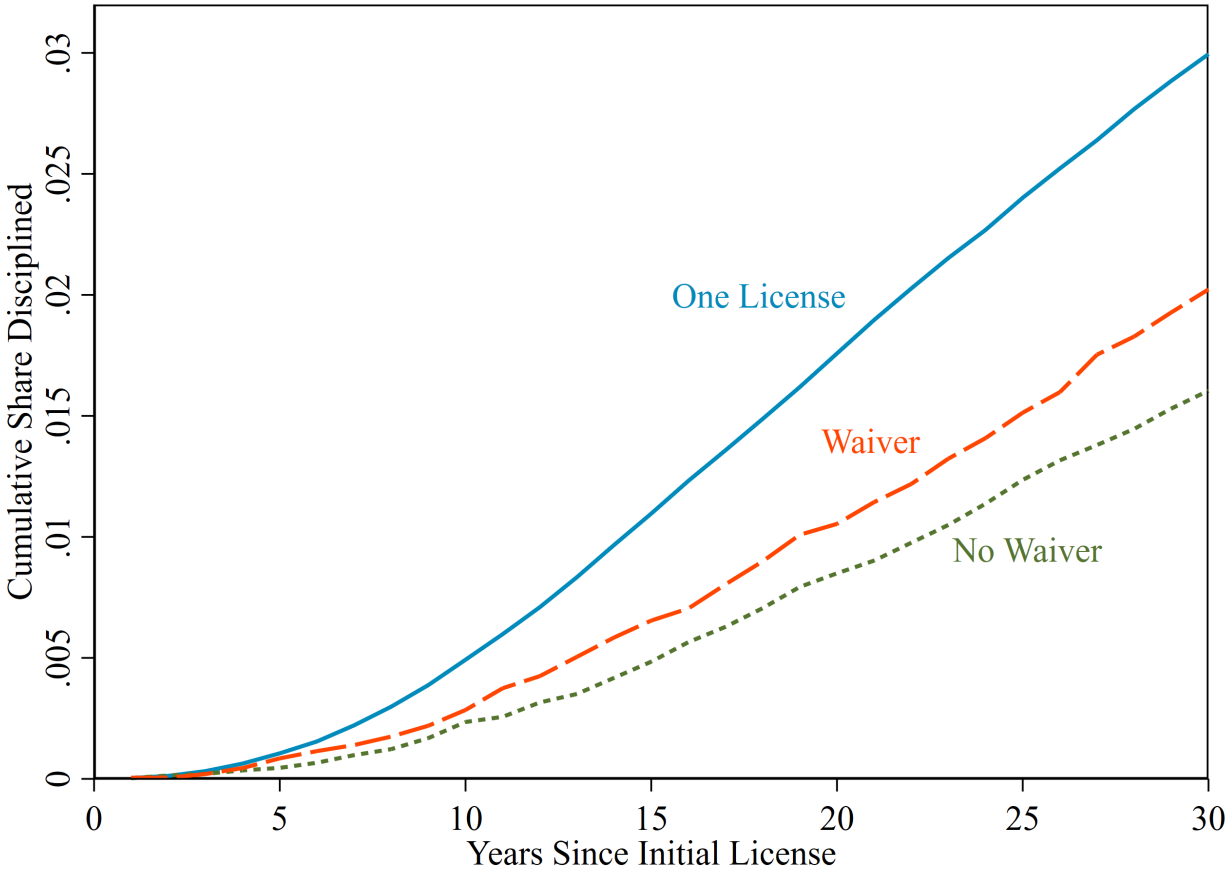
Note: This figure reports the number of lawyers obtaining their first license (solid line), inexperienced lawyers receiving their second or subsequent license (dashed line), and the number of experienced lawyers receiving a second or subsequent license (dotted line) over time.

Figure 5: Average Annual Labor Market Mobility at the Corridor-Level



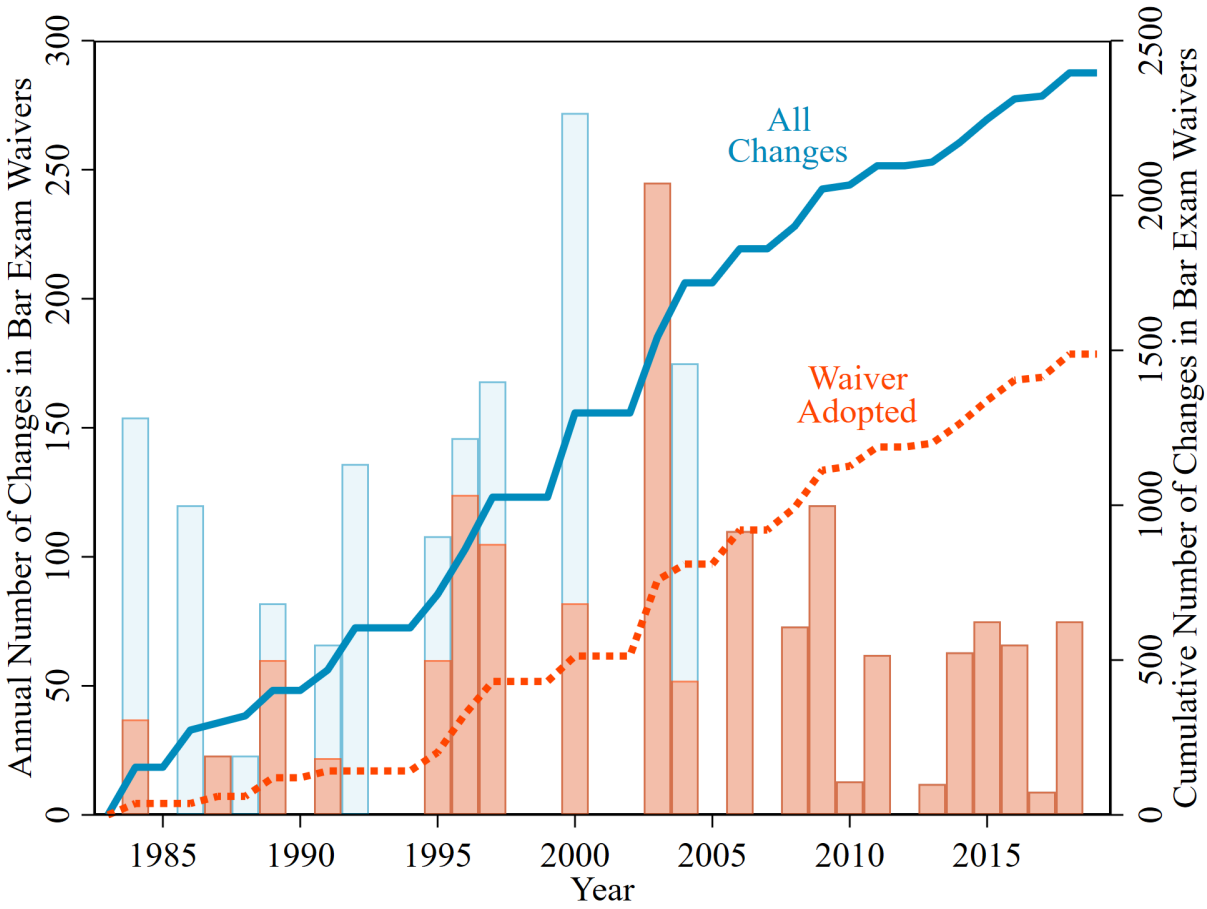
Note: This figure reports the distribution of the average annual number of experienced lawyers obtaining an additional license at the corridor-level between 1983 and 2019. With 50 states plus D.C., there are $51 \times 50 = 2550$ origin-destination corridors. The y-axis is the percent of corridors. The x-axis is the average annual number the experienced lawyers flowing through a corridor (log scale).

Figure 6: Discipline Rate Over Career by Labor Market Mobility Status



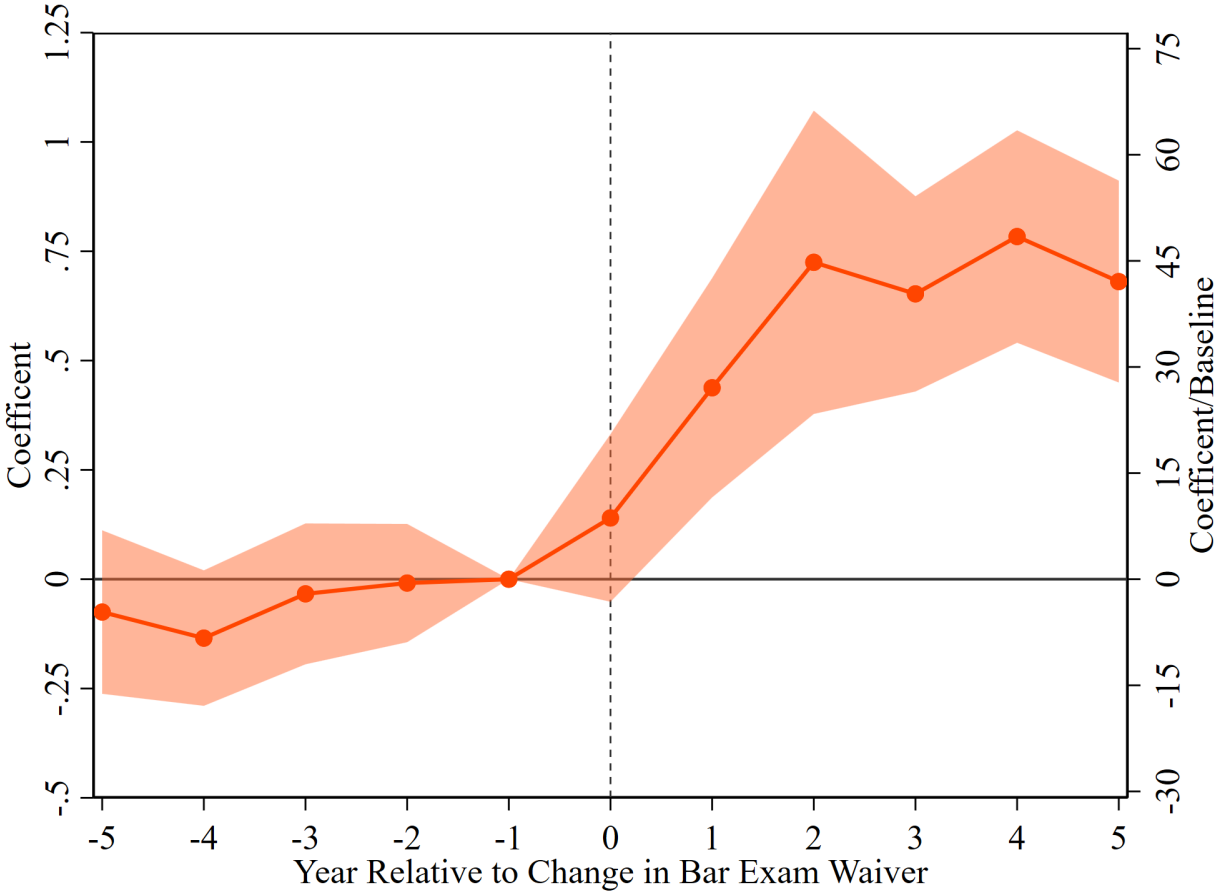
Note: This figure reports the share of lawyers who are disciplined over their career, separately by labor market mobility status. The sample is lawyers who were initially licensed between 1970 and 1990 and the 37 states where we observe disciplinary actions. For lawyers who obtain multiple licenses, the sample is restricted to lawyers who obtained a second license between 1983 and 2019 (a period when we observe whether they qualified for a bar exam waiver). The solid line labeled "One License" is for lawyers who only obtained one license as of 2019. The long dashed line labeled "Waiver" is for lawyers who obtained multiple licenses and qualified for a bar exam waiver at least once. The short dashed line labeled "No Waiver" is for lawyers who obtained multiple licenses but never qualified for a bar exam waiver.

Figure 7: Changes in Bar Exam Waivers



Note: This table reports changes in bar exam waivers at the corridor level over time. On the left y-axis, the light shaded bars are the annual number of changes in bar exam waivers, and the dark shaded bars are the annual number of changes in bar exam waivers from no bar exam waiver to a bar exam waiver. On the right y-axis, the solid line is the cumulative number of changes in bar exam waivers since 1983, and the dashed line is the cumulative number of changes in bar exam waivers from no bar exam waiver to a bar exam waiver since 1983.

Figure 8: The Impact of Bar Exam Waivers on Lawyer Labor Market Mobility



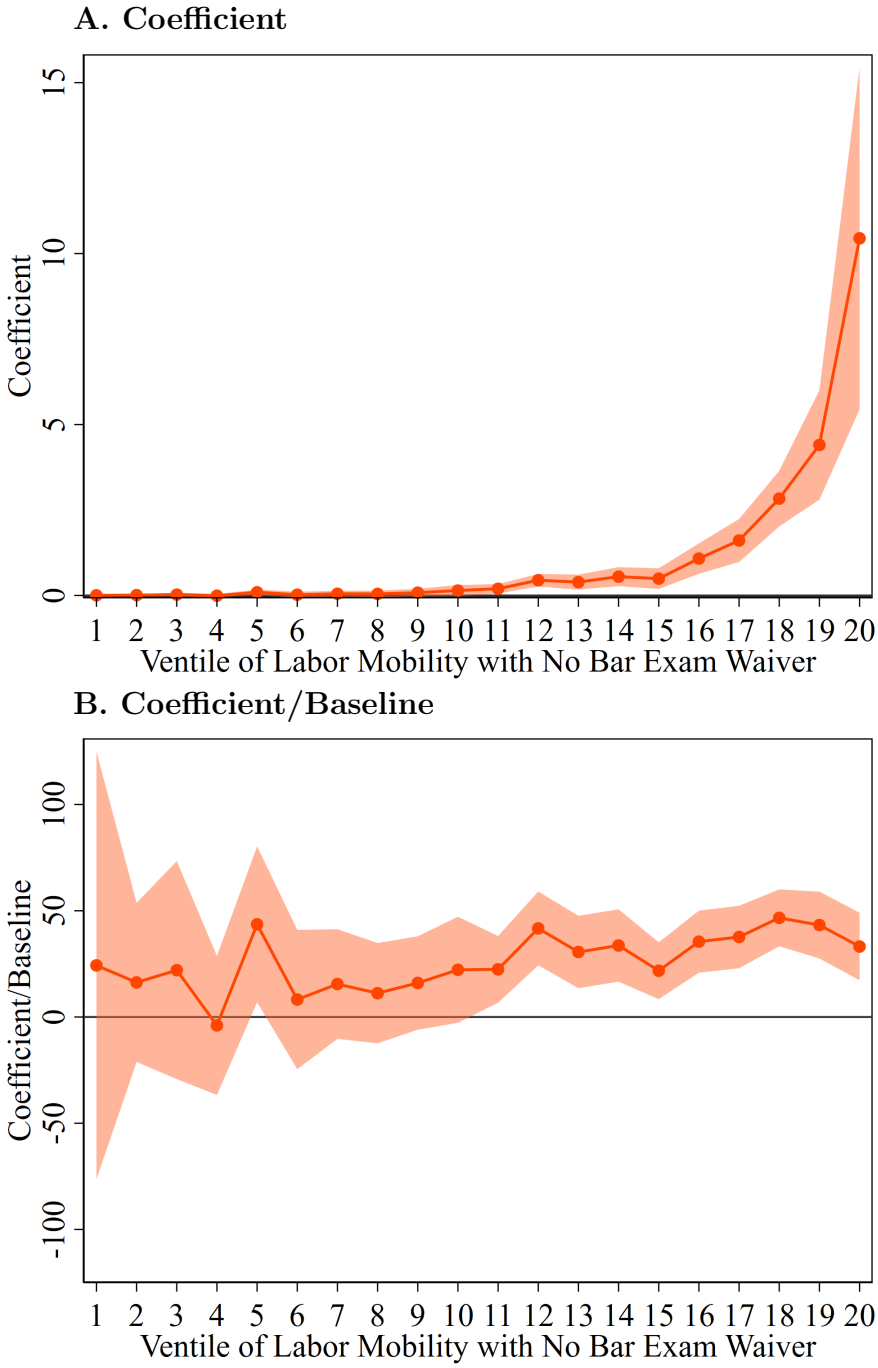
Note: This figure estimates the impact of a bar exam waiver on the number of experienced lawyers obtaining a license in the state in the 5 years before and after a change in a bar exam waiver. The results come estimating Equation 2 in a stacked event study framework that uses 1,212 events. The left-side y-axis reports the estimated coefficients, and the right-side y-axis reports the estimated coefficients divided by the mean of the treated group without a bar exam waiver ($\times 100$). The shaded region reports 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Table 1: The Impact of Bar Exam Waivers on Lawyer Labor Market Mobility

	Sample						
		Opening Event		Difficult Bar Exam		Adjacent State	
	All	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bar Exam Waiver	0.62***	0.70***	0.44***	1.50***	0.26***	2.97***	0.45***
	(0.10)	(0.13)	(0.11)	(0.28)	(0.07)	(0.63)	(0.09)
Mean # Lawyers	3.55	3.85	2.90	3.85	3.43	3.67	3.54
Treated Corridors	1.62	1.46	1.91	2.25	1.37	6.57	1.26
Control Corridors	3.45	3.63	3.11	3.68	3.36	3.54	3.44
Covariates							
Event-Corridor FE	✓	✓	✓	✓	✓	✓	✓
Event-Year FE	✓	✓	✓	✓	✓	✓	✓
Events	1,212	832	380	353	859	82	1,130
Observations	25,699,960	17,611,341	8,088,619	7,409,116	18,290,844	1,727,605	23,972,355

Note: This table estimates the impact of a bar exam waiver on the number of experienced lawyers obtaining a license in the state. The results come estimating Equation 1 in a stacked event study framework that uses 1,212 events and an event window from 5 years before to 5 years after a change in the bar exam waiver. The row labeled "Mean # Lawyers" is the overall mean for both treated and control corridors in all years in the event window. The rows labeled "Treated Corridor" and "Control Corridor" are the means when the treated corridor does not have a bar exam waiver. Column 1 reports the results for all events. Columns 2-7 separately subset the events by characteristics about the treated corridor in the event: whether the treated corridor was opened (Columns 2-3), had a difficult bar exam (Columns 4-5), and involved two adjacent states (Columns 6-7). Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Figure 9: The Impact of Bar Exam Waivers on Lawyer Labor Market Mobility By Amount of Mobility



Note: This figure estimates the impact of a bar exam waiver on the number of experienced lawyers obtaining a license in the state from estimating Equation 1, separately by the ventile of labor market mobility in the treated corridor with no bar exam waiver. Panel A reports the estimated treatment effect in levels (the estimated coefficients). Panel B reports the estimated treatment effect in percentage terms (the estimated coefficients divided by the mean in the treated group without a bar exam waiver ($\times 100$)). The shaded regions report 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Table 2: The Impact of Bar Exam Waivers on Lawyer Quality: Professional Discipline

	Specification				
	Reduced Form (1)	First Stage (2)	IV (3)	Comparison Group (4)	P-Value (5)
Bar Exam Waiver	0.025***	0.683***			
	(0.005)	(0.160)			
Lawyers Obtaining Another License			0.036***		
			(0.009)		
Always Movers Discipline Share				0.017	p<0.046
Covariates					
Event-Corridor FE	✓	✓	✓		
Event-Year FE	✓	✓	✓		
Outcome Mean	0.063	4.725	0.063		
Treated Corridors	0.03	1.92	0.03		
Control Corridors	0.07	4.55	0.07		
Events	681	681	681		
Observations	13,295,997	13,295,997	13,295,997		

Note: This table investigates the impact of bar exam waivers on lawyer quality using professional discipline as a proxy for lawyer quality and an instrumental variables design. Column 1 estimates the reduced form regression of the number of experienced lawyers obtaining another license who are disciplined at some point during their career on the bar exam waiver (Equation 3). Column 2 estimates the first stage regression of the total number of experienced lawyers obtaining an additional license on a bar exam waiver (Equation 1). Column 3 estimates the IV regression, where the coefficient is interpreted as the discipline rate of the lawyers who were induced to obtain an additional license by the bar exam waiver. Column 4 reports the discipline rate of lawyers who obtain an additional license even without a bar exam waiver (always movers). Column 5 reports the p-value of whether the estimated discipline rate of the marginal movers from Column 3 is different than the discipline rate of the always movers from Column 4.

Table 3: The Impact of Bar Exam Waivers on Lawyer Quality: Law School Selectivity

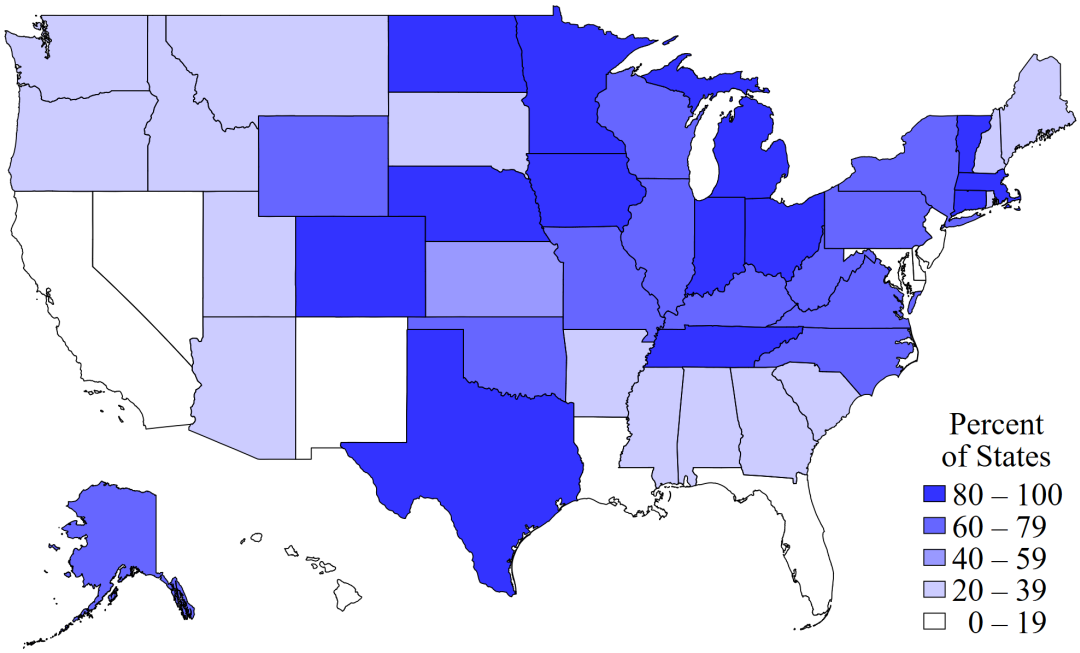
	Specification				
	Reduced Form (1)	First Stage (2)	IV (3)	Comparison Group (4)	P-Value (5)
Bar Exam Waiver	0.319***	0.596***			
	(0.046)	(0.085)			
Lawyers Obtaining Another License			0.535***		
			(0.028)		
Share of Always Movers from 101+ Rank				0.477	p<0.039
Covariates					
Event-Corridor FE	✓	✓	✓		
Event-Year FE	✓	✓	✓		
Outcome Mean	1.485	3.288	1.485		
Treated Corridors	0.74	1.52	0.74		
Control Corridors	1.46	3.23	1.46		
Events	1,212	1,212	1,212		
Observations	25,699,960	25,699,960	25,699,960		

Note: This table investigates the impact of bar exam waivers on lawyer quality using low-ranked law schools (consistently ranked 100+ in the U.S. News and World Report) as a proxy for lawyer quality and an instrumental variables design. Column 1 estimates the reduced form regression of the number of experienced lawyers from low-ranked law schools obtaining another license on the bar exam waiver (Equation 3). Column 2 estimates the first stage regression of the total number of experienced lawyers obtaining an additional license on a bar exam waiver (Equation 1). Column 3 estimates the IV regression, where the coefficient is interpreted as the share of the lawyers who were induced to obtain an additional license by the bar exam waiver from low-ranked law schools. Column 4 reports the share of lawyers from low-ranked law schools who obtain an additional license even without a bar exam waiver (always movers). Column 5 reports the p-value of whether the estimated low rank share of the marginal movers from Column 3 is different than the low rank share of always movers from Column 4.

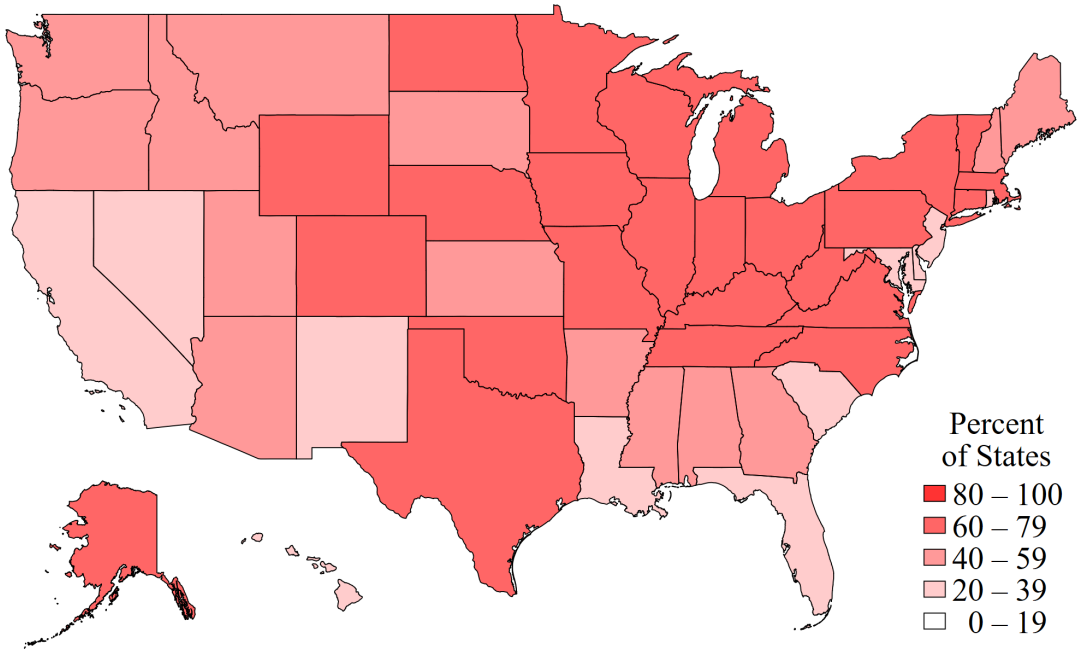
Supplemental Appendix A: Additional Results

Figure A1: Bar Exam Waivers of Inbound and Outbound Lawyers

A. Bar Exam Waivers for Inbound Lawyers

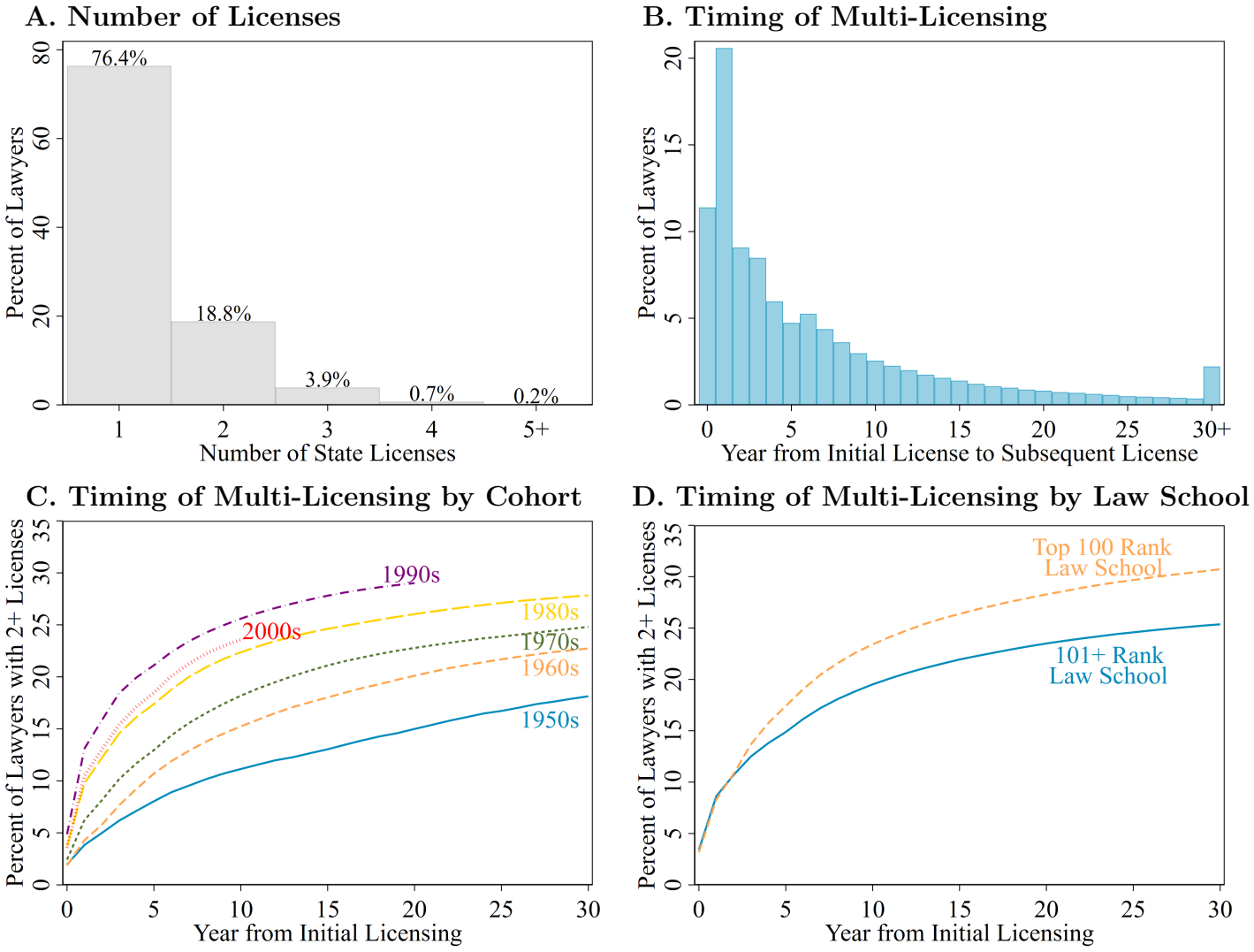


B. Bar Exam Waivers for Outbound Lawyers



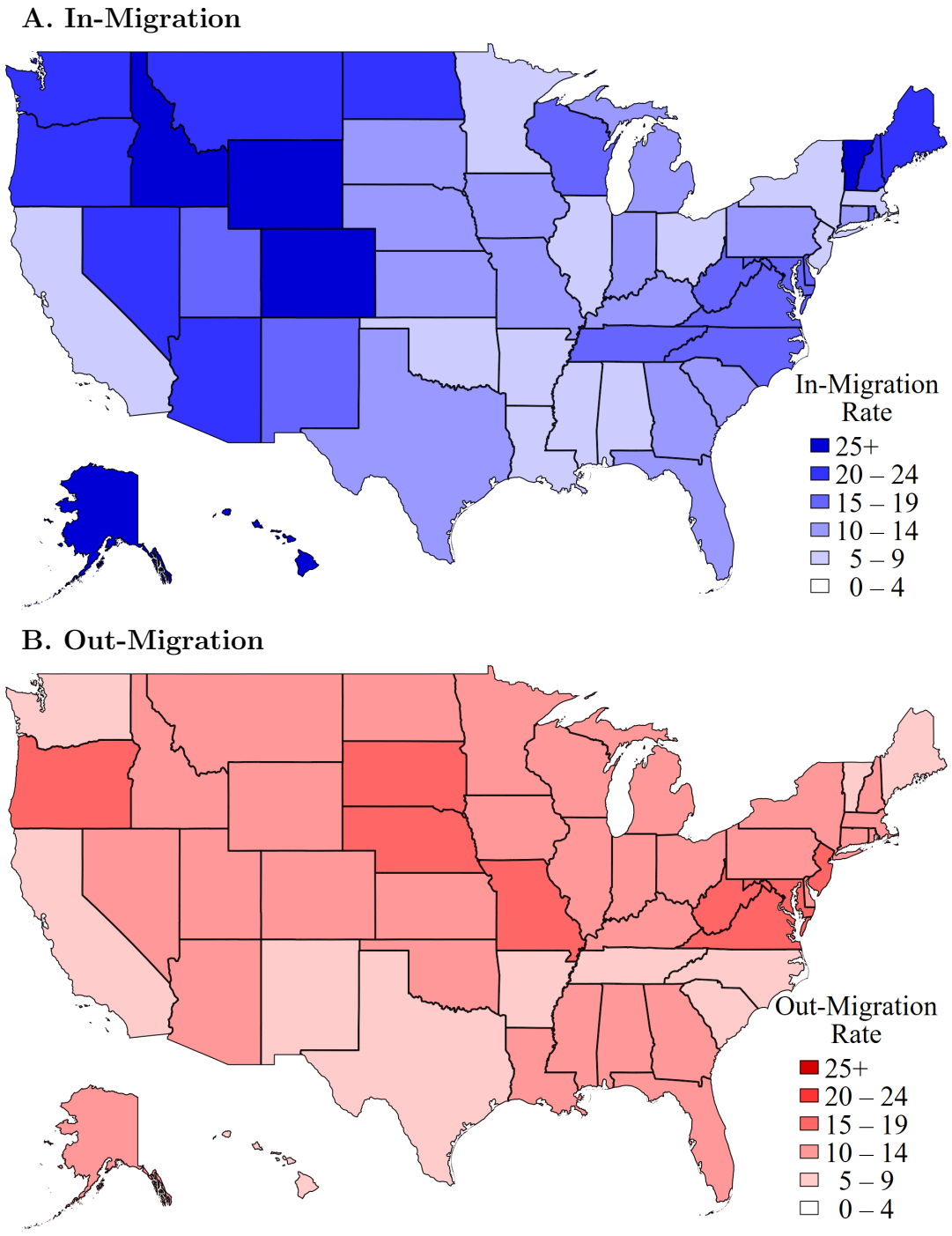
Note: This figure describes bar exam waivers for inbound versus outbound lawyers. Panel A reports waivers for inbound lawyers; for example, 40 percent means that experienced lawyers from 40 percent of other states do not have to take the bar exam to obtain a license in that state. Panel B reports waivers for outbound lawyers; for example, 70 percent means that experienced lawyers in the state do not have to take the bar exam to obtain a license in 70 percent of other states. Both panels report averages over annual percentages from 1983 to 2019.

Figure A2: Descriptive Statistics of Labor Market Mobility



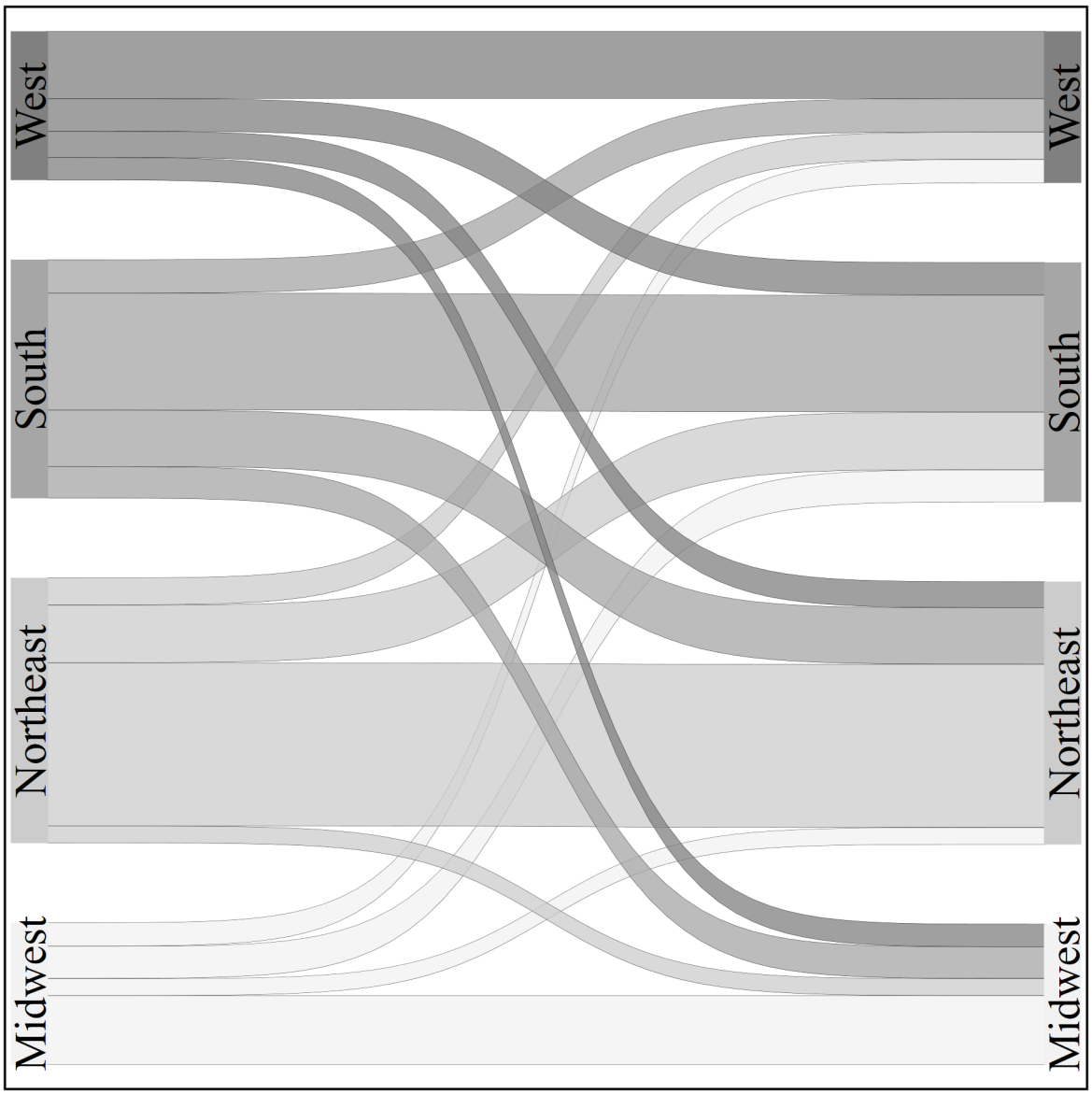
Note: Panel A reports the distribution of the number of licenses that lawyers obtain throughout their career. Panel B reports the distribution of the years from a lawyer's first license to a subsequent license. Panel C reports the cumulative share of lawyers with multiple licenses over their career, separately by the decade they obtained their first license. Panel D reports the cumulative share of lawyers with multiple licenses over their career, separately by whether they graduated from a law school consistently ranked in the top 100 by the U.S. News and World Report.

Figure A3: In-Migration and Out-Migration of Experienced Lawyers



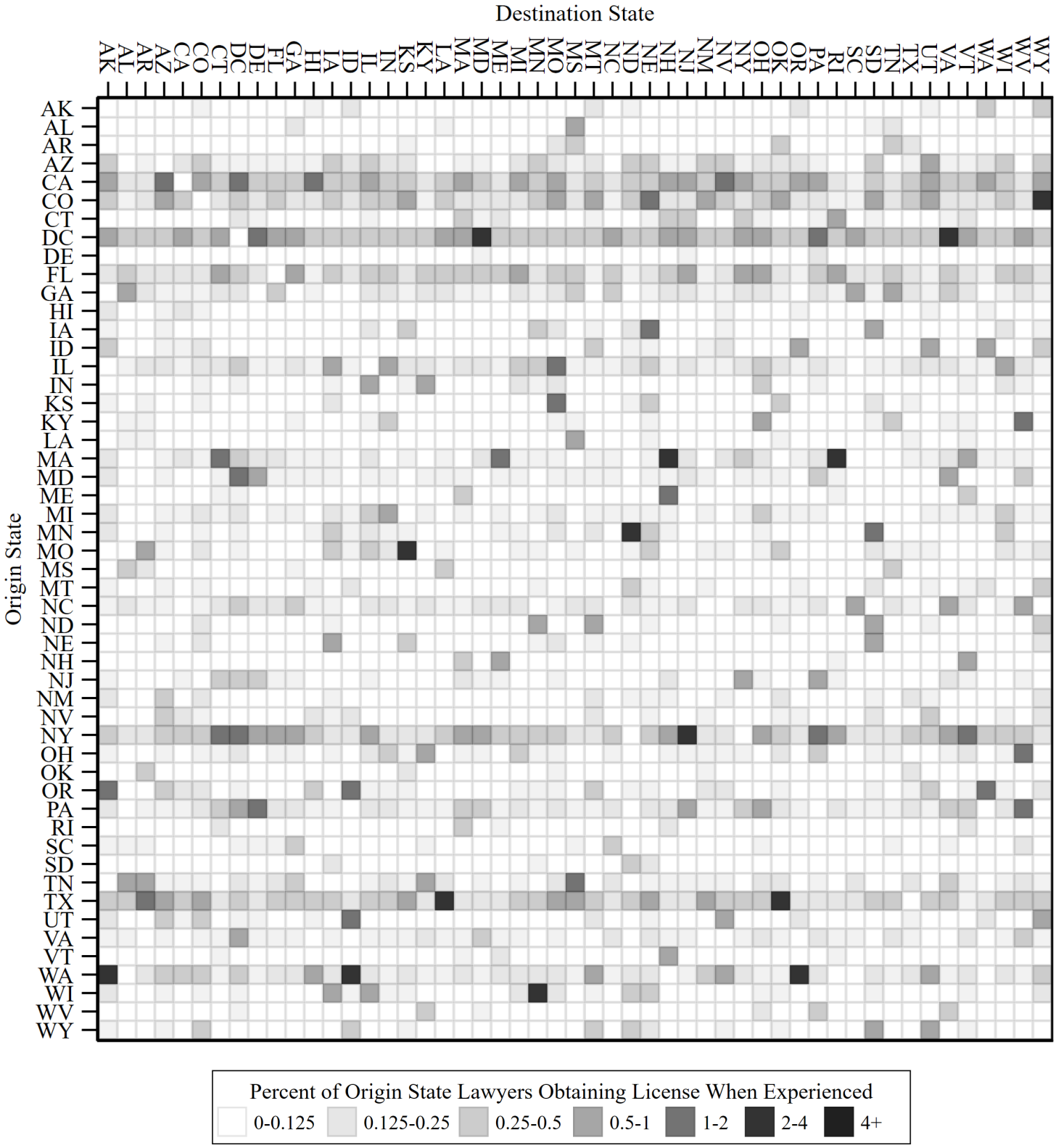
Note: Panel A reports the percent of a state's experienced lawyers who in-migrated, defined as the percent of lawyers who obtained their first license from another state (the number of experienced lawyers moving in divided by the number of experienced lawyers). Panel B reports the percent of a state's experienced lawyers who out-migrated, defined as the percent of lawyers who subsequently obtained a license in another state (the number of experienced lawyers moving out divided by the number of experienced lawyers).

Figure A4: Multi-Licensing for Pairs of Regions



Note: This figure reports a Sankey diagram of the number of experienced lawyers obtaining multiple licenses in different regions, where the width of the line corresponds to the number of experienced lawyers obtaining at least one license in each region.

Figure A5: Multi-Licensing for Pairs of States



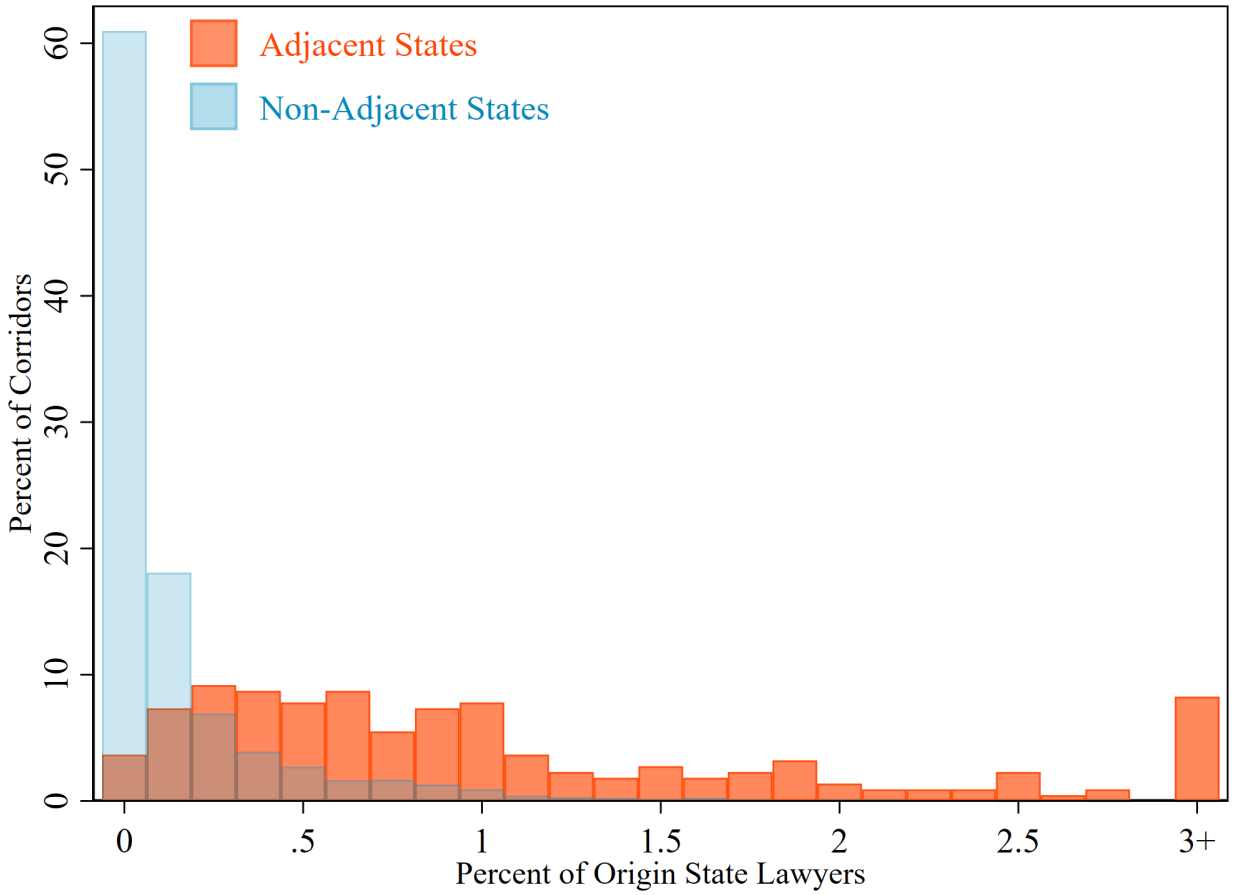
Note: This figure reports the percent of experienced lawyers in a state who obtain a license in another state. The left hand side of the graph indicates the state where the share of lawyers is being calculated. For example, at the top left for Alaska on the y-axis and Alabama on the x-axis, the cell divides the number of experienced Alaska lawyers who obtained a license in Alabama by the total number of lawyers who first obtained a license in Alaska.

Table A1: Most Common Destination State by Origin State

Origin	Rank of Destination State			Origin	Rank of Destination State		
State	1	2	3	State	1	2	3
AK	WA (19%)	CA (12%)	OR (8%)	MT	WA (8%)	ND (6%)	CO (6%)
AL	GA (8%)	FL (6%)	MS (5%)	NC	SC (6%)	GA (4%)	DC (4%)
AR	TX (9%)	TN (5%)	MO (4%)	ND	MN (38%)	MT (6%)	TX (6%)
AZ	CA (12%)	NY (4%)	IL (4%)	NE	IA (16%)	CO (7%)	KS (4%)
CA	NY (6%)	DC (3%)	IL (2%)	NH	MA (46%)	ME (13%)	NY (7%)
CO	CA (8%)	TX (6%)	NY (5%)	NJ	NY (50%)	PA (29%)	DC (6%)
CT	NY (38%)	MA (11%)	DC (5%)	NM	TX (13%)	CO (6%)	CA (5%)
DC	NY (12%)	MD (8%)	CA (7%)	NV	CA (31%)	UT (6%)	AZ (6%)
DE	PA (31%)	NJ (13%)	NY (8%)	NY	NJ (14%)	CA (5%)	CT (5%)
FL	NY (9%)	DC (4%)	GA (4%)	OH	KY (4%)	IL (3%)	MI (3%)
GA	FL (8%)	NY (4%)	TN (4%)	OK	TX (12%)	AR (3%)	MO (3%)
HI	CA (23%)	WA (5%)	NY (5%)	OR	WA (25%)	CA (11%)	ID (3%)
IA	NE (11%)	IL (10%)	MN (6%)	PA	NJ (25%)	NY (10%)	DC (5%)
ID	WA (21%)	UT (10%)	OR (9%)	RI	MA (52%)	CT (8%)	NY (7%)
IL	MO (7%)	WI (4%)	NY (4%)	SC	NC (11%)	GA (7%)	DC (3%)
IN	IL (12%)	KY (5%)	OH (5%)	SD	NE (12%)	MN (12%)	IA (11%)
KS	MO (42%)	CO (4%)	TX (4%)	TN	GA (6%)	MS (5%)	FL (3%)
KY	OH (13%)	IN (7%)	TN (4%)	TX	CA (4%)	NY (3%)	CO (2%)
LA	TX (10%)	MS (3%)	FL (2%)	UT	CA (10%)	ID (5%)	NV (5%)
MA	NY (14%)	RI (5%)	CT (5%)	VA	DC (19%)	MD (6%)	NY (4%)
MD	DC (24%)	VA (5%)	NY (5%)	VT	NY (18%)	MA (16%)	NH (9%)
ME	MA (27%)	NH (8%)	NY (7%)	WA	OR (12%)	CA (10%)	ID (4%)
MI	IL (6%)	OH (4%)	CA (3%)	WI	IL (13%)	MN (10%)	CA (4%)
MN	WI (9%)	ND (5%)	CA (4%)	WV	PA (18%)	OH (10%)	KY (5%)
MO	IL (23%)	KS (20%)	TX (4%)	WY	CO (29%)	UT (6%)	MT (5%)
MS	TN (11%)	AL (8%)	LA (7%)				

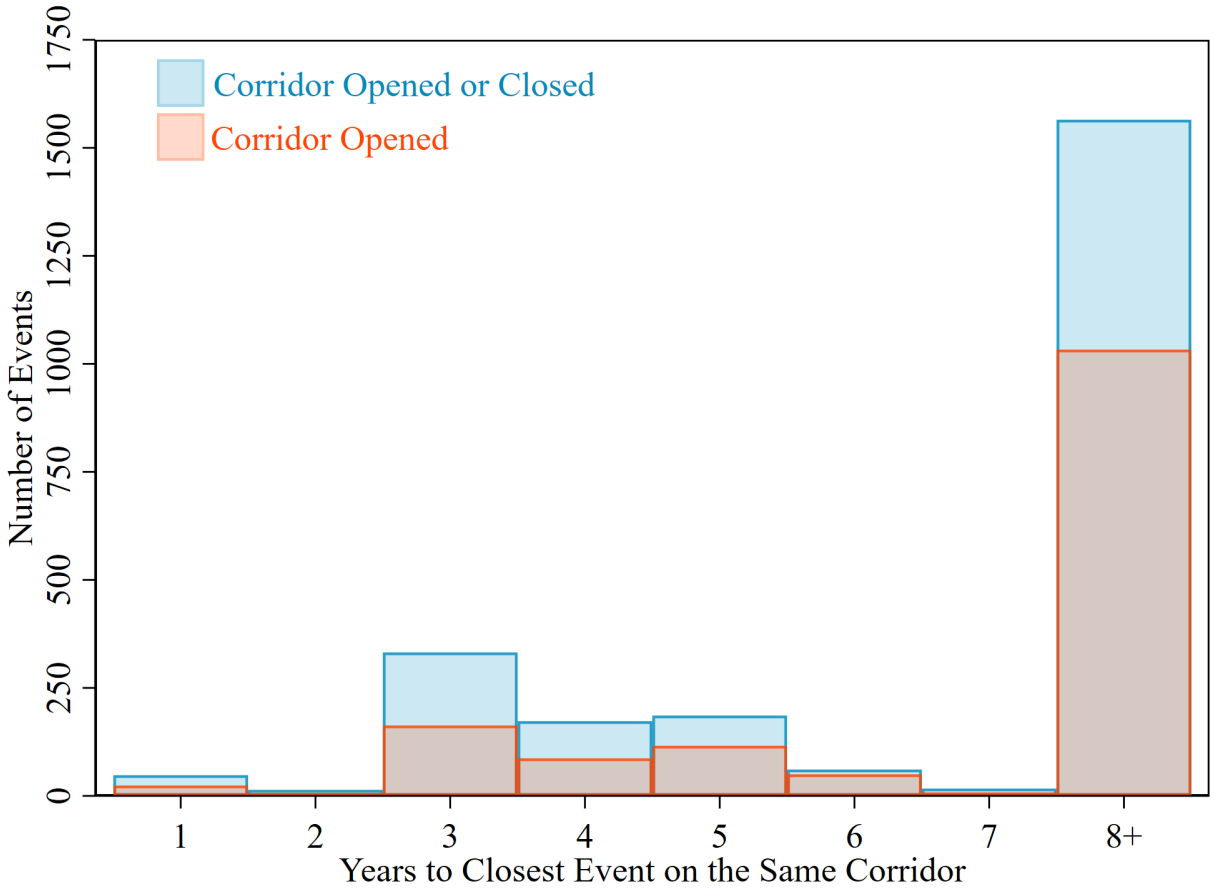
Note: This table reports the top three most common destination states for lawyers in each origin state. The origin state is indicated in the column on the left. Columns 1, 2, and 3 indicate the 1st, 2nd, and 3rd most common destination states for experienced lawyers in the origin state. The percentages listed in parentheses next to the state abbreviations are the listed destination state's share of total mobility from the origin state. For example, the first row reports that out of all multi-licensed experienced lawyers in Alaska, 19 percent obtain a license in Washington State.

Figure A6: Labor Market Mobility by if the Corridor is Adjacent States



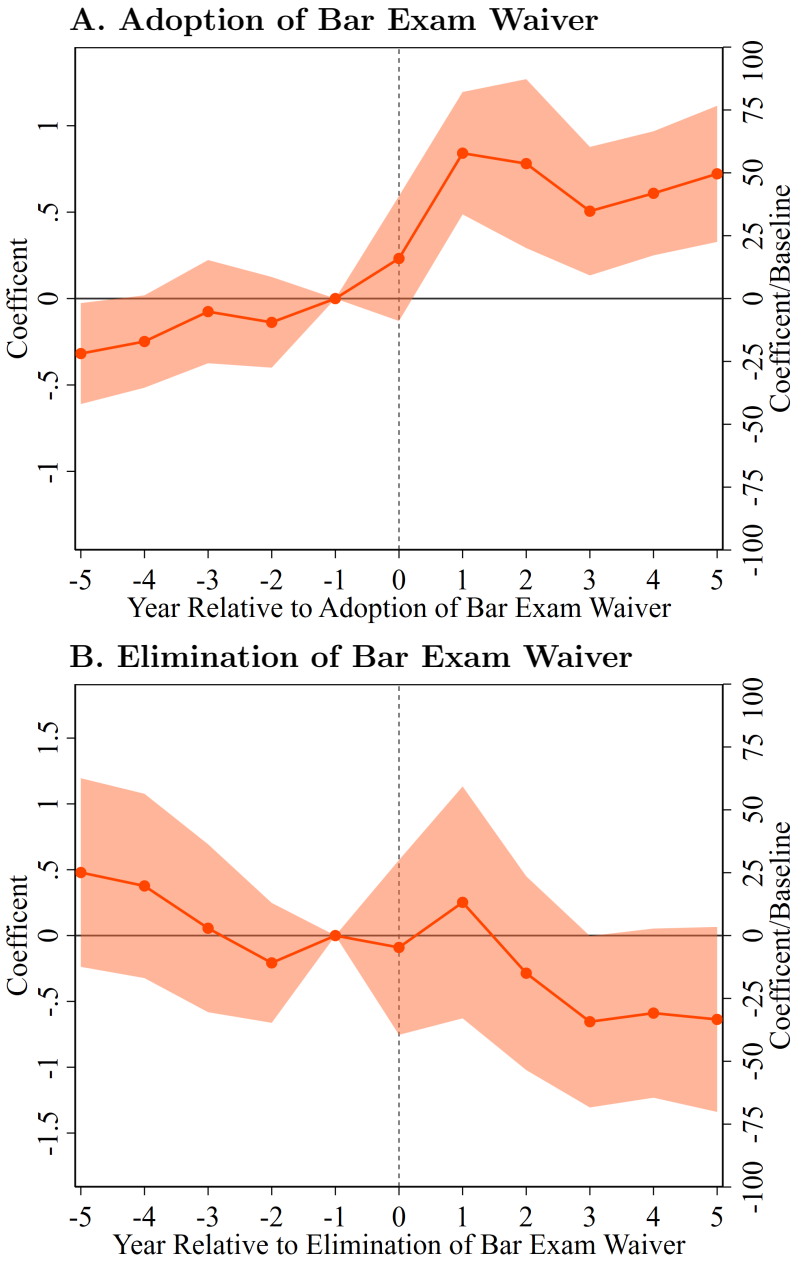
Note: This figure reports the distribution of corridor-level mobility, separately by whether the state is adjacent or non-adjacent. The x-axis is the share of experienced lawyers from a state that obtain a license in a destination state. The y-axis is the percent of corridors.

Figure A7: Years between Changes in Bar Exam Waivers within a Corridor



Note: This figure reports the distribution of the number years between a change in a bar exam waiver and the next-closest change in the bar exam waiver occurring in the same corridor. For example, if there was no bar exam waiver in a corridor between 1983 and 1994, there was a bar exam waiver in the same corridor from 1995-2000, and there was no bar exam waiver in the same corridor from 2001-2019, then the number of years to the closest event for both events is $|1994 - 2000| = 6$. We top code the years to closest event at 8. If only one event occurred in a corridor during our sample period, then the value would be coded as "8+".

Figure A8: The Impact of Bar Exam Waivers on Labor Market Mobility, By Adoption and Elimination of Bar Exam Waivers



Note: This figure estimates the impact of a bar exam waiver on the number of experienced lawyers obtaining a license in the state, separately by whether a bar exam waiver was adopted (Panel A) or eliminated (Panel B). The results come estimating Equation 2 in an event study framework. The left-side y-axis reports the estimated coefficients, and the right-side y-axis reports the estimated coefficients divided by the treatment mean without a bar exam waiver ($\times 100$). The shaded region reports 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Table A2: Substitution: Excluding Control Corridors

	Excluding Control Corridors		
	Origin	Dest.	Both
	(1)	(2)	(3)
Bar Exam Waiver	0.62***	0.62***	0.62***
	(0.10)	(0.10)	(0.10)
Mean Number of Lawyers	3.56	3.55	3.56
Treated Corridors	1.62	1.62	1.62
Control Corridors	3.46	3.45	3.45
Covariates			
Event-Corridor FE	✓	✓	✓
Event-Year FE	✓	✓	✓
Events	1212	1212	1212
Observations	25,244,241	25,461,799	25,006,080

Note: This table explores the extent that lawyers may substitute one destination state for another and the impact on our primary estimates. The results come estimating Equation 1. The row labeled "Mean # Lawyers" is the overall mean for both treated and control corridors in all years in the event window. The rows labeled "Treated Corridor" and "Control Corridor" are the means when the treated corridor does not have a bar exam waiver. The results specifically investigate the extent that violations of the stable unit treatment value assumption (SUTVA) affect the primary results by excluding subsets of control corridors. Column 1 excludes the treated corridor's origin state from the control group. Column 2 excludes the treated corridor's destination state from the control group. Column 3 excludes both the treated corridor's origin state and destination state from the control group. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A3: Substitution: Destination versus Origin State Policy Changes

	Triggering State Policy	
	Dest.	Origin
	(1)	(2)
Bar Exam Waiver	0.76***	0.25*
	(0.13)	(0.11)
Mean Number of Lawyers	3.45	3.83
Treated Corridors	1.76	1.24
Control Corridors	3.37	3.67
Mean Change in # Open Corridors	2	21
Covariates		
Event-Corridor FE	✓	✓
Event-Year FE	✓	✓
Events	876	336
Observations	18647442	7,052,518

Note: This table explores the extent that lawyers may substitute one destination state for another and the impact on our primary estimates. The results come estimating Equation 1. The row labeled "Mean # Lawyers" is the overall mean for both treated and control corridors in all years in the event window. The rows labeled "Treated Corridor" and "Control Corridor" are the means when the treated corridor does not have a bar exam waiver. The results specifically investigate the magnitude of related substitution effects by separately subsetting the events by whether the event was driven by a change in the policy of the treated corridor's destination state (which would result in relatively few opportunities for substitution) versus the treated corridor's origin state (which would result in relatively many opportunities for substitution). A smaller estimate for events driven by origin state policies is thus suggestive evidence of substitution effects. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A4: Substitution: Few versus Many Initially Open Corridors

	# Initially Open Corridors	
	< 25	≥ 25
	(1)	(2)
Bar Exam Waiver	0.68**	0.34*
	(0.26)	(0.15)
Mean Number of Lawyers	2.92	3.87
Treated Corridors	2.11	1.20
Control Corridors	2.98	3.86
Covariates		
Event-Corridor FE	✓	✓
Event-Year FE	✓	✓
Events	316	230
Observations	6,915,777	4,939,011

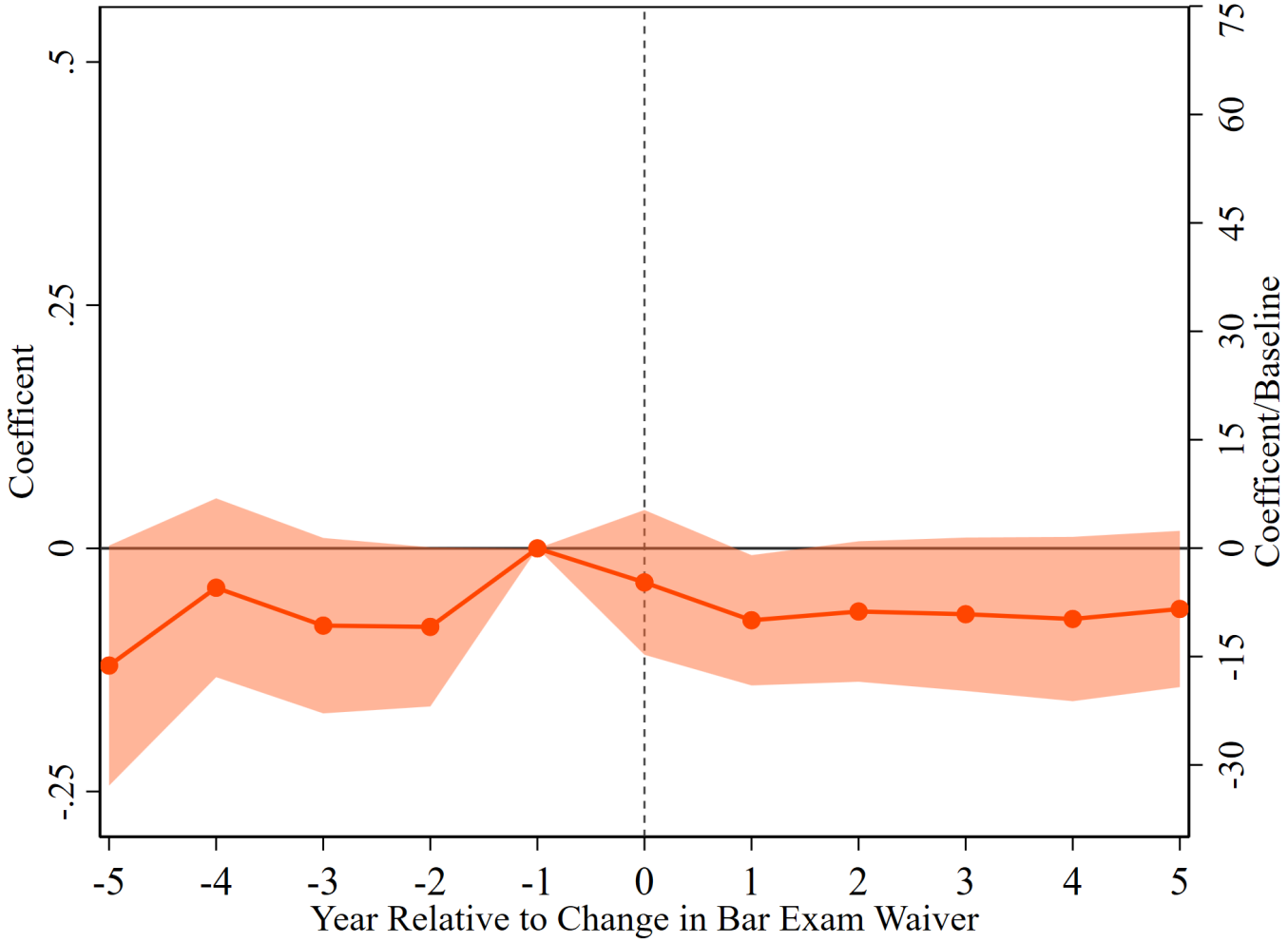
Note: This table explores the extent that lawyers may substitute one destination state for another and the impact on our primary estimates. The results come estimating Equation 1. The row labeled "Mean # Lawyers" is the overall mean for both treated and control corridors in all years in the event window. The rows labeled "Treated Corridor" and "Control Corridor" are the means when the treated corridor does not have a bar exam waiver. The table specifically investigates the magnitude of related substitution effects by separately subsetting the events by whether the treated corridor involved fewer alternative destination states with a bar exam waiver (Columns 6-7). Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A5: Accounting for Intertemporal Substitution

	Outcome	
	Inexp. + Exp. (1)	Inexp. Only (2)
Bar Exam Waiver	0.62***	0.00
	(0.11)	(0.04)
Mean Number of Lawyers	5.05	1.50
Treated Corridors	2.36	0.74
Control Corridors	4.97	1.52
Observations	25,699,960	25,699,960

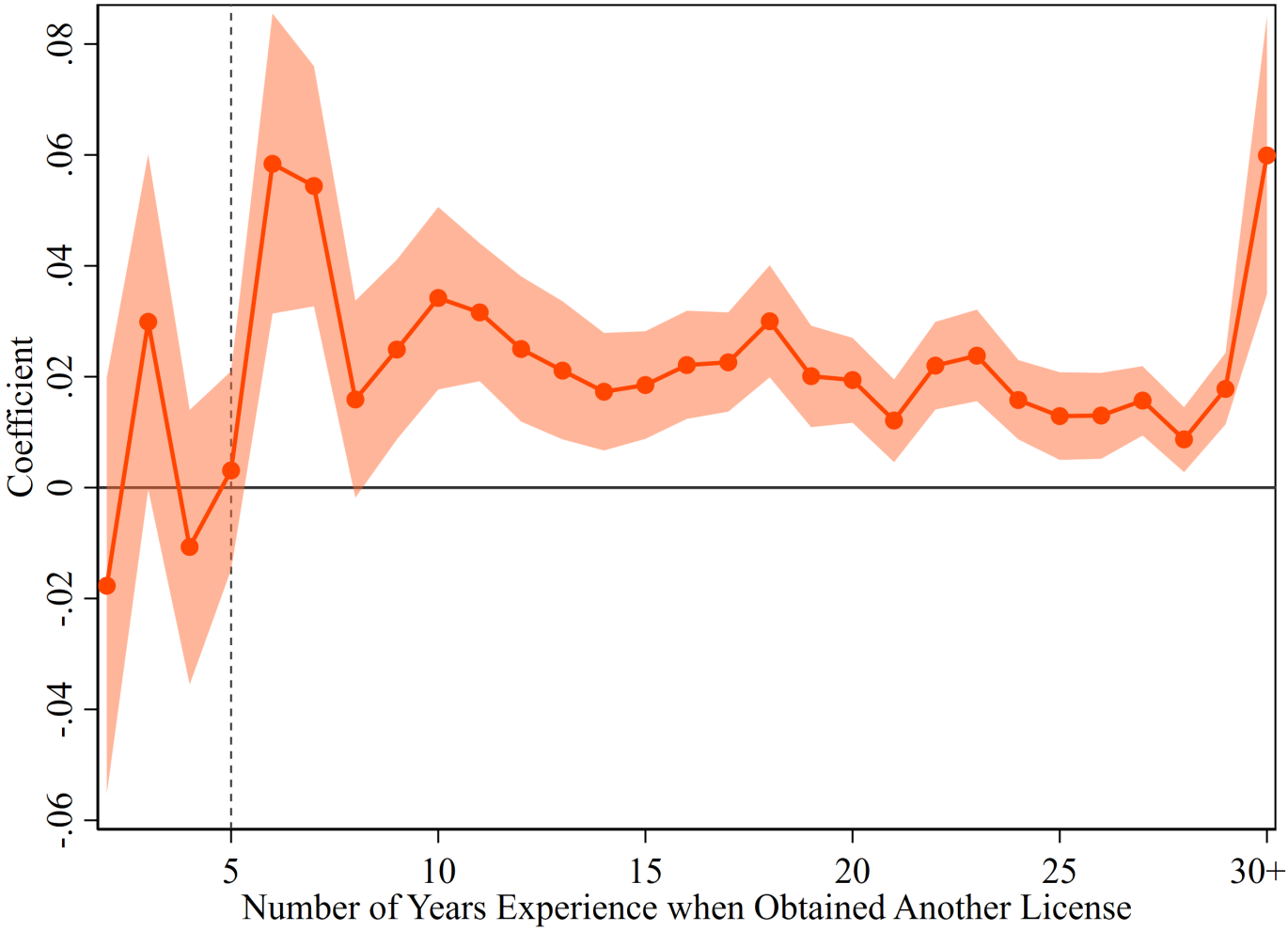
Note: This table reports regression results from Column 1 of Table 1 but for different outcomes. The outcome in Column 1 is the number of experienced and inexperienced licensed lawyers obtaining a license in the state. The outcome in Column 2 is the number of inexperienced licensed lawyers obtaining a license in the state. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Figure A9: Intertemporal Substitution



Note: This figure estimates the impact of a bar exam waiver on the number of inexperienced lawyers obtaining a license in the state in the 5 years before and after a change in a bar exam waiver. The results come estimating Equation 2 in a stacked event study framework that uses 1,212 events. The left-side y-axis reports the estimated coefficients, and the right-side y-axis reports the estimated coefficients divided by the treatment mean without a bar exam waiver ($\times 100$). The shaded region reports 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Figure A10: Experience-Specific Treatment Effects



Note: This figure reports estimates from a series of regressions estimating Equation 1 using different outcomes. The outcome in each regression is the number of lawyers flowing through a corridor with a specific number of years experience, as indicated on the x-axis. The shaded regions reports 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Table A6: Accounting for Confounding Changes in Policies and Market Conditions

	(1)
Bar Exam Waiver	0.70***
	(0.11)
Mean Number of Lawyers	2.67
Treated Corridors	1.70
Control Corridors	2.65
Events	975
Observations	217,261

Note: This table investigates potential bias from confounding policies and market conditions by exploiting particular events that allow us to isolate the impact of a bar exam waiver by selecting control corridors that would be equally affected by all other policies and market conditions in the state that triggered the event. For events triggered by destination state changes, we restrict to events involving changes to, or from, waiving reciprocally because in these situations there are some kj corridors that do not experience a change in their access to state j, and our control group consists of corridors with the same destination state j that did not experience a change in bar exam waiver but experienced a change in the other polices and market conditions in the destination state. For an event triggered by an origin state change, our control group consists of corridors with the same origin state i that did not experience a change in bar exam waiver but experienced a change in the other polices and market conditions in the origin state. The results come estimating Equation 1 in an event study framework using this sample. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A7: Restricting Control Group Based on Bar Exam Waiver Status

	Control Group			
	Status Before	Always Waiver	Never Waiver	Status Changed
	(1)	(2)	(3)	(4)
Bar Exam Waiver	0.59***	0.51***	0.64***	0.75***
	(0.10)	(0.10)	(0.11)	(0.10)
Mean Number of Lawyers	3.67	4.57	3.80	2.24
Treated Corridors	1.62	1.62	1.62	1.62
Control Corridors	3.55	4.40	3.71	2.21
Observations	22,184,767	11,145,552	4,919,508	9,661,564

Note: This table reports regression results from Column 1 of Table 1 but using different control groups. Column 1 uses only control corridors where the bar exam waiver status did not change before the event during our sample period. Column 2 uses only control corridors that never had a bar exam waiver during our sample period. Column 3 uses only control corridors that always had a bar exam waiver during our sample period. Column 4 uses only control corridors where the bar exam wavier status changed over the sample period but not within the event window. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A8: Restricting Events Based on Other Changes in Bar Exam Waiver in the Same Corridor

	Events	
	No Change 10 Years	First Event
	(1)	(2)
Bar Exam Waiver	0.49***	0.70***
	(0.11)	(0.14)
Mean Number of Lawyers	3.09	3.82
Treated Corridors	1.85	1.46
Control Corridors	3.21	3.61
Observations	9,397,014	16,302,946

Note: This table reports regression results from Column 1 of Table 1 but for different sets of events. Column 1 restricts to events where the bar exam waiver status in the treated corridor did not change within the 10 years before the event. Column 2 restricts to events that are a corridor's first event during our the sample period. Standard errors are in parentheses and are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A9: Standard Error Correction

	Level of Clustering				
	Corridor +Event	Corridor By Event	Event Only	Policy Only	Policy+ Corridor
	(1)	(2)	(3)	(4)	(5)
Bar Exam Waiver	0.62***	0.62***	0.62***	0.62**	0.62**
	(0.10)	(0.09)	(0.09)	(0.21)	(0.21)
Mean Number of Lawyers	3.55	3.55	3.55	3.55	3.55
Observations	25,699,960	25,699,960	25,699,960	25,699,960	25,699,960

Note: This table reports regression results from Column 1 of Table 1 but clustering standard errors at different levels. Column 1 multi-way clusters at the corridor and event level. Column 2 clusters at the corridor by event level. Column 3 clusters at the event level. Column 4 clusters at the policy change level. Column 5 multi-way clusters at the policy change and corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Table A10: Accounting for Missingness: Implied Impact of a Bar Exam Waiver on Interstate Mobility

	Effect for Lawyers Not in MH				
	0%	50%	100%	150%	200%
Levels Effect	0.62	0.74	0.86	0.98	1.10
Percent Effect	0.23	0.31	0.38	0.46	0.53

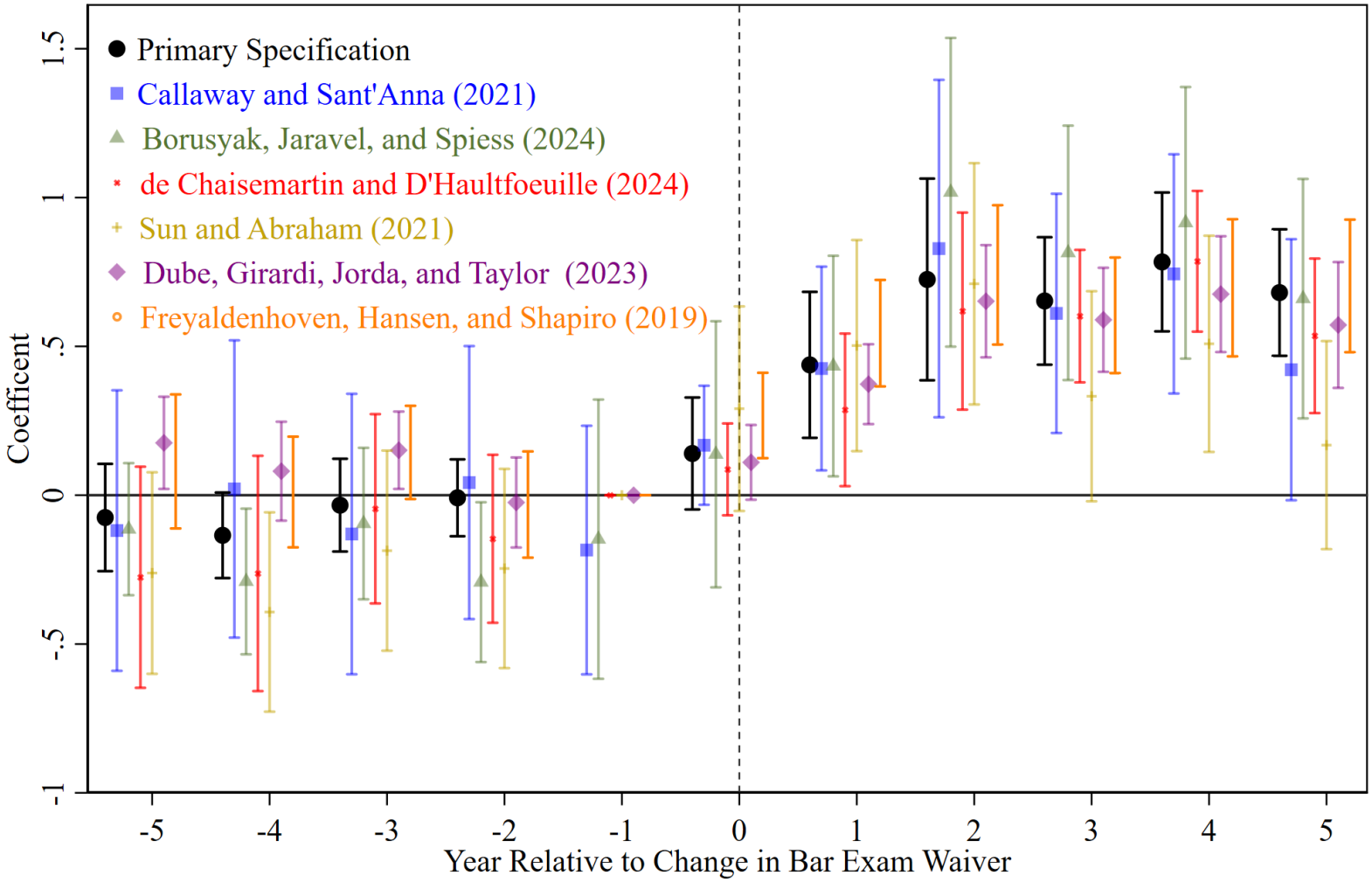
Note: This table reports the implied impact of a bar exam waiver on labor market mobility under various assumptions about the impact for lawyers not in the Martindale-Hubbell (MH) sample. The MH sample includes 61 percent of lawyers (Rozema, 2024c). The first row labeled "Levels Effect" is the implied effect in levels, computed by $\beta + \beta \times \eta \times (1 - m)$, where $\beta = 0.62$ is the estimated effect from Column 1 of Table 1, η is the assumed relative effect of a bar exam waiver on lawyers not in the MH sample as indicated at the top of the table, and $m = 0.61$ is the share of lawyers observed in the MH sample. The second row labeled "Percent Effect" is the implied effect in percent terms, computed by $\beta \times m/\bar{y} + (1 - m) \times \eta \times \beta/\bar{y}$, where $\bar{y}$ is the mean labor market mobility of lawyers in the treated group without a bar exam waiver from Column 1 of Table 1.

Table A11: Different Estimation Approaches

Specification	Coef. (1)	SE (2)	Mean (3)	Obs. (4)
Primary Specification	0.62***	(0.10)	3.55	25,699,960
Arkhangelsky et al. (2021)	0.52***	(0.16)	2.58	35,372
Borusyak et al. (2024)	0.48*	(0.19)	2.40	43,660
Callaway and Sant'Anna (2021)	0.68***	(0.17)	4.31	30,044
de Chaisemartin and D'Haultfoeuille (2024)	0.14**	(0.06)	3.06	37,101
Goodman-Bacon (2021)	0.90***	(0.23)	2.58	35,372
Roth and Sant'Anna (2023)	1.48***	(0.26)	3.06	94,350
Sun and Abraham (2021)	0.46***	(0.06)	3.06	94,350
TWFE	0.49***	(0.11)	3.06	94,350
Wooldridge (2023)	0.48*	(0.20)	2.40	43,660

Note: This table investigates the sensitivity of our results to a range of alternative proposed methods for estimating causal effects in panel data settings with staggered treatment timing, as indicated in the table. Column 1 reports the estimated coefficient. Column 2 reports the standard error. Column 3 reports the mean number of lawyers obtaining a license at the corridor-year level in the sample. Column 4 reports the number of observations in the sample. Standard errors are clustered at the corridor level. $^{+}$ p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

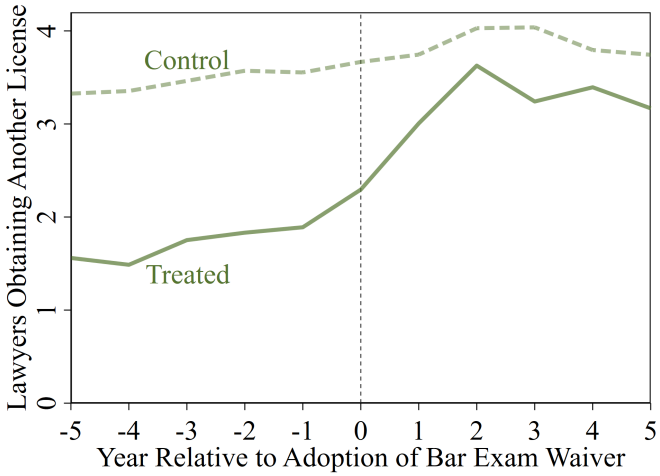
Figure A11: Different Estimation Approaches



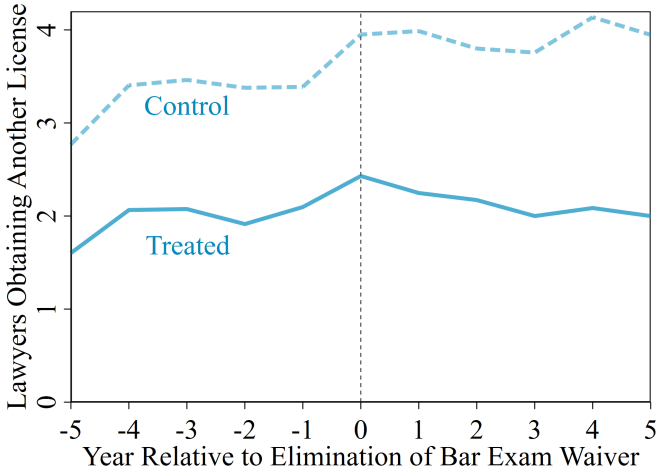
Note: This figure investigates the sensitivity of our results to a range of alternative proposed methods for estimating causal effects in panel data settings with staggered treatment timing, as indicated in the figure. The y-axis reports the estimates. The vertical bars report 95 percent confidence intervals. Standard errors are clustered at the corridor level.

Figure A12: Synthetic Difference-in-Differences

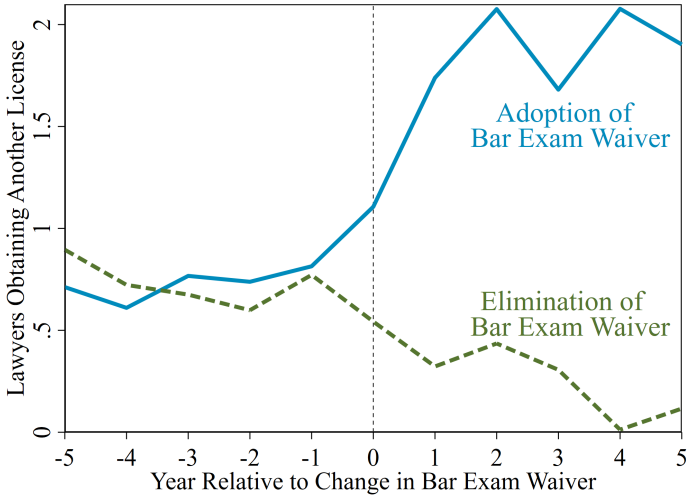
A. Adoption of Bar Exam Waiver



B. Elimination of Bar Exam Waiver



C. Difference Between Treated and Control



Note: This figure reports results using the synthetic difference-in-differences design from Arkhangelsky et al. (2021). For each event, we estimate a synthetic control group using the stata command `syndd`. We then compute the mean for the treated group and synthetic control groups in each event time, separately by whether a bar exam waiver was adopted or eliminated. Panel A reports the mean of the treated and control groups for events adopting a bar exam waiver. Panel B reports the mean of the treated and control groups for events eliminating a bar exam waiver. Panel C reports the difference between the mean of the treated and control groups separately by whether the event adopted or eliminated the bar exam waiver.

Table A12: Method of Counting Labor Market Mobility of Lawyers Obtaining Third or Subsequent License

	Method of Counting Mobility			
	ij	ij, ik	ij, jk	Dependent
	(1)	(2)	(3)	(4)
Bar Exam Waiver	0.35***	0.44***	0.48***	0.47***
	(0.07)	(0.08)	(0.08)	(0.08)
Mean Number of Lawyers	1.63	2.45	2.46	2.31
Treated Corridors	0.65	1.09	1.04	0.98
Control Corridors	1.59	2.39	2.41	2.27
Events	1,212	1,212	1,212	1,212
Observations	25,699,960	25,699,960	25,699,960	25,699,960

Note: This table reports regression results from Column 1 of Table 1 but counting the labor market mobility of lawyers obtaining their third or subsequent license in different ways. Column 1 counts the lawyer as moving through the ij and ik corridors. Column 2 counts the lawyer as moving through the ij and jk corridors. Column 3 counts the lawyer as just moving through the ij corridor. Column 4 counts the lawyer as moving through the ij corridor but then uses information on how lawyer obtained a license in j as well as waivers in the ik and jk corridors to classify whether the lawyer had a bar exam waiver into k from either i or j, where our specific approach is as follows. First, we determine whether the ij corridor when the lawyer obtained a license had a bar exam waiver and whether the lawyer had enough experience to benefit from it (if the j license was obtained 5 years before the i license, then the lawyer had to take the j bar exam). Second, if the lawyer did not take the j bar exam, then we assume that the lawyer moved through the ik corridor. Third, if the lawyer took the j bar exam, then what corridor we assume they moved through depends on policies of the ij and ik corridor: (1) if there is a bar exam waiver for the ik corridor but not the jk corridor, then we assume that the lawyer moves through the ik corridor only, (2) if there is a bar exam waiver for the jk corridor but not the ik corridor, then we assume that the lawyer moves through the jk corridor only, (3) if there is a bar exam waiver for both the ik and jk corridors, then we assume that the lawyer moves through both corridors, and (4) if there is no bar exam waiver for either the ik and jk corridors, then we assume that the lawyer moves through both corridors.

Table A13: Excluding Corridors Involving DC

	Excluding Corridors Involving DC		
	Origin Only	Dest. Only	Dest. +Origin
	(1)	(2)	(3)
Bar Exam Waiver	0.59***	0.62***	0.60***
	(0.09)	(0.09)	(0.09)
Mean Number of Lawyers	3.27	3.24	2.95
Treated Corridors	1.58	1.62	1.58
Control Corridors	3.17	3.14	2.85
Observations	24,802,349	25,033,360	24,503,127

Note: This table reports regression results from Column 1 of Table 1 but excludes events or control corridors involving DC. Column 1 excludes events where the treated corridor has a DC origin and excludes control corridors that have a DC origin. Column 2 excludes events where the treated corridor has a DC destination and excludes control corridors that have a DC destination. Column 3 excludes events where the treated corridor has a DC destination or origin and excludes control corridors that have a DC destination or origin.

Table A14: Confounding Changes in Disciplinary System

	Specification				
	Reduced Form (1)	First Stage (2)	IV (3)	Comparison Group (4)	P-Value (5)
Bar Exam Waiver	0.029**	0.854***			
	(0.010)	(0.192)			
Lawyers Obtaining Another License			0.034**		
			(0.011)		
Always Movers Discipline Share				0.017	p<0.116
Covariates					
Event-Corridor FE	✓	✓	✓		
Event-Year FE	✓	✓	✓		
Outcome Mean	0.068	5.626	0.068		
Treated Corridors	0.03	1.92	0.03		
Control Corridors	0.08	5.55	0.08		
Events	681	681	681		
Observations	107,371	107,371	107,371		

Note: This table reproduces Table 2 but using only control corridors that share the same destination state as the treated corridor. Column 1 estimates the reduced form regression of the number of experienced lawyers obtaining another license who are disciplined at some point during their career on the bar exam waiver (Equation 3). Column 2 estimates the first stage regression of the total number of experienced lawyers obtaining an additional license on a bar exam waiver (Equation 1). Column 3 estimates the IV regression, where the coefficient is interpreted as the discipline rate of the lawyers who were induced to obtain an additional license by the bar exam waiver. Column 4 reports the discipline rate of lawyers who obtain an additional license even without a bar exam waiver (always movers). Column 5 reports the p-value of whether the estimated discipline rate of the marginal movers from Column 3 is different than the discipline rate of the always movers from Column 4.

Supplemental Appendix B: Simulations

Our simulations use (1) state-level bar exam waiver policies in 1983 as the initial conditions, and (2) the transition probabilities from one policy to another policy. Each of these parameters are given in Appendix Table B1. Using these parameters, we randomly assign states a 1983 policy. Next, given the randomly assigned 1983 policies, we simulate each state's 1984 policy using the transition probabilities and a random number generator; given the 1984 policies, we simulate each state's 1985 policy using the transition probabilities and a random number generator; and so on. For each simulation, this provides a counterfactual set of bar exam waiver policies at the state level. We then use the simulated state-level policies to construct bar exam waivers for each corridor. We run 5,000 simulations, which generates 5,000 sets of corridor-year level bar exam waivers.

To assess the extent this approach matches the actual policy variation, Appendix Figure B1 plots the distribution of number of total events for each simulation against the actual number of events (Panel A, blue distribution). The mean and median number of events across the simulations are 2,310 and 2,291, and the 5th and 95th percentiles of the simulated policies are 1,755 and 2,911. The actual number of events is 2,396. The actual number of total events thus fits within the simulated distribution.

We then use the same stacked event study approach for our actual analysis to generate a stacked dataset for each simulated set of corridor-year level bar exam waivers, applying the same set of restrictions. To assess the extent that the stacked event study restrictions affect the number of events in the simulated bar exam waivers in a similar way to how it affects the number of events in actual bar exam waivers, Figure B1 plots the distribution of the number of events after the event study restrictions for each simulation against the actual number of events after the restriction (Panel A, orange distribution). After the stacked event study restrictions, the mean and median number of events across the simulations is 1,309 and 1,302, and the 5th and 95th percentiles of the simulations are 958 and 1,683. The actual number of events after stacked event study restrictions is 1,212. The

actual number of remaining events after the stacked event study restriction thus fits within the simulated distribution. Combined, these results suggest that the simulation approach produces a set of bar exam waiver changes that largely reflect the actual changes in terms of the number (captured by the total number of events) and timing (captured in the restricted number of events).

Next, for each simulation, we match the actual number of lawyers flowing through each corridor in each year (y_{ijt}) to the simulated stacked event study datasets and estimate the primary specification. Figure B2 reports the distribution of estimated coefficients. The mean and median of the simulated coefficients are -0.0012 and -0.00009, and the 5th and 95th percentiles are -0.31 and 0.30. The actual estimate is 0.62 (Table 1, Column 1). Only 2.1 percent of estimates from the simulations have an estimate larger than the actual estimate.

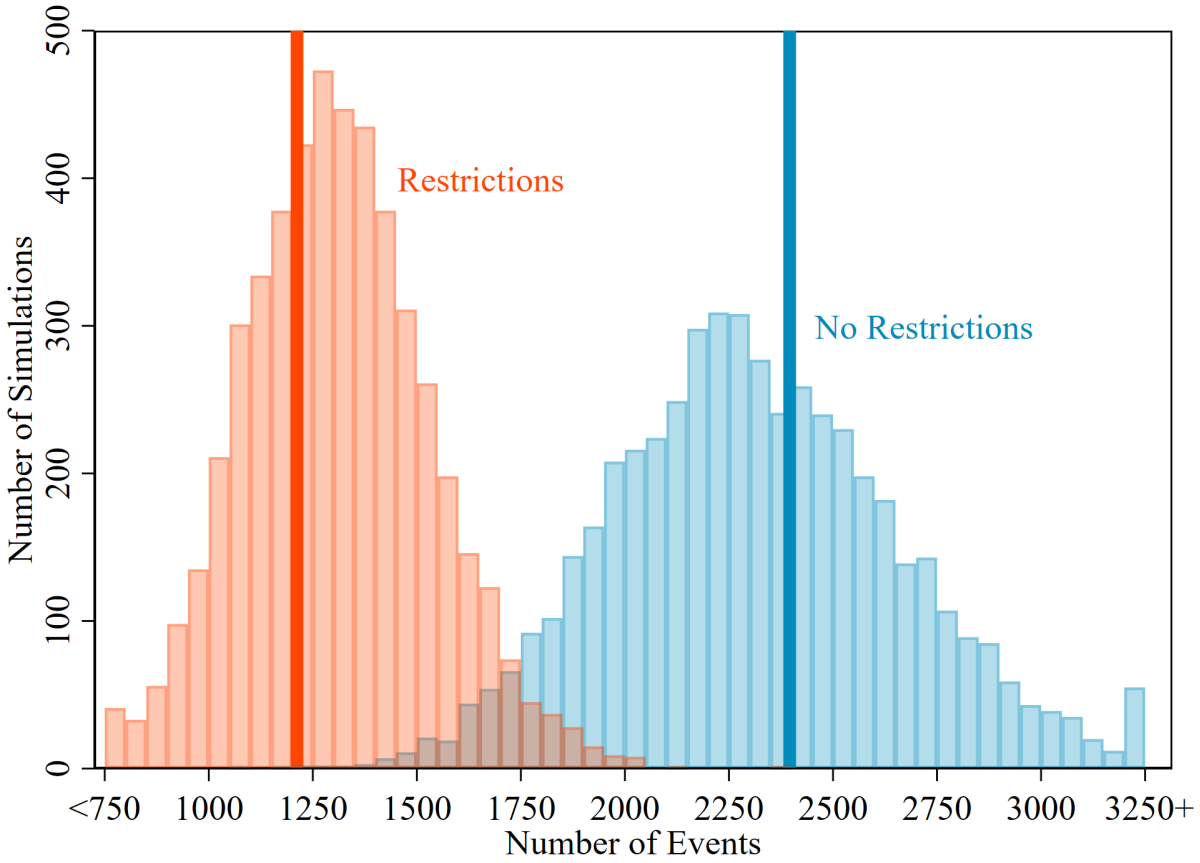
Finally, we compute the mean flow in the treated corridors without a waiver in the simulations, and we document the relative effect size in the simulations. Figure B3 reports the results. Panel A plots the distribution of the mean size of the corridor in the treated group without a waiver against the actual mean. We find that the simulated corridors that are subject to treatment are much larger than the actual corridors subject to treatment. The average and median corridor size in the simulations are 3.12 and 3.06, and the 5th and 95th percentiles are 2.15 and 4.26. The actual mean corridor size is 1.62. Treated corridors in the simulations thus have much smaller flows than in the actual treated corridors, and only 7/5000 simulations had a lower mean. Panel B reports the distribution of the coefficients divided by the treated mean without a bar exam waiver, and Panel C reports a scatter plot of the coefficients against the treated mean. It reveals that the actual coefficient-mean pair is an outlier. The mean and median relative effect in the simulations are 0.01 and 0.03 percent, and the 5th and 95th percentiles are -9.4 and 10.1 percent. The largest simulated estimated relative effect is 26.8 percent. The actual estimated relative effect is 38 percent. This provides strong evidence that the actual coefficient-mean pair is an outlier.

Table B1: Simulation Approach: Transition Probabilities

		Policy in $t+1$		
Policy in t	Initial Conditions (1)	Waive for None (2)	Waive Reciprocally (3)	Waive for All (4)
Waive for None	39.21	96.19	2.74	1.07
Waive Reciprocally	17.65	0.97	98.06	0.97
Waive for All	43.14	1.25	1.78	96.98

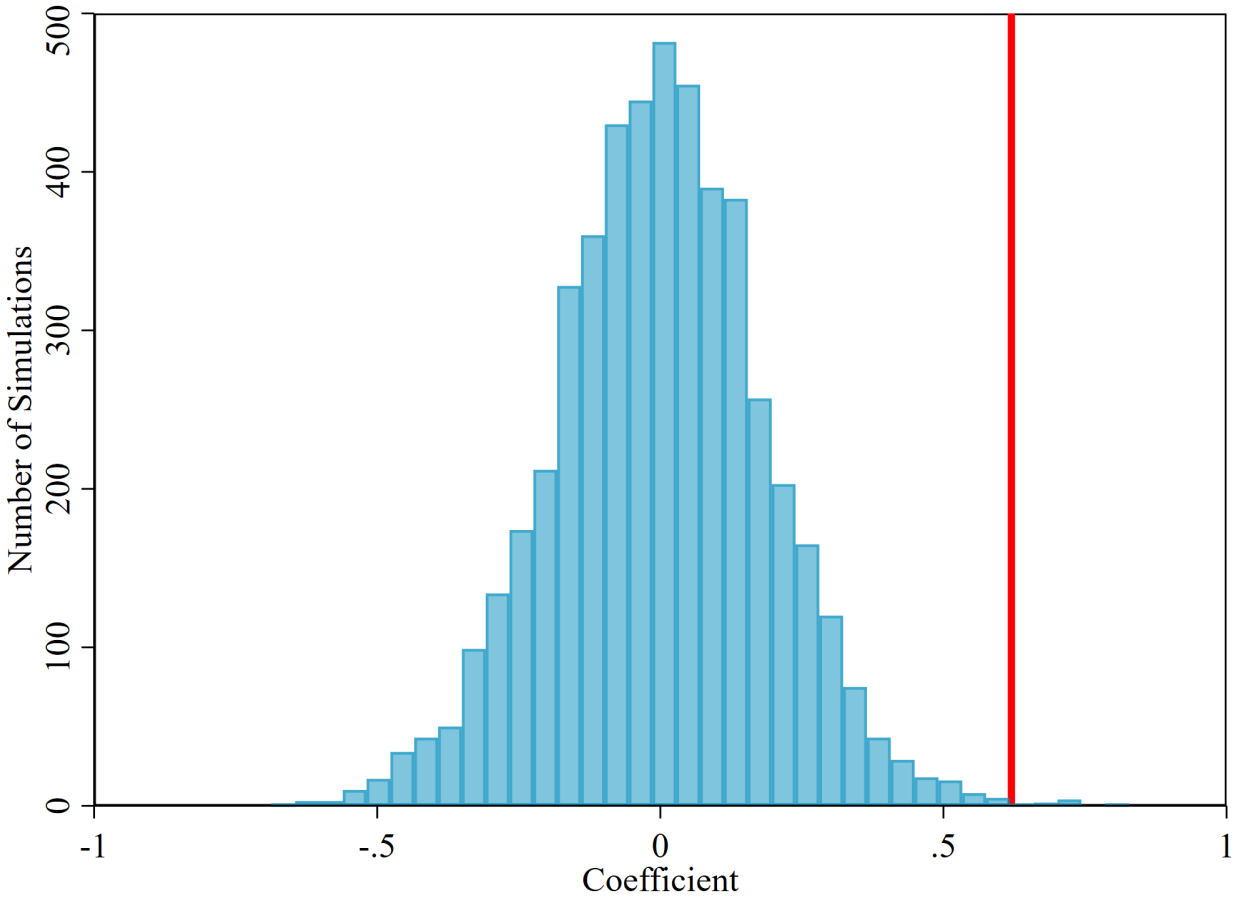
Note: The table reports the share of the bar exam waiver policies in 1983 (Column 1) and the annual transition probabilities from one policy to another from 1983 to 2019 (Columns 2-4, where each row is a separate initial policy in time t, and each column is the subsequent policy in time $t+1$.)

Figure B1: Assessing Simulation Approach



Note: This figure reports the results of a simulation exercise where we use the initial conditions of one of three policies in 1983 and then simulate policy changes using the transition probabilities from one policy to another policy. We then apply the same stacked event study restrictions to the simulated data. The figure assesses the extent this approach matches the actual policy variation by plotting the distribution of number of total events for each simulation against the actual number of events (blue distribution) and the distribution of the number of events after the event study restrictions for each simulation against the actual number of events after the restriction (orange distribution).

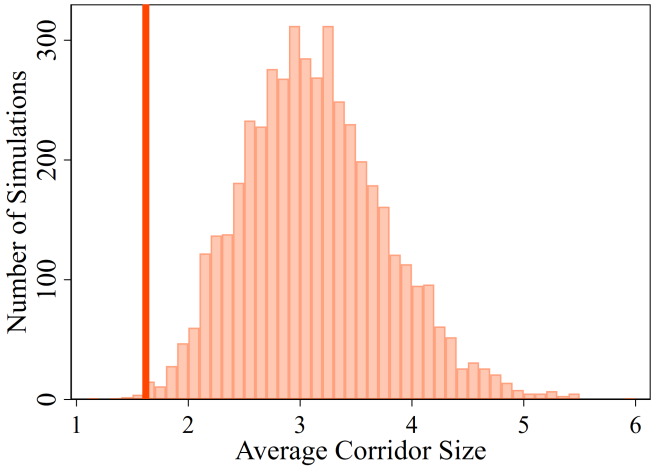
Figure B2: Simulation Results: Estimated Coefficients



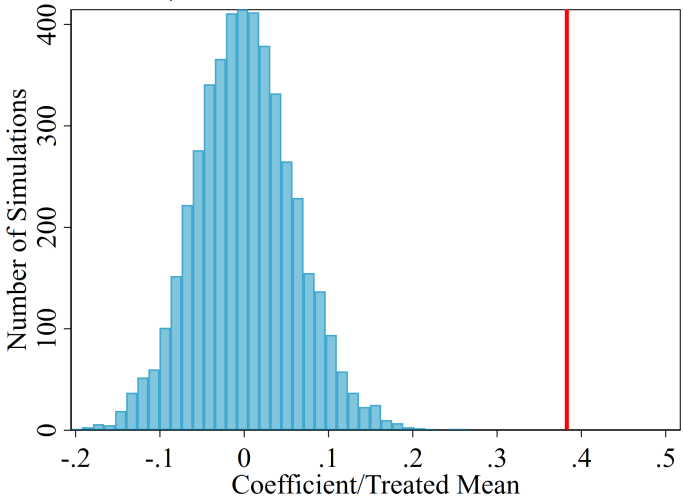
Note: This figure reports the results of a simulation exercise where we use the initial conditions of one of three policies in 1983 and then simulate policy changes using the transition probabilities from one policy to another policy. We then apply the same stacked event study restrictions to the simulated data, and we estimate the main specification (Table 1, Column 1). We run 5,000 simulations. The figure reports the distribution of estimated coefficients against the actual estimated coefficient (vertical line).

Figure B3: Simulation Results: Relative Effect

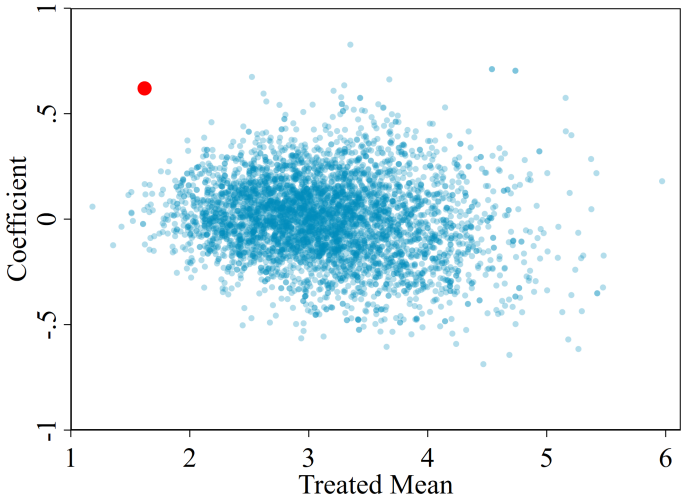
A. Average Corridor Size



B. Coefficient/Treated Mean



C. Scatterplot of Mean and Coefficient



Note: This figure reports results assessing the relative corridor size and relative effect size of the simulations. Panel A plots the distribution of the mean size of the corridor in the treated group without a waiver against the actual mean (the vertical bar). Panel B reports the distribution of the coefficients divided by the treated mean against the actual relative estimates (vertical line). Panel C reports a scatterplot of the coefficients against the treated mean for the simulated estimates (small, blue circles) and the actual estimate (large, red circle).