

Does the Bar Exam Protect the Public?

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I study the effects of requiring lawyers to pass the bar exam on whether they are later publicly disciplined for misconduct. In the 1980s, by abolishing what is known as a diploma privilege, four states began to require graduates from all law schools to pass the bar exam. My research design exploits these events to estimate the effect of the bar passage requirement on the share of lawyers publicly sanctioned by state discipline bodies. I find that during the first decade of their careers lawyers licensed without a bar passage requirement are publicly sanctioned at similar rates to lawyers licensed after passing a bar exam. Small differences do begin to emerge after a decade, however, and larger though still modest differences form after two decades.

Keywords: bar exam; legal profession; occupational licensing; professional discipline

I. INTRODUCTION

Governments regulate many labor markets by requiring that anyone working in those markets hold a license. The main justification for occupational licensing regimes is that they are necessary to protect the public (Kleiner, 2006), especially by shielding consumers from the worst outcomes (Larsen et al., 2020). One field that has long been considered to have some of the strictest licensing requirements is the legal profession. Licensing requirements for lawyers are set at the state level and thus differ from state to state. But there are typically three requirements to obtain a law license: lawyers must obtain a 3-year law degree from an accredited law school, show that they possess good

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For helpful comments, I am grateful to Scott Baker, Adam Chilton, Albert Choi, Danielle D'Onfro, Ofer Eldar, Dan Epps, Michael Frakes, Jacob Goldin, Michael Heise, Jim Hines, Erik Hovenkamp, William Hubbard, Peter Joy, Don Kenkel, Bob Kuehn, Emily Nix, Wendy Muchman, JJ Prescott, Sarath Sanga, Dave Schwartz, Nadav Shoked, and seminar participants at the 2019 Conference on Empirical Legal Studies, the Duke University Law and Economics Colloquium, the Indiana University Maurer School of Law Faculty Workshop, the Northwestern University Pritzker School of Law Faculty Workshop, the University of Chicago Law School Workshop, the University of Michigan Law and Economics Workshop, and the Washington University School of Law Workshop. For excellent research assistance, I thank James Bambury, Garrett Bell, Hyla Bondareff, Mack Bowles, Victoria Bush, Amira Harmon, Jordan Hoffman, Jeffrey Jiang, Ryan Lantry, Jake Law, Paul Luhmann, Lydia Marik, Emil Risk, Christina Schrantz, Chiajung Tsai, and Weixian Zhang.

moral character by submitting to a background check, and pass a bar exam. The requirement to pass a bar exam is thought to be the most stringent of the licensing requirements to enter the legal profession.

In this article, I study the effects of the bar passage requirement on whether lawyers are later publicly disciplined for misconduct. Public sanctions capture conduct that violate ethical rules and that was discovered, reported, and determined to be serious enough to warrant a public (rather than a private) sanction. The underlying conduct that leads to public sanctions can vary widely, but it includes a lack of diligence in representing a client (e.g., missing court deadlines and thereby causing a case to be dismissed), representing a client where a conflict of interest exists (e.g., advising a family member in a way that personally benefits the lawyer), and participating in conduct that is prejudicial to the administration of justice (e.g., knowingly making false statements to a court).¹ Although public sanctions are an imperfect proxy of harm to the public because they reflect a nonrandom set of incidents of misconduct, they nonetheless serve both as a source of information on the damage inflicted by lawyers and as a signal of the presence of incompetent or unethical lawyers.

To study the effects of the bar passage requirement, I exploit the only extensive margin changes in it since the 1970s. In the 1980s, Mississippi, Montana, South Dakota, and West Virginia imposed a bar passage requirement for graduates from the flagship law schools in those states. Before, those graduates were exempt from the bar passage requirement because of a “diploma privilege.” In my research design, graduates of the four law schools who thereby lost their diploma privilege were “treated” by the bar passage requirement. In a difference-in-differences framework, I assess changes in public sanctions against graduates of these four law schools who obtained a law license in the years before and after the bar passage requirement was imposed. To form control groups, I use lawyers in other states—from the same geographic region—that did not change their bar passage requirement over the same time period.

My research design requires rich data on public sanctions at the lawyer level. The problem is that these data are not made available to the public by state discipline bodies. To address this problem, I built a novel panel dataset at the state-cohort level of the share of lawyers who received public sanctions by state discipline bodies at each year in their career. To do so, I first identified a source of information on public sanctions for each state. Depending on the state, I then built the dataset from public websites that state regulatory bodies maintain, state bar magazines, and newspapers. For some states, hundreds of issues of state bar journals were obtained and searched to document the details of each public sanction. Searches at the lawyer level were then conducted to identify the lawyer’s year of admission and the law school they attended. Through this process, I am able to estimate the share of lawyers who received public sanctions by state discipline bodies at the state-cohort level at each year in their career.

¹For examples of the behavior leading to a public sanction in two states that have detailed descriptions that are reasonably accessible (Maryland and New Jersey), see Tables A1 and A2 in Appendix A. For a breakdown of the type of conduct for which a lawyer was publically disciplined in one state that has the information reasonably accessible (the State of Washington), see Table A3 in Appendix A.

Using these data, I first estimate the effect that a bar passage requirement has on the overall share of lawyers who receive public sanctions. Over the first decade following licensing, the estimates suggest that lawyers licensed without a bar passage requirement are publicly sanctioned at similar rates to lawyers licensed after passing the bar exam. However, differences begin to emerge in the second decade, and the differences become larger in the third decade. The estimates suggest that the bar passage requirement decreased the share of lawyers who received a public sanction within 25 years after admission from 5.1% to 3.9%. Under most specifications, these differences are only marginally significant at the 10% level, meaning that I find only weak evidence that there are differences between lawyers licensed under a diploma privilege and lawyers licensed after passing the bar exam. Overall, these findings suggest that bar passage requirements have modest, negative effects on public sanctions. Although the point estimates indicate there is a 24% decrease in public sanctions (1.2 percentage points relative to the baseline of 5.1%), this reflects the cumulative share of lawyers receiving public sanctions over a 25-year period. At an annualized rate, the average effect is 0.9%. Moreover, the confidence intervals suggest that the effect is not greater than an annualized rate of 1.6% (2.1 percentage points relative to the baseline of 5.1% but spread out over 25 years). These results thus provide evidence against the presence of large effects.

Next, I investigate the underlying mechanisms that drive the overall results. One way the bar passage requirement could influence public sanctions is by preventing lawyers who would have received public sanctions at high rates from ever obtaining a law license. Another way the bar passage requirement could influence public sanctions is by changing lawyers' behavior in a way that prevents them from being publicly sanctioned (e.g., lawyers learn how to be more ethical by studying for the bar exam). Although the results do not allow me to disentangle these two mechanisms, I build a conceptual framework to estimate bounds for them. On the one hand, suppose that the bar exam's effect of withholding a law license from some individuals drives *none* of the effects of the bar passage requirement on public sanctions. Under this assumption, the estimates would suggest that the bar passage requirement changed the behavior of roughly 12 out of every 1000 lawyers who actually passed the bar exam in a way that prevented them from being publicly sanctioned within 25 years.² On the other hand, suppose that the bar exam's effect of withholding a law license from some individuals drives *the entire* effect of the bar passage requirement. Under this assumption, the estimates would suggest that 18% of lawyers licensed only because of the lack of a bar passage requirement (i.e., individuals who would have never passed a bar exam) would be publicly sanctioned within 25 years.³

²This behavioral change could be driven by any number of channels. For example, to the extent that a decrease in the total entry of lawyers into the profession leads to higher wages, higher wages for good behavior could decrease misconduct by decreasing the relative value of bad behavior.

³These findings are based on the assumption that lawyers with different propensities to be publicly sanctioned do not sort to states on the basis of the bar passage requirement. To the extent that lawyers with a higher propensity to be publicly sanctioned sort to a state because of a bar passage requirement, the differences between lawyers who would and would not pass the bar exam would be reduced.

Conversely, of lawyers licensed without a bar exam requirement—but who would have eventually passed it if required—4% would be publicly sanctioned within 25 years.⁴ Overall, although not distinguishing between competing mechanisms is a limitation of this research,⁵ this bounding exercise helps to document the potential magnitude of the underlying mechanisms.

Although the results do not necessarily support one position in the debates over the bar passage requirement over the other, certain aspects of the results could be argued to support a diploma privilege while others could be argued to support a bar passage requirement. On the one hand, one might argue that the results support the diploma privilege because they show that the bar exam is too inaccurate as a tool for screening out lawyers who would later be publicly sanctioned. The argument would be that (1) the bar passage requirement prevented less than one third of lawyers who would be publicly sanctioned from practicing law, and (2) the diploma privilege increased the size of the legal profession who would not be publicly sanctioned by 9%. On the other hand, one might argue that the results support a bar passage requirement because the bar exam is accurate enough as a tool for screening out lawyers who would later be publicly sanctioned. The argument would be that (1) the bar passage requirement prevented a much larger share of lawyers who would be publicly sanctioned than lawyers who would not be publicly sanctioned from practicing law, and (2) the bar passage requirement decreased the overall share of lawyers who would be publicly sanctioned within 25 years by one fifth.

Given that both of these arguments could be supported by the results, the evidence presented here is unlikely to settle debates on the bar passage requirement. However, it is important to also note that even larger negative effects associated with a bar passage requirement would not have been able to truly settle the debate. This is because the impact of the bar passage requirement on public sanctions is not the only or even the primary consideration in this debate. The bar passage requirement can decrease access to legal services, increase the cost of legal services, decrease interstate mobility of lawyers, and disparately impact groups of aspiring lawyers and client populations. Moreover, the welfare implications of the results depend not only on the accuracy of the bar exam in identifying lawyers who would later be publicly sanctioned but also on the relative welfare costs of passing a lawyer who would be publicly sanctioned and failing a lawyer who would not be publicly sanctioned. The results therefore do not speak directly to the welfare implications of the bar passage requirement.

This article contributes to the growing empirical literature studying the effects of occupational licensing on the quality of services across a range of professions (e.g., Anderson

⁴Another way to view these findings is in terms of the accuracy of the bar exam: of all the lawyers licensed without a bar passage requirement but who have not received a public sanction, the estimates would suggest that 7% would have never passed the bar exam; of all the lawyers licensed without a bar passage requirement but who have received a public sanction, the estimates would suggest that 29% would have never passed the bar exam.

⁵Most empirical research estimating the effects of changes in licensing regimes suffer from the same limitation (e.g., Larsen et al., 2020).

et al., 2020; Currie & Hotz, 2004; Hall et al., 2020; Larsen et al., 2020). Although the legal profession is one of the largest and most important professions for the US economy, it has largely been excluded from this enterprise.

This article proceeds as follows. Section II provides a background. Section III introduces the original dataset, describes the identification strategy, and reports the primary results. Section IV investigates mechanisms. Section V reports robustness checks. Section VI concludes.

II. BACKGROUND

A growing empirical literature studies the effects of occupational licensing on the quality of services across a range of professions.⁶ One challenge is finding a measure of the quality of output or services. Quality of services is often measured by negative outcomes,⁷ which include injuries, illnesses, accidents, or mishaps related to the profession (Kleiner, 2000). Much of the research on the effects of occupational licensing on the quality of services is the medical context.⁸ With a few exceptions,⁹ there is limited empirical evidence on the effects of occupational licensing on the legal profession generally and on negative outcomes in particular.¹⁰

⁶To date, the literature has investigated the effects of occupational licensing on the outcomes of services of building contractors (Maurizi, 1980), day care center directors (Currie & Hotz, 2004), dentists (Kleiner & Kudrle, 2000), electricians (Carroll & Gaston, 1981), midwives (Anderson et al., 2020), mortgage brokers (Kleiner & Todd, 2007), nurse practitioners (Kleiner et al., 2016), opticians (Timmons & Mills, 2018), physicians (Kugler & Sauer, 2005), residential home services (Farronato et al., 2020), teachers (e.g., Kane et al., 2008; Kleiner & Petree, 1988; Larsen et al., 2020), and Uber drivers (Hall et al., 2020).

⁷Consumer complaints have also been used as a measure of negative outcomes from services rendered (e.g., Johnson & Loucks, 1986; Kleiner & Kudrle, 2000; Maurizi, 1980).

⁸These studies typically assess negative patient outcomes. For example, Kleiner and Kudrle (2000) study the licensing of dentists by assessing untreated dental disease, and Kleiner et al. (2016) study licensing restrictions of nurse practitioners (who perform a number of medical tasks with and without the supervision of physicians) by assessing infant mortality rates.

⁹Ramseyer and Rasmusen (2015) investigate how the difficulty of the Japanese bar exam influences the credentials of lawyers and find that a change in the bar exam that made it less difficult to pass actually increased the quality of young lawyers. Anderson and Muller (2019) use data on lawyers licensed in one state (California) to examine the relationship between bar exam scores and career discipline rates. In particular, they regress the number of sanctions against a lawyer on an estimate for the lawyer's bar exam score that is based on their law school's average bar passage rate.

¹⁰In addition to studies on licensing requirements, there have also been a few studies that have assessed lawyer sanctions more generally (e.g., Bartlett, 2008; Hatamyar & Simmons, 2004; Kinsler, 2017; Sklar et al., 2019). The literature typically finds the following characteristics to be associated with higher risk factors for sanctions: old age, incorporated law practice, men, law practice in rural areas, solo and small law practice, trust account authority, and failed the bar exam on the first attempt (Arnold & Hagan, 1992; Baer & Corneille, 1992; Carlin, 1966; Curtis & Kaufman, 2004; Davies, 1999; Hatamyar & Simmons, 2004; Joy, 2004; Kinsler, 2017; Lee, 2020; Levin et al., 2013; Zemans & Rosenblum, 1981).

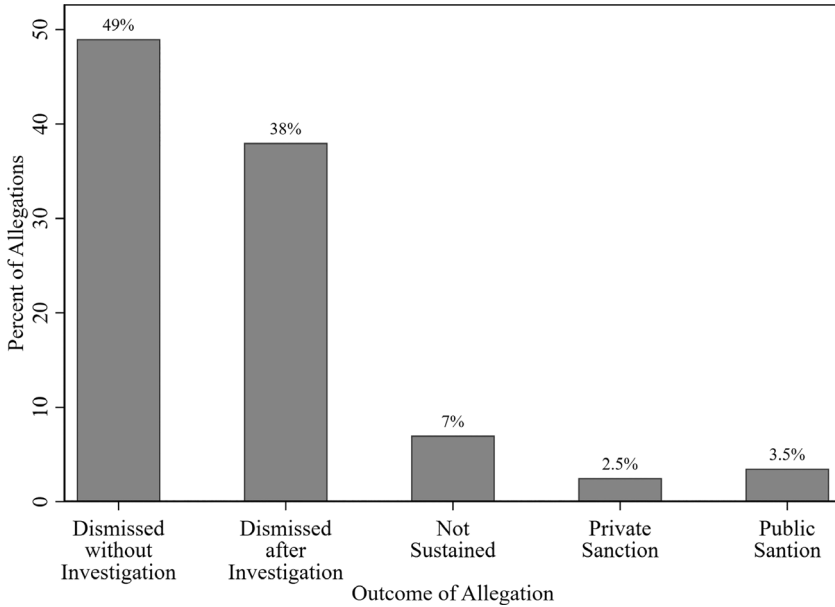
To practice law in a state, a lawyer must be licensed in that state. To obtain a license in a state, a lawyer must typically graduate from law school, show that they possess good moral character by submitting to a background check (resulting in what is known as a character and fitness report), and pass a bar exam. A lawyer can be licensed in multiple states, and there are no limits on the number of states in which someone can be licensed. For initial licensure, some states use one or more national level exams (e.g., the Multistate Bar Exam) and allow the scores to be transferred from an exam taken in another state. As a result, a law school graduate seeking to be licensed in several states can take an exam in one state and have part of it count for at least part of the bar passage requirement in other states. For experienced lawyers who seek to obtain a license in a state after having practiced law in another state, the rules for transferring a license differ by state and within a state over time. Some states allow experienced lawyers who have practiced for some time in another state, such as the last 5 of 7 years, to be licensed without having to take the bar exam (a practice known as admission on motion).¹¹

Once lawyers are licensed, they are subject to the state's rules of conduct. To enforce the rules, each state has a lawyer disciplinary system and a department that runs it ("discipline body"). Discipline bodies do not typically start investigations of lawyer misconduct *sua sponte*, but rather the bodies investigate an incident only after receiving information about lawyer conduct that might constitute a violation of the professional conduct rules. In 2017, roughly one allegation was filed for every 10 lawyers (American Bar Association, 2017). Once an allegation is filed, there are three main steps in the process before it could result in a public sanction. Figure 1 illustrates the outcome of allegations filed against lawyers for misconduct based on statistics reported by the American Bar Association that are based on the 2017 Survey on Lawyer Discipline Systems.

The first step in the process is for the discipline body to determine if the alleged conduct violates the ethical rules. Even if an allegation is true, allegations that would not violate the rules are dismissed without further investigation. In 2017, 49% of allegations were dismissed without an investigation, and these dismissals occurred an average of 1 month after the allegation was filed. The second step in the process involves an investigation by the discipline body and a determination of whether there is enough evidence supporting the allegation for formal charges to be filed. In 2017, 38% of all allegations were dismissed after an investigation. If an allegation was dismissed after an investigation, the average time from filing to dismissal was 3 months. After these first two steps, 87% of all allegations were dismissed. The final step begins with hearings and ends with the disposition of the remaining cases. If the accused lawyer and the discipline body can stipulate to a set of facts and to the conditions for the disposition of the case, the case can settle. If the case does not settle, a determination of the case on the merits is made by a panel typically consisting of three members. Of the 13% of allegations not dismissed in 2017, 46% resulted in a finding of misconduct.

¹¹Some states require the other state to have a reciprocal policy in order for experienced lawyers to be admitted on motion (Comprehensive Guide to Bar Admission Requirements, 2020). Based on data from 2005 to 2015, Johnson and Kleiner (2020) find that 3% of lawyers moved between states each year and that a reciprocity agreement in a state increases migration of lawyers into the state.

Figure 1: Disposition of allegations of lawyer misconduct.



NOTES: The figure reports the outcomes of allegations filed against lawyers in 2017 from American Bar Association (2017). Allegations that are “dismissed without investigation” are those where the alleged conduct, even if true, does not violate the rules of conduct and are dismissed without an investigation. Allegations that are “dismissed after investigation” are those where there was an investigation by the discipline body and a determination was made that there was not enough evidence supporting the allegation for formal charges to be filed. Allegations that are “not sustained” are those where formal charges were filed but there was no evidence to support a violation of the rules of conduct. Allegations that lead to a “private sanction” or “public sanction” are those where there was evidence to support a violation of the rules of conduct. If a state follows Rule 10 of the American Bar Association Model Rules for Lawyer Disciplinary Enforcement, then sanctions are made public if they result in disbarment, suspension, probation, or reprimand.

If a lawyer is found to have violated the rules of conduct, the lawyer can be sanctioned privately or publicly.¹² Private sanctions are typically for conduct that is less serious in nature (Levin, 1998). For cases that result in a private sanction, the entire process takes on average 6 months from the filing of the allegation to disposition. If a state follows Rule 10 of the American Bar Association Model Rules for Lawyer Disciplinary Enforcement, sanctions would be made public if they result in disbarment, suspension, probation, or reprimand. Of all the sanctions in 2017, 58% were public, meaning that

¹²Under the American Bar Association Model Rules for Lawyer Disciplinary Enforcement, Rule 10, the rule for “Factors to be Considered in Imposing Sanctions” states that, “[i]n imposing a sanction after a finding of lawyer misconduct, the court or board shall consider the following factors, as enumerated in the ABA Standards for Imposing Lawyer Sanctions”: (1) “whether the lawyer has violated a duty owed to a client, to the public, to the legal system, or to the profession;” (2) “whether the lawyer acted intentionally, knowingly, or negligently;” (3) “the amount of the actual or potential injury caused by the lawyer’s misconduct;” and (4) “the existence of any aggravating or mitigating factors.”

3.5% of all allegations filed in 2017 resulted in a public sanction. For cases that result in a public sanction, the entire process takes on average 10 months from the filing of the allegation to disposition.

III. PRIMARY ANALYSIS

A. Research Design

To study the effects of the bar passage requirement, I exploit four state-level changes in the so-called diploma privilege policy, which are the only extensive margin changes in the bar passage requirement since 1970. Under a diploma privilege policy, graduates of certain law schools (normally, only certain in-state schools) need not sit for the bar exam to be licensed to practice law in that particular state. In the early 1900s, over 30 states had a diploma privilege. By 1980, only five states—Mississippi, Montana, South Dakota, West Virginia, and Wisconsin—still had a diploma privilege. During the 1980s, all except Wisconsin abolished the diploma privilege. As a result, graduates from the four flagship law schools in those states (the only schools covered by the privilege) lost their diploma privilege and were required to pass a bar exam to obtain a law license. Mississippi abolished its diploma privilege for graduates of the University of Mississippi in 1985. Montana abolished its diploma privilege for graduates of the University of Montana in 1984. South Dakota abolished its diploma privilege for graduates of the University of South Dakota in 1983. West Virginia abolished its diploma privilege for graduates of West Virginia University in 1989. Appendix B discusses the circumstances that lead to each of these changes and provides some evidence that the policy changes are exogenous to graduates of the four relevant law schools.

I define the treatment group as lawyers graduating from these four law schools who obtain a law license in the same state in the 5 years before and 5 years after the event. Let Y_{st} be the percent of lawyers with public sanctions who graduated from the treatment group in state s and who obtained a law license t years relative to the policy change.¹³ Because the variation in the bar passage requirement is at the state-cohort level, cohort-relative-to-the-event t can be thought of as event time. Define $t = -1$ as the last cohort licensed without a bar passage requirement and $t = 1$ as the first cohort

¹³This means that I use information only from a lawyer's first public sanction to identify whether they have been publicly sanctioned as of each year into their career. I do not use second or later public sanctions to generate the annual rate of public sanctions because of data limitations. In particular, I only observe the public sanctions and not the underlying conduct generating the sanction. Lawyers who are sanctioned multiple times often receive second or later sanctions for the same underlying conduct from the first sanction because of a violation of the terms related to the first sanction. These follow-on sanctions are often referred to as continuations of a prior sanction. Because I cannot identify whether a second or later sanction is a continuation of a prior sanction, I use the share of lawyers who have been publicly sanctioned as an outcome. In addition to data limitations, another reason to focus on the first disciplinary action is that state discipline bodies might be more likely to publicly sanction a lawyer with a discipline record. If prior public sanctions have an independent effect on whether state discipline bodies publicly sanction someone for misconduct, the overall rate of public sanctions would be a biased estimate of the actual rate of misconduct.

required to pass the bar exam (by construction, $t = 0$ is not a cohort). The preperiod contains cohorts $t < 0$ and the postperiod contains cohorts $t > 0$.

Importantly, lawyers who fail the bar exam can take it again,¹⁴ so the $t = 1$ cohort includes only lawyers who passed the bar exam on their first attempt, but the $t > 1$ cohorts also include lawyers who failed the bar exam at least once but passed it on another attempt. Of all lawyers licensed in the treatment states from the second to fifth year after bar exam was required, 8% were licensed after failing the bar exam at least once.¹⁵ From a policy standpoint, the relevant estimate is based on the equilibrium set of lawyers passing the bar exam, which includes lawyers who pass the bar exam on the second or later attempts. To the extent that there are differences in public sanctions between lawyers who pass the bar exam on the first attempt and lawyers who pass the bar exam after failing it at least once (Anderson & Muller, 2019), the inclusion of the $t = 1$ cohort would bias the estimates. I therefore exclude the $t = 1$ cohort from the main analysis. To balance the number of cohorts in the pre- and postperiod, the main analysis compares the four cohorts licensed without a bar passage requirement right before the diploma privilege was abolished to the first four cohorts containing repeat bar exam takers after bar passage was required.¹⁶

As a counterfactual for how public sanctions would change in the treatment group in the absence of the treatment, I use changes in public sanctions of lawyers in other states that are in the near proximity to the treatment state and that did not change their bar passage requirement over the same time period. I estimate difference-in-differences regressions in a stacked event study framework:

$$Y_{est} = \alpha + \beta \text{Treated}_{es} \times \text{Post}_{et} + \zeta \text{Treated}_{es} + \gamma \text{Post}_{et} + \psi_t + \eta_s + \phi_e + \epsilon_{est} \quad (1)$$

for event e , state s , and cohort-relative-to-the-event t . Treated_{est} is an indicator variable for the treatment group. Post_{et} is an indicator variable for cohorts licensed in the postperiod. Equation (1) includes event time fixed effects ψ_t , state fixed effects η_s , and event effects ϕ_e . In a preferred specification, I replace the state and event fixed effects with state-event fixed effects.¹⁷ The difference-in-differences estimate on the interaction term β is the coefficient of interest, indicating the percentage point change in the share of lawyers with public sanctions in the treatment group. Using information on the year of each

¹⁴As of 2010, 18 states put some restriction on the number of times a lawyer can retake the bar exam without obtaining special permission, ranging from two times in Iowa to six times in Utah (National Conference of Bar Examiners, 2020).

¹⁵I collected data on the number of admissions by first time and repeat takers from the National Conference of Bar Examiners, 2020.

¹⁶Section V.B. shows that the point estimates are consistent if the first cohort after the bar was required is included in the analysis.

¹⁷State fixed effects will pick up any fixed differences in the legal markets between states such as urbanicity and the share of lawyers working in national law firms.

public sanction, I generate separate outcome variables for each year since lawyers obtained a law license and estimate Equation (1) separately for each of these outcomes.

In an initial approach, I estimate Equation (1) using all the lawyers in the control states and cluster standard errors by state, cohort, and event. This clustering approach accounts for the correlation between the error terms, for the repeated observations in the stacked event study design, for common shocks in all the states in a given year, and for the correlation between the error terms within a given event.¹⁸ Multi-way clustering is performed using the approach in Correia (2016). Although this approach is standard in event studies (e.g., Rozema & Schanzenbach, 2021), there are two concerns about the reliability of the standard errors in this setting. First, there are concerns from the small sample properties resulting from only having four events (Angrist & Pischke, 2009). Second, the set of lawyers in each treatment group is a subset of lawyers in the state, but the set of lawyers in the control group is the entire set of lawyers in the state. And because the smaller subset of lawyers that make up the treatment group has considerably fewer lawyers than the states used to form the control group (e.g., number of lawyers licensed each year in Minnesota is 15 times higher than the number of lawyers from the University of South Dakota licensed in South Dakota), the cohort-to-cohort variation in the sanction rate in the treatment group is much larger in expectation than that of an entire control state, even if the groups are otherwise similar. Statistical inferences about changes in the sanction rate in the treatment group from before to after the bar passage requirement should therefore be drawn using similar sized cohorts as control groups.

In a second approach, I account for the fact that the treatment group is a subset of lawyers in the state by estimating Equation (1) by resampling a set of lawyers from the control states. I require each of these bootstrapped samples to have the same number of lawyers as in the treatment group in the particular event-cohort. Bootstrapped standard errors are estimated by the variance of the estimated coefficients (Fox, 2002). This approach accounts for the differences in expected variance of the sanction rate between the treatment groups and the control states that result from large differences in the number of lawyers in the groups. This approach also does not suffer from the small sample properties of clustering by event because it estimates standard errors from the variance in the coefficients from the bootstrapped samples.

In a final approach, I use the bootstrapped samples from the previous approach to run placebo tests in the spirit of Conley and Taber (2011). In difference-in-differences settings with few events, placebo tests are a common way to make inferences, which are drawn from asking whether the changes in some outcome in the treatment group from before to after some policy change could fit within the distribution of the changes in the outcome in the control groups in different periods (e.g., Baker et al., 2008; Cameron et al., 2008; Cameron et al., 2011; Cameron & Miller, 2015; Cotropia & Rozema, 2018). I tailor the placebo tests to this setting to draw inferences in a way similar to what is suggested and commonly done in prior research. To again account for the fact that the

¹⁸Figure A1 in Appendix A shows that the standard errors are similar if clustered separately by state, cohort, or event.

treatment group is a subset of lawyers in the state, for each state-cohort in the treatment group I draw a random sample of the same number of lawyers licensed in the same year from a randomly selected control state.¹⁹ For example, because the first difference in the Mississippi event compares lawyers licensed from 1981 to 1984 under the diploma privilege to lawyers licensed between 1986 and 1989 after passing the bar exam, the placebo test takes a random sample of the same number of lawyers at the cohort-event level from a single control state. More formally, let $\delta_i = \hat{Y}_i^{\text{Post}} - \hat{Y}_i^{\text{Pre}}$ be the average percentage point change in the share of lawyers with public sanctions in the control group in placebo test i . I run 10,000 placebo tests and recover a distribution of δ_i for each year since obtaining a law license. Using the simulated distribution of δ_i , I recover the counterfactual estimate and the simulated confidence intervals of the public sanction rate in the treatment group in the postperiod from Equation (2).²⁰

$$\Delta \hat{Y}_i^{\text{Bar}} = \hat{Y}^{\text{Bar}} - \delta_i \quad (2)$$

where \hat{Y}^{Bar} is the sanction rate of lawyers in the treatment group licensed after passing the bar exam. The difference-in-differences estimate is then the difference between $\Delta \hat{Y}_i^{\text{Bar}}$ and the sanction rate of lawyers in the treatment group licensed on diploma privilege.

These placebo tests have three differences from the second approach. First, the confidence intervals are formed directly from the distribution of changes in a single state and thus rely on fewer functional form assumptions.²¹ Second, this approach more fully captures any state-cohort specific differences in public sanctions, such as from peer effects, because it draws samples from a single control state rather than multiple states as in the previous regression approach. To the extent that state-cohort effects are present, drawing samples from a single state would thus be a more conservative approach because it would increase the cohort-to-cohort variation in the control group.²²

¹⁹Figure A2 in Appendix A reports placebo tests that use entire states as a control state (i.e., without bootstrapping) and reveals that the requirement of sampling the same number of lawyers meaningfully increases the confidence intervals.

²⁰I report results using the percentage point change for δ_i . Because the overall rate of sanctions can differ between states, I also used various normalized outcomes for the treatment and control groups (e.g., the percent change relative to the baseline rate in the pre-period, a normalized rate that subtracts off the mean and divides by the standard deviation), and I find consistent results.

²¹This is similar to using bootstrap percentile interval in regressions where the confidence intervals are from the percentiles of the estimated coefficients (Fox, 2002).

²²Figure A3 in Appendix A reports results where I sample from a pooled set of lawyers from all control states. Whereas drawing from single states capture any such effects, the pooled control group would not. I find similar results if I pool all the control states into one control group and taking random draws (implying that states with larger lawyer populations make up a larger share on average than smaller control states) or if I pool all the control states into one control group but reweight smaller states such that the share of lawyers from each state will be the same on average.

B. Interpretation

There are several institutional features that influence the interpretation of the estimates. First, the diploma privilege could cause a different set of lawyers to select into the state. Some aspiring lawyers may only choose to go to law school because of the diploma privilege, and other aspiring lawyers may choose which law school to attend on the basis of the diploma privilege offered in the state. It may also be that the four law schools to which the policies applied responded to the bar passage requirement by changing their admissions decisions, such as by admitting fewer applicants who are at high risk of failing the bar exam. To the extent that the bar passage requirement had a direct effect on the selection of lawyers with different misconduct propensities into the state, the estimates capture these indirect effects in addition to any direct effects.

Second, lawyers licensed with a bar passage requirement are likely to work together with and be supervised by lawyers licensed without a bar passage requirement. To the extent that lawyers licensed with a bar passage requirement are influenced in a negative way by lawyers licensed without a bar passage requirement, then the estimates could understate the longer run effects of the bar passage requirement.

Third, there were several other changes in the requirements for initial licensure for the treatment group in the event period. In particular, each of the treatment states first required applicants to pass an ethics exam in the years leading up to the bar passage requirement,²³ Mississippi changed its rules regarding whether applicants with felony convictions can obtain a law license in the year before the bar passage requirement,²⁴ and there could be unobserved changes in other types of requirements for initial licensure as well.²⁵ Given that I study only four changes in the bar passage requirement, I do not have the statistical power to disentangle the effects of the various policy changes. Consequently, the estimates only capture the effects of the package of licensing changes around the time of the bar passage requirement.

Fourth, there can be other policy changes related to the regulation of lawyers who already have a license, such as changes in the rules or procedures for lawyer discipline,

²³This Multistate Professional Responsibility Examination (MPRE) is an exam intended to “measure the knowledge and understanding of established standards related to a lawyer’s professional conduct” (National Conference of Bar Examiners, 2019). The MPRE is a 60 question multiple choice exam. Mississippi adopted the ethics exam 2 years before the bar passage requirement, Montana adopted it 3 years before, South Dakota adopted it 2 years before, and West Virginia adopted it in the same year.

²⁴Some states have or have had an explicit rule preventing convicted felons from gaining bar admission, whereas other states have no rule that applies to the situation and admission is done on a case by case basis.

²⁵For example, states can modify the background checks they conduct and make it more difficult for lawyers to be licensed on the basis of the requisite character to practice law. However, the character and fitness requirement is typically a holistic determination without many bright line rules regarding admission. To determine whether states meaningfully changed their character and fitness policy for cohorts of lawyers licensed in the event window, I reviewed descriptions of state-level policies regarding the process from annual versions of the Comprehensive Guide to Bar Admission Requirements. I did not find any meaningful changes in state-level policies related to character and fitness during the event periods. But even though most states do not release detailed statistics on the share of lawyers prevented from obtaining a law license because of a negative character and fitness determination, some evidence suggests that only a small share, likely less than 0.5%, are outright denied based on character and fitness grounds (for a discussion, see Levin et al., 2015 and Levin, 2015).

changes in continuing legal education requirements, and changes in policies that revoke law licenses of lawyers who have unpaid child support obligations. State-level changes occurring outside the event window imply that lawyers in the treatment group who are licensed under a diploma privilege will be exposed to the policy for a different period of time and at different points in their careers than lawyers licensed with a bar passage requirement. However, because public sanctions occur throughout lawyers' careers, it is not unreasonable to expect that any state-level changes occurring outside the event window will largely affect lawyers licensed with and without a bar passage requirement in a similar way.

C. Data and Descriptive Statistics

In the four treatment states, I only have individual level data for the lawyers who were publicly sanctioned. This data identifies the name of lawyers who were disciplined, the date of the disciplinary action, the year the lawyer obtained a law license, and the law school that each disciplined lawyer in the state attended. Because I do not have individual level data for lawyers who were not publicly sanctioned, I collect data on the number of lawyers who receive public sanctions X_{st} and on the number of lawyers N_{st} . Using these data, I estimate the rate of public sanctions at the state-cohort level according to $Y_{st} = X_{st}/N_{st}$. In the preperiod, I use official annual statistics of lawyer admissions under diploma privilege for N_{st} (National Conference of Bar Examiners, 2020). In the postperiod, I do not have data on N_{st} . I estimate N_{st} by using data on the number of applicants and the bar passage rate and by assuming that the bar passage rate of the treatment group is equal to the state's overall bar passage rate and that the treatment group share of total applicants in the state is equal to the share from the preperiod. Appendix C describes data construction and the estimation of the outcome for the treatment groups in detail. Section V.B. investigates the robustness of the results to these assumptions in several ways and provides evidence that the way I estimate Y_{st} is unlikely to have a large effect on the primary results.

The selection of states used in the control group is based on data availability. Some state courts and regulatory bodies maintain public websites that have individual lawyer webpages indicating the year the lawyer obtained a law license and the year they received any public sanction. I choose the control states as those where I was able to write automated scripts in 2019 to scrape this information and where I was able to verify with reasonable confidence that I obtained the full set of lawyers and public sanctions in those states. The lawyers in the control group include all lawyers licensed, regardless of whether they are active as of the time of data collection in 2019. Table 1 reports the breakdown of the three control states used for each event, and Appendix D provides state-by-state details and replication instructions for collecting these state datasets.

Figure 2 reports the number of applicants and number of lawyers obtaining a law license in the 5 years around the events in the treatment states. Because lawyers in the treatment group were licensed without a bar passage requirement in the preperiod, the number of applicants equals the number of licensed lawyers in the preperiod for the treatment group. The figure shows that the treatment group makes up around half of the applicants and licensed lawyers in the states over the event window.

Table 1: List of Control States by Event

(1)	(2)
<i>Event</i>	<i>Control states</i>
Mississippi 1985	Louisiana North Carolina Tennessee
Montana 1984	Colorado Oregon Washington
South Dakota 1983	Iowa Michigan Minnesota
West Virginia 1989	North Carolina Pennsylvania Tennessee

NOTES: The treatment group contains lawyers who graduated from the University of Mississippi, the University of Montana, the University of South Dakota, or West Virginia University and who obtained a law license in the state the law school is located. Column (1) reports the state and year the diploma privileges were abolished. Column (2) reports the breakdown of the three control states used for each treatment group. The selection of states used in the control group is based on data availability. See Section III.C for details.

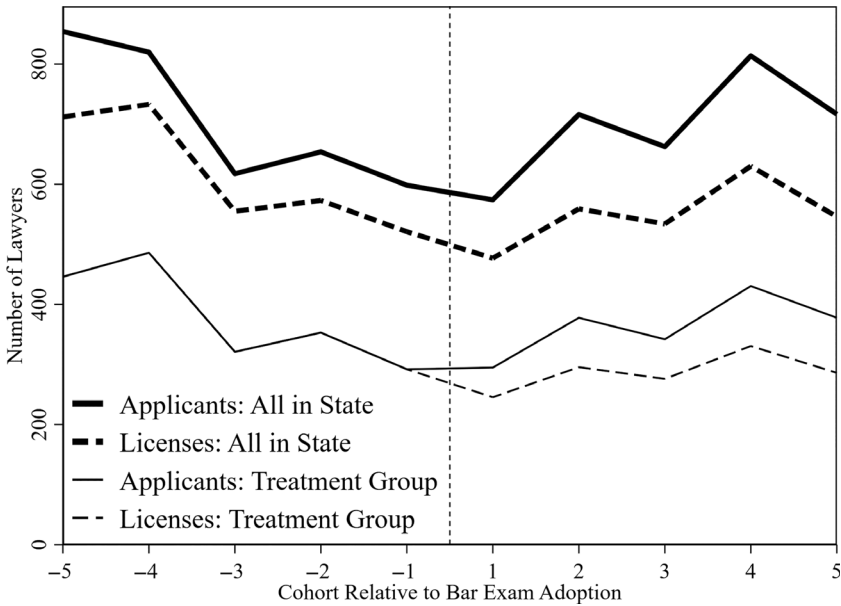
Table 2 reports descriptive statistics. The last change in the bar passage requirement occurred in 1989, so the last cohort in the event study obtained a law license in 1994 and the number of years of experience of the 1994 cohort in the year 2020 is 26. Therefore, the maximum number of years of experience I observe for the treatment group is 26. Table 2 provides evidence that lawyers in the treatment and the control groups have similar rates of public sanctions.

D. Results

Figure 3 reports the regression results. Panel (a) reports the estimates using entire states as control groups, and panel (b) reports the estimates using the bootstrapping approach. The panels report the estimates for separate regressions for each year after obtaining a law license. In both figures, the difference-in-difference estimates have been added to the baseline public sanction rate of lawyers licensed without a bar passage requirement. Figure 4 reports the results of the placebo tests.²⁶

²⁶Figure A4 in Appendix A reports the distribution of simulated first-differences in the control group δ_i along with the first-difference in the treatment group.

Figure 2: Applicants and licenses around the adoption of the bar passage requirement.



NOTES: The figure reports the estimated number of applicants and lawyers obtaining a law license in the treatment states in the 5 years around the four changes in the diploma privilege. The treatment states include Mississippi, Montana, South Dakota, and West Virginia. The treatment group contains lawyers who graduated from the University of Mississippi, the University of Montana, the University of South Dakota, or West Virginia University and who obtained a law license in the state the law school is located. Applicants and lawyers not in the treatment group graduated from law schools other than in the treatment group.

Overall, the magnitudes of the estimated effects are consistent in Figures 3 and 4, but the confidence intervals are meaningfully smaller in panel (b) of Figure 3. In panel (a) of Figure 3 and in Figure 4, the majority of the estimates for the outcomes of at least 20 years of experience are only marginally significant at the 10% level. I discuss the results together and by referencing the confidence intervals from the most conservative set of estimates (from the placebo tests in Figure 4). In the spirit of discussions in Rainey (2014) and Romer (2020), I discuss the range of estimates by referencing the 90% confidence intervals.

There are several findings from these results. First, regardless of whether lawyers were licensed without a bar passage requirement or after passing the bar exam, few lawyers receive public sanctions in the first 10 years of their career. Until year 8, less than 1% of lawyers receive a public sanction in both groups. Second, there is evidence that both groups receive public sanctions at similar rates during the first decade after obtaining a law license. The confidence intervals suggest there are not big differences between the groups. For example, 5 years after obtaining a law license, between 0.1% and 0.7% of lawyers licensed after passing the bar exam had received public sanctions,

Table 2: Descriptive Statistics

	<i>Lawyers with public sanctions after years of experience (%)</i>			<i>Bar passage rate (%)</i>	<i>Number of lawyers</i>	
	<i>Year 5</i>	<i>Year 15</i>	<i>Year 25</i>		<i>Per cohort</i>	<i>Total</i>
Treatment group						
Preperiod	0.4	2.8	5.1		91	1452
Postperiod	0.5	2.6	4.4	78	67	1078
Control group						
Preperiod	0.4	1.9	3.5	73	287	13,786
Postperiod	0.4	2.2	4.0	73	295	14,182

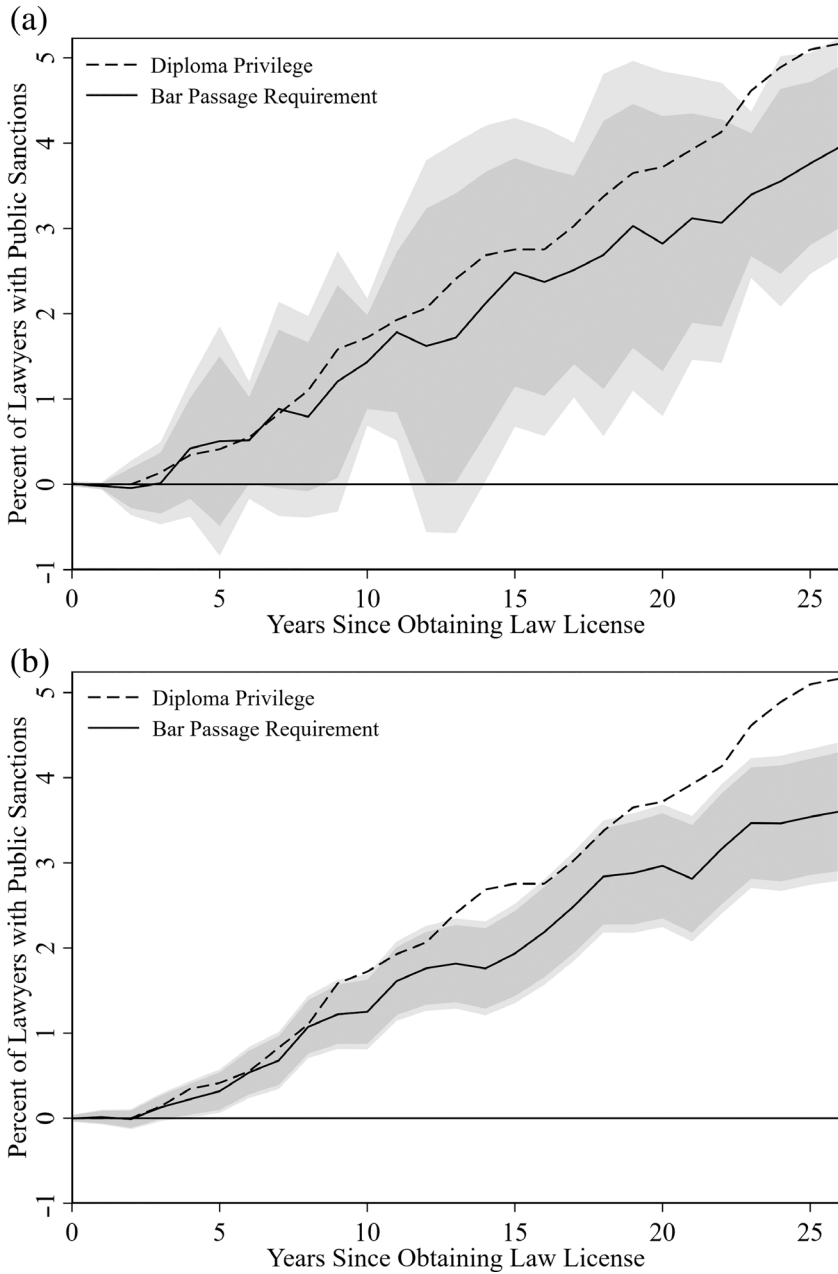
NOTES: The treatment group contains lawyers who graduated from the University of Mississippi, the University of Montana, the University of South Dakota, or West Virginia University and who obtained a law license in the state the law school is located. The control group contains lawyers from the control states for each event shown in Table 1. The preperiod contains the four cohorts of lawyers licensed in the years immediately before the diploma privilege was abolished. The postperiod contains the four cohorts of lawyers licensed in the years after the diploma privilege was abolished after the initial cohort (the initial cohort is excluded because it does not contain lawyers who passed the bar exam after failing it—see Section III.A. for details).

compared to 0.4% of lawyers licensed without a bar passage requirement; 10 years after obtaining a law license, between 0.8% and 2.0% of lawyers licensed after passing the bar exam had received public sanctions, compared to 1.7% of lawyers licensed without a bar passage requirement. Third, differences begin to emerge in the second decade after obtaining a law license. Those differences become statistically different at the 10% level after 23 years. By 25 years after obtaining a law license, the estimates suggest that 5.1% of lawyers licensed without a bar passage requirement were publicly sanctioned, compared with 3.9% of lawyers licensed after passing the bar exam. These estimates are only marginally significant at the 10% level. The 90% confidence interval suggests that between 2.9% and 4.9% of lawyers licensed after passing the bar exam were publicly sanctioned within 25 years.

The emergence of effects only long after initial licensing is perhaps unexpected, but the patterns are consistent with research documenting the relationship between initial failure of a bar exam and public discipline (e.g., Anderson & Muller, 2019). Moreover, to the extent that unskillfulness contributes to public sanctions, the fact that many young lawyers are supervised by other lawyers to some extent early in their careers could be one possible explanation for the pattern. Finally, given that the bar exam measures aptitude and skill rather than a propensity for misconduct, another possible explanation of the pattern is if misconduct is partly driven by a lack of skill or financial success. To the extent that the effects of aptitude and skill on financial success accumulate slowly over time, financial hardship might be more expected to emerge later in lawyers' careers.²⁷

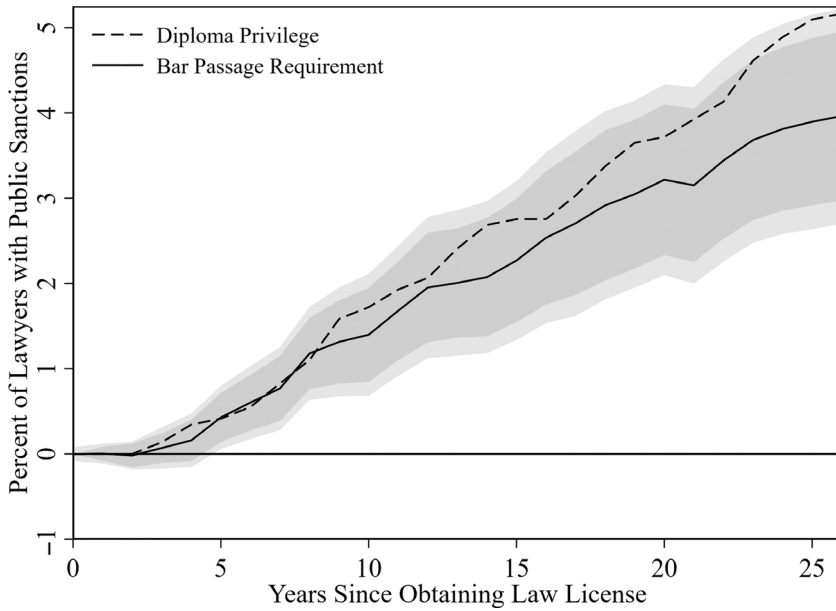
²⁷This is a time in one's life where the risk of bankruptcy is the highest (Fisher, 2019). Evidence suggests that financial troubles interfere with one's ability to manage a schedule and their budget (Mullainathan & Shafir, 2013), and these issues are related to some of the underlying conduct for which lawyers are disciplined (e.g., a pattern of failing to appear at hearings, Kinsler, 2017).

Figure 3: Percent of lawyers with public sanctions by admission type: Regression results. (a): States as control groups. (b): Bootstrap samples from states as control groups.



Continued on the next page.

Figure 4: Percent of lawyers with public sanctions by admission type: Placebo tests.



NOTES: The figure reports the estimated percent of lawyers who receive public sanctions on the y-axis over the years after lawyers obtain a law license, separately by whether they were licensed on diploma privilege or after passing the bar exam. Lawyers licensed on diploma privilege are those who graduated from the University of Mississippi, the University of Montana, the University of South Dakota, or West Virginia University and who obtained a law license in the state the law school is located in the years before the state abolished the diploma privilege. Lawyers licensed after passing the bar exam are those who graduated from these same law schools and who obtained a law license in the state the law school is located in the years after the state abolished the diploma privilege. The dashed line labeled “Diploma Privilege” is the public sanction rate of lawyers licensed on diploma privilege in the treatment groups. The solid line labeled “Bar Passage Requirement” is the estimated public sanction rate of lawyers in the treatment group who were licensed after passing the bar exam. This line and confidence intervals is estimated from placebo tests from Equation (2) where for each state-cohort in the treatment group I draw a random sample of lawyers licensed in the same year from a randomly selected control state. The placebo tests are used to recover the counterfactual estimate and the simulated confidence intervals of the public sanction rate in the treatment group in the postperiod. For comparison, this estimate is added to the public sanction rate of lawyers licensed on diploma privilege in the treatment groups. The gray areas report the 90% and 95% confidence intervals.

NOTES: The figure reports regression results from estimating Equation (1). In each panel, the estimated percentage point change in public sanction rate is added to the rate of public sanctions against lawyers licensed on diploma privilege. The dashed line labeled “Diploma Privilege” is the public sanction rate of lawyers licensed on diploma privilege in the treatment groups. The solid line labeled “Bar Passage Requirement” is the estimated public sanction rate of lawyers in the treatment group who were licensed after passing the bar exam. The gray areas report the 90% and 95% confidence intervals of the estimates. In panel (a), the solid line and confidence intervals are estimated from regressions in Equation (1) in a stacked event study framework where entire states are used as a control group. Standard errors are clustered by state, cohort, and event using Correia (2016). In panel (b), the solid line and confidence intervals are estimated from regressions in a stacked event study framework by estimating Equation (1) by resampling a set of lawyers from the control states where each of these bootstrapped samples have the same number of lawyers as in the treatment group in the particular event-cohort. Bootstrapped standard errors are estimated by the variance of the estimated coefficients (Fox, 2002).

IV. MECHANISMS

In theory, the bar passage requirement could protect the public through three main mechanisms. One way a bar passage requirement could protect the public is by preventing some law school graduates who are more prone to misconduct from practicing law (Leland, 1979). Although many lawyers who fail the bar exam on their first attempt take the bar exam again and pass on a subsequent attempt, some never pass the bar exam and are therefore not permitted to obtain a law license.

Another way a bar passage requirement could protect the public is by changing the set of people who take the bar exam (Ballou & Podgursky, ; Ramseyer & Rasmusen, 2015). There are three main steps required to be able to take the bar exam, and a bar passage requirement can alter the set of lawyers clearing each step. First, aspiring lawyers must apply to law school. Most students go to law school to practice law, so the value of a legal education largely depends on obtaining and maintaining a law license. If aspiring lawyers must pass a bar exam to practice law, individuals who would have more difficulty passing the bar exam might be discouraged from pursuing a legal education in the first place. Second, law school applicants must be admitted to law school. In part because of American Bar Association requirements on law schools' bar passage rates, current law school admissions decisions partly depend on the ability of graduates to pass the bar exam.²⁸ With a diploma privilege, law schools might admit more students who are at a higher risk of failing the bar exam. Third, admitted law students must complete their law degree. With a diploma privilege, law school faculty might feel a "special obligation to ensure that the students who graduate are competent to practice law" because "[they] are the last stop."²⁹ With a bar passage requirement, faculty might be less likely to fail law students in their classes because they can rely on the bar exam to serve as a final screening mechanism.³⁰ On the other hand, it is also possible that faculty are more likely to fail law students if there is a bar passage requirement, in part to help ensure that the law school meets the American Bar Association's requirements on law schools' bar passage rates.

A final way a bar passage requirement could protect the public is by affecting the behavior of practicing lawyers through several mechanisms. First, it could influence the behavior of aspiring lawyers who eventually practice law through a studying or learning

²⁸Bar passage was not explicitly stated as a consideration in American Bar Association accreditation of law schools until 1997 (American Bar Association, 1997, Standard 301, Interpretation 301-1: "Among the factors to be considered in assessing the extent to which a law school complies with the Standard are the attrition rates of the school's students, and the bar passage and career placement rates of its graduates.") A new interpretation of bar passage considerations was adopted in 2008, which provided target bar passage rates for the first time (although these targets were arguably low, see American Bar Association, 2008, Standard 301, Interpretation 301-6). However, changes in the standard in 2019 meaningfully increased these targets.

²⁹Ward (2020), quoting Gordon Smith who is currently the dean of J. Reuben Clark Law School at Brigham Young University.

³⁰To the extent that any heightened faculty screen due to the absence of a bar passage requirement is better at screening lawyers who would be publicly sanctioned than the bar exam itself, a bar passage requirement could actually harm the public.

effect. By requiring lawyers to demonstrate their knowledge of the law through a bar passage requirement, aspiring lawyers have more incentives to study hard in law school and continue learning the law in preparation for the bar exam. Second, a bar passage requirement can influence the behavior of practicing lawyers through a wage effect. Requiring lawyers to pass a bar exam decreases total entry of lawyers into the profession, which decreases competition and thus can increase wages. This in turn could decrease negative outcomes because higher wages for good behavior decrease the relative value of bad behavior (Shapiro, 1986), or it could increase negative outcomes by lowering competition for legal services (Peltzman, 1976; Stigler, 1971).³¹

The results cannot distinguish between these mechanisms. Although not distinguishing between competing mechanisms is a limitation of this research, it is worth noting that most empirical research estimating the effects of changes in licensing regimes suffer from the same limitation (e.g., Larsen et al., 2020). Although the results cannot distinguish between these mechanisms directly, I build a simple conceptual framework to estimate bounds on the first mechanism (screening out lawyers). The framework builds these bounds by assuming that the bar passage requirement only affects public sanctions by screening out lawyers. However, note that the bounds for all other combined mechanisms can be estimated directly from the above results. In particular, under the assumption that none of the effect of the bar passage requirement on public sanctions is driven by a higher rate of public sanctions against lawyers who would never pass the bar exam, then by construction there is an equal public sanction rate for lawyers licensed without a bar passage requirement but who would and would not have ever passed the bar exam. Under this alternative assumption that the bar exam does not screen out lawyers on the basis of a propensity for public sanctions, the estimate that the bar passage requirement decreased the share of lawyers who received a public sanction within 25 years after admission can be interpreted directly as the decrease in the sanction rate of all lawyers. In particular, the estimates under this assumption would suggest that the bar exam changed the behavior of roughly 12 out of every 1000 lawyers who actually pass the bar exam in a way that prevents them from being publicly sanctioned within 25 years.

A. Conceptual Framework

Assume that the bar passage requirement only affects public sanctions by screening out lawyers. Let y_i be a random variable that takes the value of 0 if lawyer i would not be publicly sanctioned and the value of 1 if the lawyer would be publicly sanctioned. Suppose there are two time periods. In the preperiod, a state does not have a bar passage requirement but instead has a diploma privilege policy. In the postperiod, the state abolishes the diploma privilege policy and imposes a bar passage requirement.

³¹ Consumers might respond to higher prices by opting for lower quality services, which can increase negative outcomes (Koumenta et al., 2014).

The population sanction rate is the expected value of y_i . I estimate the population sanction rate by the sanction rate of lawyers who obtain a law license under the diploma privilege in the preperiod:

$$E(y_i) = \hat{Y}^{\text{Diploma}}$$

Define bar exam failure as a lawyer taking the bar exam at least once but never passing the bar exam on their first or any subsequent attempt. Define the eventual bar passage rate p as the percent of lawyers who take the bar exam at least once and pass on their first or any subsequent attempt. I estimate the sanction rate of lawyers who would pass the bar exam $E(y_i | \text{Pass})$ by the sanction rate of lawyers who obtain a law license by passing the bar exam in the postperiod according to Equation (3):

$$E(y_i | \text{Pass}) = \hat{Y}^{\text{Bar}} \quad (3)$$

The sanction rate of lawyers who would fail the bar exam $E(y_i | \text{Fail})$ is never observable. This is because there is either (1) a diploma privilege where lawyers who would fail the bar exam practice law but are indistinguishable from lawyers who would pass the bar, or (2) a bar passage requirement where lawyers who fail the bar exam do not practice law. However, by assuming that the sanction rate for lawyers who would pass the bar exam and the sanction rate for lawyers who would fail the bar exam is the same in both periods, I estimate of the sanction rate of lawyers who would fail the bar exam $E(y_i | \text{Fail})$ by applying the eventual bar passage rate p to an equation for the population sanction rate where the equation is formulated as the weighted average of lawyers who would pass the bar exam and lawyers who would fail the bar exam: $E(y_i) = pE(y_i | \text{Pass}) + (1 - p) E(y_i | \text{Fail})$. Plugging in $E(y_i)$ and $E(y_i | \text{Pass})$ and solving for $E(y_i | \text{Fail})$:

$$E(y_i | \text{Fail}) = (\hat{Y}^{\text{Diploma}} - p\hat{Y}^{\text{Bar}}) / (1 - p) \quad (4)$$

The bar exam makes errors if it is not perfectly accurate in screening out lawyers who would be publicly sanctioned. There are two types of bar exam errors. First, the bar exam can mistakenly pass a lawyer who would be publicly sanctioned. Let e^+ be the false positive error rate, that is, the rate at which lawyers who would be publicly sanctioned pass the bar exam. I estimate the false positive rate by applying the percent of lawyers who are publicly sanctioned if a bar exam is administered: $\hat{Y}^{\text{Bar}} = e^+(1 - \theta)$, where θ is the percent of lawyers who are not publicly sanctioned if all lawyers obtain a law license. I estimate θ from lawyers licensed under the diploma privilege: $\theta = 1 - \hat{Y}^{\text{Diploma}}$. Plugging in θ and solving for e^+ :

$$\hat{e}^+ = \hat{Y}^{\text{Bar}} / \hat{Y}^{\text{Diploma}} \quad (5)$$

Second, the bar exam can mistakenly fail a lawyer who would not be publicly sanctioned. Let e^- be the false-negative error rate, that is, the rate at which lawyers who would not be publicly sanctioned fail the bar exam. I estimate the false negative error rate using

the formula for the bar failure rate: $1 - p = \theta(1 - e^-) + (1 - \theta)e^+$. In this equation, the first component— $\theta(1 - e^-)$ —is the percent of lawyers who are not publicly sanctioned and who pass the bar exam, and the second component— $(1 - \theta)e^+$ —is the percent of lawyers who are sanctioned and who pass the bar exam. Plugging in θ and \hat{e}^+ and solving for e^- :

$$\hat{e}^- = (p + \hat{Y}^{\text{Bar}} - \hat{Y}^{\text{Diploma}}) / (1 - \hat{Y}^{\text{Diploma}}) \quad (6)$$

Applying this framework requires estimates on \hat{Y}^{Diploma} , \hat{Y}^{Bar} , and p . Section III provides estimates for \hat{Y}^{Diploma} and \hat{Y}^{Bar} . Although official statistics on bar passage rates are reported separately by first time and repeat test takers at the state-year level, I am unaware of official estimates of the eventual bar passage rate at the state-year or even state level.

To estimate the eventual bar passage rate in the treatment states in the postperiod, I divide the number of first-time test takers in the 5 years after the diploma privilege was abolished by an estimate of how many of them passed. As an estimate of how many passed, I count up the number of first-time test takers that passed in the 5 years after the diploma privilege was abolished and the number of repeat test takers that passed from the year after the privilege was abolished to the sixth year after the diploma privilege was abolished.³² Using this approach, I estimate the eventual bar passage rate to be 91% in the treatment states in the postperiod (this includes the 83% of test takers passing on a first attempt and an additional 8% who pass on a subsequent attempt).³³ To put this in

³²Using the lagged number of repeat passers is informed by Wightman (1998), which provides evidence related to when repeat test takers who pass the bar exam first took the bar exam. In particular, of all lawyers who fail the bar exam at least once but pass on a later attempt, they find that 74% pass on their second attempt, 21% pass on their third attempt, 4% pass on their fourth attempt, and the remaining 1% pass on later attempts. One limitation of this approach is that it does not include the lawyers in the treatment group who first took the bar exam in periods 1 to 5 but who passed after period 6. If the estimates from Wightman (1998) are applicable to the treatment group, this means that I am missing roughly 26% of period 5 applicants who failed the bar exam but eventually passed, 5% of period 4 applicants who failed the bar exam but eventually passed, 1% of period 3 applicants who failed the bar exam but eventually passed, and less than 1% of applicants before period 3 who failed the bar exam but eventually passed. As a result, this approach could understate the true eventual bar passage rate. A second limitation of the approach is that it includes those first-time test takers from before the diploma privilege was abolished but who passed the bar exam in periods 1 to 6.

As a result, this approach could overstate the true eventual bar passage rate. Given both limitations, the fact that they bias the estimates in opposite directions suggests that the approach is unlikely to meaningfully understate or overstate the eventual bar passage rate. The reason I choose to not include repeat passers after period 6 is that, to the extent that the estimates from Wightman (1998) are applicable to the treatment group, the set of repeat passers after period 6 contains a higher share of repeat passers from applicants who passed on a second attempt outside the event window than repeat passers from inside the event window.

³³The eventual bar passage rate does not include data from West Virginia because the reporting of repeat test takers appears to differ from other states. In particular, the number of first-time passers and repeat passers is greater than the number of first-time test takers over different time periods, suggesting that not all the repeat takers were included in the number of first-time takers at some point in the past.

perspective, Wightman (1998) uses data on most students who started law school in the United States in 1991 and estimates the eventual bar passage rate to be 95%.

B. Results

Using the estimates from the placebo tests, Figure 5 reports the implied sanction rates by whether lawyers would pass the bar exam from Equations (3) and (4). Assuming the entire effect of the bar passage requirement on public sanctions is driven by a higher rate of public sanctions against lawyers who would never pass the bar exam, the estimates suggest that 17.7% of lawyers licensed without a bar passage requirement but who would have never passed the bar exam were publicly sanctioned within 25 years, compared with 4.4% of lawyers licensed without a bar passage requirement but who would have passed the bar exam.

Next, I estimate the bar exam error rates according to Equations (5) and (6).³⁴ Using the error rate estimates from the 25th year after lawyers obtain a law license along with the eventual bar passage rate of 91%, Figure 6 reports evidence on the trade-offs involved in the choice over a bar passage requirement. Of all the lawyers licensed without a bar passage requirement who have not received a public sanction, the estimates suggest that 8% would have failed the bar exam.³⁵ Of all the lawyers licensed without a bar passage requirement who have received a public sanction, the estimates suggest that 71% would have passed the bar exam.³⁶ Both of these estimates assume that the entire effect of the bar passage requirement on public sanctions is driven by a higher rate of public sanctions against lawyers who would never pass the bar exam.

V. ROBUSTNESS CHECKS

A. Number of Cohorts around the Events

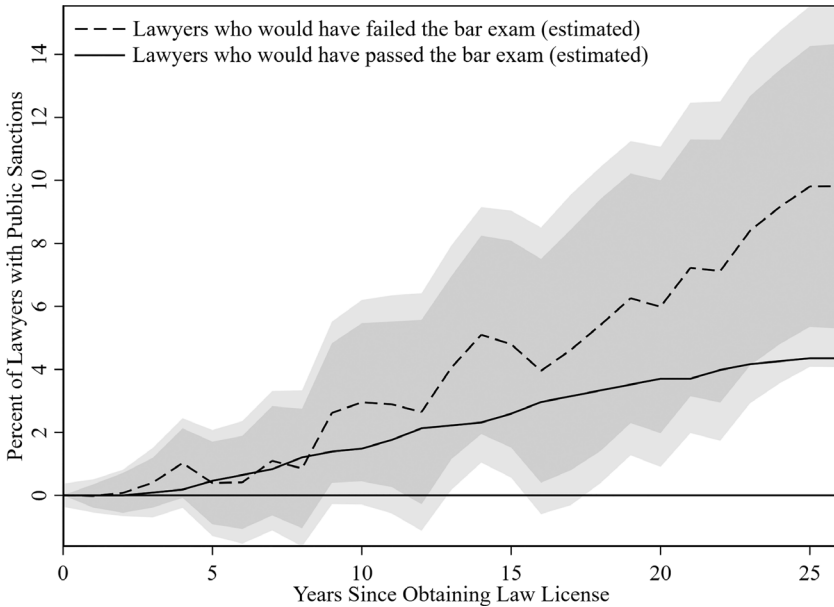
The main analysis compared the four cohorts of lawyers licensed without a bar passage requirement right before the diploma privilege was abolished to the first four cohorts containing repeat bar exam takers after bar passage was required. The choice of how

³⁴Figure A5 in Appendix A reports the error rates estimated over lawyers' careers. The standard errors of the error rates are extremely large in the first 10 years after obtaining a law license, so the first 10 years are excluded from the figure.

³⁵To calculate this, divide the percent of lawyers who have not received a public sanction and who would have failed the bar exam (7.4%, as represented in solid red) by the percent of lawyers who have not received a public sanction (94.4%, which is the 7.4% in solid red plus the 87.0% as represented in solid gray).

³⁶To calculate this, divide the percent of lawyers who have received a public sanction but who would have passed the bar exam (4.0%, as represented in the gray pattern) by the percent of lawyers who received a public sanction (5.6%, which is the 4.0% in the gray pattern plus the 1.6% as represented in the red pattern).

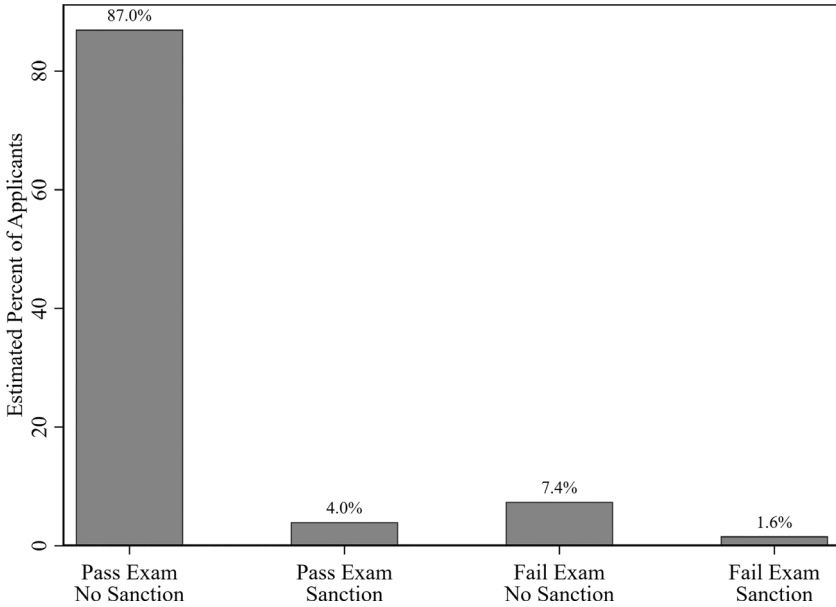
Figure 5: Percent of lawyers licensed on diploma privilege with public sanctions by estimated bar exam passage.



NOTES: The figure reports estimates under the assumption that the entire effect of the bar passage requirement on public sanctions is driven by a different rate of public sanctions for lawyers licensed on diploma privilege but who would never have passed the bar exam. The estimates are recovered using the estimates from Figure 4 and the eventual bar passage rate. The solid line is the implied percent of lawyers licensed on diploma privilege but who would have passed the bar exam receiving sanctions estimated from Equation (3). The dashed line is the implied percent of lawyers who would fail the bar exam receiving public sanctions estimated from Equation (4).

many cohorts to include around the policy change is admittedly arbitrary, and there are ambiguous predictions about how narrowing the number of cohorts around the event would affect the magnitude and precision of the estimates. On the one hand, the treatment group (law school graduates from the University of Mississippi, the University of Montana, the University of South Dakota, and West Virginia University) only contains roughly 330 lawyers in each cohort. Because I estimate bootstrapped confidence intervals that match the number of lawyers in the control group with the number of lawyers in the treatment group, one might expect the confidence intervals to decrease as the number of cohorts included in the analysis increases. On the other hand, law school graduates further away from the event might be more likely to differ in ways that are unrelated to the bar passage requirement. For example, while cohorts licensed a few years apart are largely subject to the same disciplinary system over their career, cohorts licensed 10 years apart are potentially subject to different disciplinary systems over much their career. As a

Figure 6: Estimated accuracy of the bar exam.



NOTES: Bar exam failure is defined as a lawyer taking the bar exam at least once but never passing the bar exam on their first or any subsequent attempt. Sanction is defined as a lawyer who receives a public sanction within 25 years of obtaining a license. The figure reports the implied estimates under the assumption that the entire effect of the bar passage requirement on public sanctions is driven by a different rate of public sanctions for lawyers licensed on diploma privilege but who would never had passed the bar exam.

result, one might expect the confidence intervals to increase as more cohorts are included in the analysis.

I repeat the analysis for different numbers of cohorts around the event. Figure A6 in Appendix A reports the results. As the number of cohorts around the event is narrowed, the estimated differences by admission type typically increase. In panel (d), the results are also consistent if I include the first cohort of lawyers after the bar passage requirement (i.e., the cohort that includes only first-time passers).

B. Estimate on the Outcome

The main outcome is the share of lawyers with a public sanction: $Y_{st} = X_{st}/N_{st}$ where X_{st} is the number of lawyers in the treatment group who receive public sanctions and N_{st} is the number of lawyers in the treatment group. The outcome is estimated by obtaining information on both the numerator and denominator. However, both are estimates and

involve some uncertainty. In this section, I assess how bias and measurement error in the estimate of Y_{st} would change the results.

V.B.1 | Bias

I observe N_{st} in the preperiod from official statistics. I do not observe N_{st} in the postperiod because graduates of the four law schools in the treatment group are not distinguishable from graduates of other law schools once the treatment group was required to pass the bar exam. To formalize the concerns, let $N_{st} = \hat{p}_{st} \hat{A}_{st}$ be the number of lawyers in the treatment group, where \hat{p}_{st} is the estimate for the bar passage rate of the treatment group and \hat{A}_{st} is the estimate for the number of applicants that will become the treatment group if they pass the bar exam. Then, the main outcome can be rewritten as:

$$Y_{st} = X_{st} / (\hat{p}_{st} \hat{A}_{st}) \quad (7)$$

I made two assumptions to estimate Y_{st} in the postperiod. First, because I do not have bar passage rates by law school over this time period, I assumed that the bar passage rate for the treatment group is equal to the state's overall bar passage rate in the postperiod. This estimate is likely to understate the true bar passage rate because the treatment groups are graduates from the state flagship universities that tend to have higher bar passage rates than the state average,³⁷ which would bias the estimates for the difference in public sanctions by admission type toward zero.

Second, I assume that the treatment group share of total applicants in the state in the postperiod is equal to the share from the preperiod. I made this assumption because I only have law school identifiers for the lawyers who were publicly sanctioned. As shown above in Figure 2, the total number of applicants in the treatment states over the event window is fairly stable, which provides at least some suggestive evidence that this assumption is reasonable.

Bias from these two assumptions therefore likely cut in opposite directions for the difference-in-differences estimate. To assess the likely effect these assumptions have on the estimates, I run simulations where I vary (1) the bar passage rate from the state bar passage rate to up to 10 percentage points higher, and (2) the number of applicants in the treatment group from the estimated number to 10% smaller.³⁸ In particular, using the confidence intervals from the placebo test, I generate a resulting distribution of

³⁷For example, in the 2020 US News and World Report, which reports bar passage rates of first-time takers by law school, University of Mississippi graduates had a 14 percentage point higher bar passage rate in Mississippi than the state average (76% compared to 62%), University of Montana graduates had an 11 percentage point higher bar passage rate in Montana than the state average (83% compared to 72%), University of South Dakota graduates had a 2 percentage point higher bar passage rate in South Dakota than the state average (80% compared to 78%), and West Virginia University had the same bar passage rate as the state average (69%).

³⁸Figure A7 in Appendix A reports the outcome variable (percent of lawyers with public sanctions) for different simulated values of \hat{A}_{st} and \hat{p}_{st} .

estimates for \hat{Y}_{st} for each combination of \hat{A}_{st} and \hat{p}_{st} . Note that this simulation not only changes the standard errors but also changes the average estimates because I am simulating actual differences in the estimates for \hat{Y}_{st} . Figure A8 in Appendix A reports the results and provides no evidence that the estimate for the number of lawyers in the treatment group in the postperiod meaningfully affects the results.

V.B.2 | Measurement Error

Next, I assess how noise in the estimates of N_{st} would affect the results. To introduce measurement error in the number of lawyers in the treatment group in the postperiod, let $\hat{N}_{st} = N_{st} \times \xi_{st}$ be the estimate of the number of lawyers in the treatment group in the postperiod, where N_{st} is the (unobserved) true number of lawyers and ξ_{st} is a random component reflecting the uncertainty of the estimate. A value of $\xi_{st} = 0.9$ would imply that I under-estimate the true number of lawyers in the treatment group in the postperiod by 10%. A value of $\xi_{st} = 1.1$ would imply that I overestimate the true number of lawyers in the treatment group in the postperiod by 10%. I simulate values for ξ_{st} between 0.9 and 1.1 from a uniform distribution. Figure A9 in Appendix A reports the results and provides no evidence that measurement error in N_{st} in the postperiod meaningfully affects the results.

In addition to measurement error in N_{st} , there is also measurement error in the number of lawyers in the treatment group who receive sanctions X_{st} . An accurate count of X_{st} requires information on the law schools of lawyers who obtained a law license in the event window and who were publicly sanctioned at some point in their career. For the lawyers who were publicly sanctioned, I obtained law school information for all lawyers in the Mississippi and Montana events, but I was unable to obtain law school information for 12% of publicly sanctioned lawyers in the South Dakota event and 27% of lawyers in the West Virginia event. For missing law school identifiers to bias the results, it would have to be the case that I systematically fail to identify lawyers more before or after the event. However, the missing information is roughly split from lawyers in the preperiod and the postperiod. In particular, of all the publicly sanctioned lawyers with missing law school identifiers, 43% were licensed in the preperiod. For lawyers with missing law school information, I included them in the sample for the main analysis, but I find consistent results if I exclude them. As a final robustness check, I ran simulations where I dropped different size shares of these lawyers between 0% and 100% (and randomly dropping lawyers) and also find consistent results.

VI. CONCLUSION

This article studied the effects of requiring lawyers to pass a bar exam to obtain a law license. To do so, it exploited four events in the 1980s where states abolished their diploma privilege for graduates of a single law school within the state. The difference-in-differences research design assessed changes in public sanctions against graduates of these four law

schools who obtained a law license in the years before and after the diploma privilege was abolished. Control groups were formed using lawyers in other states from the same geographic region that did not change their bar passage requirement over the same time period. Using this research design, I found evidence that lawyers licensed without a bar passage requirement received public sanctions at similar rates to lawyers licensed after passing the bar exam until roughly the second decade after receiving a law license. Within 10 years after obtaining a law license, between 0.8% and 2.0% of lawyers licensed after passing the bar exam had received public sanctions, compared to 1.7% of lawyers licensed without a bar passage requirement. Within 25 years after obtaining a law license, the bar passage requirement decreased the share of licensed lawyers who received a sanction from 5.1% to 3.9%. The annualized rate of this cumulative share is 0.9%, and the confidence intervals suggest that the effect is not greater than an annualized rate of 1.6%.

There are several limitations of the study. First, I do not have individual level data for lawyers who graduated from the four relevant law schools and who were not publicly sanctioned, so I estimate the number of lawyers in the treatment groups after the bar passage requirement was imposed using available information from the number of applicants and bar passage rates. Although I present robustness checks suggesting that these estimates do not materially affect the results, better estimates could be obtained if state bars or state discipline bodies release data on licensed lawyers. Second, I use lawyers in other states from the same geographic region as a control group, but the legal markets for graduates of the four law schools in the treatment group are likely to be structurally different than the legal market for lawyers practicing in large cities in the control states. Although the research design accounts for the difference in number of lawyers in the treatment and control groups, a better control group could be formed with data on the law schools that individual lawyers attended and the legal markets that they work in. Third, the states that abolished the diploma privilege that I study also changed one or more other licensing requirements around the same time, including requiring passage of an additional exam on ethics and changing rules whereby applicants with a felony conviction are categorically barred from admission. As a result, the estimates only capture the effects of the package of licensing changes around the time the diploma privilege was abolished. Finally, the estimates are based on changes in the diploma privilege in four small states in the 1980s. The policy changes studied here thus occurred in states with legal markets that might not be comparable to other legal markets, and the effects of a bar passage requirement in the states studied here could be different than the effects in states that have always required lawyers to pass the bar exam.

Future research should explore the appropriate role of the bar passage requirement and other quality control systems in preventing negative outcomes. There are many different systems that could potentially serve as a quality control measure through their gatekeeping role of the legal profession, including the American Bar Association's certification of law schools, law schools' admissions and grading decisions, and state licensing bodies' character and fitness reviews. The legal malpractice system and the regulation of malpractice insurance could also serve as quality control measures. Future research should explore the extent to which these other quality control systems overlap with the bar passage requirement. For example, how much overlap is there between the set of

lawyers who fail the bar exam at least once, the set of lawyers that state licensing bodies identify as having questionable moral character and fitness, and the set of lawyers who are eventually subjected to discipline? And how much overlap is there between the conduct that is subject to legal malpractice and the conduct that lawyers are disciplined for by state discipline bodies?³⁹ Future research should also explore the extent to which the bar passage requirement interacts with other quality control systems. For example, given that the bar exam measures aptitude and skill, which are the same characteristics that drive law school admissions decisions, do law school admissions and grading decisions respond to changes in the bar passage requirement? And do malpractice insurance costs change after a change in the bar passage requirement? Understanding the extent that the bar passage requirement influences the decisions of the American Bar Association and law schools would help inform a longstanding debate about the roles that governing bodies, law schools, and courts do and should play in regulating the legal profession.

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³⁹For recent studies of the extent of overlap between medical malpractice and medical disciplinary systems, see Hyman et al. (2021) and Liu and Hyman (2019).

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APPENDIX A: ADDITIONAL RESULTS

Table A1: Random Sample of 30 Disciplinary Actions in New Jersey

Discipline	Description
Suspended	Committed misconduct in eleven matters, including gross neglect, failure to cooperate with disciplinary authorities, failure to return client files, misrepresentation, conduct prejudicial to the administration of justice and conduct intended to disrupt a tribunal.
Disbarred	Was criminally convicted for conspiring with an employee of the New Jersey Division of Motor Vehicles to arrange for the alteration of official records relating to respondent's motor vehicle violations history, preparing and assisting in the preparation of wills in which the respondent was given a residuary interest in the estate in violation of ethics rules, continuing to represent a client after the effective date of a prior suspension from practice, failing to inform the client of his suspension, falsely certifying to the Office of Attorney Ethics that he had no matters pending after his suspension, and for attempting to persuade a grievant to withdraw a grievance against him.
Suspended	Failed to answer or otherwise appear before the District Ethics Committee, Disciplinary Review Board and Supreme Court to respond to findings of gross neglect and failure to communicate with clients.
Admonished	Engaged in a conflict of interest by representing one client in the incorporation of a business and in the renewal of a liquor license and who then filed a lawsuit against that client on behalf of the original liquor license owner.
Disbarred	Abandoned his law practice and engaged in misconduct in six matters, including gross neglect, lack of diligence, failure to communicate, conduct prejudicial to the administration of justice and failure to cooperate with ethics authorities.
Suspended	Grossly neglected a municipal court matter and then failed to file an answer to a formal ethics complaint.
Suspended	Agreed to represent clients in recovering \$100,000 from their former accountant who had sold them fraudulent municipal bonds.
Reprimanded	Failed to notify beneficiaries of an estate of their interest in a \$120,000 estate for a period of nine years.
Disbarred	Was disbarred in the state of Pennsylvania for knowingly misappropriating \$19,000 of clients' trust funds.
Suspended	Falsely accused her babysitter of being involved in an automobile accident which in fact involved respondent in order to secure the dismissal of criminal charges.
Admonished	Failed to diligently prosecute a medical malpractice claim and failed to communicate with his client.

Table A1: *Continued*

<i>Discipline</i>	<i>Description</i>
Suspended	Failed to inform his clients, the sellers, of the buyers contract to sell the property to a third party, executed before closing of title with respondent's client and for representing both parties in negotiating a contract of sale and in negotiating a modification thereof.
Reprimanded	Admitted that he engaged in conduct involving dishonesty, when he collected unemployment benefits from the State of New Jersey while employed as an attorney in a Pennsylvania law firm.
Reprimanded	Grossly neglected a client's personal injury matter and failed to adequately communicate with the client.
Reprimanded	Grossly neglected to take any substantial action in an estate matter for a period of eleven years, including the failure to prepare or file an inheritance tax return, open an estate account or deposit checks forwarded to the estate.
Reprimanded	Agreed to handle a complex litigation matter on behalf of a client who had previously been represented by another law firm.
Admonished	Failed to act diligently and failed to properly communicate with a client concerning the status of a subdivision application and subsequent litigation.
Reprimanded	Grossly neglected 14 collection cases and failed to protect his clients' interests upon termination of the representation.
Suspended	Engaged in conflicts of interest, misrepresentations and failures to safeguard client's funds in connection with the exchange of real estate owned by respondent and his clients.
Admonished	Failed to act diligently in a litigated matter and failed to reply to the client's reasonable requests for information regarding the status of the matter.
Admonished	Negligently misappropriated \$1450 in client trust funds as a result of a failure to maintain proper trust and business account records as required under Rule 1:21-6.
Disbarred	Accepted \$18,000 to appeal a federal criminal conviction and then misled the client that the appeal was proceeding, when, in fact, it had been dismissed.
Reprimanded	Failed to disclose secondary financing to a mortgage company contrary to the company's written instructions.
Suspended	Practiced law while ineligible and then grossly neglected the defense of a litigated matter, allowing a judgment to be entered against his client on two occasions and failing to timely move to vacate these judgments.
Reprimanded	Failed to act diligently, and failed to communicate with a client in connection with an automobile accident case which was dismissed because of respondent's failure to supply answers to interrogatories.
Suspended	Engaged in the practice of law during the period of December 1994 to September 1995, despite the fact that, during that period, he was declared ineligible to practice law because of his failure to pay the annual attorney registration fee.
Reprimanded	Accepted a personal injury action after the statute of limitations had already expired and, thereafter, failed to act for at least six months.
Suspended	Represented a client in a wrongful termination of employment lawsuit, but then failed to contact his client and ignored her repeated telephonic and written inquiries about the status of the case.
Reprimanded	Engaged in a conflict of interest by representing multiple parties with competing interests and also notarized a deed that was signed out of his presence.
Suspended	Failed to disclose on his New Jersey, California and Pennsylvania bar applications that he had been convicted in 1979 of shoplifting.

Table A2: Random Sample of 30 Disciplinary Actions in Maryland

<i>Discipline</i>	<i>Description</i>
Censured	For failing to safekeep the client's property.
Censured	For periodically failing to file and pay taxes from 2010 to 2014.
Disbarred	For engaging in criminal conduct that adversely reflected on his honesty, trustworthiness and/or fitness as an attorney in other respects and conduct prejudicial to the administration of justice when he possessed child pornography with the intent to distribute same.
Censured	For failing to explain a potential conflict of interest related to a joint representation and, when the conflict became apparent, failing to obtain the clients' informed consent confirmed in writing to continue the representation.
Censured	For failing to obtain the client's consent to deposit an unearned fee into an account that was not a client trust account and for failing to timely return unearned fees.
Disbarred	For failing to provide competent, diligent and prompt representation to his clients.
Censured	For failing to properly advise his client, causing the client to be unable to make an informed decision concerning an admission and for relaying to opposing counsel that his client would make the admission.
Disbarred	For failing to represent his clients with reasonable diligence and promptness.
Suspended	For failing to safekeep client funds in an attorney trust account.
Suspended	For failing to represent his clients with competence and for neglecting his clients' cases.
Disbarred	For failing to represent her client competently and diligently, failing to abide by the scope of representation, failing to adequately communicate with her client, collecting unreasonable fees, failing to safekeep unearned fees in an attorney trust account, failing to properly withdraw from her clients' cases and failing to surrender client property upon termination of her representation, failing to maintain fairness to opposing parties and counsel, knowingly failing to respond to bar counsel, engaging in conduct involving dishonesty, fraud, deceit or misrepresentation, and engaging in conduct that is prejudicial to the administration of justice. respondent repeatedly neglected her clients' cases, did not maintain client funds in trust, and made material representations to clients, opposing counsel, bar counsel and the courts.
Disbarred	For committing a criminal act that that reflects adversely on her honesty, trustworthiness and fitness as an attorney. respondent plead nolo contendere to conspiracy to commit theft of less than \$1,000.00 in connection with her role as a getaway driver in a robbery and plead guilty to obtaining prescription by fraud and possession of morphine following the execution of a search and seizure warrant on her home.
Disbarred	For engaging in a scheme in which he and [another person] enriched themselves at the expense of a trust.
Suspended	For failing to adequately communicate with his clients and failing to recognize that his representation of one client would be directly adverse to and would materially limit his responsibilities to another client. respondent represented a government contractor and construction company and invited a friend and client to serve as an indemnitor for surety bonds on future construction projects, resulting in a conflict of interest.
Censured	For representing clients when the representation involved a conflict of interest, providing financial assistance to a client in connection with pending litigation, and failing to properly terminate representation. respondent represented the passenger and the driver involved in an auto accident, failed to properly terminate representation after the driver was found to be at fault, and loaned the passenger \$1,500 unrelated to court costs or litigation expenses.
Disbarred	For failing to disclose his criminal activities on his bar application and submitting a false affirmation to the board of law examiners.

Table A2: *Continued*

<i>Discipline</i>	<i>Description</i>
Suspended	For failing to appropriately safeguard property and maintain complete records of his attorney trust account, and for making cash withdrawals from his attorney trust account. respondent transferred funds from his attorney trust account to his political campaign account, made cash withdrawals from his attorney trust account, failed to remove earned fees from his attorney trust account, and failed to keep records and perform monthly reconciliations of his attorney trust account.
Disbarred	For misappropriation of client funds and for failing to respond to lawful requests for information by bar counsel.
Disbarred	For misappropriating client funds held in trust.
Censured	For failing to recognize that her representation of one or more clients would be materially limited by her responsibilities to another client and failing to obtain the orphans' court's approval in advance of making periodic distributions of estate funds that created a conflict of interest.
Disbarred	For engaging in a pattern of neglect that affected at least five clients over two years.
Disbarred	For breaching his fiduciary duties as trustee of an irrevocable trust for the benefit of his elderly client.
Disbarred	For failing to provide competent representation to his clients.
Disbarred	For lack of competence and diligence.
Disbarred	For failing to safekeep client funds in trust, failing to maintain adequate records for his attorney trust account, failing to timely respond to bar counsel, knowingly making false statements to bar counsel, committing a criminal act that that reflects adversely on his honesty, trustworthiness and fitness as an attorney and engaging in conduct involving dishonesty, fraud, deceit or misrepresentation that is prejudicial to the administration of justice.
Disbarred	For abandoning his practice of law without notice to his clients.
Disbarred	For failure to maintain an accurate accounting of client funds, engaging in a pattern of negligent and deceitful accounting practices, knowingly making a false statement to bar counsel in connection with a disciplinary matter, submitting false client ledgers and settlement sheets to bar counsel.
Disbarred	For failing to honor assignments securing the repayment of a loan to a third party with settlement proceeds in a personal injury matter. upon receipt of the proceeds, he failed to notify the creditor that he received the funds and disbursed the funds to his client.
Suspended	For failure to pay a recordation tax, failure to keep client and third party property separate from his own, failure to respond to requests for information by bar counsel and failure to maintain sufficient funds in his attorney trust account.
Suspended	For maintaining his attorney trust account solely for the purpose of managing his personal funds after he ceased practicing law.

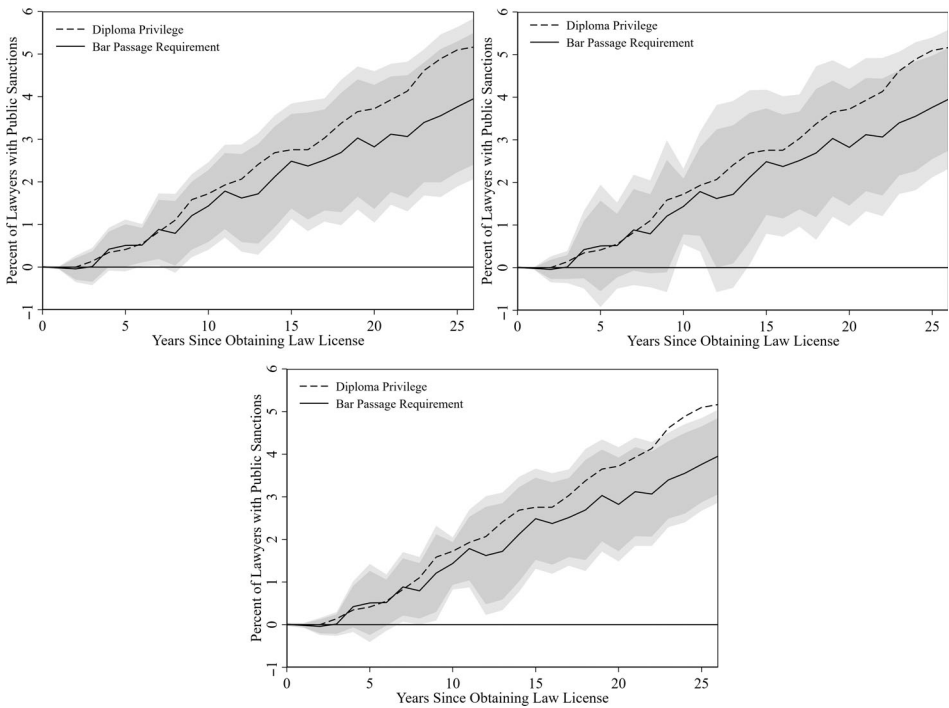
Table A3: Breakdown of Types of Publicly Disciplined Conduct in the State of Washington

<i>Conduct</i>	<i>Share of disciplinary actions relative to most common conduct</i>
Communication	100
Diligence	82
Dishonesty, fraud, deceit, or misrepresentation	69
Declining or terminating representation	48
Fees	48
Conflict of interest	46
Criminal act	43
Safeguarding property	42
Preserving identity of funds and property of a client	39
Conduct prejudicial to the administration of justice	38
Misconduct	33
Violate ELCs	33
Expediting litigation	22
Moral turpitude	22
Competence	19
Scope of representation	18
Candor toward the tribunal	18
Fairness to opposing party and counsel	18
Required trust account records	17
Responsibilities regarding nonlawyer assistants	15
Unauthorized practice of law	13
Bar admission matters	8
Truthfulness in statements to others	8
Conduct demonstrating unfitness to practice law	7
Meritorious claims and contentions	7
Disobey court order	7
Violate the RPCs	7
Misconduct involving disbarred, suspended, resigned, or inactive lawyers	4
Violate oath of attorney	4
Respect for rights of third person	4
Confidentiality	3
Professional independence of a lawyer	2
Responsibilities of a partner or supervisory lawyer	2
Duties to former clients	1
Impartiality and decorum of the tribunal	1
Advisor	1
Prejudice or bias	1
Advertising	1
Imputed disqualification; general rule	1
Client with disability/diminished capacity	1
Direct contact with prospective clients	1
Special responsibilities of a prosecutor	<1
Violate CJC	<1
Trial publicity, guidelines for applying RPC 3.6	<1
Firm names and designations	<1
Judicial and legal officials	<1
Improperly influence a governmental official	<1

NOTES: The table reports the breakdown of conduct of public disciplinary actions in the State of Washington from 1996 to 2019. Disciplinary actions can include multiple citations of conduct, and these statistics treat each citation of conduct as an individual violation. The outcome is the share of conduct cited in disciplinary actions compared to the most common conduct (communication).

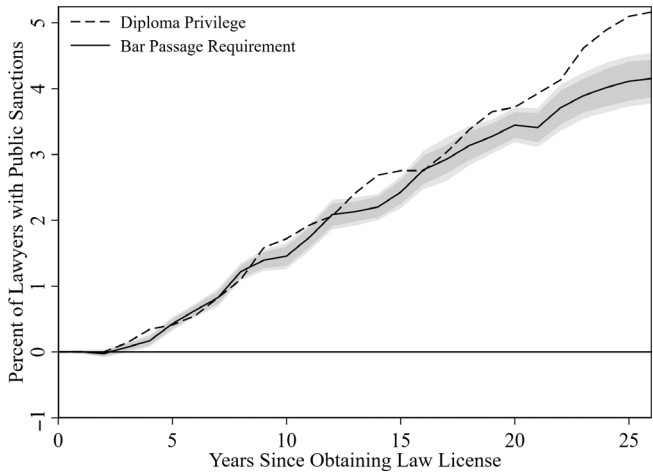
Abbreviations: CJC, code of judicial conduct; ELCs, enforcement of lawyer conducts; RPC, rules of professional conduct.

Figure A1: Regression estimates for different approaches to standard error correction. (a) Level of clustering: State. (b): Level of clustering: Cohort. (c) Level of clustering: Event.



NOTES: Each panel shows estimated coefficients and 90% and 95% confidence intervals from Equation (1), estimated separately for each year since obtaining law license. The estimated coefficients are the same as in Figure 3 (a). The panels differ from Figure 3(a) and from each other by the standard error correction. Panel (a) clusters standard errors by state. Panel (b) clusters standard errors by cohort. Panel (c) clusters standard errors by event.

Figure A2: Placebo tests without bootstrapping.



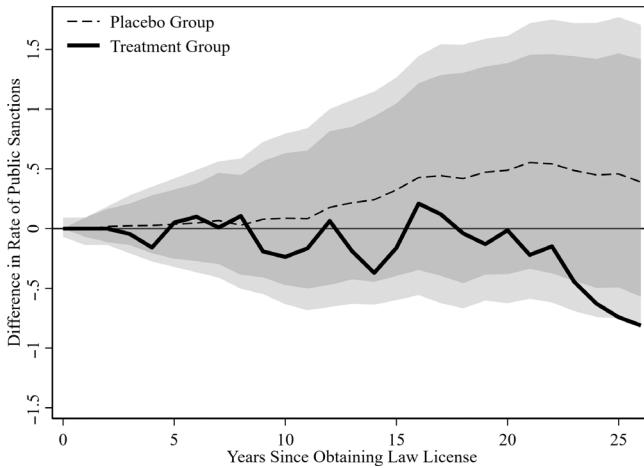
NOTES: The dashed line labeled “Diploma Privilege” is the public sanction rate of lawyers licensed on diploma privilege in the treatment groups. The solid line labeled “Bar Passage Requirement” is the public sanction rate of lawyers in the treatment group who were licensed after passing the bar exam. This line and confidence intervals is estimated from placebo tests where for each state-cohort in the treatment group I draw a random control state as the control group. The placebo tests are used to recover the counterfactual estimate and the simulated confidence intervals of the public sanction rate in the treatment group in the postperiod. The gray areas report the 90% and 95% confidence intervals.

Figure A3: Placebo tests with pooled control group.



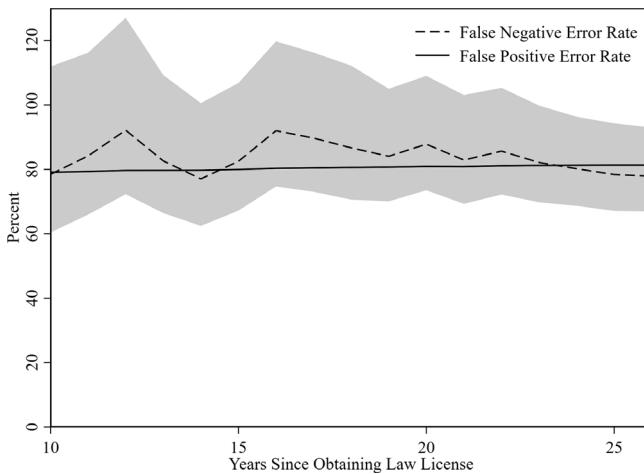
NOTES: The figure reports the mean and 90% confidence intervals of three versions of the placebo tests described in the text. The “Unpooled” lines are from placebo tests taking a random sample a single control state from Figure 4. The “Unweighted Pooled” lines are from placebo tests taking a random sample of the pooled set of lawyers from all the control states. The “Weighted Pooled” lines are from placebo tests taking a random sample of the pooled set of lawyers from all the control states but where the states with fewer lawyers are sampled more frequently such that the share of lawyers from each state will be the same on average.

Figure A4: Difference in rate of public sanctions in the placebo tests.



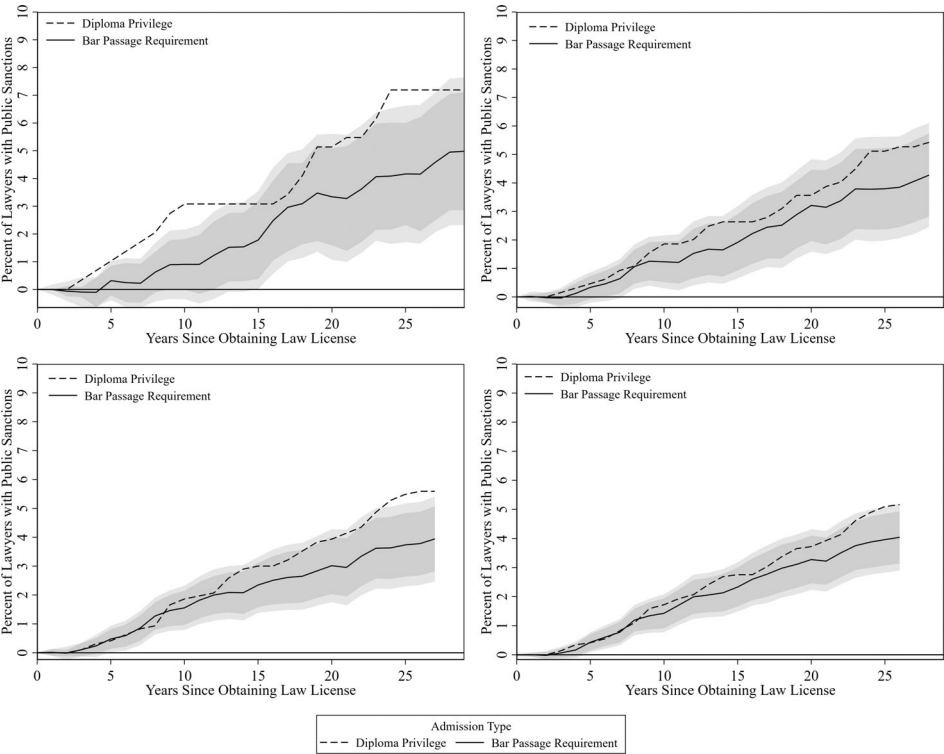
NOTES: The figure reports the mean, 90% confidence interval, and 95% confidence interval of the simulated first-differences in the control group (δ_i from Equation 2, labeled “Placebo Group”) and the first-difference in the treatment group (\hat{Y}^{Bar} , labeled “Treatment Group”). See text for details.

Figure A5: Implied error rates.



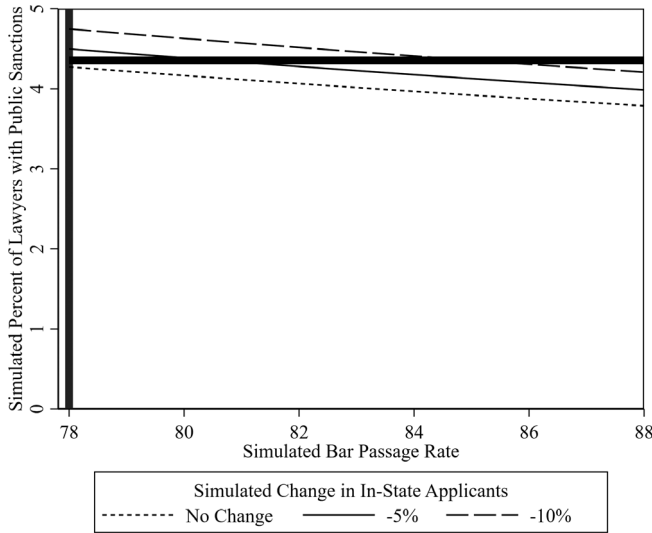
NOTES: The false positive error rate is the rate at which lawyers licensed on diploma privilege but who would have passed the bar exam were publicly sanctioned (estimated according to Equation 5). The false negative error rate is the rate at which lawyers licensed on diploma privilege but who would never have passed the bar exam were publicly sanctioned (estimated according to Equation 6). The figure reports estimates under the assumption that the entire effect of the bar passage requirement on public sanctions is driven by a different rate of public sanctions for lawyers licensed on diploma privilege but who would never had passed the bar exam.

Figure A6: Assessing the choice of the event window. (a) 1-Year event window. (b): 2-Year event window. (c) 3-Year event window. (d) Full with first-year post change.



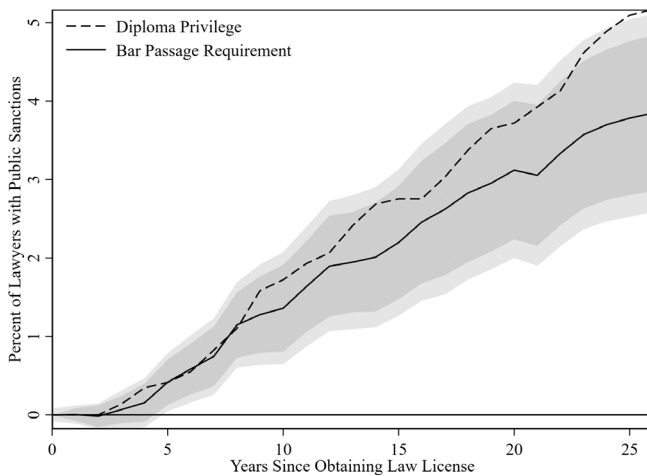
NOTES: Each panel reproduces the estimates from Figure 4 but with different number of cohorts around the events. Panel (a) reports one cohort around the events. Panel (b) reports two cohorts around the events. Panel (c) reports three cohorts around the events. Panel (d) reports five cohorts around the events and includes the cohort licensed the year after the diploma privilege was abolished which only contains lawyers in the treatment group who passed the bar exam after their first attempt. The range of the number of years since obtaining a license on the x-axis differs between the panels because the observed career length of lawyers licensed at different times is different in the panels. In particular, the last event occurred in 1989, so the number of years of experience that is observed is based on cohorts in the years before and after this event.

Figure A7: Percent of lawyers with public sanctions by simulated bar passage and simulated in-state applicants.



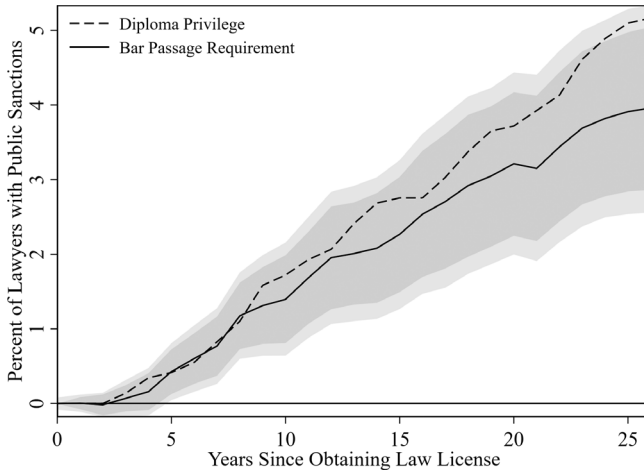
NOTES: The figure reports the results of simulations assessing the extent that the assumptions made to estimate the percent of sanctions at the state-cohort level in the treatment groups affect the primary results. The figure reports the estimated percent of lawyers with public sanctions from Equation (7) for different simulated values the bar passage rate of the treatment group, separately by simulated percent changes in the number of applicants that will become the treatment group if they pass the bar exam as indicated in the legend. See text for details.

Figure A8: Assessing bias: Simulated bar passage and simulated in-state applicants.



NOTES: The figure reports the results of simulations assessing the extent that the assumptions made to estimate the sanction rate at the state-cohort level in the treatment groups bias the primary results. The simulated percent of lawyers with public sanctions is estimated according to Equation (7). The simulations vary the bar passage rate from the state bar passage rate to 10 percentage points higher and vary the number of applicants in the treatment group from the estimated rate to up to 10% smaller. See text for details.

Figure A9: Assessing measurement error: Percent of lawyers with public sanctions by simulated measurement error.



NOTES: The figure reports the results of simulations assessing the extent that measurement error in the estimates of the sanction rate at the state-cohort level in the treatment groups affect the primary results. The simulated percent of lawyers with public sanctions is estimated according to Equation (7). The simulations introduce measurement error into the bar passage rate of the treatment group and the number of applicants that will become the treatment group if they pass the bar exam. See text for details.

APPENDIX B: DETAILS ABOUT CHANGES IN DIPLOMA PRIVILEGES

B.1. Mississippi

In 1917, Mississippi adopted a diploma privilege for graduates of the University of Mississippi.⁴⁰ In the 1970s, Mississippi lowered its bar exam standard for required test takers. Legislation passed in 1979 abolished the diploma privilege beginning with the 1985 cohort. Documenting the history of the Mississippi bar leading up to the diploma privilege change, Hafter (1985) concludes that that backlash faced by the board of bar examiners after lowering the bar exam standard was a motivating factor in the decision to repeal the diploma privilege.

⁴⁰See Association of American Law Schools (1976), citing Letter from George Van Zant, Secretary, Mississippi State Bar, October 17, 1968 ("compiled from the Bar's Annual Meeting Minutes from 1910 to 1917, and reports of the Committee on Legal Education and Admissions to the Bar.")

B.2. Montana

In 1915, the Montana Supreme Court adopted a rule granting a diploma privilege to graduates of the University of Montana.⁴¹ After law professor Jim Goetz failed the bar exam in 1968, he challenged the diploma privilege in court. The case made it up to the Montana Supreme Court (*Goetz v. Harrison*). Although he lost the case, the case led to the abolition of the diploma privilege, effective in 1984.

B.3. South Dakota

In 1973, South Dakota adopted a diploma privilege for graduates of the University of South Dakota. After this adoption, law professors at the University of South Dakota questioned the ability of students to “qualify for the great privilege of law practice on a piecemeal basis, without ever having to show in a comprehensive way that [they are] qualified overall on the completion of [their] law school course” (Davidson & Hagemann, 1984, at 20). After the law school appointed a committee to “explore the possibility of using an internal, comprehensive exam” (Davidson & Hagemann, 1984, at 18–19), it proposed requiring law students the Multistate Bar Exam after completing three semesters of law school. The diploma privilege was abolished in 1983.

B.4. West Virginia

In 1887, West Virginia first required a written bar exam for applicants, but a diploma privilege applied to graduates of West Virginia University.⁴² In 1981, the West Virginia state legislature passed legislation abolishing the privilege to be effective for anyone graduating after July 1, 1983.⁴³ Four law students challenged the legislation, and the West Virginia Supreme Court struck down the legislation.⁴⁴ In 1989, the West Virginia Supreme Court abolished the diploma privilege. The West Virginia Board of Bar Examiners stated that “[t]he major reason for the change in West Virginia was to bring West Virginia in line with other states, to allow [West Virginia University] graduates more flexibility in becoming admitted in other states [after becoming admitted to West Virginia] if that was their desire and to complete the educational process” (Hall, 2000).

⁴¹In 1895, legislation required all applicants to take a bar exam. See Association of American Law Schools (1976), citing Rules of the Supreme Court of Montana, Rule XVII, Sec. I, dated March 6, 1894. 13 Montana 578. See Association of American Law Schools (1976), citing Montana Laws of 1915, Sec. 6382 as amended by oh. 18; Mont. Rev. Codes Ann. Sec. 93-2002.

⁴²See Association of American Law Schools (1976), citing W.Va. Code, ch. 30, Art. 2, Sec. I.

⁴³The legislature amended W.Va.Code, 30-2-1.

⁴⁴State Ex Rel. Quelch v. Daugherty. 306 S.E.2d 233 (1983).

APPENDIX C: DATA CONSTRUCTION: ESTIMATION OF THE PUBLIC SANCTION RATE IN THE TREATMENT GROUP

In the four treatment states, I only have individual level data for the lawyers who were publicly sanctioned, which identifies the name of lawyers who were disciplined, the date of the disciplinary action, the year the lawyer obtained a law license, and the law school that each disciplined lawyer in the state attended.⁴⁵ For the treatment groups, I do not have individual level data for lawyers who were not publicly sanctioned. Therefore, I build a dataset on the percent of lawyers in the treatment group who have received public sanctions in a state-cohort Y_{st} by collecting data on the number of lawyers who receive public sanctions X_{st} and on the number of lawyers N_{st} and estimating it according to Equation (A1).

$$Y_{st} = X_{st} / N_{st} \quad (\text{A1})$$

I start with a dataset of all public sanctions in the states that changed their bar passage requirement from 5 years before each event to 2020. This includes any public sanction against lawyers in the treatment group and any public sanction against other lawyers practicing in the state who either attended another law school or who were licensed outside of the event window. For lawyers who received a public sanction over this time period, I establish the year they obtained a law license and the law school they attended. After excluding public sanctions from lawyers not in the treatment group, I calculate the number of lawyers who were publicly sanctioned in the treatment group in each state-cohort (X_{st}).

In the preperiod, I use official annual statistics of lawyer admissions under diploma privilege for N_{st} (National Conference of Bar Examiners). There are no official statistics on N_{st} in the postperiod because the lawyers from these four law schools are combined with lawyers from other law schools in the official statistics. Let the number of licensed lawyers in the treatment group in the postperiod be given by $N_{st} = p_{st}A_{st}$, where p_{st} is the bar passage rate of the treatment group and A_{st} is the number of applicants who would be in the treatment group if they pass the bar exam.

To estimate N_{st} in the postperiod, I make two assumptions. First, I assume that the bar passage rate of the treatment group is equal to the state's overall bar passage rate.⁴⁶ In the treatment states where the treatment law school is the only law school in the state (Montana, South Dakota, West Virginia), the state bar passage rate is from both the treatment law school and test takers who attended out of state law schools. In Mississippi, the state bar passage rate is from the treatment law school, other law schools in the state, and test takers who attended out of state law schools. Because the treatment law schools make up a large share of the total number of applicants in the state in the postperiod, the state's overall bar passage rate is largely driven by the treatment law school's bar passage

⁴⁵Appendix C describes how I collected this information for each treatment group.

⁴⁶Official statistics on state-year bar passage rates are from the National Conference of Bar Examiners, 2020.

rate. But this estimate is likely to understate the true bar passage rate because the treatment groups are graduates from the state flagship universities that tend to have higher bar passage rates than the state average (see Section V.B. for a discussion). To the extent that the estimate understates the true bar passage rate for the treatment group, this would cause the estimates of $\hat{p}_{st}\hat{A}_{st}$ to understate the true number of lawyers.

Second, I assume that the treatment group share of total applicants in the state in the postperiod is equal to the share from the preperiod. One concern arises here because the estimate of the number of applicants in the treatment group also depends on the number of applicants in the state that are not in the treatment group. To the extent that the number of applicants from other law schools increases relatively more than the treatment group in the postperiod, the estimate of the number of applicants in the treatment group in the postperiod overstates the true number. However, the total number of applicants is fairly stable over the event window (see Figure 2), which provides at least some suggestive evidence that the assumption is reasonable. Another concern arises because the bar passage requirement might cause lawyers in the treatment group to practice law in another state. To the extent that graduates of these four flagship state schools are less likely to practice law within the state because of the bar passage requirement, the estimate for \hat{A}_{st} in the postperiod would overstate the true number of applicants. However, if lawyers who are discouraged from practicing in the state because of the bar passage requirement are also those lawyers who would be less likely to pass the bar exam, then the state level bar passage rate would be more likely to underestimate the true bar passage rate for the treatment group. In other words, the more the estimate of \hat{A}_{st} overstates the number of applicants, the more likely it will be counteracted by a higher bar passage rate.

Under these assumptions, the number of lawyers obtaining a law license in the treatment group in the postperiod is the number of applicants multiplied by the bar passage rate.

APPENDIX D: DATA COLLECTION

This appendix provides an overview of the source of data and the data collection process for each of the states.⁴⁷ I scraped state websites in 2019.

D.1. Colorado

I scraped the Colorado Supreme Court's lawyer-level webpages that contain information about the lawyer, including the date of admission and details about disciplinary actions taken against the lawyer. <http://www.coloradosupremecourt.com>

⁴⁷For more details on the data collection process, see the appendix of the working paper version of this article (the working paper can be accessed from www.kylerozema.com/research).

D.2. Iowa

I scraped the Iowa Judicial Branch's lawyer-level webpages that contain information about the lawyer, including the date of admission and details about disciplinary actions taken against the lawyer. <https://www.iacourtcommissions.org>

D.3. Louisiana

I scraped the Louisiana Attorney Disciplinary Board's lawyer-level webpages that contain information about the lawyer, including the date of admission and any disciplinary actions. <https://www.ladb.org/Search/>

D.4. Mississippi

I obtained Mississippi disciplinary actions since 1982 reported in *The Mississippi Lawyer*. I compiled a list of all disciplinary actions taken since 1982 that were reported in the magazine. To identify the admission date and law school of these lawyers, I used several sources. The Mississippi Bar has an searchable member directory with admission dates (<https://www.msbar.org/lawyer-directory>). The website does not contain the full list of lawyers ever licensed in the state. The website also does not include the law school a lawyer graduated from. I scraped the website and used the available information. To obtain the admission date for lawyers not on the state bar website and to collect law school information for the lawyers, research assistants searched the West Legal Directory, the Mississippi bar roll from the Mississippi courts website, Martindale-Hubbell directory, Lawyers.com, Google searches, and a list of University of Mississippi law school graduates by year.

D.5. Michigan

I scraped the State Bar of Michigan's lawyer-level webpages that contain information about the lawyer, including the date of admission. <https://www.michbar.org>. These webpages do not contain disciplinary actions. I obtained disciplinary actions since October 1978 from the State of Michigan's Attorney Discipline Board website. <https://www.adbmich.org/attorney-information/attorney-database>

D.6. Minnesota

I scraped the Minnesota Lawyers Professional Responsibility Board's lawyer-level webpages that contain information about the lawyer, including the date of admission and details about disciplinary actions taken against the lawyer. <http://lprb.mncourts.gov/LawyerSearch/Pages/default.aspx>

D.7. Montana

As of 2019, the Montana State Bar website allows the public to search for lawyers but it restricts the public from viewing most lawyer webpages (e.g., <https://www.montanabar.org>).

org/search/custom.asp?id=2249). The State Bar of Montana has a publication of “Public Discipline of Attorneys,” which was “compiled based on public orders of discipline issued by the Montana Supreme Court.” <https://montanaodc.org/attorney-discipline>. From this list, I first compiled information on lawyer admission date from the public opinions on the Montana Supreme Court website. These were available for actions since 2006. For the remaining lawyers, I spent a few hours on the phone with Lee Bruner at the Montana Supreme Court clerks’ office where I read him the names of lawyers and he told me the year the lawyer was licensed. I thank Lee Bruner for his help with data collection. For the lawyers who were barred between 1979 and 1988 and who were disciplined, I identified whether the lawyers attended the University of Montana using annual commencement handouts.

D.8. North Carolina

I scraped the North Carolina State Bar’s lawyer-level webpages that contain information about the lawyer, including the date of admission. The link of the webpages is the following followed by a lawyer’s bar number: <https://www.ncbar.gov>. I obtained disciplinary records from same website by scraping all the PDF orders and opinions and then parsing the documents. <https://www.ncbar.gov/lawyer-discipline/>

D.9. Oregon

I scraped the Oregon State Bar’s lawyer-level webpages that contain information about the lawyer, including the date of admission and details about disciplinary actions taken against the lawyer. <https://www.osbar.org>.

D.10. Pennsylvania

I scraped the Disciplinary Board of the Supreme Court of Pennsylvania’s lawyer-level webpages that contain information about the lawyer, including the date of admission and details about disciplinary actions taken against the lawyer. <https://www.padisiplinaryboard.org>

D.11. South Dakota

For disciplinary actions in South Dakota after 1975, a South Dakota Supreme Court rule required: “Newspaper publication of suspension or disbarment. The clerk of the Supreme Court shall cause a notice of every suspension or disbarment to be published in a newspaper of general circulation in the judicial circuit or circuits in which the disciplined attorney maintained an office for the practice of law.”⁴⁸ To compile the list of disciplinary actions taken, searches were conducted for South Dakota newspapers on newspapers.com between 1972 and 2020. Search terms included:

⁴⁸Source: Supreme Court Rule 78-1, Rule XIII (c); SL 2018, ch 298 (Supreme Court Rule 18-07), eff. July 1, 2018.

- “Disbarment”
- “Attorney disbarred” OR “lawyer disbarred”
- “Court disbars lawyer” OR “court suspends lawyer” OR “court censures lawyer” OR “Court disbars attorney” OR “court suspends attorney” OR “court censures attorney”
- “Lawyer reprimanded” OR “attorney reprimanded”
- “Admonishment” AND (“Attorney” or “lawyer”)
- “Public Censure” AND (“Attorney” or lawyer”)
- “Code of Professional Responsibility” AND (“Attorney” or lawyer”)
- “Probationary status” AND (“Attorney” or lawyer”)
- “Violation of canon” AND (“Attorney” or lawyer”)
- “Reimburse the state bar” AND (“Attorney” or lawyer”)
- “Referee” AND “state bar” and (“Attorney” or “lawyer”)
- “Attorney disciplined” OR “lawyer disciplined”
- “Lawyer sanctioned” OR “attorney sanctioned”
- “Lawyer suspended” OR “attorney suspended”
- “Misconduct” AND (“Attorney” or lawyer”)
- “Disciplinary Board” AND “State Bar Association”

Some of the newspapers obtained information on the lawyer’s law school and year of admission. For the admission year and law school for the other disciplined lawyers, searches were conducted for every lawyer with missing information in Google, Martindale Hubble, newspapers.com, and legacy.com (obituaries). For the disciplinary actions that did not identify the admission date, the South Dakota Supreme Court clerks’ office provided me with the dates.

D.12. Tennessee

I scraped the Board of Professional Responsibility of the Supreme Court of Tennessee’s lawyer-level webpages that contain information about the lawyer, including the year of admission and details about disciplinary actions taken against the lawyer. <https://www.tbpr.org/for-the-public/online-attorney-directory>

D.13. West Virginia

I scraped the West Virginia Bar’s lawyer-level webpages that contain information about the lawyer, including the date of admission. <https://www.mywvbar.org>. These webpages do not contain details on disciplinary actions. I obtained disciplinary actions from the West Virginia Office of Disciplinary Counsel website, which has a link to all West Virginia Supreme Court decisions. <http://www.wvdc.org/decisionslist.htm>. These include lawyer discipline. Note that this is not limited to opinions but instead includes orders as well. (The large majority of disciplinary actions are from orders.)