How Do Professional Licensing Regulations Affect Practitioners? New Evidence*

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Abstract

I estimate the effect of professional licensing policy on training. The licensing regulations data come from a panel which contains rich variation in professional licensing policy for four diverse occupations. The individual-level data come from two supplements to the Current Population Survey. More stringent licensing regulations are not associated with higher vocational class enrollment, although increased stringency among some regulations is positively related to whether respondents have acquired training since the current job began. I find limited evidence of a licensing wage premium. This suggests that the cumulative effect of these licensing regulations on the supply of practitioners is small.

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1 Introduction

Professional licensing is one of the most prevalent institutions in the US labor market. Kleiner and Krueger (2013) estimate that in 2008 about 29% of workers were required by law to obtain a license in order to exercise their profession.[1] Kleiner (2000) notes that in 2000, more workers practiced in a licensed occupation than earned the minimum wage or belonged to a union.

A prospective practitioner can obtain a license by satisfying a list of education and training requirements. Those who are unable to fulfill these requirements may not practice lawfully. Consequently, each requirement creates a barrier to entry into the occupation. For instance, many states require attorneys to attend an ABA-approved law school. This licensing requirement imposes large direct and indirect costs on potential attorneys. Proponents of licensing claim that such barriers to entry screen out relatively low quality practitioners. However, empirical support for this claim is scant.[2] The difficulty of measuring service quality complicates the search for such evidence.

In this paper, I examine the relationship between professional licensing policy and training. Practitioners acquire human capital through training, thereby generating an important input into the production of service quality. The empirical analysis presented here thus contributes to the debate over whether licensing affects service

[1]This estimate refers to the percentage of workers required by some level of government to obtain a license for their current or most recent occupation. Licensing regulations are most frequently administered at the state level. This is the case for each of the occupations which I consider.

[2]Carroll and Gaston (1981) conclude that licensing reduces the stock of practitioners and thereby reduces received service quality. They show similar evidence across a variety of occupations and according to various measures of service quality. Kleiner and Todd (2009) find evidence that states requiring mortgage brokers to maintain a larger minimum net worth have a higher percentage of homes in foreclosure. Kleiner and Kudrle (2000) argue that more stringent licensing laws for dentists do not affect untreated dental health deterioration, complaints against dentists, or malpractice insurance premia. There is mixed evidence regarding whether traditionally certified teachers are more effective than alternatively certified or uncertified teachers. Kane, Rockoff and Staiger (2008) find little, if any, difference in student test scores among traditionally certified and uncertified teachers. By contrast, Clotfelter, Ladd and Vigdor (2007) conclude that students assigned to traditionally certified teachers score substantially better.

quality.[3] Evidence that licensing increases investment in quality inputs would lead us to pose two questions for future research. First, are the available measures of output quality adequate? The estimated effects of licensing on these noisy measures of output quality might differ from the true effect of licensing on output quality. Second, if these measures are adequate, where does the relationship between inputs to the production of service quality and outputs break down?

Licensing might affect training through a variety of mechanisms. One such direct effect is a compliance effect. Practitioners might acquire more training simply to comply with licensing policies.[4] A second, indirect effect is a selection effect. By setting a minimum human capital standard, licensing policies might screen out potential practitioners who find it more costly to acquire human capital. Licensing restrictions might thus select for practitioners who are more likely to obtain training, even if they are not required to do so by law.

I estimate the impact of licensing on two binary measures of training: current vocational class enrollment and training since obtaining the current job. I exploit variation in licensing regulations over time and across states to identify the effect of each dimension of licensing policy (e.g. educational requirements, exam requirements) on these measures of training. I control for state-level unobserved heterogeneity since little is known about the process generating variation in licensing policy across states.[5] Models without state fixed effects can yield a biased estimate of the

[3]Larsen (2013) makes a similar contribution to this debate in studying the impact of licensing exams on the quality of teachers' inputs and outputs. He shows that more restrictive licensing laws increase the selectivity of colleges attended by more experienced teachers at most quantiles of this input quality distribution. He goes on to argue that this increase in input quality translates into an increase in student test scores, mostly among high-performing students.

[4]Shapiro (1986) considers a moral hazard model in which licensing is related to human capital investments through this mechanism. In this model, regulators can improve welfare in some contexts by implementing licensing, as the additional human capital investments required by licensing increase both service quality and prices on average. Klee (2010) extends this model to show that a certification policy weakly dominates licensing in a welfare sense in this theoretical environment. A necessary condition for certification to strictly improve welfare in this environment is that the licensing regulations bind. The empirical analysis presented here examines whether licensing regulations bind.

[5]Blau (2007) and Hotz and Xiao (2011) emphasize the importance of including state fixed effects

correlation between professional licensing regulations and training. Additionally, I use only within-occupation variation in training to identify the effects of licensing, which mitigates selection bias and bias from omitted factors that influence training.

State-level data on licensing regulations come from an underutilized panel collected by Morris Kleiner.[6] This panel contains information on the relative stringency of various dimensions of licensing policy for four diverse occupations. Within-state variation in relative stringency over time and across policy dimensions allows for the inclusion of state fixed effects. The individual-level data on training and demographics come from two supplements to the Current Population Survey. Such a large sample mitigates concerns about the precision of estimates that confront many investigations of the effects of professional licensing.

I find mixed evidence of the relationship between professional licensing and training. Even within occupations, some dimensions of licensing policy seem to affect training while others seem to have no effect or even a counteracting effect.[7] More stringent licensing policies do not seem to increase enrollment in a vocational class. On the contrary, I estimate this relationship to be negative to the extent that it exists. I present evidence that some professional licensing regulations are positively related to training since the current job began. In addition, I find limited evidence of a licensing wage premium. Indeed, I estimate that in some cases more stringent licensing regulations are associated with a wage discount. Consequently, the cumulative effect of more stringent licensing regulations on the supply of practitioners seems to be small. This conclusion is consistent with the generally weak estimated effects of licensing regulations and the offsetting estimated effects of licensing regulations on

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when estimating the effects of professional licensing.

[6]The data were collected for use in Kleiner (2006). No other study uses this data, to my knowledge.

[7]All comparisons are statistically significant at the 90 percent level. The estimates in this paper are based on responses from a sample of the population and may differ from actual values because of sampling variability or other factors. As a result, apparent differences between the estimates for two or more groups may not be statistically significant. For more information on the source of the data and the accuracy of the estimates, see http://www.census.gov/cps/methodology/techdocs.html.

class enrollment and training on the current job.

Blau (2007) presents an empirical analysis which is closely related to the one presented here. Blau (2007) includes cross-sectional evidence regarding how the regulation of child care workers affects their training. He exploits variation in licensing policy across worker types (i.e. director, teacher, and assistant) and across states to identify the effect of each dimension of policy on training. The empirical examination presented here differs from Blau (2007) in two important ways. First, I estimate a model with occupation-specific effects of licensing regulations on training. This assumption allows me to examine whether licensing regulations are more binding for certain types of workers. Second, I employ licensing regulations data that exhibit variation over time and over more regulatory jurisdictions. Longitudinal variation allows me to control for unobserved heterogeneity at the state-occupation level which might bias estimates of the effects of professional licensing.[8]

The remainder of this paper is structured as follows. In Section 2, I provide some background about professional licensing. Section 3 presents the econometric model and problems complicating estimation. In Section 4, I describe the data. I discuss the results in Section 5. Section 6 concludes.

2 Professional Licensing: Background

Some theoretical analyses predict that licensing improves average service quality in labor markets with asymmetric information. Generally, licensing screens out practitioners who are likely to be of low quality by imposing minimum requirements on

[8]For instance, consumers in some states might have relatively strong preferences for the quality of lawyers' services and relatively weak preferences for the quality of cosmetologists' services. Lawyers in this state might acquire specific training in order to attract consumers. On the other hand, cosmetologists might not acquire specific training, recognizing that such training is unlikely to attract consumers. Additionally, consumers might pressure legislators for more stringent regulations on lawyers in order to screen out low quality practitioners. On the other hand, consumers might accept weaker regulations for cosmetologists in order to keep prices low. Without controlling for the state-occupation-level component of unobserved heterogeneity, estimation would yield an upwardly biased estimate of the relationship between licensing regulations and training.

quality or quality inputs. In these models, licensing provides a degree of assurance to consumers who cannot observe the quality of a particular practitioner.[9] Leland (1979) uses an adverse selection model to show that licensing also can attract high quality practitioners who would not have participated in the licensed occupation without regulation. In this model, licensing increases average service quality so consumers are willing to pay more for the services of a practitioner of unknown quality. These increased earnings attract entrants from alternative occupations. When a practitioner's occupation-specific quality is positively correlated across occupations, these entrants further improve average service quality.

By contrast, other theoretical analyses predict that licensing can reduce average service quality in labor markets with asymmetric information. One common argument for this claim is that licensing distorts the optimal allocation of workers to tasks, potentially leading high skill workers to pursue an unlicensed occupation.[10] A second common concern is that licensing might require some practitioners to invest in inefficiently high levels of human capital. If practitioners pass along this entry cost to consumers, some consumers might substitute toward less expensive, lower quality services including home production. A final concern is that by limiting competition, licensing policies allow low quality incumbents to remain in the market longer than they might otherwise. On balance, many economists suspect that professional licensing might be motivated in practice by a desire to capture rents more than by a desire

[9] The mechanism through which licensing accomplishes this depends on the modeling assumptions. Leland (1979) assumes that the regulator can observe quality directly, and thereby administers licensing by setting a minimum quality requirement. Shapiro (1986) assumes that the regulator can observe human capital investments but not quality. The regulator then administers licensing by setting a minimum human capital requirement. Human capital and quality are strategic complements in this model. Licensing thus indirectly improves average quality in equilibrium by decreasing the incremental cost of providing high quality. Klee (2010) also follows this assumption. Atkeson, Hellwig and Ordoñez (2012) use a model in which regulators tax entry. This tax screens out low quality practitioners who will be unlikely to accumulate a reputation good enough to allow them to recoup the entry costs. This policy can be interpreted as a form of licensing.

[10] Wiswall (2007) incorporates this mechanism into a model of occupational sorting to argue that licensing can reduce average service quality. He estimates the model, and uses the estimated model to simulate the effects of a more stringent licensing policy. He concludes that more stringent teacher licensing requirements reduce average teacher quality.

to protect the public interest. These suspicions are strengthened by what many claim is an overrepresentation of practitioners' interests on many regulatory boards.[11]

Fewer theoretical analyses have studied the mechanism by which professional licensing might affect inputs to the production of quality. Primarily, these analyses have assumed that licensing affects training through the mechanism of compliance. Regulations might require practitioners to obtain more training than they would have chosen independently. The effect of licensing on training through the compliance mechanism might also be less direct. For example, a regulatory board can revoke a lawyer's license as punishment for particularly low quality service. Lawyers might acquire training as a quality input to reduce the likelihood of such an occurrence.

Second, licensing might affect training through the mechanism of selection. Licensing requires all practitioners to satisfy a set of minimum human capital standards. In response, the set of practitioners choosing to enter the occupation might have relatively low costs of acquiring human capital on average. If this is the case, practitioners that select into a licensed occupation are more likely to acquire training on average, even if they are not required to do so. Similarly, licensing imposes potentially large costs of entering an occupation. In response, the set of practitioners choosing to enter the occupation might have relatively strong ties to the occupation on average. If this is the case, practitioners that select into a licensed occupation are more likely on average to acquire occupation-specific training, anticipating a long occupational employment spell.

Through either mechanism, we might expect binding licensing regulations to improve human capital through training. This training grants practitioners access to the latest and best accepted knowledge and methodologies. Such information acts as

[11]See Smith (1976) and Friedman (2002) for classic examples of economists' arguments against licensing. See Stephenson and Wendt (2009) and Svorny (2004) for summaries of the more recent literature on professional licensing. See Law and Hansen (2010) and Summers (2007) for a discussion of the overrepresentation of practitioners' interests on regulatory boards. Law and Hansen (2010) find that concerns about overrepresentation are not borne out in the data; the degree of public oversight does not significantly affect regulatory boards' use of disciplinary action.

an input to the production of service quality. Consequently, licensing might improve average service quality, depending upon how inputs are translated into output quality.

3 Econometric Model

I estimate the strength of the relationship between professional licensing and binary measures of training using a probit model with state and year fixed effects. Blau (2007) and Hotz and Xiao (2011) emphasize the importance of controlling for state-level unobserved heterogeneity which could bias estimates of the effects of professional licensing policies. For example, consider a state in which parents have relatively strong preferences for teacher quality. Parents might pressure teachers in this state to obtain training in order to ensure that their children are exposed to the latest and best accepted teaching methodologies. Parents in this state might also pressure legislators to pass more stringent licensing regulations for teachers. Preferences for quality that vary across states can thus lead us to infer a positive relationship between licensing policy and training, even if this relationship is not causal. We could obtain unbiased estimates of the effects of licensing by properly modeling the process generating variation across states in licensing policy. However, we understand this process poorly. For similar reasons, I include year fixed effects to control for unobserved national time trends which might be correlated with both licensing policy and training.

I estimate the following model of the relationship between professional licensing policy and training:

$$Train^*_{icst} = X_{icst}\beta_c + L_{cst}\left[Young_{icst}\lambda^y_c + (1 - Young_{icst})\,\lambda^o_c\right] + \delta_{cs} + \gamma_{ct} + \varepsilon_{icst} \quad (1)$$

$$Train_{icst} = \begin{cases} 1 & \text{if } Train^*_{icst} > 0 \\ 0 & \text{if } Train^*_{icst} \leq 0 \end{cases} \quad (2)$$

Here, $Train_{icst}$ is a binary measure of training for individual i, who is a member of occupation c in state s and year t. Individual-level demographic explanatory variables are included in X_{icst}. These include education, a quadratic in age, and SMSA, among other variables. δ_{cs} and γ_{ct} represent occupation-state and occupation-year fixed effects, respectively. ε_{icst} is an individual-specific residual, assumed to be normally distributed. State-level licensing variables that describe the policy relevant in year t are included in the vector L_{cst}. $Young_{icst}$ is an indicator variable which is defined to have value one if respondent i is below some threshold age in year t.

The above model allows for cohort-specific effects of professional licensing, where cohorts are determined by age. The limitations of the data make this characteristic of the model essential for identifying the effect of professional licensing on training. For such an analysis, we need information on the set of licensing regulations that might impact the training behavior of a particular practitioner. This set includes the regulations in effect when a practitioner entered the occupation, regardless of whether licensing affects training through the compliance mechanism or the selection mechanism.[12] The current regulations might not reflect those relevant for a practitioner's training behavior; changes in regulation over time are often coupled with a grandfather clause that exempts incumbent practitioners from meeting the new licensing standards. I could match each respondent to the regulations in effect at entry into occupation c, given information on occupational tenure and a sufficiently long panel of regulations. In the absence of such data, I estimate the effects of the current regulations on two cohorts of practitioners.[13] Similar to Law and Marks (2009), I define the cohorts using age as a proxy for occupational tenure. This exploits the empiri-

[12]Additionally, this set should include the current continuing education requirements. However, the panel of licensing regulations that I employ does not contain this information.

[13]The Occupational Mobility, Job Tenure, and Training Supplement to the CPS does include information on occupational tenure. I attempted to conduct an analysis similar to the one presented in Section 5 using the matched regulations in effect at the implied entry into occupation c. However, the implied date of occupational entry occurred before the start of my panel of regulations in a majority of cases. In these cases, I assumed that the relevant regulations are the first set of regulations observed in my panel. I present and discuss the results of this empirical analysis in Appendix A.

cally negative relationship between age and occupational mobility.[14] Consequently, the parameter of interest is λ_c^y. I expect the current regulations to resemble more closely those in effect when younger practitioners entered the occupation.[15] I am also interested in λ_c^o though, since age is only a noisy measure of occupational tenure.

The model given by (1) and (2) also allows for occupation-specific coefficients. This assumption allows me to estimate the effects of professional licensing policy using only within-occupation variation in training behavior. In a model without occupation-specific coefficients, estimates of the correlation between licensing and training might have a positive bias due to selection into occupations. Kleiner and Krueger (2010) provide evidence that licensing is more common among highly educated workers. These workers might also find training less costly to acquire. This is a relevant concern even among the occupations for which I have licensing data, due to the diversity in point estimates of average education levels across these occupations. Furthermore, these occupation-specific coefficients allow me to assess whether licensing regulations are more binding for some types of workers than for others.

4 Data

I estimate this model on data from two sets of sources. First, state-level data on licensing regulations come from a panel containing information on the relative stringency of various dimensions of professional licensing policy for four diverse occupations. Second, the individual-level data on training and demographics come from two supplements to the Current Population Survey (CPS).

[14] See Kambourov and Manovskii (2008), Moscarini and Vella (2008), and Klee (2012) for evidence regarding this relationship. These studies also describe other occupational mobility patterns over the course of my panel of licensing regulations.

[15] This assumes no interstate mobility. However, the relatively short panel nature of the CPS would necessitate this assumption even I had information on occupational tenure for all respondents and a sufficiently long panel of licensing regulations.

4.1 Licensing Regulations Data

Information about licensing regulations toward accountants, attorneys, cosmetologists, and teachers comes from a panel dataset collected by Morris Kleiner.[16] This panel covers the period of 1980-98 for accountants, 1980-99 for attorneys, 1981-98 for cosmetologists, and 1984-98 for teachers. These occupations are licensed in all states throughout my panel of regulations, although different states require different qualifications of licensed practitioners. The data provide information about the relative stringency of various dimensions of licensing policy for each state and D.C. Two examples of dimensions of licensing policy observed in my data are specific education requirements and exam requirements. The dataset categorizes each observed dimension of a state's policy in a particular year as either relatively stringent or relatively lax. For instance, the general education dimension for cosmetologists is characterized as relatively stringent in states requiring a high school degree or equivalent. The binary relative stringency variable for specific education takes the value one in these states. Each relative stringency variable observed for occupation c is included as an element of the vector L_{cst} in the model given by (1) and (2). Table 1 lists the policy dimensions that I employ for each occupation and the criterion by which each is judged as relatively stringent.

Two characteristics of this dataset make it particularly desirable for estimation of the effects of professional licensing on training. First, it includes rich policy variation. I exploit variation in relative stringency across states and across years in order to identify the effect of each policy dimension on training. This second form of variation, in addition to within-state variation across policy dimensions, allows for the inclusion of state fixed effects.

Figure 1 depicts the within-state variation in policy across years. It indicates for

[16]See Figure 5.2 and the associated discussion in chapter 5 of Kleiner (2006) for details regarding how the data were collected.

each state the number of changes to licensing policy. A change is recorded between two year observations if any dimension of licensing policy changed. Licensing policy toward attorneys and cosmetologists exhibits less time variation than one might hope, as over half of states exhibit no changes to licensing policy. However, there is considerably more time variation within states for accountants and teachers, as more than 75

Figure 2 depicts the within-state variation in policy across dimensions. It indicates for each state the time average of the number of policy dimensions that are characterized as relatively stringent. There is less variation across dimensions for attorneys and cosmetologists than there is for accountants and teachers. Nevertheless, there is considerable variation across dimensions for all four occupations. This indicates that for many state-year observations, some dimensions of licensing policy were characterized as relatively stringent and others were characterized as relatively lax.

Table 2 presents a summary of the within-dimension variation in the stringency variables. This table lists for each dimension the percentage of state-year observations that are characterized as relatively stringent. There is considerable variation for each dimension of policy. For the dimension of policy that was least frequently characterized as stringent, 12.226% of state-year observations were considered relatively stringent. For the dimension of policy that was most frequently characterized as stringent, 18.328% of state-year observations were considered relatively lax.

Alternatively, I could employ variation in the introduction or repeal of professional licensing measures to estimate the effects of licensing. On one hand, these policy changes might generate more variation in training than the policy changes I consider here, to the extent that they represent larger changes in the cost of occupational entry. On the other hand, there are two limitations of such an identification strategy. Many of these measures were introduced in the late 19th or early 20th centuries.

This drastically limits the individual-level data that are available for econometric analysis. Additionally, the introduction or repeal of professional licensing regimes occurs relatively infrequently in practice. Such limited longitudinal variation makes precision of estimates a more important concern, especially for models with state fixed effects.

A second desirable characteristic of this dataset is that it provides information about four diverse occupations. These occupations vary widely in the tasks that practitioners perform. Additionally, these occupations vary widely in the demographic characteristics of practitioners. Table 3 presents mean demographics from the individual-level data that illustrate this point. For example, the point estimate of attorneys' average weekly earnings exceeds that of cosmetologists' average weekly earnings by a factor of four. Point estimates also suggest that almost all lawyers possess at least a bachelor's degree (97.377%), whereas few cosmetologists possess at least a bachelor's degree (9.062%). By comparing the effects of licensing policy across these diverse occupations, I can more confidently make a general statement about the effects of professional licensing.

One disadvantage of the regulations data is that two of the four occupations are not licensed in the strictest sense. In all states and D.C., policy toward accountants and teachers is one of certification. In this type of policy regime, practitioners may be issued a certification on the basis of human capital investments. This certification acts similarly to a license by signaling quality to potential employers. Certification and licensing differ fundamentally only because entry is not prohibited to practitioners lacking a certification, while entry is prohibited to practitioners lacking a license.[17]

This distinction is potentially problematic for my econometric analysis. My individual-level data do not include certification status. A corresponding licensing variable is less useful for attorneys or cosmetologists, since a license is required by

[17]For a more extended analysis of the difference between licensing and certification of professionals, see Klee (2010) and Shapiro (1986).

law to practice in these occupations. Those individuals reporting to be attorneys or cosmetologists must be subject to licensing regulations, and their training behavior might be affected by the relative stringency of these regulations.[18] By contrast, some individuals reporting to be accountants or teachers might be uncertified, and thus might not be subject to or affected by the stringency of regulations.[19] However, my econometric analysis does not disregard accountants and teachers. Rather, I acknowledge that any estimated effect of licensing policy for these occupations is a weighted average of potentially heterogeneous treatment effects on certified and uncertified practitioners. The estimates of λ_c^y and λ_c^o will be attenuated if the true effect of professional licensing policy on the training behavior of uncertified practitioners is zero.

A second potential disadvantage of the regulations dataset is that the relative stringency of each policy dimension is observed at intervals ranging from two to five years. The length of these intervals varies by occupation and by observation year within occupation.[20] These gaps between observations limit the frequency at which individual-level observations may be utilized in estimation. As a result, precision of estimates would be more concerning, especially because I condition on state and membership in a three-digit occupation. For attorneys and cosmetologists, I also observe time frames over which the observed regulations are relevant. However, even this information is often insufficient to make the regulations continuously observed.

[18]This assumes that practitioners comply with the law and that occupational affiliation is measured without error.

[19]This issue is less problematic for teachers than for accountants. I used the data described in Kleiner and Krueger (2013) to determine the relative frequencies of certified and uncertified practitioners. This dataset includes self-reported certification status. Among the 69 reported K-12 teachers, only 10 reported not being certified. However, among the 27 reported accountants and auditors, 19 reported not being certified.

[20]Regulations for accountants were observed five times over the span 1980-98, with the intervals between observations lasting from three to five years. Regulations for attorneys were observed five times over the span 1980-99, with the intervals between observations lasting from four to five years. Regulations toward cosmetologists were observed five times over 1981-98, with the intervals between observations lasting from three to five years. Regulations toward teachers were observed six times over 1984-98, with the intervals between observations lasting from two to four years.

I endeavor to mitigate the problem of infrequent observation by assuming that the relative stringency of a policy dimension remained unchanged between two temporally adjacent observations if these observations exhibit the same relative stringency. This assumption would introduce measurement error into the data if a policy's relative stringency changed and returned to its previous value between observations.

4.2 CPS Supplements

Individual-level training information comes from two supplements to the CPS. First, I employ the School Enrollment Supplement, which is administered annually in October. I use these supplements from 1980-84 and from 1987-99. Second, I employ the Occupational Mobility, Job Tenure, and Training Supplement, which was administered in January of 1983 and 1991. Individual-level demographic information comes from the basic monthly CPS, which is available for both supplements.

The School Enrollment Supplement asks respondents about current school enrollment at any level. I use the answer to the following question as a measure of participation in training:

> *Excluding (regular college courses and) on the job training, is ... taking any business, vocational, technical, secretarial, trade, or correspondence courses?*

I assume that this question indicates whether the respondent was taking a course to acquire skills relevant for the current occupation.[21] As a result, this variable is

[21] There is insufficient data about the respondent's motivation for enrolling in such a class to assess the validity of this assumption. For instance, we cannot observe whether the class helped generate general skills, skills specific to the current occupation, or skills specific to another occupation. This is potentially problematic if we hope to understand better the relationship between licensing policy and service quality inputs by examining the relationship between licensing policy and training. Only general skills and skills specific to the current occupation feasibly serve as inputs to the production of service quality among licensed practitioners. However, I expect that licensing policy stringency will affect service quality inputs more strongly than it affects training in general. In particular, I expect that licensed practitioners will be more likely to acquire skills specific to another occupation in states and occupations with relatively less stringent licensing policies. By imposing a barrier to entry,

a coarse measure of the respondent's investment in human capital. The information gleaned in this class might aid the production of service quality. Given the absence of tenure variables in this dataset, this enrollment variable has the advantage of being a flow measure of training. Additionally, the high frequency at which this enrollment variable is observed allows for relatively precise estimates of the effect of licensing on this training measure.

The Occupational Mobility, Job Tenure, and Training supplement asks respondents about attachment to their current jobs. I use the answer to the following question as a measure of participation in training:

> *Since you obtained your present job did you take any training to improve your skills?*

I assume that this question indicates whether the respondent was taking training to acquire skills relevant for the current occupation.[22] As a result, this variable is a coarse measure of the respondent's investment in human capital. The information gleaned from this training might aid the production of service quality. While this question does not yield a flow measure of training, the dataset also allows me to control for employer tenure. The relatively low frequency at which this training variable is observed might yield relatively imprecise estimates of the effect of licensing on this training measure. Furthermore, I observe this training variable only once during my panel of regulations for teachers. Consequently, the estimates presented in Section 5 for teachers do not

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professional licensing policies screen out potential practitioners with the weakest ties to the licensed occupation. States imposing the strongest barriers to entry thus select for the practitioners who are relatively less likely to change occupations and who are thereby less likely to acquire skills specific to a different occupation. For this reason, a given differential in training between licensed practitioners in a state with more lax licensing policies and those in a state with more stringent licensing policies amounts to a larger differential in service quality inputs between licensed practitioners in these two states.

[22] However, such training might instead reflect specific training which cannot be used in the licensed occupation. Similar to the School Enrollment Supplement, the Occupational Mobility, Job Tenure, and Training Supplement does not include a variable which would allow me to verify this assumption. My estimates of the effects of licensing on training will be biased if respondents systematically train for alternative occupations in states with more stringent licensing regulations.

reflect longitudinal variation in training.

Table 3 reports summary statistics for these two measures of training. The point estimate of the percentage of workers currently enrolled in a vocational class is relatively small in all four occupations. The point estimate of the percentage of workers who have attended training during the current job spell is considerably higher. One potential explanation for this difference is that the vocational class enrollment variable measures enrollment at a particular point in time, while the training variable measures enrollment at any time during a potentially long interval. For example, 5.899% of accountants are estimated to be enrolled in a vocational class. During their 7.156 years of employer tenure on average, 55.812% of accountants have attended training since the present job began. The corresponding estimates for attorneys, cosmetologists, and teachers are broadly comparable in magnitude.

One desirable aspect of the CPS and its supplements is the large sample size, both cross-sectionally and longitudinally. Precision of estimates is generally a central concern when assessing the effect of professional licensing on some outcome of interest. Since conditioning on state and membership in a particular three-digit occupation drastically narrows the estimation sample, precision is a serious problem for all but the largest nationally representative datasets.

One disadvantage of the CPS is that it includes only state of residence on the public use file. However, employer state determines the set of licensing regulations relevant for a particular practitioner. In the absence of data on employer state, I assume that respondents live and work in the same state. My estimates of the effects of licensing will be biased if practitioners who find human capital acquisition relatively more expensive systematically work in states with less stringent regulations than their state of residence.[23]

[23]The distinction between employer state and state of residence is less problematic for my analysis of accountants and attorneys. Many states prohibit these two types of licensed practitioners from residing outside the state.

5 Results

This section presents the estimates of the model given by (1) and (2). I begin with the vocational class enrollment outcome. I then present the results for training since the current job began. Finally, I examine the cumulative effect on the supply of practitioners as implied by a wage regression. If more stringent licensing regulations restrict the supply of practitioners, we will observe a licensing wage premium.

5.1 Vocational Class Enrollment

Tables 4-7 contain the estimated marginal effects of licensing regulations on vocational class enrollment. Panel A of each table presents the estimated effects for the younger cohort, which includes respondents age 35 and younger. Panel B of each table presents the estimated effects for the older cohort. I consider two types of specifications for each measure of training, where only λ_c^y and λ_c^o vary across specifications. The first constrains all of the elements of the parameter vector λ_c^y to be equal, and a similar set of constraints is placed on λ_c^o. The estimates of this specification with and without state fixed effects are presented in columns (1) and (3), respectively. I interpret the resulting estimates of λ_c^y and λ_c^o as capturing the effect of overall licensing policy on training behavior. The second specification drops the constraints on λ_c^y and λ_c^o. The estimates of this specification with and without state fixed effects are presented in columns (2) and (4), respectively. I interpret the resulting estimates of λ_c^y and λ_c^o as capturing the effect of each dimension of licensing policy on training behavior.[24]

Table 4 presents the estimates for accountants. Column (1) illustrates that overall licensing policy does not seem to have an effect on class enrollment for younger practitioners. Furthermore, column (2) illustrates that only one dimension of policy has

[24] An equivalent description of the difference between the two specifications is one in which only the vector L_{cst} varies across specifications. In the first, L_{cst} has only one element: the sum of the stringency variables for each observed policy dimension for occupation c in state s and year t. In the second, L_{cst} has as many elements as there are observed policy dimensions for occupation c in state s and year t, with each element containing a different stringency variable.

a statistically significant estimated effect on class enrollment, and its sign is unexpected. Accountants in states with more stringent graduate education requirements are 2.7% (standard error 1%) less likely to be enrolled in a vocational class outside of on-the-job training. Neither overall licensing policy nor any dimension of policy seems to be related to class enrollment for older practitioners.

Table 5 displays the pattern for attorneys. Overall licensing policy seems to have no effect on vocational class enrollment for younger practitioners. Only practitioners in states requiring attorneys to pass an exam in order to transfer a license across states seem to be affected by licensing policy. These practitioners are less likely to be enrolled in a vocational class outside of on-the-job training. Neither overall licensing policy nor any dimension of policy significantly seems to affect class enrollment for older practitioners.

Table 6 shows the estimates for cosmetologists. Younger cosmetologists in states with more stringent licensing policies are no more likely to be enrolled in a vocational class. Neither overall licensing policy nor any dimension of licensing policy seems to affect these practitioners. Older cosmetologists are also unaffected.

Evidence of a statistically significant relationship between licensing policy and class enrollment is also scant for teachers, as Table 7 indicates. Younger teachers in states not accepting an education degree are 1.3% (standard error 0.6%) less likely to be enrolled. Older teachers in states with more stringent policies overall are less likely to be enrolled. No other measure of professional licensing policy seems to have an effect on the enrollment of either younger or older teachers.

Several patterns emerge upon comparing across occupations the estimated relationship between licensing and class enrollment. There is no evidence that more stringent licensing policies increase class enrollment. The relative precision with which these effects are estimated strengthens the conclusion that licensing is unrelated to vocational class enrollment. To the extent that licensing policy does affect class enrollment, I estimate this effect to be negative. One potential explanation for this lack of evidence of a positive relationship is that there is relatively little variation in class enrollment, as Table 3 indicates. If relatively few practitioners enroll in vocational courses, there might be little scope for different licensing policies to impact enrollment. Consequently, it would be desirable to estimate the relationship between licensing policy and a training variable which exhibits more variation.

Finally, there is mixed evidence regarding how the inclusion of state fixed effects impacts estimates. Table 4 and Table 6 show that for accountants and cosmetologists the statistical significance of estimated coefficients does not depend on whether we include state fixed effects. By contrast, Table 5 and Table 7 show that the inclusion of state fixed effects does sometimes change the statistical significance of estimates for attorneys and teachers. However, for all four occupations point estimates of marginal effects change by no more than 3 percentage points after the inclusion of state fixed effects. This suggests that any omitted variable bias due to the exclusion of state-level unobservables is small.

5.2 Training Since the Current Job Began

Tables 8-11 contain the estimated marginal effects of licensing regulations on training since obtaining the current job. When estimating the effect of licensing policy on this stock measure of training, omitting job tenure might yield biased estimates of λ_c^y and λ_c^o. Individuals with longer job tenure are more likely to answer affirmatively, since they have had more opportunities to acquire training since obtaining the current job. If individuals are less mobile across jobs in states with more stringent licensing policies, this would lead to a positive bias in the estimate of λ_c. Although the Occupational Mobility, Job Tenure, and Training Supplement does not include information on job tenure, it does include an employer tenure variable. Consequently, I include employer tenure in X_{icst}, exploiting the positive correlation between employer tenure and job tenure. This constitutes the only change to the regressor set from section 5.1.

Table 8 presents the estimates for accountants. Accountants are more likely to have acquired training in states requiring candidates to pass failed exam sections in a shorter time window and in states requiring more experience. By contrast, accountants are less likely to have acquired training in states requiring better performance on failed exam sections. These relationships are apparent for younger accountants and older accountants. Furthermore, states requiring more graduate accounting education are also states in which younger accountants are more likely to have acquired training. Overall licensing policy seems to have no effect on accountants' training behavior, perhaps due to the counteracting estimated effects of the individual dimensions of policy.

Table 9 presents the results for attorneys. Attorneys in states requiring an exam in order to transfer a license across states are 19.2% (standard error 6.5%) more likely to have acquired training since obtaining their current job. Evidence of such an effect exists for both younger attorneys and older attorneys. Nevertheless, overall licensing policy does not seem to impact training behavior for either younger or older attorneys.

The evidence for cosmetologists is consistent with that for accountants and attorneys, as seen in Table 10. Cosmetologists in states requiring at least a high school degree or equivalent are more likely to have obtained training since the current job began. This effect is evident for both younger and older cosmetologists. Additionally, younger cosmetologists are 18.4% (standard error 10.1%) more likely to have acquired training on the current job in the presence of one additional relatively stringent dimension of policy.

The estimates shown in Table 11 stand in slight contrast to those presented in Tables 8-10. Teachers in states not accepting a professional education degree are 19.6% (standard error 3.3%) less likely to have acquired training since the current job began. Additionally, overall licensing policy stringency is negatively associated with the probability of having acquired training.[25] These two effects hold for both younger teachers and older teachers. However, older teachers in states requiring a subject major or minor for secondary school teachers are more likely to have acquired training.[26]

Several patterns emerge upon comparing across occupations the estimates of the effect of licensing on training since the current job began. First, several dimensions of licensing policy seem to display strong positive impacts on accountants' training. One potential explanation for this finding is that these estimates reflect mainly the compliance mechanism. Many states require accountants to accumulate relevant work experience before they become licensed. Consequently, some of the accountants observed in the CPS might be acquiring training in order to comply with observed regulations.[27] Second, for all four occupations, there is evidence that more stringent licensing policies are positively related to training. Third, examples from all four occupations illustrate that the inclusion of state fixed effects can change the statistical significance of estimated marginal effects more for this outcome than for class enrollment. Finally, precision is much more of a concern for this outcome than for class enrollment. Precision is an especially relevant issue for attorneys and cosmetologists, since these are smaller occupations compared to accountants and teachers.

5.3 Wages

Sections 5.1 and 5.2 present mixed evidence regarding the relationship between professional licensing policy and training. There is a small, negative association, if any,

[25]These findings, together with the negative relationship between licensing policy stringency and vocational class enrollment, are consistent with the evidence presented by Hanushek and Pace (1995). They conclude that teachers are less likely to complete training in states with more stringent licensing requirements.

[26]Note that these effects are estimated relatively precisely, considering that they do not reflect longitudinal variation in training behavior.

[27]Alternatively, these accountants might be acquiring training in order to comply with unobserved regulations which are correlated with observed regulations.

between licensing and vocational class enrollment. There is some evidence of a positive association between licensing and training since obtaining the current job. The former relationship suggests that licensing regulations do not restrict the supply of practitioners. The latter relationship suggests that some licensing regulations do restrict the supply of practitioners on balance. In order to gauge the cumulative effect of licensing policy on the supply of practitioners, I estimate the effect of professional licensing on wages. If licensing regulations effectively limit supply, we will encounter evidence of a licensing wage premium. To that end, I estimate the following model:

$$\ln W_{icst} = X_{icst}\beta_c^w + L_{cst}\lambda_c^w + \delta_{cs}^w + \gamma_{ct}^w + \varepsilon_{icst}^w \tag{3}$$

The parameter of interest in this model is λ_c^w. This model does not allow for cohort effects. The set of regulations that is most relevant for wages includes the current regulations. These affect recent flows into the occupation, and thereby the current stock of practitioners. The size of this stock is expected to have a similar effect on the wages of older and younger practitioners.

Table 12 contains the estimates of this model for accountants. Column (1) of this table illustrates that overall licensing policy has a statistically insignificant estimated effect on log wages. Column (2) provides some insight into this inference. Accountants in states requiring better performance on failed exam sections earn a 16.3% (standard error 6.5%) wage premium. On the other hand, accountants in states requiring more graduate education receive a 10.1% (standard error 6.1%) wage discount. This unexpected result suggests that more stringent graduate education requirements do not restrict entry among potential accountants, but rather they stimulate entry. No other dimension of professional licensing policy seems to be related to wages.

Table 13 presents the estimates of the wage regression for attorneys. Attorneys in states requiring an exam to transfer a license across states earn significantly smaller wages. No other measure of licensing policy has a statistically significant estimated effect on wages.

Table 14 includes the estimates of the wage regression for cosmetologists. Cosmetologists seem to earn smaller wages in states with more stringent licensing policies overall. Cosmetologists receive a 15% (standard error 6.6%) wage discount in the presence of one additional relatively stringent dimension of licensing policy. Additionally, cosmetologists in states requiring relatively more cosmetology education earn significantly smaller wages.

Table 15 presents the estimates for teachers. Teachers in states requiring an exam to become certified or to enter a teacher training program earn significantly higher wages. By contrast, teachers in states requiring a subject major or minor for secondary school teachers earn significantly lower wages. Overall licensing policy has a statistically insignificant estimated effect on teachers' wages.

The patterns evident in Tables 12-15 pose a stark contrast to the existing empirical evidence regarding the wage effects of licensing. The literature generally has documented a significant wage premium associated with licensing.[28] I find limited evidence of such a relationship. Nevertheless, the results presented here are generally consistent with the evidence discussed by Kleiner (2006).[29] He estimates the effect of

[28]Evidence of a wage premium has been documented using a variety of identification and estimation techniques. Kleiner (2000) compares average incomes across licensed and comparison unlicensed occupations. Kleiner and Krueger (2010) exploit variation across individuals in self-reported licensure status to estimate the wage effects of licensing. Kleiner (2006) uses individuals' mobility across licensed and unlicensed occupations to estimate the wage effects of licensing. While many of the estimates of the licensing wage premium rely on across-occupation variation, others use only within-occupation variation. Kugler and Sauer (2005) exploit the institutional details of a relicensing program to obtain an instrumental variables estimate of the wage effects of licensing for physicians in Israel. Kleiner (2006) also uses variation across states for occupations that are licensed in some states but not in others in order to estimate the wage effects of licensing. Previous studies have also documented evidence of a licensing wage premium among the occupations I consider. Angrist and Guryan (2008) find mixed evidence suggesting the existence of an earnings premium in states requiring teachers to pass an exam. Adams, Jackson and Ekelund (2002) present evidence that more stringent general and specific education regulations are associated with higher prices for cosmetology services, which might be associated with increased incomes.

[29]The results presented here are also generally consistent with the findings of Gittleman and Kleiner (2013). They pursue a variety of identification strategies to estimate the wage effects of membership in a state and occupation which are covered by a licensing law. They conclude that in

licensing policy on wages using decennial Census data and the same panel of licensing regulations that I use. He finds no statistically significant effects for attorneys or teachers. In contrast to the results presented in Table 14, Kleiner (2006) estimates a statistically significant, positive effect of professional licensing on cosmetologists' wages.

The wage analysis presented here differs from the one in Kleiner (2006) in two important ways. First, Kleiner (2006) does not allow each dimension of licensing policy to affect wages differentially. The results presented here suggest that the effects of the individual dimensions do vary, and might even be offsetting. Second, I control for state-level unobserved heterogeneity, whereas Kleiner (2006) does not. I estimated a model that does not include state fixed effects and found broadly similar results to those presented here.

The scant evidence of a licensing wage premium presented in Tables 12-15 is consistent with the results discussed in Sections 5.1 and 5.2. To the extent that there is a relationship between licensing regulations and these two measures of training, I find the estimated effects to be countervailing. These results suggest that the cumulative effect of licensing regulations on the supply of practitioners is small.

6 Conclusion

In this paper, I estimate the effects of professional licensing policy on two measures of training. The individual-level data come from two supplements to the Current Population Survey. The regulations data come from a panel containing rich variation in licensing policy for four diverse occupations. I exploit this variation to identify the effect of each dimension of licensing policy on training. Together with the size of the individual-level dataset, this rich variation allows me to mitigate concerns regarding

general there is no statistically significant wage effect of licensing coverage when they restrict their analysis to exploit only wage variation within detailed occupation.

the precision of estimates. Finally, I control for state-level unobserved heterogeneity and membership in a particular three-digit occupation.

There is no evidence that professional licensing is positively related to class enrollment. To the extent that this relationship exists, I estimate it to be negative. I find some evidence of a positive effect of licensing on a stock measure of training, controlling for tenure. These countervailing effects suggest that licensing regulations are slightly positively correlated with training, on balance. These inferences generally hold for each of the diverse occupations in the licensing regulations data. I find evidence from wage regressions which suggests that the cumulative effect of licensing regulations on the supply of practitioners is small; there is limited evidence of a licensing wage premium.

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Table 1: Observed Dimensions of Policy and Stringency Criteria

Requirement type	Stringent if...
Accountants	
Time to pass failed exam section	$\leq$ 5 exam windows
Min score on failed exam section	$\geq$ 50%
Accounting experience	$\geq$ 3 years of work
Graduate accounting	$\geq$ 30 semester hours
Undergraduate accounting	$\geq$ 24 semester hours
Attorneys	
Transfer license across states	Exam required
Degree from ABA-approved law school	Required
Cosmetologists	
General education	HS degree or equivalent
Cosmetology education	$\geq$ 1650 credit hours
Passing exam score	$\geq$ 75%
Teachers	
Professional Education Degree	Not Accepted
Education for (required) 2nd stage certificate	Master's or 5th year
Exam	Required
Subject major or minor for secondary school teachers	Required

Source: Licensing restrictions data from Kleiner (2006)
Notes for Accountants: Some states only require candidates to retake failed exam sections, as opposed to retaking the entire exam. Among these states, some states limit the amount of time in which candidates must pass all exam sections. One exam window is a period of three months, within which an exam may be taken only once. Additionally, some states require candidates performing poorly enough on the failed section to retake the entire exam. The graduate accounting policy variable includes the 150 hour rule. It is possible in some states to satisfy this rule without taking graduate-level accounting courses. Regulations vary by state.
Notes for Teachers: Some states followed a tiered certification system during the time frame I examine. This system initially grants teachers a first stage certificate. Teachers may obtain more advanced certificates, such as the second stage certificate, by satisfying additional requirements. Among the states with a tiered certification system, some states require teachers to obtain a second stage certificate. The exam policy variable includes exams required to obtain a license, as well as exams required to enter a teacher education program.

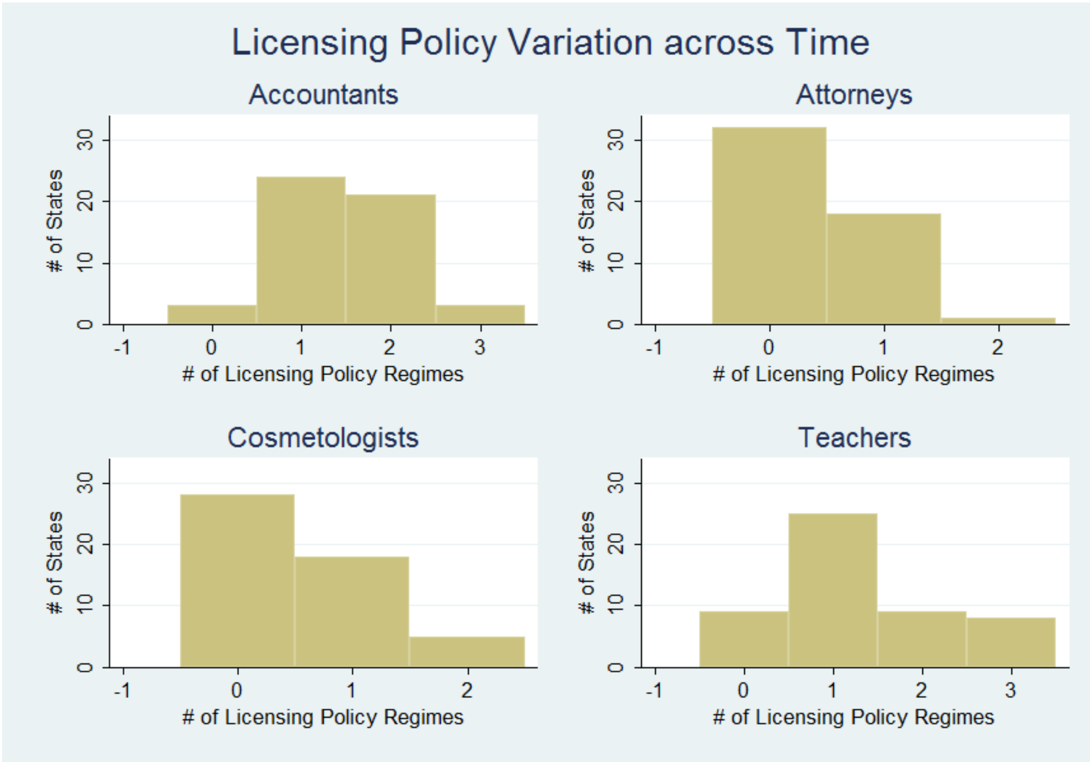

Figure 1: This figure plots the number of policy changes observed for each state in my panel of regulations. A regime change is observed if any dimension of policy changes between observations. The height of each bar represents the number of states in that bin. The maximum number of regimes is five for accountants, attorneys, and cosmetologists and six for teachers. These values are achieved when some dimension of a state's policy changes between each observation.
Source: Author's calculations from licensing restrictions data of Kleiner (2006)

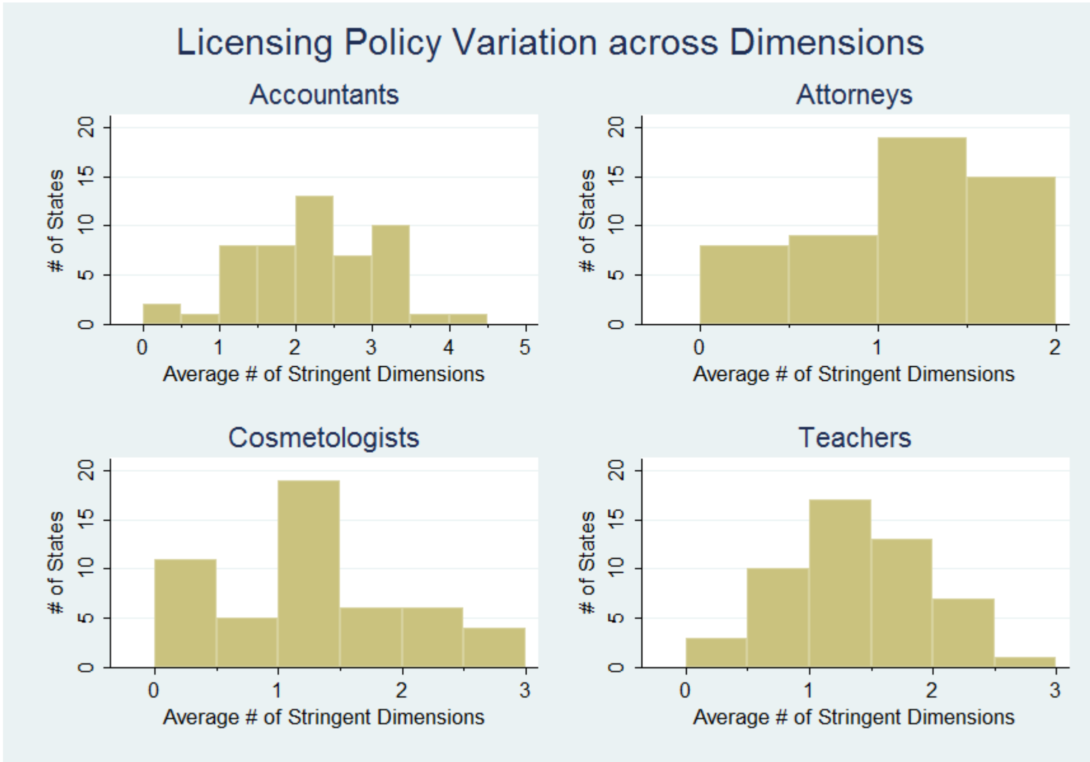

Figure 2: This figure plots within-state averages over time of the number of stringent policy dimensions for each occupation. The height of each bar represents the number of states with averages in that bin. The maximum value that a within-state average over time can take is five for accountants, two for attorneys, three for cosmetologists, and four for teachers. These values are achieved when each dimension of a state's policy is categorized as stringent in each observation year. Averages were computed before imputing policy stringency between observations exhibiting the same stringency.
Source: Author's calculations from licensing restrictions data of Kleiner (2006)

Table 2: Licensing Policy Variation within Dimensions

Requirement type	% Stringent
Accountants	
Time to pass failed exam section	35.645%
Min score on failed exam section	64.391%
Accounting experience	26.367%
Graduate accounting	12.226%
Undergraduate accounting	66.957%
Attorneys	
Transfer license across states	43.620%
Degree from ABA-approved law school	67.231%
Cosmetologists	
General education	21.093%
Cosmetology education	32.711%
Passing exam score	59.868%
Teachers	
Professional Education Degree	12.517%
Education for (required) 2nd stage certificate	14.661%
Exam	81.672%
Subject major or minor for secondary school teachers	19.973%

Source: Author's calculations from licensing restrictions data of Kleiner (2006)
General Note: This table presents the percentage of state-years exhibiting stringent policies. Percentages were computed after imputing stringency between observations exhibiting the same stringency. Computations before this imputation differed only slightly from those in this table.
Notes for Accountants: Some states only require candidates to retake failed exam sections, as opposed to retaking the entire exam. Among these states, some states limit the amount of time in which candidates must pass all exam sections. One exam window is a period of three months, within which an exam may be taken only once. Additionally, some states require candidates performing poorly enough on the failed section to retake the entire exam. The graduate accounting policy variable includes the 150 hour rule. It is possible in some states to satisfy this rule without taking graduate-level accounting courses. Regulations vary by state.
Notes for Teachers: Some states follow a tiered certification system, in which teachers are initially granted a first stage certificate. Teachers may obtain more advanced certificates, such as the second stage certificate, by satisfying additional requirements. Among the states with a tiered certification system, some states require teachers to obtain a second stage certificate. The exam policy variable includes exams required to obtain a license, as well as exams required to enter a teacher education program.

Table 3: Individual-level Summary Statistics

	Accountants	Attorneys	Cosmetologists	K-12 Teachers
Class Enrollment (%)	5.899	3.673	3.126	3.951
Training (%)	55.812	61.915	51.487	74.813
Employer Tenure	7.156	8.941	7.169	9.298
Weekly Earnings ($)	898.11	1535.30	361.01	801.78
Hours Last Week	40.547	45.852	34.487	40.110
Male (%)	49.526	77.517	9.323	24.951
Age	38.089	41.417	37.343	40.154
College Grad (%)	70.597	97.377	9.062	89.434
SMSA (%)	89.864	91.279	76.838	75.677

Source: Author's calculations from the Current Population Survey.
Table presents sample weighted averages by occupation of percent currently enrolled in a vocational class, percent acquiring training since obtaining the current job, employer tenure, weekly earnings (measured in 2009 dollars), hours worked last week, percent male, age, percent college graduates, and percent living in a metropolitan area. Vocational class enrollment is taken from the School Enrollment Supplement to the CPS (October 1980-84, 1987-99). Training since acquiring the current job and employer tenure are taken from the Occupational Mobility, Job Tenure, and Training Supplement (January 1983, 1991). The remaining variables are taken from the Basic Monthly CPS.

Table 4: Accountants' Class Enrollment

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	-0.011		-0.002	
	(0.008)		(0.003)	
Consecutive Exams		0.003		0.005
		(0.018)		(0.009)
Minimum Failing Score		-0.015		-0.003
		(0.015)		(0.008)
Experience		-0.008		-0.005
		(0.019)		(0.008)
Graduate Accounting Education		-0.027***		-0.021*
		(0.010)		(0.011)
Undergraduate Accounting Education		0.005		0.005
		(0.015)		(0.008)
Panel B: λ_c^o				
Summary	-0.007		0.003	
	(0.008)		(0.003)	
Consecutive Exams		-0.004		-0.001
		(0.016)		(0.009)
Minimum Failing Score		-0.004		0.010
		(0.016)		(0.008)
Experience		-0.003		-0.000
		(0.019)		(0.007)
Graduate Accounting Education		-0.020		-0.010
		(0.014)		(0.011)
Undergraduate Accounting Education		0.007		0.007
		(0.015)		(0.008)
Pseudo- R^2	0.0544	0.0555	0.0460	0.0477
Observations	9041	9041	9041	9041
State-Year Clusters	593	593	593	593
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data and the Current Population Survey. Note: Table presents marginal effects of a probit regression of class enrollment indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the class enrollment of the younger cohort, while λ_c^o contains the effect of licensing on the class enrollment of the older cohort. Younger practitioners are assumed to be of age 35 and younger. Here, the specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. The estimation sample is only accountants. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 5: Attorneys' Class Enrollment

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	0.003		-0.000	
	(0.010)		(0.006)	
Exam		-0.022***		-0.005
		(0.008)		(0.007)
ABA-approved Law School		0.024		0.004
		(0.018)		(0.008)
Panel B: λ_c^o				
Summary	0.010		0.007**	
	(0.009)		(0.003)	
Exam		-0.008		0.022***
		(0.012)		(0.006)
ABA-approved Law School		0.014		-0.005
		(0.013)		(0.005)
Pseudo- R^2	0.0794	0.0841	0.0475	0.0550
Observations	7076	7076	7320	7320
State-Year Clusters	753	753	826	826
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of class enrollment indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the class enrollment of the younger cohort, while λ_c^o contains the effect of licensing on the class enrollment of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only attorneys. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 6: Cosmetologists' Class Enrollment

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	-0.014		-0.006	
	(0.009)		(0.004)	
General Education		-0.014		-0.001
		(0.010)		(0.009)
Cosmetology Education		-0.005		-0.007
		(0.013)		(0.007)
Minimum Exam Score		-0.014		-0.007
		(0.012)		(0.006)
Panel B: λ_c^o				
Summary	-0.007		0.003	
	(0.009)		(0.004)	
General Education		-0.013		0.002
		(0.011)		(0.009)
Cosmetology Education		0.003		0.002
		(0.017)		(0.011)
Minimum Exam Score		-0.006		0.004
		(0.013)		(0.007)
Pseudo- R^2	0.0740	0.0744	0.0382	0.0384
Observations	5659	5659	5938	5938
State-Year Clusters	680	680	745	745
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of class enrollment indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the class enrollment of the younger cohort, while λ_c^o contains the effect of licensing on the class enrollment of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only cosmetologists. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 7: Teachers' Class Enrollment

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	-0.005		0.004*	
	(0.003)		(0.003)	
No Education Degree		-0.013**		-0.004
		(0.006)		(0.006)
Master's/5th Year		0.006		0.023***
		(0.010)		(0.008)
Exam		0.000		-0.003
		(0.006)		(0.006)
Subject Major/Minor		-0.004		0.001
		(0.007)		(0.007)
Panel B: λ_c^o				
Summary	-0.005*		0.004**	
	(0.003)		(0.002)	
No Education Degree		-0.008		0.002
		(0.006)		(0.004)
Master's/5th Year		-0.002		0.011**
		(0.008)		(0.004)
Exam		-0.000		-0.002
		(0.005)		(0.005)
Subject Major/Minor		-0.005		-0.000
		(0.006)		(0.005)
Pseudo- R^2	0.0633	0.0639	0.0418	0.0443
Observations	20562	20562	20562	20562
State-Year Clusters	552	552	552	552
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of class enrollment indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the class enrollment of the younger cohort, while λ_c^o contains the effect of licensing on the class enrollment of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only K-12 teachers. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 8: Accountants' Training Since Obtaining Current Job

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	0.011		0.009	
	(0.067)		(0.024)	
Consecutive Exams		0.370***		0.041
		(0.072)		(0.077)
Minimum Failing Score		-0.336***		0.018
		(0.060)		(0.059)
Experience		0.256***		0.012
		(0.061)		(0.050)
Graduate Accounting Education		0.321***		0.009
		(0.061)		(0.135)
Undergraduate Accounting Education		0.034		-0.058
		(0.081)		(0.048)
Panel B: λ_c^o				
Summary	0.046		0.032	
	(0.061)		(0.029)	
Consecutive Exams		0.368***		0.042
		(0.074)		(0.102)
Minimum Failing Score		-0.320***		0.023
		(0.086)		(0.087)
Experience		0.357***		0.130**
		(0.059)		(0.061)
Graduate Accounting Education		0.107		-0.144
		(0.118)		(0.112)
Undergraduate Accounting Education		0.041		-0.058
		(0.061)		(0.058)
Pseudo- R^2	0.1056	0.1083	0.0579	0.0661
Observations	806	806	814	814
State-Year Clusters	53	53	55	55
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the training of the younger cohort, while λ_c^o contains the effect of licensing on the training of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. The estimation sample is only accountants. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 9: Attorneys' Training Since Obtaining Current Job

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	0.096		0.061	
	(0.119)		(0.054)	
Exam		0.192***		0.002
		(0.065)		(0.058)
ABA-approved Law School		0.008		0.111*
		(0.208)		(0.066)
Panel B: λ_c^o				
Summary	0.076		0.061	
	(0.115)		(0.049)	
Exam		0.182***		0.028
		(0.069)		(0.058)
ABA-approved Law School		-0.015		0.090
		(0.199)		(0.059)
Pseudo- R^2	0.1275	0.1282	0.0338	0.0369
Observations	574	574	617	617
State-Year Clusters	69	69	81	81
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the training of the younger cohort, while λ_c^o contains the effect of licensing on the training of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only attorneys. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 10: Cosmetologists' Training Since Obtaining Current Job

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	0.184*		0.142**	
	(0.101)		(0.061)	
General Education		0.553***		0.355***
		(0.032)		(0.063)
Cosmetology Education		-0.007		0.045
		(0.121)		(0.089)
Minimum Exam Score		-0.006		0.057
		(0.103)		(0.081)
Panel B: λ_c^o				
Summary	0.141		0.117**	
	(0.103)		(0.055)	
General Education		0.485***		0.138
		(0.080)		(0.135)
Cosmetology Education		0.054		0.120
		(0.082)		(0.095)
Minimum Exam Score		0.012		0.081
		(0.100)		(0.068)
Pseudo- R^2	0.1240	0.1349	0.0486	0.0612
Observations	576	576	590	590
State-Year Clusters	81	81	88	88
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the training of the younger cohort, while λ_c^o contains the effect of licensing on the training of the older cohort. Young practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only cosmetologists. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 11: Teachers' Training Since Obtaining Current Job

	(1)	(2)	(3)	(4)
Panel A: λ_c^y				
Summary	-0.036***		0.023	
	(0.013)		(0.018)	
No Education Degree		-0.196***		0.016
		(0.033)		(0.061)
Master's/5th Year		-0.003		0.025
		(0.040)		(0.058)
Exam		0.027		-0.036
		(0.035)		(0.052)
Subject Major/Minor		-0.024		-0.008
		(0.037)		(0.073)
Panel B: λ_c^o				
Summary	-0.047***		0.003	
	(0.007)		(0.021)	
No Education Degree		-0.190***		0.003
		(0.029)		(0.043)
Master's/5th Year		-0.059		-0.014
		(0.044)		(0.057)
Exam		-0.025		-0.104***
		(0.021)		(0.038)
Subject Major/Minor		0.059**		0.078***
		(0.025)		(0.030)
Pseudo- R^2	0.1848	0.1887	0.1228	0.1309
Observations	1759	1759	1769	1769
State-Year Clusters	49	49	50	50
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and year effects. λ_c^y contains the effect of licensing on the training of the younger cohort, while λ_c^o contains the effect of licensing on the training of the older cohort. Younger practitioners are assumed to be of age 35 and younger. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only K-12 teachers. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 12: Accountants' Wages

	(1)	(2)
Summary	0.016	
	(0.029)	
Consecutive Exams		0.084
		(0.062)
Minimum Failing Score		0.163**
		(0.065)
Experience		0.098
		(0.079)
Graduate Accounting Education		-0.101*
		(0.061)
Undergraduate Accounting Education		0.014
		(0.044)
Adjusted R^2	0.3724	0.3744
Observations	2078	2078
State-Year Clusters	540	540
State Fixed Effects?	Y	Y

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents coefficients of an OLS regression of log weekly wages (measured in 2009 US dollars) on log weekly hours worked, white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and state and year effects. Column (1) includes all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Column (2) includes all dimensions of state licensure policy without restricting coefficients. This table presents the estimates of the parameter vector λ_c^w. The estimation sample is only accountants. Labor supply weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 13: Attorneys' Wages

	(1)	(2)
Summary	-0.054	
	(0.082)	
Exam		-0.224*
		(0.116)
ABA-approved Law School		0.012
		(0.102)
Adjusted R^2	0.3752	0.3756
Observations	1029	1029
State-Year Clusters	443	443
State Fixed Effects?	Y	Y

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents coefficients of an OLS regression of log weekly wages (measured in 2009 US dollars) on log weekly hours worked, white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and state and year effects. Column (1) includes all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Column (2) includes all dimensions of state licensure policy without restricting coefficients. This table presents the estimates of the parameter vector λ_c^w. The estimation sample is only attorneys. Labor supply weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 14: Cosmetologists' Wages

	(1)	(2)
Summary	-0.150**	
	(0.066)	
General Education		-0.147
		(0.096)
Cosmetology Education		-0.248**
		(0.105)
Minimum Exam Score		0.002
		(0.182)
Adjusted R^2	0.4036	0.4039
Observations	889	889
State-Year Clusters	440	440
State Fixed Effects?	Y	Y

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents coefficients of an OLS regression of log weekly wages (measured in 2009 US dollars) on log weekly hours worked, white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and state and year effects. Column (1) includes all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Column (2) includes all dimensions of state licensure policy without restricting coefficients. This table presents the estimates of the parameter vector λ_c^w. The estimation sample is only cosmetologists. Labor supply weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Table 15: Teachers' Wages

	(1)	(2)
Summary	-0.006	
	(0.015)	
No Education Degree		0.014
		(0.033)
Master's/5th Year		-0.014
		(0.037)
Exam		0.056**
		(0.026)
Subject Major/Minor		-0.053**
		(0.023)
Adjusted R^2	0.4843	0.4846
Observations	5222	5222
State-Year Clusters	569	569
State Fixed Effects?	Y	Y

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents coefficients of an OLS regression of log weekly wages (measured in 2009 US dollars) on log weekly hours worked, white indicator, black indicator, male indicator, married indicator, a quadratic in age, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and state and year effects. Column (1) includes all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Column (2) includes all dimensions of state licensure policy without restricting coefficients. This table presents the estimates of the parameter vector λ_c^w. The estimation sample is only K-12 Teachers. Labor supply weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

A Appendix: Regulations in Effect at Occupational Entry

I estimate the following model in order to assess the relationship between the licensing regulations relevant at entry into occupation c and training, given by $Train_{icst}$:

$$Train^*_{icst} = X_{icst}\beta_c + L^e_{icst}\lambda^e_c + L^{nm}_{icst}\lambda^{nm}_c + \delta_{cs} + \gamma_{ct} + \varepsilon_{icst} \tag{4}$$

$$Train_{icst} = \begin{cases} 1 & \text{if } Train^*_{icst} > 0 \\ 0 & \text{if } Train^*_{icst} \leq 0 \end{cases} \tag{5}$$

The measure of training is the flow measure from the Occupational Mobility, Job Tenure, and Training Supplement, which also includes information about employer tenure and occupational tenure. The availability of occupational tenure enables me to compute an implicit year of entry into occupation c for the respondent. I then define L^e_{icst} to include the regulations that were relevant when the respondent entered the occupation, assuming no interstate mobility. Respondents whose implied entry occurred before my regulations panel begins cannot confidently be matched to the regulations in effect at entry. For these respondents, L^e_{icst} is defined to be zero and L^{nm}_{icst} contains the first observed set of licensing regulations in occupation c. The parameter estimates that I am mainly concerned with are λ^e_c. I am also interested in λ^{nm}_c to some extent, since the first observed regulations might closely resemble those in effect at the respondent's implied entry date. As in the model for class enrollment, I consider two specifications. The first constrains all of the elements of the parameter vector λ^e_c to be equal, and a similar set of constraints is placed on λ^{nm}_c. These results are presented in columns (1) and (3). The second specification drops these constraints. These results are presented in column (2) and (4).

The estimated marginal effects of the model given by (4) and (5) for training since obtaining the current job are presented in Tables 16-19. Panel A of each table contains the estimate of the parameter vector λ_c^e, while Panel B of each table contains the estimates of the parameter vector λ_c^{nm}. Statistical power is much more of a concern for this regression, as illustrated by Tables 16-18 for accountants, attorneys, and cosmetologists, respectively. This measure of training is drawn from the Occupational Mobility, Job Tenure, and Training Supplement, which was administered only twice. Much of the remaining longitudinal variation is removed by the matching algorithm. A substantial fraction of respondents' implied occupational entry dates occurred before my panel of regulations begins.[30] Although the CPS is relatively large in the cross-section, this limited longitudinal variation results in noisy estimates of the marginal effects of licensing policy. Consequently, neither overall licensing policy nor any particular dimension of policy has a statistically significant estimated impact on training for practitioners in these occupations.

Despite this limited longitudinal variation, Table 19 reveals evidence of a statistically significant relationship between licensing policy at occupational entry and training. Younger teachers in states requiring a Master's degree or a fifth year of education in order to receive a required second stage teaching certificate are more likely to have acquired training. Younger teachers in states requiring an exam to become certified or enter an education program are less likely to have acquired training. Although these effects counteract each other, column (1) displays that the estimated effect of overall licensing policy on the probability of having acquired training is negative.

[30]Among 1983 respondents only 32.6% of accountants entered the current occupation during my panel of regulations. The corresponding statistics are 27.1% for attorneys, and 20.3% for cosmetologists. Among 1991 respondents, only 66.2% of accountants entered the current occupation during my panel of regulations. The corresponding statistics are 60% for attorneys, 54.9% for cosmetologists, and 35.2% for teachers.

Appendix Table 16: Accountants' Training in Current Job Spell

	(1)	(2)	(3)	(4)
Panel A: λ_c^e				
Summary	-0.052		-0.006	
	(0.042)		(0.016)	
Consecutive Exams		-0.137		-0.067
		(0.107)		(0.084)
Minimum Failing Score		-0.144		0.027
		(0.130)		(0.066)
Experience		-0.130		0.086**
		(0.092)		(0.034)
Graduate Accounting Education		-0.082		0.057
		(0.182)		(0.069)
Undergraduate Accounting Education		0.201		-0.046
		(0.138)		(0.038)
Panel B: λ_c^{nm}				
Summary	-0.042		-0.000	
	(0.041)		(0.024)	
Consecutive Exams		0.017		0.024
		(0.126)		(0.083)
Minimum Failing Score		-0.198		-0.028
		(0.163)		(0.077)
Experience		-0.139		0.059
		(0.095)		(0.051)
Graduate Accounting Education		-0.041		0.187
		(0.318)		(0.161)
Undergraduate Accounting Education		0.191		-0.043
		(0.137)		(0.048)
Pseudo- R^2	0.0994	0.1030	0.0529	0.0633
Observations	1177	1177	1178	1178
State-Year Clusters	91	91	92	92
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^e contains the effect of the regulations in effect at a practitioner's entry into the occupation, while λ_c^{nm} contains the effect of the earliest observable regulations on practitioners entering the occupation before the start of my panel of regulations. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. The estimation sample is only accountants. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Appendix Table 17: Attorneys' Training in Current Job Spell

	(1)	(2)	(3)	(4)
Panel A: λ_c^e				
Summary	0.009		0.068	
	(0.098)		(0.060)	
Exam		-0.102		-0.010
		(0.212)		(0.069)
ABA-approved Law School		0.052		0.127**
		(0.106)		(0.064)
Panel B: λ_c^{nm}				
Summary	0.031		0.045	
	(0.091)		(0.046)	
Exam		-0.084		0.034
		(0.227)		(0.057)
ABA-approved Law School		0.074		0.057
		(0.103)		(0.057)
Pseudo- R^2	0.1287	0.1291	0.0327	0.0363
Observations	595	595	633	633
State-Year Clusters	73	73	85	85
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^e contains the effect of the regulations in effect at a practitioner's entry into the occupation, while λ_c^{nm} contains the effect of the earliest observable regulations on practitioners entering the occupation before the start of my panel of regulations. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only attorneys. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Appendix Table 18: Cosmetologists' Training in Current Job Spell

	(1)	(2)	(3)	(4)
Panel A: λ_c^e				
Summary	-0.148		0.013	
	(0.090)		(0.056)	
General Education		-0.165		0.092
		(0.112)		(0.127)
Cosmetology Education		0.223		0.122
		(0.245)		(0.109)
Minimum Exam Score		-0.181		-0.044
		(0.179)		(0.075)
Panel B: λ_c^{nm}				
Summary	-0.028		0.111**	
	(0.094)		(0.047)	
General Education		-0.165		0.150
		(0.150)		(0.108)
Cosmetology Education		0.188		0.039
		(0.280)		(0.087)
Minimum Exam Score		-0.009		0.122**
		(0.195)		(0.060)
Pseudo- R^2	0.1286	0.1329	0.0429	0.0471
Observations	578	578	591	591
State-Year Clusters	83	83	89	89
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, self-employed indicator, licensing variables, and year effects. λ_c^e contains the effect of the regulations in effect at a practitioner's entry into the occupation, while λ_c^{nm} contains the effect of the earliest observable regulations on practitioners entering the occupation before the start of my panel of regulations. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only cosmetologists. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$

Appendix Table 19: Teachers' Training in Current Job Spell

	(1)	(2)	(3)	(4)
Panel A: λ_c^e				
Summary	-0.076**		-0.019	
	(0.038)		(0.015)	
No Education Degree		-0.106		0.009
		(0.169)		(0.048)
Master's/5th Year		0.107**		0.058
		(0.054)		(0.041)
Exam		-0.183***		-0.134**
		(0.050)		(0.053)
Subject Major/Minor		-0.110		0.150***
		(0.200)		(0.038)
Panel B: λ_c^{nm}				
Summary	-0.066*		-0.002	
	(0.037)		(0.013)	
No Education Degree		-0.100		-0.026
		(0.154)		(0.050)
Master's/5th Year		0.032		-0.022
		(0.066)		(0.040)
Exam		-0.054		0.017
		(0.059)		(0.025)
Subject Major/Minor		-0.330*		0.008
		(0.189)		(0.033)
Pseudo- R^2	0.1380	0.1421	0.1060	0.1122
Observations	3235	3235	3244	3244
State-Year Clusters	92	92	93	93
State Fixed Effects?	Y	Y	N	N

Source: Author's calculations from licensing restrictions data of Kleiner (2006) and the Current Population Survey.
Note: Table presents marginal effects of a probit regression of training since obtaining current job indicator on white indicator, black indicator, male indicator, married indicator, a quadratic in age, employer tenure, SMSA, some college indicator, college-plus indicator, part-time indicator, government-employed indicator, licensing variables, and year effects. λ_c^e contains the effect of the regulations in effect at a practitioner's entry into the occupation, while λ_c^{nm} contains the effect of the earliest observable regulations on practitioners entering the occupation before the start of my panel of regulations. The specifications presented in columns (1) and (2) also include state fixed effects. Columns (1) and (3) include all dimensions of state licensure policy, restricting the coefficients on each dimension to be equal. Columns (2) and (4) include all dimensions of state licensure policy without restricting coefficients. The estimation sample is only K-12 teachers. Sample weights are used in estimation. Standard errors are clustered at the state-year level. Robust standard errors of coefficient estimates are included in parenthesis below estimates. * $p < .1$; ** $p < .05$; *** $p < .01$