EFFECTS OF OCCUPATIONAL LICENSING LAWS ON MINORITIES: EVIDENCE FROM THE PROGRESSIVE ERA

2nd Draft—Do Not Cite.

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July 16, 2006

Abstract

This paper investigates the effect of occupational licensing regulation on the representation of minority (female and black) workers in a range of skilled and semi-skilled occupations representing 12 percent of the civilian labor force. We take advantage of a natural experiment afforded by the introduction of state-level licensing regulation during the late nineteenth and to mid twentieth centuries to identify the effects of licensing on minority representation. We find that licensing laws seldom harmed minority workers. In fact, licensing sometimes helped minorities, particularly in occupations where information about worker quality was difficult to ascertain.

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For classic economic reasons, we conjecture that the craft unions are more likely to have monopolistic powers than industrywide unions. Therefore we would expect to observe more such discrimination in the first type of union than in the second. And included in the category of craft unions are such organizations at the American Medical Association, and any profession in which admission involves the approval of a governing board.

(Alchian and Kessel 1962).

Occupational licensing coupled with white-dominated craft unions has been a particularly effective tool for reducing employment for Negroes.

(Williams 1982, p. 90-91)

I. INTRODUCTION

Many scholars have claimed that occupational licensing regulations disadvantage minorities such as women, blacks, and Jews in professional occupations (Kessel 1958, 1970; Alchian and Kessel 1962; Frech 1975; Sorkin 1977; Williams 1982). Licensing, it is argued, functions as a barrier that raises the costs of practicing a given occupation. Licensing regulations may disadvantage minorities, either because minorities find it more costly to obtain a license or because they are under-represented among incumbent practitioners and regulatory authorities. While in the first instance discrimination against minorities is an unintended consequence of licensing, in the second instance, licensing, by reducing the extent of competition, allows incumbent practitioners and regulatory authorities to further indulge in their taste for discrimination (Becker 1957).[1] In either case, however, the observable implications of licensing are the same: licensing will have a negative effect on the prevalence of disadvantaged groups within regulated occupations.

Occupational licensing need not have a negative effect on minorities, however. Indeed, whether licensing harms minorities will depend on the broader function that it serves. Generally, it is argued that occupational licensing reflects capture by the regulated

[1] The claim is not that there was no discrimination against minorities prior to licensing but rather that licensing may enhance the extent of discrimination. Because we employ a differences-in-differences approach, our empirical methodology already controls for state-specific discriminatory tastes as well as discriminatory tastes that are changing over time across all states.

group (Stigler 1971). To the extent that it introduces entry barriers that increase producer rents at the expense of efficiency, licensing may facilitate discrimination against disadvantaged groups. However, this is not the only role that licensing may play. Since Arrow (1963), economists have recognized that licensing can also help reduce informational asymmetries about professional quality (Akerlof 1970; Leland 1979). In fact, recent scholarship suggests that the desire to reduce informational asymmetries was an important motivation for the rise of licensing in early twentieth century America (Law and Kim 2005). If uncertainty about worker quality gives rise to statistical discrimination over observable characteristics like sex or race, then licensing regulation that serves as an improved signal of quality may help minority workers and increase their presence in regulated occupations (Lundberg and Startz 1983; Coate and Loury 1993).

Curiously, relatively little empirical work has systematically examined the relationship between occupational licensing and the representation of minorities or disadvantaged workers. This is a significant omission given that 20 percent of today's labor force continues to be licensed by state governments (Kleiner 2006). In this paper we attempt to remedy this deficiency. Specifically, we take advantage of the natural experiment afforded by cross-state and temporal variation in the adoption of licensing regulation across a broad sample of occupations representing approximately 12 percent of the civilian labor force during the late nineteenth and early twentieth centuries (the Progressive Era) to identify how licensing affected women and blacks. By including a range of occupations within the sample that represent a spectrum of high and low skill jobs, we are able to speak generally about the effects of licensing on minority groups. Additionally, for two occupations (teachers and physicians) we have data on specific

licensing requirements that allow us to measure licensing more precisely. By comparing the differential effect of the adoption of licensing regulation on the majority group (white men) and minorities in various occupations, we use a "difference-in-differences" estimator to determine the effect of licensing on minority group representation. Methodologically, our approach is similar to recent work that examines how exogenous changes in the extent of market competition affect the prevalence and performance of minorities in specific different industries (Heywood and Peoples 1984; Black and Strahan 2001).

The remainder of this paper is structured as follows. In section II we review the relevant empirical literature. Section III presents information on the evolution of licensing regulations for various occupations over time and discusses the validity of our identification strategy. Section IV discusses the data. Section V outlines the empirical methodology and presents the regression results. Section VI examines the effect of specific teacher and physician licensing requirements on the gender and racial composition of these two occupations. Section VII concludes.

II. LITERATURE REVIEW

To date much of the evidence on the effects of licensing on the presence of women and minorities in particular occupations has focused on aggregate time series trends on the female or minority share of the occupation during periods when licensing laws were introduced or strengthened. In particular, scholarship has examined changes in the share of blacks, women, and Jews in professions like medicine during the early decades of the twentieth century, an era when medical licensing regulations became

increasingly strict. Frech (1975), for instance, claims that stricter licensing requirements that were enacted in response to the Flexner Report of 1910 halted a steady increase in the percentage of black physicians; between 1890 and 1920, the black share of physicians increased from 0.9 percent to 2.7 percent, but subsequently declined to 1.4 percent by the late 1960s. Looking at medical school enrolment figures, Sorkin (1977) believes that stricter medical licensing regulations also reduced the share of female medical students. He notes that the percentage of female medical students peaked in 1910 and was greater in 1910 than in 1950. This negative correlation between periods of licensing and the female share of the occupation at the national level is taken as evidence that licensing harms women.

While these figures are suggestive of a possible relationship between licensing and minority representation, it would clearly be premature to conclude on the basis of this evidence that there is in fact a causal relationship at work. First, there is a problem of aggregation. One must be cautious about attributing changes in the composition of an occupation at a national level to changes in legislation that are enacted at the state level. Second, there is a problem of omitted variables. Clearly, many factors apart from licensing will influence the share of an occupation that is female or black over time. As a result, the effect of licensing on minority representation within an occupation is unlikely to be correctly identified.

An alternative approach to analyzing the effect of licensing on minority representation involves comparing licensing exam pass rates. In his examination of cosmetology licensing in Illinois and Missouri (two states that license cosmetologists) in the early 1970s, Dorsey (1980, 1983) finds that black license applicants were 30 percent

less likely to pass the licensing exam than white applicants, holding constant education and training. Along similar lines, it has also been found that black insurance license applicants in Illinois were far more likely to fail the licensing exam than white applicants (Revised Agent-Broker Exams 1978). The differential between white and black licensing exam pass rates is attributed to discrimination on the part of licensing authorities.

While this body of evidence is more compelling than the time series approach that merely analyzes national trends, it is still problematic. First, factors apart from education and training are likely to affect examination pass rates. Second, the evidence from these studies is limited to a small number of occupations in a small number of states. Third, in these studies, the effect of licensing is estimated by comparing white and black pass rates within states that already license the occupation. Identification of the effects of licensing is therefore limited entirely to one source of variation—race. If one is determined to use licensing pass rates as the outcome variable, this is clearly the only approach available (since licensing pass rates will only be generated by states that already license a given occupation) but in so doing, this approach ignores variation across states in the timing and nature of the licensing regime.

Methodologically, our approach takes its cue from the literature that examines how exogenous changes in market competition affect the representation and performance of minorities in certain industries. The specific hypothesis tested in these studies is whether competition in product markets influences the degree of employment discrimination. Since Becker (1957), it has often been argued that non-competitive product markets allow employers to indulge in their taste for discrimination. When product markets are less than competitive, employers earn above normal rents; employers

may spend these rents by hiring workers in a discriminatory fashion (Alchian and Kessel 1962). Variation in the degree of competition in product markets can therefore be used to test this hypothesis.

Within this literature, three approaches have been adopted. In the first approach, identification comes from variation in market structure across different industries. Some measure of industry concentration is used to proxy for the degree of market power in product markets and cross-sectional variation in industry concentration is exploited to test the proposition that greater product market competition reduces the degree of employment discrimination (Shepard 1969; Oster 1975; Luksetich 1979). The main problem with this approach is that omitted industry characteristics may bias the relationship between product market competition and employment discrimination. Hence, a second approach uses geographic variation in competitive market structure within a given industry to determine whether product market competition influences employment discrimination. Ashenfelter and Hannan (1986) adopt this approach in their analysis of sex discrimination in banking. They divide the state into different geographic regions and use firm level data on the share of female employees in banks in Pennsylvania in 1976 to measure the degree of employment discrimination. They find that the share of female employees at the firm level was higher in those regions where competition among banks was greatest.

A third approach uses changes in regulatory regime (which in turn, influence the degree of competition) rather than regional variation concentration ratios to identify the degree of product market competition. State or federal deregulatory legislation potentially offers a more exogenous source of variation in competitive market structure than

variation in concentration ratios. Two studies in particular stand out in this regard. The first, by Heywood and Peoples (1994), examines how federal deregulation of trucking influenced the prevalence of black truckers. Using individual-level data from the Current Population Survey (CPS), Heywood and Peoples find that federal trucking deregulation during the 1970s significantly increased the presence of blacks in the for-hire trucking sector. Since trucking is regulated at the federal level, identification in this study comes from a pre and post deregulation comparison. The second study, by Black and Strahan (2001), analyzes how changes in state-level banking regulation over a 20 year period (from the 1970s until the 1990s) affected the earnings of female versus male banking employees. Year and state variation in banking regulation is their source of identification. Using bank level data as well as individual level CPS data to measure employee earnings, Black and Strahan find that while deregulation lowered the earnings of all bank employees during the sample period, deregulation reduced the real wages of male workers faster than those of female workers. They also find that the share of managerial positions held by women increased following deregulation.

The approach we employ in this paper most closely resembles that of Black and Strahan. First, we use individual level data for our analysis. Second, identification comes from cross-state and temporal variation in state regulation (in our case, occupational licensing regulation). Licensing laws, like some banking regulations, fall within the jurisdiction of state governments. Additionally, during the Progressive Era, licensing laws were adopted by various states at different times. Depending on the specific occupation studied, variation comes from as many as 48 states over 8 different census years.

However, our approach differs from these studies in several important respects. First, because the Census of Population did not report data on earnings prior to 1950, we restrict ourselves to occupational status. Our dependent variable is thus whether an individual works in a given occupation in a given census year. Second, whenever possible, we examine two types of minority status: minority sex status (female) and minority race status (black). Third, because our focus is on occupational licensing regulation, there is an important difference in interpretation. In these other studies, the hypothesis under investigation is whether changes in product market competition change the extent of discrimination by employees. For us, the hypothesis is whether the introduction of licensing laws reduces the prevalence of minorities in that occupation, either as an unintended consequence of the fact that licensing laws disproportionately disadvantage minorities, or because licensing laws allow regulatory authorities to indulge in their own discriminatory tastes. Finally, we have a clearly articulated alternative hypothesis. Theoretically, it is possible that licensing regulation will increase the presence of minorities, particularly in occupations where information about worker quality is an issue, because licensing may also function as an impartial signal of quality that reduces the extent of statistical discrimination over observable worker characteristics.

III. OCCUPATIONAL LICENSING AS A NATURAL EXPERIMENT

During the Progressive Era government at all levels—state, local, and federal—began to play an increasingly active role in the economy. In the labor market, occupational licensing regulations governing the standards of practice and the minimum

qualifications needed to practice occupations ranging from medicine and dentistry to barbering and plumbing were enacted in earnest by state legislatures. We are interested in determining whether licensing disproportionately affected minority worker participation in these newly regulated occupations. Our outcome variable is thus whether an individual participates in one of these occupations. In order to make valid causal inferences we need to establish that licensing is exogenous with respect to other factors that might influence minority participation in these fields. Our goal in this section is to argue that the introduction of state level occupational licensing regulations during this period constitutes a natural experiment.

As a starting point it makes sense to examine national trends in the adoption of licensing regulation for different occupations during the late nineteenth and early twentieth centuries. Our measure of the extent to which an occupation is licensed is the percentage of workers in a given occupation who operate in a state that regulates that occupation. Information on the introduction of state licensing laws is from the Council of State Governments (1952), which, in the early 1950s, surveyed state government agencies about the characteristics of their licensing laws.

The Council of State Government (1952) survey reports the year in which a state enacted a licensing law. For our purposes, we code a state as having introduced licensing in a given census year, say 1920, if the state enacted a licensing law between 1909 and 1919.[2]

Figure 1 presents information on the growth of occupational licensing between 1860 and 1950 for ten occupations: accountants, barbers, beauticians, dentists, engineers,

[2] We also allowed a five year lag for licensing to take effect. Our empirical results were unaffected by this change.

midwives, pharmacists, plumbers, practical nurses and registered nurses. For an occupation to be included in our sample, it had to meet three criteria. First, it had to be included in the Council of State Government (1952) survey. Second, for the purpose of econometric identification, the adoption of licensing regulation had to span at least two census periods. Finally, the occupation had to have a sufficiently large sample within the Integrated Public Use Microdata Samples (IPUMS) of the Census of Population.

Several interesting facts emerge from an analysis of this figure. First, while it is clear that the extent of licensing overall increased during these decades, there are no obvious temporal patterns regarding the growth of licensing across these occupations. Some occupations, like dentistry and pharmacy, began licensing earlier (in the 1860s and 1870s) than other occupations, like engineers and beauticians (which did not begin to become licensed until the 1900s and 1910s, respectively). Second, the rate at which licensing diffused within an occupation also differs dramatically across these occupations. For instance, while regulation of accountants, engineers, and registered nurses spread very quickly, licensing of barbers, plumbers, midwives and practical nurses spread more gradually. Third, the extent to which each occupation was licensed by the end of the sample period varies dramatically across occupations. For dentists, accountants, pharmacists, and practical nurses, virtually all of the occupation was regulated by the end of the sample period. In contrast, for other occupations like midwives, plumbers, and practical nurses, a large portion of the occupation was still unregulated by 1950. Accordingly, while the Progressive Era did witness the rise of state level occupational licensing of various professions, these facts suggest that licensing was not clearly correlated with time, at least at a national level.

For our purposes, this point is important because it helps establish our claim that licensing regulation was exogenous, at least with respect to the desire to discriminate. It is often argued that licensing is introduced to control entry. The historical literature, however, does not suggest that licensing was ever introduced specifically out of a desire to discriminate against women or minority workers. Figure 1 provides some evidence in support of this view. If licensing regulation were introduced in response to a general desire to discriminate, then presumably we would observe licensing emerge first in occupations with a significant presence of black or female workers. This does not appear to be the case. Dentistry and pharmacy were among the first occupations to license; in neither occupation were women or blacks well represented. Accordingly, the fact that there are no obvious temporal patterns regarding the diffusion of licensing during this period suggests that a specific desire to discriminate against particular groups cannot explain the growth of licensing regulation.

If licensing is to serve as a natural experiment, then licensing laws for a given occupation should ideally appear to be more or less randomly adopted across states. This ensures that licensing states (the treatment group) are similar to non-licensing states (the control group), which is a key precondition for a natural experiment where state level variation is the source of identification.

Figures 2 through 4 display maps that show, by census decade interval, when states adopted regulation licensing accountants, engineers, and registered nurses, respectively.[3] A glance at these maps suggests that there are no easily discernable geographical patterns with respect to the adoption of licensing regulation for these four

[3] For the sake of economy, we present only the maps for these three occupations. Maps for the other occupations are similar in most important respects; hence, we restrict our attention to these three.

occupations. States that are shaded in darker colors adopted licensing later than those shaded in lighter colors. Early or late adopting states do not appear to be concentrated in particular geographic regions. The timing of adoption of licensing legislation for each of these occupations does not appear to be related to region, urbanization, demography, or level of economic development any obvious way. For instance, New England and Mid-Atlantic states (states that were more urbanized, wealthy, faster growing, and politically progressive) did not always adopt regulation sooner than southern or western states. Curiously, states that may have been relatively less developed (South Atlantic, Gulf Coast, and Western) and less politically progressive are often among the earlier adopters of regulation. Indeed, states that adopt licensing regulation at any given point in time do not appear to share much in common. Consider, for instance, Figure 3, which shows the timing of engineering licensing. Among states that adopted engineering licensing regulation, Louisiana was the earliest adopter, followed in the subsequent decade by Florida, Idaho, and Wisconsin. Additionally, the pattern of adopters varies across occupations. Compare, for instance, Figure 3 with Figure 4, which shows the timing of registered nursing regulation. States that were the earliest adopters of nursing regulation (California, the Midwest, and parts of the Northeast) were among the later adopters of engineering licensing. Hence, the evidence presented in these maps suggests that the timing of adoption of licensing regulation is not well correlated with geographic region.

While the adoption of licensing regulation is not well correlated with geography, it may be correlated with other factors that would render this a poor natural experiment. For instance, if it were the case that, in a given period, states that tend to adopt licensing and other labor market regulations were also states where minorities were increasing their

representation in skilled jobs like plumbing or engineering, then we might misattribute the growth of minority workers in these occupations to the adoption of licensing laws. Alternatively, if urbanization leads to greater tolerance of female labor force participation and is also correlated with political activity that generates licensing regulation, then any positive correlation between licensing and female participation in licensed professions may be spurious.

If there were only two time periods in our sample, one would simply analyze descriptive statistics of treatment and control states to test for differences in means. Clearly this is not possible for us because the adoption of licensing regulation spanned several decades and states that are in the control group in one period may be in the treatment group in a later period. Our solution is to estimate probit regressions for each occupation. In these regressions the dependent variable is an indicator that equals 1 if a state is regulated in a given census period and 0 otherwise. Since our goal is to predict change in licensing status at the state level, for each occupation, the sample begins in the last census year when no state had adopted licensing, and ends in the first census year in which every state had adopted licensing, or 1950, whichever occurs first. Sample sizes will therefore vary across regressions because the interval over which states adopted licensing varied across occupations.

The independent variables are a series of state-year controls that proxy for state-level characteristics that may be correlated with minority representation in a given occupation. If these controls are not systematically correlated with licensing regulation across occupations, then we are more confident that we have valid control state-years. The control variables that interest us most are those that directly measure minority

representation. There are many ways to control for minority representation in a given state-year. One possibility is the minority's share of the labor force. Another is the minority's share of the relevant occupation. Because it is often argued that licensing is introduced in order to reduce competition from potential entrants, we present results using the first measure of minority representation. Qualitatively similar results were obtained, however, when we used the second measure.

Table 1 presents the probit regression results. The first row shows the correlation between an occupation's share of the labor force (a proxy for the size and political influence of an occupation) and licensing, other things equal. For four occupations—dentists, engineers, midwives, and registered nurses—there is a positive and significant relationship between the occupation's share of the labor force and licensing at the state-year level. These results provide some support for the capture theory of regulation, which argues that the occupation itself seeks licensing in order to establish entry barriers. The second and third rows of the table show the relationship between female and black labor force shares and licensing. Few of these coefficients are statistically significant and when they are significant, they vary in sign across regressions. For instance, while the female labor force share has a positive and significant correlation with dentistry licensing, it has a negative and significant correlation with registered nurse licensing. Additionally, while the black labor force share has a negative and significant relationship with barber licensing, it has a positive and significant relationship with engineering and practical nurse licensing. Hence, it would appear that there is no systematic relationship between minority representation and occupational licensing regulation at the state-year level.

As additional state year controls we included the average age of the population, literacy rates, urbanization rates, the share of the population that is domestic born, census region dummies and year dummies. No systematic patterns emerge from an analysis of these coefficients. For instance, the coefficient on urbanization is statistically significant in only three of the ten regressions. In two instances (accountants and midwives) it is positive and significant while in one case (engineers) it is negative and significant. Additionally, while the year dummies are significant in some regressions, they are not significant in others. Finally, there is no obvious pattern between licensing and the census region dummies. For accountants, engineers and pharmacists, none of the region dummies are significantly different from New England, the omitted census region. For the remaining occupations, some subset of the region dummies is significant, but seldom are the same regions significant across occupations. This provides further evidence for our claim that geography is not systematically correlated with licensing.

We also estimated these regressions using the first difference in minority labor force shares instead of the level to control for minority representation. Perhaps licensing responds to changes in minority representation rather than the level of minority representation. We find no systematic relationship between changes in minority representation and licensing at the state-year level. In only one instance (dentists) is there a significant correlation between changes in female labor force share and licensing regulation. In no case is there a statistically significant relationship between changes in black labor force share and licensing. Qualitatively similar results were obtained when we used changes in the minority's share of the occupation instead of changes in the minority's share of the labor force.

Hence, our analysis shows that there are no systematic differences in observable characteristics between treatment and control state-years. This suggests that the adoption of occupational licensing regulation may indeed furnish a natural experiment that will allow us to make causal inferences about how licensing laws affected women and blacks.

IV. THE DATA

The data for our empirical analysis are from IPUMS which represent a sample of individual returns from the United States Census of Population. Our full sample includes individual-level observations from the 1860 through 1960 censuses (with the exception of 1890, for which IPUMS has not yet collected data). The population censuses include information on occupation (self reported), race, sex, state of residence, and other individual and household level characteristics. For consistency, we use the 1950 Census of Population occupational definitions. We restricted our attention to individuals aged 14 years and older. Additionally, we dropped housewives, inmates, retired persons, military personnel, and individuals living on reservations from our sample. Since we do not have data on licensing for Hawaii, Alaska, and the District of Columbia, we also focus exclusively on the 48 contiguous states.

As control variables, we include individual and household demographic-level data that were consistently reported across the various population censuses. Our key variables, of course, pertain to the race and sex of the individual. We restricted our attention to blacks since they are by far the largest racial minority group within our sample and the only racial group reasonably well represented across the regulated occupations.[4] As other

[4] We experimented with included foreign born workers as a group subject to discrimination and found little evidence that they were harmed by licensing.

controls, we included indicators for marital status (excluded category is never married), residence in a metropolitan area, foreign born, school attendance, the number of families living in the household, age, and the number of children. Because the 1860 Census of Population did not include information on marital status, the first two indicator variables were excluded for our analysis of those occupations where licensing began very early in the sample period. Since questions about current school attendance were not asked in the 1950 Census of Population, we exclude this variable in those regressions that include 1950 in the sample.

Unfortunately, variables measuring the level of an individual's educational attainment are not consistently reported across the various population censuses. Prior to the 1940 Census of Population, data on years of schooling attained were not recorded. The only consistently reported information on educational attainment in the 1860 through 1930 censuses is literacy. Hence, whenever possible, we also include a binary variable that equals 1 if an individual is literate. For 1940 and 1950, no information on an individual's literacy is available. For 1940, we coded any individual with a 1st grade education or less as illiterate. This gives us an illiterate share of the population that is similar to that which existed in 1930. For 1950 it was not possible to construct a similar variable. Hence, for those regressions that include data from 1950, the literacy variable is excluded as a control variable.

Table 2 presents information on the size of the sample available for each of the occupations under investigation. The size of the sample (in person years) depends on which census years are included and the presence of workers in that occupation by sex or race. As shown in the table, the sample size varies dramatically by occupation. For

instance, for dentists, every state had adopted licensing regulation by 1940, while for practical nurses, no state introduced licensing prior to 1900. Hence, for dentists, we omitted information from 1950 while for practical nurses we omitted years prior to 1900. We also omitted those states for which the Census of Government (1952) did not have information on the year in which licensing was introduced. Finally, the size of the sample used depended on the presence of workers by sex or by race. For instance, there were no black accountants in the IPUMS sample. Hence, for our accountant regressions, we focus exclusively on non-black workers. Similarly, IPUMS only included data on two male midwives; hence, we omit men from our analysis of midwives. Additionally, since barbers are only men while beauticians are only women, the sample for these occupations excludes members of the other sex. While the number of person years in our sample is very large, it is important to bear in mind that identification is coming from cross-state and temporal variation in licensing regulation. Hence, in the fourth column, we also report the number of state-years of data available for each occupation.

Table 2 also shows the total number of workers in each occupation, the share of the sample that worked in each occupation, as well as the number and share of female and black workers in each of the regulated occupations. Clearly the size of each occupation as well as the number of female and black workers varies significantly. For barbers and beauticians, two relatively large occupations, the sample includes a large number of black workers (1,000 and 584, respectively), representing fairly sizable shares (11 and 12 percent, respectively) of the total number of workers in those fields. Identification of the effect of licensing on black workers should be relatively easy for these occupations. For other occupations, however, the number of female or black

workers is small. For instance, there are only 32 female and 37 black dentists. These small numbers may make it difficult to isolate the effect of dental licensing laws on these groups.

More descriptive information about our data can be furnished through regression analysis. Table 3 presents probit regressions of the factors influencing whether an individual is in a given occupation in a given year as a function individual and household-level characteristics and whether that occupation is licensed in her state in a given year. Our dependent variable is a binary variable that equals 1 if an individual located in a given state is a member of a given occupation in a given year (and 0 otherwise). State and year fixed effects are included to control for unobserved heterogeneity at the state level and within census years. Robust standard errors, clustered at the state level, are reported in parentheses. The probit coefficients on the individual and household controls are generally significant and have the predictable signs. Women are under-represented as dentists, plumbers, engineers, pharmacists, but are over-represented as either practical or registered nurses. Blacks are more likely to be barbers but less likely to be plumbers, engineers, pharmacists, or dentists. Literacy is positively and significantly correlated with more technical occupations like accounting, dentistry and pharmacy. Finally, individuals living in cities are more likely to be involved in most of these occupations.

To control for licensing we use an indicator that equals 1 if an individual resides in a state that regulates that occupation in a given year and 0 otherwise. The coefficient on this variable is seldom significant; when it is significant, it is positive, which suggests that licensing increases the probability that an individual works in a given occupation.

These results are roughly consistent with other work on the effects of Progressive Era licensing laws that use state level data (Law and Kim 2005). For barbers, accountants, pharmacists, practical nurses, and registered nurses, the licensing indicator variable is negative but not significant. Licensing may have reduced the growth of these occupations (which is consistent with the standard entry barrier explanation for licensing), but its effects are not precisely estimated. If licensing is to facilitate discrimination against females or blacks, it is most likely to do so within these occupations since licensing can only be used to disadvantage particular groups if in fact it functions as an effective entry barrier. For beauticians, engineers, and dentists, the coefficient on the licensing variable is positive but not significant, while for plumbers and practical nurses, the coefficient is positive and significant. Hence, we do not expect licensing to facilitate discrimination in these occupations.

V. DIFFERENCE-IN-DIFFERENCES ANALYSIS

Our primary goal in this paper is to determine whether licensing disadvantaged female or black workers. To estimate the effect of occupational licensing regulation on the prevalence of female or black workers in each occupation, we use a "difference-in-differences" (DID) estimator. In terms of the regression framework, we can obtain the DID estimator by interacting the licensing indicator variable with the race or female indicator variable. The coefficient on this interaction term is the DID estimate. The interaction term tells us if female or black workers are disproportionately affected by licensing, controlling for the effect that licensing has on the likelihood of any individual belonging to this occupation.

For each occupation, we estimate the following regression equation separately for female and black workers:

$$P(y_{ijt} = 1) = \beta_1 L_{jt} + \beta_2 M_{ijt} + \beta_3 L_{jt} M_{ijt} + \beta_4 X_{ijt} + \beta_5 S_j + \beta_6 T_t + \varepsilon_{ijt}$$

$P(y_{ijt} = 1)$ is the probability that individual i in state j in census year t works in the occupation; L_{jt} is the licensing indicator variable; M_{ijt} is the minority status (black or female) indicator variable; $L_{jt}M_{ijt}$ is the interaction term; X_{ijt} is a vector of other individual and household level controls; S_j and T_t are state and year fixed effects; and ε_{ijt} is the error term. The variable of interest is β_3, the coefficient on the interaction term. Omitting the individual and household level controls alters neither the sign nor significance of any of the regressions that we report.

Table 4 displays the DID estimates of the effects of occupational licensing regulation on black workers. Each column shows the coefficient estimates for a given occupation. The coefficient on the interaction term is negative and significant for only barbers, suggesting that black representation in barbering was reduced by licensing. Interestingly, it is positive and significant for pharmacists and practical nurses, and statistically insignificant for the remaining occupations in our sample. The coefficients for the other control variables continue to have plausible signs and significance levels. The failure to find much evidence that occupational licensing harmed black representation is perhaps not surprising, given that we do not find much evidence that licensing functioned as an effective entry barrier overall.

Table 5 shows the DID estimates of the impact of licensing regulation on female workers. Again, each column displays probit coefficient estimates for a given occupation. For none of the occupations in our sample was the coefficient negative and significant. In

fact, practical nursing is the only occupation where the coefficient is negative. For plumbers, engineers, pharmacists, and registered nurses, the interaction term is positive and significant, implying that occupational licensing increased female representation. For the remaining occupations, the coefficient on the interaction term is positive but not significantly different from zero.

Overall, the DID results suggest the following tentative conclusions. In general, the introduction of occupational licensing regulation during the Progressive Era did not tend to harm female or black workers. Only for barbers did licensing have a negative impact on minority representation. By functioning as an entry barrier, barber licensing may have reduced opportunities for blacks, which is consistent with the conventional view of how licensing affects minorities. However, for some occupations, licensing had no effect. For five of the ten occupations—specifically, plumbing, engineering, pharmacy, practical nursing and registered nursing—licensing regulation appears to have had a positive effect on the presence of female or black workers.[5] It is revealing that the occupations where licensing seems to have helped female or black workers are also relatively technical occupations, where information about worker quality is likely to be a serious concern. Markets for these services are likely to be characterized by poor information about worker quality. In such markets, consumers might rationally engage in statistical discrimination over observable worker characteristics like race or sex. The fact that licensing appears to have helped female and minority workers in these occupations is consistent with the statistical discrimination hypothesis, which argues that licensing, by

[5] For nursing, a profession that is disproportionately female, it is unclear which sex is the potential target of discrimination. Hence, the registered nursing results are somewhat difficult for us to interpret.

serving as an signal of quality, reduces the extent of statistical discrimination and helps traditionally disadvantaged workers.

Robustness check: grandfathering and licensing

So far our analysis has focused on the effects of licensing on all the workers within a given occupation. Perhaps one reason why we do not find significant negative effects of licensing on minority representation is because licensing laws invariably grandfather existing workers. In general, licensing requirements are only binding for new entrants in an occupation. As a result, a significant portion of our sample may be unaffected by the introduction of a licensing law in a given year. As a robustness check, we re-estimated our DID regressions focusing on young workers (less than 35 years old), the sub-sample of workers for whom licensing requirements are most likely to be binding.[6]

Table 6 displays the DID regressions for the sub-sample of young black workers. For pharmacists, the coefficient on the interaction term is positive and significant, suggesting that pharmacy licensing increased the representation of young black workers in pharmacy. For the remaining occupations, the interaction term is insignificantly different from zero. While our earlier results suggested that black representation in barbering was harmed by licensing, when we restrict attention to the group most likely to

[6] Because licensing laws grandfather existing workers, one might be tempted to estimate a differences-in-differences-differences (triple diff) regression that interacts licensing, minority status and an indicator for young workers. An advantage of this approach is that it controls for within-state omitted factors that are potentially correlated with minority representation. The validity of a triple diff approach rests in part on the assumption that old minority workers function as a good control for young minority workers with regards to occupational choices. This assumption is unlikely to be valid in our context. For instance, the old sub-sample of our data set likely includes old black workers who were former slaves. These workers are unlikely to be a good control for young black workers. Additionally, marriage and family obligations and the influence they have on female labor force participation and occupational choices make older women a poor control for younger women. Hence, we choose to present our analysis that focuses exclusively on young minority workers. The triple diff approach does, however, generate qualitatively similar results.

be disadvantaged by licensure, we find no such effect. Accordingly, these regressions show that the introduction of licensing laws did not disproportionately harm young black workers. Table 7 shows the corresponding regressions for young female workers. For pharmacy, plumbing, and registered nursing, the interaction term is positive and significant while for the other occupations it is insignificantly different from zero. The evidence therefore suggests that the adoption of licensing legislation did not reduce the representation of young female workers and often increased it. In our view, it is revealing that the three occupations where licensing increased the representation of young black and female workers are also occupations where we found that licensing improved black or female representation overall. These occupations, as discussed earlier, are ones where uncertainty about worker quality was likely an issue. If licensing requirements only apply to new entrants, and licensing laws reduce uncertainty about worker quality, then new entrants should disproportionately benefit from licensing. Our regression results provide some support for this perspective.

VI. PHYSICIAN AND TEACHER LICENSING CASE STUDIES

We now turn our attention to analysis of two occupations: medicine and teaching. A detailed analysis of how medical and teaching licensing regulations affected black and female workers is of interest for several reasons. First, for these two occupations, we have detailed information on the characteristics of state licensing regulation. Instead of measuring the effect of licensing by the presence of an initial licensing law, we now measure licensing using specific licensing requirements that are consistently measured across all states. A potential problem with the licensing variable we used in the previous

sections is that it treats all states that license a given occupation as having identical licensing regimes. An advantage of using a specific licensing requirement is that it furnishes a more accurate indicator of licensing and should reduce measurement error that biases coefficient estimates towards zero. Second, teaching and medicine are two occupations where information about worker characteristics was likely to be a serious issue, particularly during the period under investigation. During the Progressive Era, advances in basic science dramatically altered the nature of the medical profession, making the issue of physician quality increasingly salient to consumers of medical services. (Law and Kim 2005; Ludmerer 1985; Starr 1982). Similarly, in teaching, the growing importance of high school education and training in more technical scientific subjects also increased the knowledge base required to be an effective teacher (Goldin 1998). An examination of physician and teacher licensing regulation may therefore provide additional evidence on the statistical discrimination hypothesis. Finally, critics of medical licensing requirements have often claimed that medical licensing laws were used by organized medicine to shut down medical schools that trained female and black doctors (Kessel 1958; Starr 1982). It is often asserted that these medical licensing laws harmed minority and female workers (Kessel 1970). An examination of the effects of medical licensing laws will therefore furnish a test of this claim.

For each occupation, we measure licensing in two ways. For physicians, we use (i) the year in which a four-year medical degree was required for a medical license and (ii) the year in which some pre-medical college education was required for a medical license. Data on these requirements are from Baker (1984) and from the American Medical Association's Council on Medical Education (1930). For teachers, we use (i) the

year in which graduation from high school was required for the lowest level of teacher certification; and (ii) the year in which some college education was required for the lowest level of teacher certification. Our data on teacher licensing requirements is taken from the US Bureau of Education's semi-annual survey of state-level teacher licensing requirements (US Bureau of Education).

Figures 5 and 6 displays information on the timing of adoption of the four-year medical school requirement for physicians and the high school graduation requirement for teachers. The maps for the pre-medical college requirement for physicians and the high school requirement for teachers are similar. For physicians we focus our attention on the years from 1880 to 1930 since we do not have complete data on this requirement after 1930. For teachers, the data are from 1910 to 1940. Prior to 1910, no state required teachers to have a high school diploma. While there is no obvious pattern regarding the adoption of the four year medical school requirement, for teachers, it would seem that later adopters of the high school requirement for teachers are heavily concentrated in the southern states while northern states are well represented among the early adopters. This suggests to us that it may be more difficult for us to make definitive causal inferences regarding the effects of teacher licensing requirements on blacks.

Table 8 displays probit regression estimates of the relationship between state-year characteristics and the presence of teacher and physician licensing requirements. As before, our goal is to see if there are systematic correlations between occupational licensing and minority representation. If no systematic correlations are found, then we have more confidence that our control state-years are valid. Columns (1) and (2) display the corresponding regressions for our two measures of physician licensing. In these

regressions, the coefficients on minority labor force shares are never significant. Columns (3) and (4) show the regression results for our two measures of teacher licensing. While the coefficient on the female labor force share is positive and significant for the first measure of teacher licensing, it is negative and insignificant for the second. In contrast, the coefficient on the black labor force share is negative and insignificant for both measures of teacher licensing. Qualitatively similar results were obtained when we examined the first difference in minority labor force participation and when we replaced minority labor force shares with the minority share of the occupation. Accordingly, it appears that licensing of teachers and doctors was not systematically related to minority labor force representation at the state-year level.

Table 9 shows the regression results from our analysis of the impact of physician licensing requirements on females and blacks. Columns (1) through (3) display the results when the four-year medical degree requirement is used to measure physician licensing. In columns (4) through (6) the pre-medical college requirement is used to measure physician licensing. We present our results with and without interaction effects. Qualitatively similar results are obtained when we estimate the regressions without individual and household level controls. While licensing did not reduce the growth of the medical profession overall, there is some evidence that it increased the representation of blacks; the coefficient on the black-licensing interaction term is positive and significant when the pre-medical college requirement is used to measure doctor licensing. For women, the coefficient on the interaction term is positive but not significant regardless of how we measure physician licensing. Thus, it would seem that medical licensing laws enacted in the early decades of the twentieth century had a positive impact on black representation

in the medical profession (which is consistent with the statistical discrimination hypothesis) but no effect on women. These results contrast sharply with the conventional view, which argues that minority representation in medicine was adversely affected by Progressive era physician licensing laws.

Table 10 displays the regression results for teachers. In the first three columns the high school graduation requirement measures teacher licensing. In the last three columns the requirement that license holders have attended some college is our measure of teacher licensing. The coefficients on the sex and race interaction terms are qualitatively similar regardless of how licensing is measured. Teacher licensing requirements increased black representation but reduced female representation. The results for blacks are, in our view, consistent with the statistical discrimination hypothesis. Within the teaching profession, blacks were clearly a minority group that might have been disadvantaged as a result of statistical discrimination. Licensing regulation, as a signal of quality, may have helped blacks enter the teaching profession. The results for women, however, are more difficult to interpret. Taken at face value, they are consistent with the standard hypothesis that argues that entry barriers facilitate discrimination. However, we are uncertain as to whether this is the correct interpretation, since, as noted earlier, in occupations that are disproportionately female it is unclear who is the target of discrimination.

In the case of teachers, southern states are heavily represented among the late adopters of teacher licensing regulation. It is possible that opportunities for blacks in the south changed at a different rate than in other regions. For instance, if educational opportunities for blacks evolved differently in the south, then, since the timing of licensing is correlated with region, our DID estimates may incorrectly attribute

improvements in the representation of blacks to teacher licensing regulation. As a robustness check, we re-estimated the teaching regressions excluding states from the South Atlantic (OK, AR, TX, LA) and West South Central (MS, AL, TN, KY) census regions. Eliminating these southern states from our sample does not alter our findings, either qualitatively or quantitatively. Teacher licensing reduces female representation but increases black representation, regardless of which measure of teacher licensing we use.

The time period during which teacher licensing requirements were introduced was also a period when marriage bars—laws that prevented married women from working—were also adopted by local school districts (Goldin 1990). If licensing requirements are positively correlated with marriage bars, then our regression results will overestimate the negative effect of teacher licensing laws on female representation in the teaching profession. Because women who never married were not subject to these laws, as a robustness check we re-estimated the teacher regressions on the sub-sample of workers who were never married.[7] The significantly negative impact of licensing on female representation in the teaching profession persists in these regressions.

The early twentieth century was also a period of rising public school enrollments, particularly at the high school level (Goldin 1998). As high school enrollments rose, the demand for teachers with advanced skills also increased, which may have favored male representation in the teaching profession. If teacher licensing is positively related to rising school enrollments, then our regressions will overestimate the negative effect of licensing on female representation in teaching. To determine if this was the case, we included the public school enrollment ratio in our regressions on the relationship between

[7] An additional check would be to include state-level information on the prevalence of marriage bars directly in our regressions. However, we were unable to locate state-level data on marriage bars in the teaching profession.

state-year characteristics and our two measures of teacher licensing (see Table 8). The coefficient on this variable is positive but not significant in one case, and negative and significant in the other. The negative impact of licensing on female representation in teaching is therefore unlikely to be the product of rising school enrollments.

As a final robustness check, we also re-estimated the teacher and physician regressions focusing exclusively on the young sub-sample of our data set (see Tables 11 and 12). For physicians, the representation of young female and black workers was unaffected by either measure of physician licensing. For teachers, we find that licensing increased the representation of young blacks and decreased the representation of young women, regardless of how teacher licensing is measured. These results mirror those found using the full data set.

VII. CONCLUSION

It is widely believed that licensing laws, by functioning as an entry barrier, reduce opportunities for traditionally disadvantaged workers. In this paper we take advantage of cross-state and temporal variation in the introduction of occupational licensing regulation during the Progressive Era to determine if in fact this is the case. By merging information on the timing of state licensing laws with detailed, individual-level data, we are able to investigate the effects of these laws on a broad sample of occupations, ranging from barbers and beauticians to plumbers and pharmacists. Additionally, for two occupations (teachers and physicians), we gathered detailed information on the nature of state licensing requirements and we examine the effects of these requirements on minority

representation. Taken together, these occupations represent approximately 12 percent of the civilian labor force from the late nineteenth to mid twentieth centuries.

Overall, our empirical analysis suggests that turn of the century licensing legislation did not generally harm black or female workers. In only one occupation (barbering) was licensing harmful to blacks, but even this result weakens when we restrict our sample to young workers to account for the effect of grandfathering. Teacher licensing requirements also had a negative effect on the representation of females in the teaching profession. For the remaining occupations in our sample licensing either had no effect on female or black participation or it had a positive effect. For instance, we find that licensing increased the representation of blacks in the medical profession, and women in engineering and pharmacy. In our view, it is revealing that those occupations where licensing appears to have helped minorities are generally those where information about worker quality was likely to be an important concern. Hence, our findings suggest that the conventional wisdom about how licensing affects minorities is not well supported, at least during the Progressive Era.

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Figure 1: The growth of licensing, 1860-1950

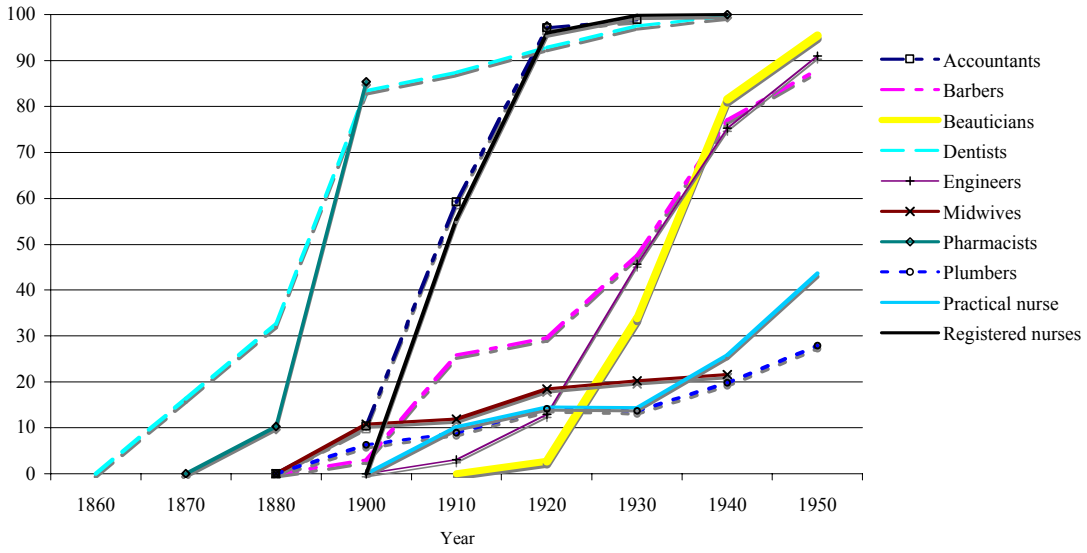


Figure 2: Adoption of accounting licensing regulation, 1890-1930

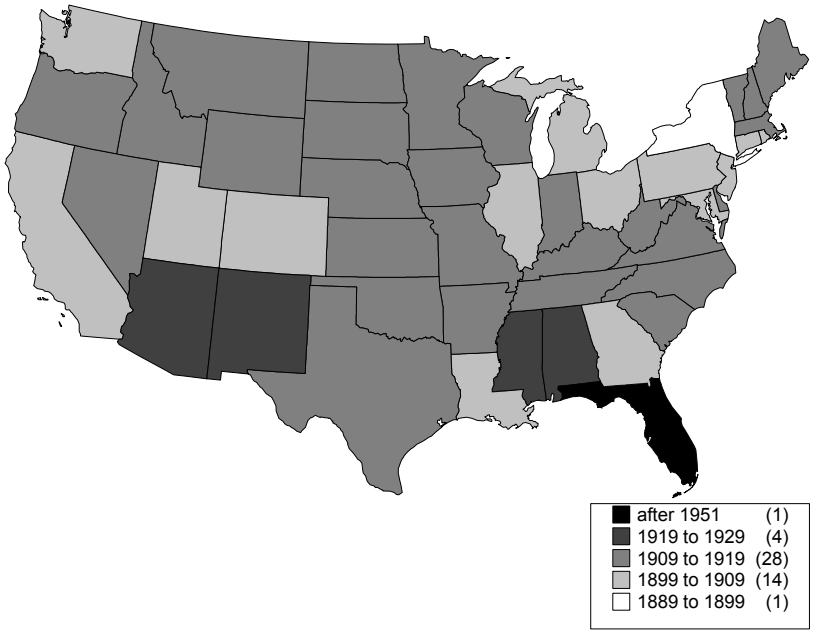


Figure 3: Adoption of engineering licensing regulation, 1900-1950

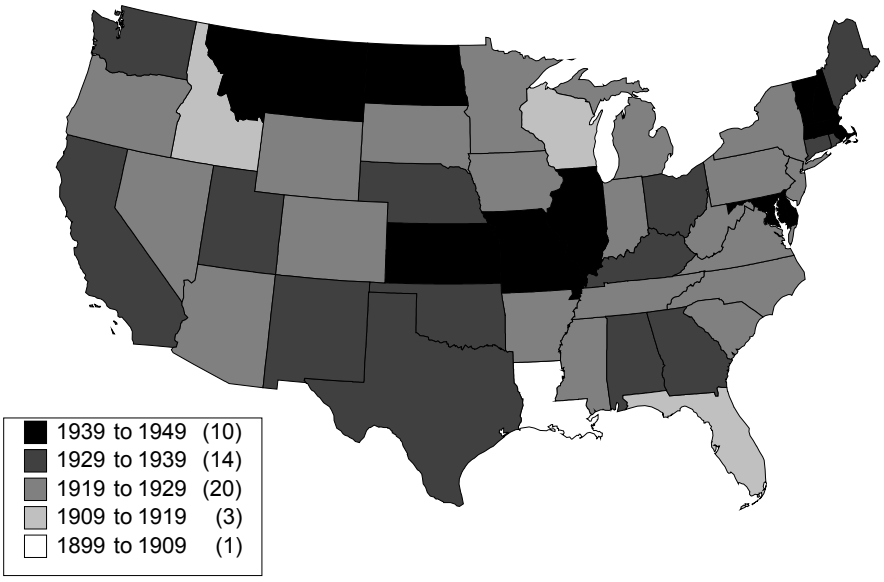


Figure 4: Adoption of registered nurse licensing, 1890-1940

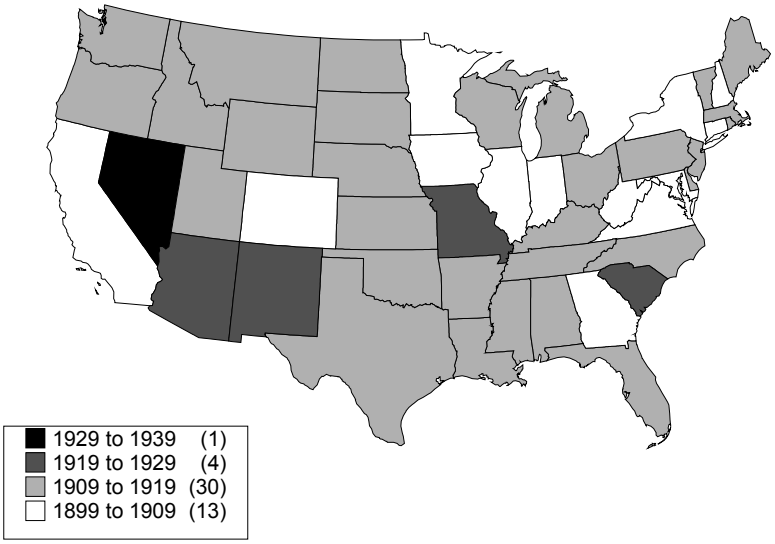


Figure 5: Adoption of the four-year medical degree requirement, 1890-1930

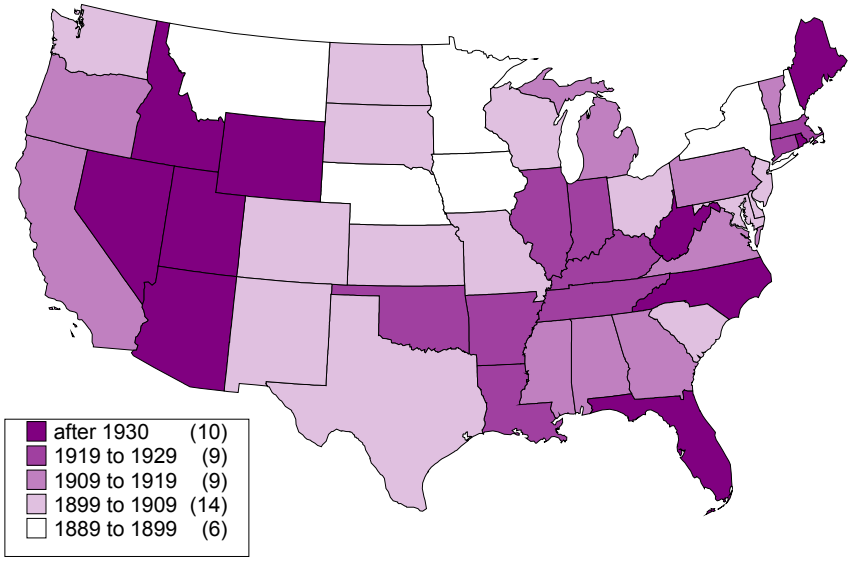
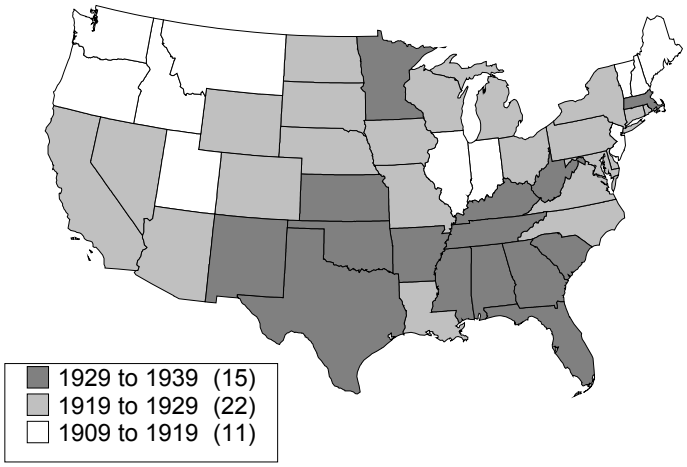


Figure 6: Adoption of high school graduation requirement for lowest level of teacher certification requirement, 1910-1940



Notes for Figures 2 through 6: States not included in the map either (i) never introduced licensing during the sample period; or (ii) licensing status unknown during the sample period.

Table 1: Correlates of licensing

	Accountant	Barber	Beautician	Dentist	Engineer	Midwives	Pharmacist	Plumber	Practical Nurse	Registered Nurse
Occ. share	-142.27	-57.18	15.71	345.05*	153.08*	1,424.90*	-165.69	17.67	70.59	491.92**
	(123.88)	(85.39)	(161.23)	(165.86)	(73.15)	(584.37)	(181.76)	(96.77)	(101.97)	(83.00)
Female LF	5.99			16.87**	-3.53		-0.35	11.30	-18.42*	8.16
	(6.26)			(4.74)	(7.06)		(6.23)	(5.89)	(6.06)	(6.10)
Black LF		-3.20*	0.82	-1.83	6.50*	1.98	-2.38	-2.36	4.565*	-4.42
		(1.25)	(1.95)	(1.37)	(2.73)	(1.74)	(2.90)	(2.58)	(2.24)	(2.41)
Age	-0.08	-0.25	-0.29**	-0.33**	0.14*	-0.60	0.29	-0.13	0.03	-0.31*
	(0.25)	(0.14)	(0.08)	(0.07)	(0.06)	(0.25)*	(0.16)	(0.15)	(0.07)	(0.09)
Literacy	-6.35			6.83***			-5.06			5.38
	(4.46)			(1.70)			(2.80)			(2.75)
Domestic	-5.70	1.31	6.58	0.52	-12.55	-0.61	1.95	-9.07	0.62	2.43
	(3.26)	(2.43)	(3.50)	(1.49)	(3.11)**	(2.39)	(1.98)	(2.64)**	(2.93)	(2.11)
Urbanization	8.21	0.60	1.58	1.39	-4.37**	2.91*	0.48	0.711	-0.07	-0.03
	(2.13)**	(0.66)	(0.91)	(0.74)	(1.07)	(1.42)	(1.07)	(1.16)	(1.01)	(0.93)
Statistically significant year dummies	None		+1930**, +1940**, +1950**	+1900**, +1910*, +1920**, +1930**	+1910*, +1920** +1930**, +1940**, +1950**	None	+1910**, +1920**, +1930**	None	+1910**, +1920**, +1930**, +1940**, +1950**	+1920**, +1930**
Statistically significant census regions	None	+WNC*,	+MA**, +ESC*, +WSC*	+ENC**, WSC**, WSC**, MT**, PC**	None	+ENC*	None	+ENC**, +WNC**, +SA**, +ESC**, +WSC**, +MT*, +PC*	-ENC*, -WNC*, -SA*, -ESC**, -WSC**, -MT**, -PC**	+SA*, +ESC*, +WSC*
Observations	192	321	240	378	240	229	288	300	280	240

Notes: Robust standard errors are reported in parentheses. ** and * denote statistical significance at the 1 percent and 5 percent levels, respectively. Regressions were weighted by state labor force.

Table 2: Descriptive data on sample

Occupation	Years included in sample	States excluded (no licensing data)	State years of data	Sample size (person years)	Number of workers in occupation (person years)	Number of female workers in occupation (person years)	Number of black workers in occupation (person years)
Accountants	1880-1930[a]		192	957,787	1,745	188	0
(percent)					(0.18)	(10.8)	
Barbers	1880-1950	MI, NV	321	1,811,048	8,953	n/a[b]	1,000
(percent)					(0.49)		(11.2)
Beauticians	1910-1950		240	441,193	4,700	n/a[b]	584
(percent)					(1.07)		(12.43)
Dentists	1860-1940		378	1,877,274	2,092	32	37
(percent)					(0.11)	(1.5)	(1.7)
Engineers	1900-1950		288	2,157,112	10,920	141	55
(percent)					(0.51)	(1.3)	(0.5)
Midwives	1880-1930	IL, PA	229	188,039[c]	158	158	115
(percent)					(0.07)	(100)	(72.8)
Pharmacist	1870-1940[a]		288	1,631,502	2,732	98	24
(percent)					(0.17)	(3.59)	(0.88)
Plumbers	1880-1950	ME, MI	321	2,331,786	9,712	47	225
(percent)					(0.42)	(0.5)	(2.3)
Practical nurses	1900-1960	MA, MI, MO, OK, PA, SC, SD, TN	280	2,374,246	7,653	7,283	1,163
(percent)					(0.32)	(95.2)	(15.2)
Physicians	1900-1930		192	1,117,373	4,332	185	61
(percent)					(0.39)	(4.3)	(1.4)
Registered nurses	1900-1940		240	1,470,335	5,736	5,580	138
(percent)					(0.39)	(97.3)	(2.4)
Teachers	1910-1940		192	1,187,781	22,388	17,614	1,321
(percent)					(1.9)	(78.7)	(5.9)

Notes:
[a] 1910 not included because accounting and pharmacy not reported as an occupation in that year.
[b] By the census definitions, barbers are always men and beauticians are always women.
[c] We exclude men from this sample because there were only two male midwives.

Table 3: Determinants of occupational participation

	Accountant	Barber	Beautician	Dentist	Engineer	Midwife	Pharmacist	Plumber	Prac Nurse	Reg Nurse
Licensing	-0.068	-0.012	0.037	0.037	0.038	0.325	-0.036	0.124	-0.036	-0.038
	(0.080)	(0.022)	(0.027)	(0.046)	(0.020)	(0.106)**	(0.034)	(0.038)**	(0.033)	(0.052)
Female	-0.268			-0.784	-0.985		-0.585	-1.212	1.295	1.424
	(0.029)**			(0.057)**	(0.027)**		(0.041)**	(0.045)**	(0.032)**	(0.040)**
Black		0.136	-0.053	-0.539	-0.940	-0.000	-0.682	-0.468	0.001	-0.616
		(0.043)**	(0.033)	(0.092)**	(0.054)**	(0.096)	(0.063)**	(0.032)**	(0.025)	(0.054)**
Age	-0.002	0.001	-0.007	0.004	0.000	0.025	0.004	-0.001	0.012	0.005
	(0.001)**	(0.001)	(0.001)**	(0.000)**	(0.000)	(0.002)**	(0.000)**	(0.000)	(0.001)**	(0.001)**
At school	-0.114			-0.436		0.513	-0.337			0.382
	(0.041)**			(0.083)**		(0.203)*	(0.079)**			(0.054)**
Literate	0.776			0.855		0.020	0.728			0.527
	(0.130)**			(0.151)**		(0.101)	(0.075)**			(0.092)**
Domestic	0.327	-0.226	0.034	0.387	0.278	-0.567	0.153	0.217	-0.003	0.029
	(0.028)**	(0.059)**	(0.023)	(0.043)**	(0.017)**	(0.115)**	(0.034)**	(0.021)**	(0.015)	(0.035)
Widowed	0.199	0.106	0.236		0.005	0.325		0.073	0.116	-0.301
	(0.021)**	(0.025)**	(0.024)**		(0.025)	(0.124)**		(0.024)**	(0.019)**	(0.028)**
Married	0.049	0.196	0.291		0.307	0.666		0.116	-0.087	-0.263
	(0.054)	(0.023)**	(0.019)**		(0.018)**	(0.127)**		(0.019)**	(0.017)**	(0.018)**
Children	-0.051	-0.039	-0.066	-0.043	-0.042	-0.012	-0.046	0.003	-0.031	-0.098
	(0.006)**	(0.005)**	(0.005)**	(0.005)**	(0.003)**	(0.026)	(0.004)**	(0.004)	(0.006)**	(0.008)**
Two families	0.004	0.012	-0.002	0.129	0.045	0.034	0.078	-0.038	0.179	-0.096
	(0.022)	(0.014)	(0.021)	(0.027)**	(0.012)**	(0.103)	(0.016)**	(0.022)	(0.018)**	(0.024)**
Three families	0.050	0.047	0.035	0.119	0.149	-0.343	0.037	-0.009	0.248	0.174
	(0.029)	(0.042)	(0.027)	(0.030)**	(0.020)**	(0.120)**	(0.028)	(0.027)	(0.021)**	(0.028)**
City	0.508	0.093	0.067	0.152	0.367	0.315	0.129	0.302	-0.020	0.111
	(0.041)**	(0.017)**	(0.018)**	(0.028)**	(0.017)**	(0.120)**	(0.015)**	(0.019)**	(0.017)	(0.028)**
Observations	834,382	1,737,679	441,193	1,349,643	2,157,112	164,243	1,631,502	2,225,502	2,374,246	1,470,335
State years	192	321	240	378	288	229	288	321	280	240

Notes: State and year fixed effects are included. Robust standard errors, clustered at the state level, are reported in parentheses. ** and * denote statistical significance at the 1 percent and 5 percent levels, respectively.

Table 4: Effect of occupational licensing on blacks

	Barber	Beautician	Dentist	Engineer	Midwives	Pharmacist	Plumber	Practical Nurse	Registered Nurse
Licensing indicator	0.007	0.043	0.037	0.039	0.276	-0.046	0.122	-0.054	-0.031
	(0.023)	(0.030)	(0.047)	(0.020)	(0.113)*	(0.034)	(0.037)**	(0.035)	(0.057)
(Black)*(licensing)	**-0.173**	-0.055	-0.004	-0.018	0.137	**3.099**	0.044	**0.137**	-0.086
	(0.049)**	(0.075)	(0.192)	(0.089)	(0.175)	**(0.064)****	(0.067)	**(0.067)***	(0.135)
Female			-0.784	-0.985		-0.585	-1.212	1.298	1.424
			(0.057)**	(0.027)**		(0.041)**	(0.045)**	(0.032)**	(0.040)**
Black	0.215	-0.015	-0.535	-0.927	-0.014	-3.716	-0.480	-0.060	-0.539
	(0.052)**	(0.073)	(0.160)**	(0.090)**	(0.101)	(0.000)	(0.032)**	(0.046)	(0.138)**
Observations	1,737,679	441,193	1,349,643	2,157,112	164,243	1,631,502	2,225,502	2,374,246	1,470,335
State years	321	240	378	288	229	288	321	280	240

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.

Table 5: Effect of occupational licensing on women

	Accountant	Dentist	Engineer	Pharmacist	Plumber	Practical Nurse	Registered Nurse
Licensing indicator	-0.077	0.035	0.036	-0.045	0.120	0.056	-0.252
	(0.082)	(0.046)	(0.020)	(0.034)	(0.038)**	(0.041)	(0.061)**
(Female)*(licensing)	0.110	0.079	**0.134**	**0.394**	**0.314**	-0.099	**0.239**
	(0.106)	(0.177)	**(0.046)****	**(0.133)****	**(0.043)****	(0.053)	**(0.080)****
Female	-0.371	-0.855	-1.090	-0.952	-1.298	1.333	1.202
	(0.106)**	(0.161)**	(0.044)**	(0.131)**	(0.032)**	(0.027)**	(0.081)**
Black		-0.538	-0.940	-0.681	-0.468	0.001	-0.616
		(0.092)**	(0.054)**	(0.063)**	(0.032)**	(0.025)	(0.054)**
Observations	834,382	1,349,643	2,157,112	1,631,502	2,225,502	2,374,246	1,470,335
State years	192	378	288	288	321	280	240

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.

Table 6: Effect of occupational licensing on young blacks

	Barber	Beautician	Dentist	Engineer	Pharmacist	Plumber	Practical Nurse	Registered Nurse
Licensing indicator	-0.038	0.042	0.050	0.062	-0.112	0.141	-0.025	-0.013
	(0.035)	(0.037)	(0.062)	(0.025)*	(0.050)*	(0.048)**	(0.061)	(0.060)
(Black)*(licensing)	-0.065	-0.097	-0.006	0.120	**3.387**	0.036	0.085	0.141
	(0.066)	(0.089)	(0.282)	(0.163)	**(0.081)****	(0.085)	(0.075)	(0.146)
Female			-0.735	-0.995	-0.599	-1.000	1.170	1.493
			(0.068)**	(0.034)**	(0.046)**	(0.034)**	(0.032)**	(0.043)**
Black	0.135	-0.032	-0.570	-1.060	-3.926	-0.523	0.177	-0.765
	(0.056)*	(0.073)	(0.260)*	(0.133)**	(0.000)**	(0.030)**	(0.039)**	(0.135)**
Observations	849,076	257,966	983,244	1,061,878	841,334	1,112,244	1,104,664	748,407
State years	321	240	378	288	288	321	280	240

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively. Young is defined as below 35 years of age.

Table 7: Effects of occupational licensing on young women

	Accountant	Dentist	Engineer	Pharmacist	Plumber	Practical Nurse	Registered Nurse
Licensing indicator	-0.120	0.048	0.060	-0.109	0.138	0.077	-0.340
	(0.141)	(0.061)	(0.025)*	(0.050)*	(0.050)**	(0.076)	(0.084)**
(Female)*(licensing)	0.036	0.142	0.136	**0.565**	**0.182**	-0.092	**0.375**
	(0.135)	(0.262)	(0.088)	**(0.231)***	**(0.058)****	(0.067)	**(0.097)****
Female		-0.866	-1.097	-1.123	-1.036	1.19	1.154
		(0.254)**	(0.072)**	(0.215)**	(0.038)**	(0.040)**	(0.089)**
Black	-0.288	-0.575	-0.970	-0.629	-0.515	0.210	-0.635
	(0.129)*	(0.099)**	(0.068)**	(0.078)**	(0.032)**	(0.021)**	(0.060)**
Observations	505,205	983,244	1,061,878	841,334	1,112,244	1,104,664	748,407
State years	192	378	288	288	321	280	240

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively. Young is defined as below 35 years of age.

Table 8: Correlates of physician and teacher licensing requirements

	(1) Physicians Four year medical school requirement	(2) Physicians Some pre-medical college requirement	(3) Teachers High school graduation requirement	(4) Teachers Some college requirement
Occ. Share	-63.544	343.002	42.28	52.35
	(86.279)	(188.16)	(27.31)	(36.65)
Female LF	13.868	0.330	28.14**	-12.35
	(7.114)	(9.177)	(8.12)	(13.36)
Black LF	-3.349	1.067	-1.98	-0.69
	(2.62)	(3.88)	(3.31)	(4.29)
Pub school enroll ratio			2.62	-6.07*
			(2.18)	(3.09)
Age	-0.14	-0.56*	0.34	-0.31
	(2.62)	(0.272)	(0.16)	(0.26)
Literacy	0.488	12.43	20.78**	13.97
	(3.016)	(7.74)	(7.47)	(12.08)
Domestic	-3.373	-14.32**	9.45**	4.01
	(2.040)	(4.66)	(2.45)	(4.95)
Urbanization	-0.332	-4.760**	0.48	-0.98
	(0.862)	(1.794)	(1.08)	(1.84)
Statistically significant year dummies	+1910**, +1920**, +1930**	+1910*, +1920*, +1930*	None	None
Statistically significant census regions	+MA**, +WSC**, +PC**	PC*	+MA**, +ENC**, +MT**	WNC**, -ESC*, -WSC*
Observations	192	192	192	192

Notes: Robust standard errors are reported in parentheses. ** and * denote statistical significance at the 1 percent and 5 percent levels, respectively. Regressions were weighted by state labor force.

Table 9: Effects of physician licensing requirements on women and blacks

	(1)	(2)	(3)	(4)	(5)	(6)
Four-year med school req't	0.009	0.008	0.007			
	(0.024)	(0.024)	(0.024)			
Pre-medical college req't				0.061	0.063	0.057
				(0.035)	(0.035)	(0.035)
(Female)*(Four year med school req't)		0.003				
		(0.036)				
(Black)*(Four year med school req't)			0.087			
			(0.083)			
(Female)*(Pre-medical college req't)					-0.034	
					(0.034)	
(Black)*(Pre-medical college req't)						**0.180**
						(0.077)*
Female	-0.412	-0.413	-0.412	-0.412	-0.399	-0.412
	(0.035)**	(0.039)**	(0.035)**	(0.035)**	(0.032)**	(0.034)**
Black	-0.673	-0.673	-0.706	-0.673	-0.673	-0.754
	(0.037)**	(0.037)**	(0.050)**	(0.037)**	(0.037)**	(0.050)**
Observations	1,117,373	1,117,373	1,117,373	1,117,373	1,117,373	1,117,373
State years	192	192	192	192	192	192

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.

Table 10: Effects of teacher licensing requirements on women and blacks

	(1)	(2)	(3)	(4)	(5)	(6)
High school graduation req't	0.017	-0.074	0.006			
	(0.022)	(0.026)**	(0.023)			
Some college req't				0.029	-0.058	0.021
				(0.022)	(0.021)**	(0.024)
(Female)*(High school req't)		**-0.240**				
		(0.021)**				
(Black)*(High school req't)			**0.151**			
			(0.035)**			
(Female)*(Some college req't)					**-0.248**	
					(0.026)**	
(Black)*(Some college req't)						**0.137**
						(0.048)**
Female	1.101	1.249	1.102	1.101	1.208	1.102
	(0.026)**	(0.034)**	(0.026)**	(0.026)**	(0.028)**	(0.026)**
Black	-0.388	-0.395	-0.468	-0.388	-0.393	-0.439
	(0.035)**	(0.035)**	(0.040)**	(0.035)**	(0.036)**	(0.036)**
Observations	1,187,781	1,187,781	1,187,781	1,187,781	1,187,781	1,187,781
State years	192	192	192	192	192	192

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.

Table 11: Effects of physician licensing requirements on young women and blacks

	(1)	(2)	(3)	(4)
Four-year med school req't	-0.010	-0.008		
	(0.035)	(0.027)		
Pre-medical college req't			0.046	0.041
			(0.042)	(0.043)
(Female)*(Four year med school req't)	0.021			
	(0.088)			
(Black)*(Four year med school req't)		-0.040		
		(0.136)		
(Female)*(Pre-medical college req't)			-0.002	
			(0.106)	
(Black)*(Pre-medical college req't)				0.180
				(0.121)
Female	-0.574	-0.565	-0.565	-0.565
	(0.068)**	(0.061)**	(0.080)**	(0.061)**
Black	-0.693	-0.684	-0.693	-0.751
	(0.061)**	(0.068)**	(0.061)**	(0.076)**
Observations	600,692	600,692	600,692	600,692
State years	192	192	192	192

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.

Table 12: Effects of teacher licensing requirements on young women and blacks

	(1)	(2)	(3)	(4)
High school graduation req't	0.300	0.016		
	(0.026)**	(0.025)		
Some college req't			0.278	-0.018
			(0.037)**	(0.027)
(Female)*(High school req't)	**-0.396**			
	(0.026)**			
(Black)*(High school req't)		**0.268**		
		(0.041)**		
(Female)*(Some college req't)			**-0.402**	
			(0.028)**	
(Black)*(Some college req't)				**0.248**
				(0.056)**
Female	1.390	1.168	1.321	1.167
	(0.045)**	(0.036)**	(0.034)**	(0.036)**
Black	-0.455	-0.568	-0.451	-0.521
	(0.035)**	(0.036)**	(0.036)**	(0.034)**
Observations	591,093	591,093	591,093	591,093
State years	192	192	192	192

Notes: State and year fixed effects as well as individual and household level controls (age, literacy, domestic, married, widowed, children, two families, three families, at school) are also included. Robust standard errors, clustered at state level, are in parenthesis. ** and * denote statistical significance at the 1 and 5 percent levels, respectively.