WAGE EFFECTS OF UNIONIZATION AND OCCUPATIONAL LICENSING COVERAGE IN THE UNITED STATES

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Recent estimates in standard models of wage determination for both unionization and occupational licensing have shown wage effects that are similar across the two institutions. These cross-sectional estimates use specialized data sets, with small sample sizes, for the period 2006 to 2008. The authors' analysis examines the impact of unions and licensing coverage on wage determination using new data collected on licensing statutes that are then linked to longitudinal data from the National Longitudinal Survey of Youth (NLSY79) from 1979 to 2010. They develop several approaches, using both cross-sectional and longitudinal analyses, to measure the impact of these two labor market institutions on wage determination. The estimates of the economic returns to union coverage are greater than those for licensing statutes.

During the past 50 years in the United States, union membership has been declining but the number of individuals who work in licensed occupations has been increasing (Kleiner and Krueger 2010). Recent cross-sectional estimates in standard models of wage determination for both of these important labor market institutions across the same data sets have shown similar wage effects for the two, averaging about 15% (Kleiner and Krueger 2013). These estimates used specialized data sets from the period

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2006 to 2008. Given the small sample size and cross-sectional construction of the surveys used in these studies of the wage outcomes of unions and occupational licensing, additional research is warranted to more fully understand the specific outcomes of such regulations.

We examine the impact of unions and licensing coverage on wage determination using new data collected on licensing statutes that are then linked to longitudinal data from the National Longitudinal Survey of Youth (NLSY79) from 1979 to 2010. The approach provides an alternative examination of the role of each of these institutions in the labor market.

In our analysis of these two labor market institutions, we first review past studies of the impact of both unions and occupational licensing on wage determination. Some economists have long held that the economic effect of unions and occupational licensing are similar in the context of the rent-seeking behavior of both labor market institutions (Friedman and Kuznets 1945; Friedman 1962).[1] The literature on wage determination under unions is well-developed and includes several syntheses by researchers such as Freeman and Medoff (1984), Lewis (1986), and Hirsch (2004). In contrast, the analyses of the wage effects of occupational licensing generally consist of cross-sectional estimates with appropriate human-capital covariates but with few panel or time-series examinations of the issue. Exceptions to these approaches include panel estimates and longitudinal measures of regulation (Kleiner 2006; Thornton and Timmons 2015). In the United States, a structural shift in the economy has developed, with a movement to a service-oriented economy from manufacturing, a sector in which unions and contracts previously were prominent, and the transformation has created a demand for a "web of rules" of the workplace that licensing may have provided (Dunlop 1958). In this context, unions provided contracts that laid out the terms and conditions of work. In a somewhat similar manner, occupational licensing set the legal framework for who can work, the number of workers, and conditions for the termination of employees through revoking the license of those who are incompetent or unscrupulous.

We then present cross-sectional estimates using the NLSY79 as a baseline to compare our estimates with those in the literature, estimating the influence of licensing by linking each individual in the NLSY79 to whether the state in which he or she worked had a licensing law covering his or her occupation. Next, we specify the longitudinal methods that we use in the NLSY79, which use switchers (workers who moved into and out of unionization and licensing coverage) in addition to individuals who did not move between these categories, and consider the implications for current and future research on the role of labor market institutions on wage determination.

[1]For both unions and occupational licensing, the supply of labor is assumed to be restricted, although through different mechanisms. Consequently, which labor market institution has a greater influence on wage determination is an empirical issue that our study attempts to illuminate (Friedman and Kuznets 1945; Friedman 1962).

Labor Market Research on Unions and Occupational Licensing

Unions

Traditional economic theory has generally treated the actions of trade unions in the labor market as a variant of monopoly behavior in product markets (Cartter 1959). For example, at the time of contract negotiations, the trade union acts as a single voice representing its members. Consequently, the employer is faced with a single seller of labor. Subsequent analysis by Freeman and Medoff (1984) argued that unions have both a monopoly and a voice effect. The monopoly effect is similar to that presented by Cartter, who suggested that supply is reduced but the voice effect provides benefits beyond just the financial ones. These benefits include grievance procedures and the ability to have seniority determine promotions and wages rather than having them assigned only by the employer.

Unions also can engage in concerted activities, such as strikes or work to rule, that can raise the cost to the firm of employing organized workers relative to nonunion ones. If the companies or plants want to avoid these concerted activities, they have to pay the higher wage and benefit package. Therefore, unions have the ability to reallocate a firm's resources away from shareholders or capital investment and toward workers. Recent estimates of these reallocations are the present value equivalent of $40,500 per worker in 1998 dollars over the duration of the worker's employment with the firm (Lee and Mas 2012). To the extent that economic rents are present in the firm because of patents, location advantages, or economies of scale, unions are able to reallocate part of those resources to union members. In addition, unions in the private sector are given legitimacy and certain levels of economic protection through federal legislation, such as the National Labor Relations Act (Kleiner and Weil 2012).[2] In the public sector, laws governing unions are established at the state level. Consequently, through their ability to monopolize labor at the firm level and public policy protections through federal and state statutes, unions should be able to drive up wages and benefits.

Evidence from firms that have experienced new organizing drives shows that initially unions have small effects on wage determination (Freeman and Kleiner 1990; Lee and Mas 2012). Over time, however, unions appear to have larger influences (Lee and Mas 2012). Because unions reached their highest level of organization and influence during the 1950s, their ability to drive up wages could reflect the long duration of their existence within an establishment.

A large body of labor market literature has documented the wage gains from unionization (Lewis 1986). One of the major efforts of the empirical work has been to estimate the union-nonunion relative wage differential, or the wage gap. The estimates have been broken down by age, sex, region,

[2]The Taft–Hartley amendments to the act do, however, allow for unions to be voted out, and in right-to-work states, individuals do not have to join a union or pay union dues. When a union wins an election at an establishment, it is the exclusive representative of the workers for a minimum of one year.

industry, and occupation. The methods used have varied from cross-sectional and time-series approaches to panel estimates using difference-in-difference modeling. Further methodological techniques have included the use of both selection-correction approaches and approaches using vast arrays of observable covariates. A significant part of the literature has also focused on the simple threat or spillover interpretation of the wage effects. The estimates of the general macro-effect of unions on relative wages have ranged from 15 to 20% (ibid.). Updates in the United States found these estimates to be greater than 15% up through 2012 (Hirsch and Macpherson 2014).

Occupational Licensing

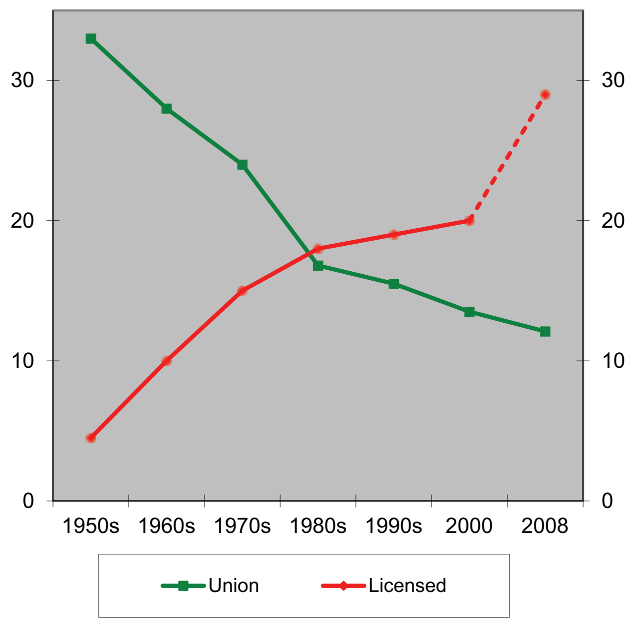
In contrast to unionization, whose stated objective is to protect workers, occupational licensing has the perceived goal of protecting the public against incompetent, untrustworthy, or irresponsible practitioners. Occupational regulation in the United States generally takes three forms: registration, certification, and licensure. The least restrictive form is registration, in which individuals file their names, addresses, and qualifications with a government agency before practicing their occupation. The registration process may include posting a bond or filing a fee. In contrast, certification permits any person to perform the relevant tasks, but the government—or sometimes a private nonprofit agency—administers an examination and certifies those who have achieved the level of skill and knowledge necessary for certification. For example, travel agents and car mechanics are generally certified but not licensed.[3] The toughest form of regulation is licensure; this form of regulation is often referred to as "the right to practice." Under licensure laws, working in an occupation for compensation without first meeting government standards is illegal. In 2003, the Council of State Governments estimated that more than 800 occupations were licensed in at least one state and that more than 1,100 occupations were licensed, certified, or registered (Council on Licensure, Enforcement, and Regulation [CLEAR] 2004). The vast majority of occupations are licensed in some states and not in others.

Figure 1 shows trends in the growth of unionization and occupational licensing from 1950 to 2008. Licensing data for earlier periods are available only at the state and occupational levels; the data gathered through the Gallup and Westat surveys for 2006 and 2008 are denoted with a dashed line in the figure and represent the attainment of an occupational license (see Kleiner and Krueger 2010, 2013). Despite possible problems in both data series, occupational licensing clearly is rising and unionization is declining.[4]

[3]For analyses of the relative wage premia of licenses versus certifications, see Kleiner and Krueger (2013) and Gittleman, Klee, and Kleiner (2015). The former uses data from the Princeton Data Improvement Initiative (PDII), and the latter uses data from the Survey of Income and Program Participation.

[4]Growth in employment in occupations requiring licensing can be decomposed into shares attributable to growth in employment in the occupations requiring licensing and to occupations becoming newly licensed. Kleiner (2006) found that, for the state of Minnesota between 1990 and 2000, three-quarters of the growth was attributable to the first factor. Although the general tendency over time is for more occupations to require licenses in more states, the majority of growth in occupational licensing seems to be coming from employment increases in occupations for which licenses were already required.

Figure 1. Comparisons in the Time-Trends of Two Labor Market Institutions: Percentage Licensed and Percentage Unionized[a]



Sources: Percentages unionized are from the Current Population Report, various years; Kleiner and Krueger (2013).
[a]Dashed line extends from state-only estimates to the Gallup and Westat PDII estimates, which include licensing by all levels of government. PDII, Princeton Data Improvement Initiative.

By 2008, approximately 29% of workers polled in the Westat survey said they were required to have a government-issued license to do their job, compared to about 12.4% who said they were union members in the Current Population Survey (CPS) for the same year. Because occupational licensing experienced its more rapid growth since the 1970s, its impact on individual wages may not be as high as unionization, which has had a longer period to use collective bargaining and other concerted activities to enhance earnings.

A basic explanation of occupational licensing suggests that administrative procedures regulate the supply of labor in the market. The regulators screen entrants to the profession and bar those whose skills or character traits suggest a tendency toward low-quality output. The regulators further monitor incumbents and discipline those whose performance is below the standards, with punishments that may include the revocation of the license needed to practice. Assuming that entry and ongoing performance are controlled in these ways, we would expect the quality of service in a profession to be raised by occupational licensing but supply and access to services to be diminished.

Additional costs could include imposition of fines and screening to prevent expelled practitioners from reentering the occupation or the requirement that incumbents put up capital that would be forfeited on loss of the license or incompetent or unscrupulous behavior. Entry requirements limit supply and potentially create monopoly rents within the licensed occupation. The threat of losing these monopoly rents could, in principle, give incentives to incumbents to meet high standards. The rents also could motivate potential entrants to invest in high levels of training to gain admittance. Demand for the services of licensed workers could increase because of the perceived higher quality and lower risk, but demand might also decrease for some segments of the occupation if some consumers demand lower-quality services that are precluded by the licensing procedures (Shapiro 1986). An outward shift in demand could accentuate the increase in the price of services resulting from diminished supply and further boost provider incomes. These models of licensing assume that consumers can choose among three markets: a market for mature producers known to sell high-quality services, a market for mature producers known to produce low-quality services, and a market for young producers whose quality of service (low or high) is not known by the consumer at the time of purchase (ibid.). The result is that seekers of high-quality services gain by regulation and seekers of low-quality services are worse off because prices are higher and choices are more limited.

The source of market influence for occupational licensing is that the members of the occupation can manipulate the pass rate to restrict entry and raise wages (Friedman 1962; Maurizi 1974; Kleiner 1990; Pagliero 2010). Across a large number of studies on occupational licensing, which are primarily cross-sectional in methodology, the main result has been that occupational licensing attainment increases wages by approximately 15% (Bryson and Kleiner 2010). This result is remarkably similar to that found for unionization. Unfortunately, the ability to examine the influence of licensing on wage determination using panel data analysis has been limited (Kleiner 2006, 2013; Thornton and Timmons 2015).

In comparing licensing to unionization, our expectation is that unionization would have a larger influence on wage determination than licensing because unions showed sizable growth during the 1940s to 1950s, well before much of the growth of occupational licensing, which largely occurred after the 1970s. Several studies on the impact of licensing over time have shown that its influence grows over time as the occupations and administrative agencies increase the requirements for entry; as the older, less-trained practitioners leave or retire; and as the more rigorous requirements restrict the supply of new practitioners (Kleiner and Vorotnikov 2012; Thornton and Timmons 2013; Han and Kleiner 2015). To the extent that duration of a labor market institution matters in wage determination, then, unions would have a larger impact on individual wages.

Data

Given our interest in applying longitudinal methods to the analysis of the impact of unionization and licensing coverage on wages, the main data set we use is the NLSY79. The NLSY79 is a nationally representative sample of young men and women who were between the ages of 14 and 22 at the time of their first interview in 1979. Individuals were surveyed annually beginning in 1979 and biennially beginning in 1994; the latest data available, at the time of analysis, were for 2010. The NLSY79 has been widely used in studies of the labor market because of the wealth of information it has collected in this area.

The NLSY79 has collected wage information on up to five jobs in each wave of the survey. For the purposes of our analysis, however, we restrict attention to the respondents' current or most recent job, also known as the CPS job, in part because other labor market variables that we need are more readily available for this job.[5] For brevity, we refer to this job as the current one. To be included in our samples, we generally require, for each year for the current job, that an individual must have been working for pay, have had a valid wage, and have had a valid occupation in the data. In addition, the individual must not have been enrolled in school (as of May of the survey year) and must have had valid information on the state of residence (which is needed to determine whether the occupation requires a license).[6] Furthermore, to limit the influence of wage outliers, we also require that the wage have been higher than one-half the real value of the minimum wage as of January 1, 1981[7] ($3.35 in 1981 dollars) and have been lower than $75 per hour (in 2010 dollars).[8]

The occupation coding that the NLSY79 uses has changed over time. From 1979 to 2000, it used the U.S. Census Bureau's 1970 occupation codes, but from 2002 on, it used the 2000 Census codes. The characteristics of the sample are summarized in Table 1.

Despite the breadth of the labor market data collected in the NLSY79, that survey, in common with other large-scale surveys, has not collected information on whether a license was required for the individual's job. Thus, we derive that information by researching state laws. CareerOneStop (2015), sponsored by the Employment and Training Administration of the U.S. Department of Labor, maintains a website containing a list of occupations by state that require licenses (http://www.acinet.org/licensedoccupations/). This information is gathered by each state's Labor Market

[5]In the event the respondent had more than one employer, the CPS job is the one with the employer for whom the respondent worked the most hours.

[6]Licensing information is not available for the District of Columbia, so all individuals with residence there are eliminated from the sample. Our information is on licensing at the state level because we do not have information on requirements at the local or federal levels.

[7]The deflator we use is the consumer price index research series (CPI-U-RS) from the Bureau of Labor of Statistics.

[8]Our results are not sensitive to the imposition of the $75 limit, which was decided on because of the way wage data are collected and computed in the NLSY79. The NLSY79 computes hourly wage rates by collecting a rate of pay and a time unit for that rate of pay (e.g., hourly, weekly, annually). No top coding is specified, but if an individual's rate of pay exceeds a maximum, his or her hourly wage is marked as missing. From 1979 to 1993, the maximum rate of pay for any time unit was $99,999.99; this was raised by a factor of 10 in 1994, and further upward adjustments were made thereafter. Thus, the lowest maximum rate of pay in real terms occurred in 1993. If individuals reporting annually were working 2,000 hours per year, then the lowest maximum is equivalent to somewhat less than $75 per hour in 2010 dollars.

Table 1. Characteristics of Sample, NLSY79, 1979 to 2008

Variable	*Mean*	*SD*
Licensing definitions		
II-Partial	0.309	0.462
I-Partial	0.110	0.313
II-Whole	0.101	0.302
Union[a]	0.172	0.377
Age (yrs.)	30.8	8.0
Female	0.473	0.499
Hispanic	0.061	0.239
African American	0.130	0.336
Schooling (yrs.)	13.0	2.3
Potential experience (yrs.)[b]	11.8	7.9
Government	0.112	0.315
Self-employed	0.062	0.242
Part-time[c]	0.147	0.356

Notes: Unless otherwise indicated, variables are coded 0 or 1, where 1 represents the presence of the characteristic. All labor market variables apply to the current/most recent job. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing, which includes the variables in the table, wage, state, and industry and occupation. Number of observations is 131,043. Weighted by NLSY longitudinal weights. SD, standard deviation.
[a]Union is an indicator variable for whether the job was covered by a collective bargaining agreement.
[b]Potential experience = max(0, min(age − 14, age − years of schooling − 6)).
[c]Part-time indicates the respondent usually worked fewer than 35 hours per week.

Information unit under a grant from the U.S. Department of Labor. The website provides four pieces of information for each listing: occupation, license name, licensing agency, and state.

The occupations are based on those in the Standard Occupational Classification (SOC) system manual so that each one listed either corresponds to a six-digit SOC code or fits neatly into one. Given that the 2000 Census codes are SOC-based, mapping the occupations requiring licenses in a given state to their corresponding 2000 Census codes is straightforward. For those 2000 Census codes involving at least some licensing requirement, we want to assess whether the state required all those in a given 2000 Census code to hold a license or whether only some in that code were required to have a license. Such partial licensing can be of two types. First, the group for which a license is required may be a subset of the 2000 Census code. For example, some states require electricians who are contractors to be licensed but not other electricians. Second, even if the occupation in the statute is the same as that in the 2000 Census code, not everyone may be required to have a license in that occupation. Common examples of this type are the engineering professions, in which licenses are required for certain tasks but not for most individuals working in the occupation (Hur, Kleiner, and Wang, forthcoming). We have more information on the first type of partial licensing than on the second.

When we concluded our data-gathering efforts, we had a data set that classifies every possible state-by-2000-Census-occupation pair into one of three categories: 1) no licensing requirement; 2) a license is required for some of the job-holders in the occupation; and 3) a license is required of all occupational incumbents. *State-by-occupation pair* merely means that, when we have a state (s) and an occupation (o), we then have a state-by-occupation pair (s, o) to which we can assign a licensing-coverage status. We use a similar procedure to categorize each state-by-1970-Census-occupation pair.[9]

Once the categorization of the state by 1970 occupation pairs was complete, we were in possession of a current licensing scheme that works for two different occupation classification systems. What we really want, however, are licensing schemes for every survey year from 1979 to 2000 using 1970 codes and for every survey year from 2002 to 2010 using 2000 codes. Put differently, we want to know the starting date for the licensing requirement for all the state-by-occupation pairs.[10] Unfortunately, other than researching the statutes for every state-by-occupation pair, we know of no source for this information. Thus, we are forced to make different sets of assumptions about when licensing began in the state-by-occupation pairs and examine how sensitive our results are to these sets of assumptions. As we indicate later, for one set of assumptions we have been able to narrow down the number of occupations with a licensing-requirement start date that needs further examination from thousands to hundreds and have been able to check state laws to see when these occupations first became licensed.

[9]As with the 2000 Census codes, the occupations requiring licenses in each state must be matched to a 1970 Census code. In many cases, this process is straightforward because many of the licensed occupations that have their own codes in the 2000 classification system either had them in the 1970 system as well or fit neatly into 1970 categories. When this is not the case, however, we had to surmount the obstacle that the occupation classification system has undergone major changes over the last four decades, particularly between the 1970 and 1980 Censuses and the 1990 and the 2000 Censuses. Because no crosswalk exists from 1970 to 2000 that we are aware of, for some occupations we had to track back over the different classification schemes to arrive at the best match in 1970. Detailed crosswalks are available for the changes from 1970 to 1980 (U.S. Bureau of the Census 1989) and from 1990 to 2000 (Scopp 2003); the changes from 1980 to 1990 are quite minor, especially in comparison. The closest we have seen to a 1970 to 2000 concordance is the valiant effort of Meyer and Osborne (2005) to put the occupation codes for the 1960, 1970, 1980, 1990, and 2000 Censuses on a single, consistent basis. Although we find their work to be a useful check on ours, we did not apply it directly for several reasons. First, our purpose is somewhat different in that, from the occupation or license name, we often know that we are working with a subset of a 2000 Census code rather than having to take all occupations in a 2000 code and match it to a 1970 code or codes. Second, for many 2000 Census codes no 1970 match exists in Meyer and Osborne (2005). Third, although aggregating occupations to get better matches over time makes sense for Meyer and Osborne, in general, doing this does not make sense for our study because we do not want to combine occupations that may be treated differently as far as licensing requirements are concerned.

[10]Implicitly, this formulation assumes that no occupations became unlicensed during the period, an assumption that, although not literally true, is almost always the case (Thornton and Timmons 2015).

We use three different licensing definitions in the empirical analysis, varying along two dimensions: 1) the assumptions made about the starting date for the licensing requirements and 2) the treatment of state–occupation pairs that are partially licensed. Our first step in the process of assigning start dates is to take all the occupations that have some sort of licensing requirement in the current data, under both the 1970 and 2000 codes, and divide them into two categories based on our knowledge of the occupations' licensing history. We placed occupations with such a lengthy history of licensing that widespread requirements were in place before the start of the NLSY79 in the commonly licensed group (group I). Examples of occupations in the commonly licensed group include barbers, physicians, and accountants. All other occupations for which licenses were required in at least one state were placed in group II. Group II, thus, includes occupations that are rarely licensed, such as roofers, cooks, and upholsterers, as well as occupations for which licensing requirements are somewhat more common, such as electricians, librarians, and massage therapists.[11]

For all three licensing definitions, we assume that for the occupations in group I the licensing start date was prior to the beginning of the NLSY79 panel. In our first definition, we do not include as licensed those state–occupation pairs in which only some of the incumbents are required to have a license. We then researched the actual dates of the start of licensing requirements for all the remaining state-by-occupation pairs in group II. We call this definition II-Whole because it includes group II occupations (all three definitions include group I occupations) but only the wholly licensed state–occupation pairs and not the partially licensed ones. For both of our other two definitions, partially licensed state–occupation pairs are included. In the second definition, we assume that the start dates of the licensing of group II occupations came after the last year we use in our NLSY sample, so only group I occupations are included; we call this I-Partial. For the third definition, we assume both group I and II occupations had licensing start dates prior to 1979; we call this II-Partial. Table 2 lays out the three different scenarios we consider.[12]

In Table 1, we show the prevalence of licensing requirements in our samples under each of the three definitions. The most inclusive definition is II-Partial, with an average of about 31% of employment during the period. The two remaining definitions include less than 10 to 11% of the sample. By way of comparison, Kleiner and Krueger (2013) estimated that 29% of the U.S. workforce attained a governmental license in 2008, but their estimates included those licensed not only by the state but also by the federal and local governments. Their state-licensed-only value was about 23% in 2008. The respondents in their sample tended to be somewhat older than those in the NLSY79 sample, which would also tend to increase their estimate of the proportion licensed.

[11]A complete list of occupations in each group is available on request.

[12]We also considered two additional scenarios not discussed here, but the results from these tended to be bracketed by the results we present here.

Table 2. Licensing Definitions and Assumptions

Licensing definition name	*Partially licensed state–occupation pairs included?*	*Groups whose occupations are included*
I-Partial	Yes	Group I only
II-Whole	No	Group I and state–occupation pairs in group II for which research indicates a start date prior to or during sample period
II-Partial	Yes	Groups I and II

Notes: Group I comprises occupations with such a lengthy history of licensing that widespread requirements were in place before the start of the NLSY79; these are commonly licensed occupations. Examples include barbers, physicians, and accountants. Group II comprises all other occupations for which licenses were required in at least one state. It thus includes occupations that were rarely licensed, such as roofers, cooks, and upholsterers, as well as occupations for which licensing requirements were somewhat more common, such as electricians, librarians, and massage therapists. A list of all occupations included in Groups I and II is available on request.

Empirical Methods

At the center of the analysis is the question, what is the impact of occupational licensing requirements on wages? Similarly, we also want to assess the wage effect of unions, the presence of which we measure by whether an individual was covered by a collective bargaining agreement.[13] Although stating what we hope to measure is easy enough, getting unbiased estimates is difficult for the usual reasons. That unions, with higher wage effects for less-skilled workers, give rise to a two-sided selection problem is well known; this happens when less-skilled workers seek union jobs while union employers seek to hire those with more skills (Abowd and Farber 1982). Although licensing poses a different selection problem, in part because many jobs are for the self-employed and because (as we discuss later) wage compression is not a goal, selection effects are likely to occur, nonetheless. Thus, even after controlling for the standard human-capital characteristics when estimating a wage regression, we might still suspect that those who entered licensed occupations or obtained union coverage might differ from those who did not in ways that are unobservable to an econometrician. As a result, the variables of interest may be correlated with the error term, which could render ordinary least squares biased.

To address our questions, we try a number of different approaches, each with advantages and disadvantages. Our multivariate analysis begins with cross-sectional regressions with fairly standard controls, an approach that allows us to compare our results to past research. In addition, in the case of unions, Freeman (1984) argued that such an approach is superior to fixed effects, at least when one uses the short (two-year) panels that are produced from matching CPS data from adjacent years, because of a greater robustness to measurement error.

[13]Determining union membership is possible only in selected years in the NLSY79, but union coverage is available for all years except for 1994, when the union questions were not asked of everyone.

Our second approach is to control for the unobserved heterogeneity that may be biasing the results by including proxies for ability. The indicators of ability that we use are the Armed Services Vocational Aptitude Battery (ASVAB) test scores, which have been standardized and adjusted for year and quarter of birth. These tests consist of 10 categories: paragraph comprehension, general science, arithmetic reasoning, mathematics knowledge, word knowledge, mechanical comprehension, numerical operations, electronic information, auto and shop information, and coding speed.

Our third and fourth methods take advantage of the longitudinal structure of the NLSY79. We employ a simple nonparametric approach that classifies individuals on the basis of their licensing and union statuses in adjacent waves of the survey.[14] Wage growth is then compared across the different categories, yielding the estimates we are interested in. For instance, we compare the wage growth of those whose licensing status did not change to that of those who moved into a job requiring a license and that of those who moved out of such a job.

Finally, in our fifth approach, we use a more formal longitudinal method and estimate individual fixed-effects regressions.[15] If we assume that for each individual a fixed effect exists that is independent of sector and that is the source of the correlation between the sector-status variables and the error term, then fixed effects will provide unbiased estimates.[16]

In the analysis in the next section, we focus on one of the licensing-definition variables, II-Whole, the one that includes our research on licensing requirement start dates for state-by-occupation pairs (s, o) in group II. In addition to the greater accuracy that comes from this research, we also gain an important source of variation in that we can observe occupations before and after they were licensed in a given state. As one of our robustness checks, however, we also present results for the two other definitions of licensing.

In addition, we know that all the licensing-definition variables are affected by measurement error, an issue that is of particular importance in our fixed-effects estimation. To address this issue, we use techniques developed by Bollinger (1996, 2001) to bound the licensing parameters.

Results

Before turning to the analysis of the data, we examine briefly how individuals who face licensing requirements differ from those who do not. In Table 3, we show the means of certain key characteristics for each definition of licensing. Across all the definitions, those who face licensing requirements tend

[14]A similar approach was used by Freeman (1984) in his analysis of union wage effects.

[15]For related work on unions, see Robinson (1989) and references therein.

[16]Whereas, as noted, little has been done using longitudinal methods in the licensing literature, that is not the case in studies of unions. For example, Lemieux (1998) estimated a model that takes into account selection by both employers and workers and allows for different rewards to the permanent component of the error term in the union and nonunion sectors. Because here we are considering licensing and union coverage simultaneously, considerations of tractability prevent us from modeling selection in an analogous fashion. Thus, the possible influence of endogeneity on our estimates cannot be ignored even though how robust its influence might be is difficult to determine.

Table 3. Means of Characteristics of Sample by Licensing Status, All Definitions

	II-Partial		*I-Partial*		*II-Whole*	
Licensing definition	*0*	*1*	*0*	*1*	*0*	*1*
Union[a]	0.177	0.159	0.170	0.188	0.170	0.192
Age (yrs.)	30.6	31.3	30.7	32.4	30.6	32.6
Female	0.473	0.471	0.459	0.583	0.460	0.587
Hispanic	0.061	0.061	0.062	0.050	0.063	0.046
African American	0.142	0.102	0.133	0.101	0.133	0.101
Schooling (yrs.)	12.8	13.6	12.8	14.7	12.8	14.8
Potential experience (yrs.)[b]	11.9	11.7	11.8	11.7	11.8	11.9
Government	0.102	0.133	0.098	0.224	0.099	0.227
Self-employed	0.057	0.075	0.062	0.064	0.062	0.064
Part-time[c]	0.153	0.149	0.150	0.167	0.150	0.162

Notes: Unless otherwise indicated, variables are coded 0 or 1, where 1 represents the presence of the characteristic. All labor market variables apply to the current/most recent job. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing, which includes the variables in the table, wage, state, and industry and occupation. Number of observations is 131,043. Weighted by NLSY79 longitudinal weights.
[a]Union is an indicator variable for whether the job was covered by a collective bargaining agreement.
[b]Potential experience = max(0, min(age – 14, age – years of schooling – 6)).
[c]Part-time indicates the respondent usually worked fewer than 35 hours per week.

to have higher levels of schooling, to be somewhat older, to be somewhat less likely to be African American, and to be more likely to work for the government.[17] Except for the most inclusive definition, Union coverage and Percentage female are somewhat higher in the licensed sector. These estimates are similar, although not identical, to the basic data found in the Princeton Data Improvement Initiative (PDII) on occupational licensing (Kleiner and Krueger 2013).

Cross-Sectional Estimates

Table 4 presents cross-sectional estimates of the effects of licensing requirements (using the II-Whole definition) and union coverage on Log wages.[18] In the first column of estimates, to get a sense of the upper bound on the impact of licensing using this variable, we show the results of using an indicator for a licensing requirement as the sole regressor.[19] We get a coefficient

[17]We save for future work an examination of whether the licensing premium differs between the public and private sectors, as does the union premium. One challenge for this research is the difference between the two sectors in the occupational mix of those who are licensed.

[18]Throughout, standard errors take account of the complex survey design of the NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron, Gelbach, and Miller 2011). Although individuals do appear in the sample multiple times, leading to the dependence of these observations, we cluster on primary sampling units to take this into account.

[19]As noted later, because of measurement error this is not actually an upper bound.

Table 4. Impact of Licensing and Union Coverage on Wages, Cross-Sectional Estimates

	(1)	(2)	(3)	(4)
Licensed	0.280**	0.118**	0.074**	0.074**
	(0.019)	(0.013)	(0.013)	(0.012)
	[0.414]	[0.174]	[0.110]	[0.109]
Union		0.157**	0.183**	0.195**
		(0.008)	(0.008)	(0.008)
R^2	0.026	0.429	0.460	0.473
Number of observations	131,043	131,043	131,043	131,043
Standard controls[a]	No	Yes	Yes	Yes
Occupational controls[b]	None	None	One-digit	Two-digit

Notes: Licensing definition: II-Whole. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. The upper bound of licensing appears in brackets. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Column (1) controls for licensing. Column (2) adds the standard set of regressors (indicators for female, Hispanic and African American, years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year). Columns (3) and (4) add controls for occupations: column (3) major-occupation dummies; column (4), two-digit-occupation dummies.
[a]Standard controls include demographic and human capital variables (indicators for female, Hispanic and African American, as well as controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.
[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.
*Indicates significant at 5%; ** significant at 1%.

of 0.280 log points, similar to the 0.297 log points that Kleiner and Krueger (2013) estimated in a similar model for attaining a license. In column (2), we add what we refer to as our standard set of regressors. These include demographic and human-capital variables (indicators for female, Hispanic and African American, years of schooling, potential experience, and potential experience squared); indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry,[20] state of residence, and year. Note that the coefficient is sharply reduced, falling to 0.118 log points, suggesting that a good portion of the higher pay for those licensed is coming from such individuals' having higher levels of schooling and other characteristics that are rewarded in the labor market.

[20]The 1970 codes have 20 categories for major industries, and the 2000 codes have 13 categories.

Our next specifications add occupation controls. Although their use has become more common in labor economics in recent years, we recognize that some controversy remains. We use them to enable within-occupation comparisons of those with licenses to those without, using both major-occupation and two-digit-occupation dummies.[21] The addition of major-occupation dummies (column (3)) leads to a further significant reduction in the return to licensing in terms of percentage, with the coefficient declining to 0.074 log points. Thus, licensed occupations seem to belong disproportionately to major occupations that tend to be higher paying even in the absence of licensing requirements. Considerable heterogeneity still exists within major occupations, so we could be comparing, say, licensed electricians to unlicensed plumbers. The replacement of controls for major occupations with those for two-digit occupations (column (4)) results, however, in no discernible impact.

What are the results for union coverage? The coefficient on that variable is always statistically significant: 0.157 log points for the second specification, 0.183 log points with major-occupation dummies, and 0.195 log points for the two-digit-occupation specifications. Thus, in contrast to the case for licensing, the results for unionization with occupation controls indicate a slight tendency for those covered by a collective bargaining agreement to be in occupations that tend to be low paying. Moreover, the simple correlation between licensing coverage and union coverage is low, at 0.02, suggesting that little collinearity exists between these two wage-determination variables.

Our second cross-sectional approach to estimating the returns to licensing requirements involves the inclusion of the ASVAB test scores as proxies for ability in the second through fourth specifications. Because we find that the inclusion of the ASVAB test scores has virtually no impact on the licensing coefficients, we do not present the results here. The ASVAB scores do appear to be correlated with both wages and with membership in a licensed occupation; however, conditional on the inclusion of the standard controls, little relationship appears to exist between the ASVAB scores and the licensing indicators. Similarly, the results for union coverage with the proxies for ability included differ little from those without them.

[21]The 1970 codes have 12 categories for major occupations and 44 for two-digit occupations. The 2000 codes have 10 categories for major occupations and 22 for two-digit occupations. We also tried specifications with detailed occupations, with more than 400 occupations for each set of codes. We do not have confidence in these estimates for two reasons. First, the large number of detailed occupations exacerbates the bias toward zero that results from measurement error. Second, there are difficulties in identifying the coefficient on licensing when detailed occupational controls are included. Because we also control for state and year when we include detailed occupation effects, for licensing to have a significant measured impact on Log wages, all else equal, workers in detailed occupations in the licensed states must either be higher paid than those in the same occupations in the states not requiring licenses or be higher paid relative to those in the same occupation in the same state when it did not require a license. We found that, under the II-Whole licensing definition, only about one-fifth to one-quarter of employment was in detailed occupations in which some individuals were licensed and some were not, with most employment being in occupations in which no individuals were licensed and a small amount (about 1%) being in occupations in which all individuals were licensed. Thus, the coefficient on licensing is identified on a subset of occupations, one in which licensing differentials may not be representative of those in all occupations.

Longitudinal Estimates

Nonparametric Comparison

We begin the longitudinal analysis with a simple, nonparametric comparison of the growth of average Log wages by group, with the groups defined on the basis of how licensing and union status evolved over time. In Table 5, panel A, we show the average growth of Log wages in adjacent survey years for three groups: 1) those who moved out of jobs having licensing coverage; 2) those who moved into jobs having licensing coverage; and 3) those whose licensing coverage did not change, which combines those who remained in unlicensed jobs and those who remained in licensed jobs. We can estimate the return to licensing either by subtracting the growth of the average Log wages in category 1 from that in category 3 or by subtracting the growth in average Log wages in category 3 from that in category 2. If the return to licensing is stable over time, then the two estimates should be equal.

The point estimate derived from comparing respondents who moved out of jobs that are covered by licensing to all who did not move is 0.004 log points, which is not statistically significant. The point estimates derived from comparing those who moved into jobs that are covered by licensing to all who did not move is 0.015 log points, which also is not statistically significant. In other words, using this approach, the return to licensing coverage is estimated to be quite small or nonexistent.

In Table 5, panel B, we repeat the analysis for union coverage instead of licensing status. Both possible estimates of union coverage are 0.059 log points and are statistically significant; this is considerably lower than the estimates from the cross-sectional results but consistent with other findings that show longitudinal results are lower.

Finally, in Table 5, panel C, we examine switches into and out of licensing and union coverage simultaneously. In any given year, an individual may be in one of four categories in terms of union and licensing coverage: both licensing and union coverage; licensing but no union coverage; union but no licensing coverage; and no licensing or union coverage. Given that, we have 16 possible patterns (4 × 4) for any two adjacent years. Excluding the cases in which an individual both switched into licensing and out of union coverage or vice versa, these categories can be summarized into the seven categories presented in the panel C. Making the appropriate subtractions yields estimates of the returns to licensing and to union coverage or to the sum of the two. The only cases in which the wage changes are statistically different from those for the nonswitching group are those for the groups that switched into and out of a union job with the licensing status unchanged. Both imply a return to union coverage of around 0.06 log points, as in panel B. The group that moved into a job with union coverage and requiring a license tends to have fairly fast wage growth, but because that group tends to be small, the difference in wage growth for this group and groups with no change in either status is not statistically significant.

Table 5. Changes in Log Wages Associated with Changes in Licensing Coverage Status and Union Coverage

A. Licensing moves

	Out of licensed job	*Into licensed job*	*No change*
Change in Log wages	0.043	0.054	0.039
Number of observations	2,352	2,298	75,068

B. Union moves

	Out of union job	*Into union job*	*No change*
Change in Log wages	–0.019**	0.099**	0.040
Number of observations.	4,607	4,683	70,428

C. Licensing and union moves

	Out of licensed and union job	*Out of licensed job, but union status unchanged*	*Out of union job, but licensing status unchanged*	*Into licensed job, but union status unchanged*	*Into union job, but licensing status unchanged*	*Into licensed and union job*	*No change*
Change in Log wages	0.010	0.040	–0.022**	0.053	0.098**	0.100	0.039
Number of observations	162	2,046	4,300	1,998	4,384	155	66,384

Notes: Licensing definition: II-Whole. Each observation represents an individual's activity in a pair of adjacent survey years. Includes all adjacent pairs of years of the NLSY from 1979–1980 to 2008–2010, except for those involving 1994, when union questions were not asked of everyone in the sample. To be included in the sample, individuals must, for both years in the pair, have been working for pay and have a wage greater than one-half the real value of the minimum wage as of January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state of residence data and have been a resident of 1 of the 50 states; have valid occupation data; have valid union coverage data; and have been usually working more than 35 hours per week. Weighted by NLSY79 longitudinal weights. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Number of observations: 79,718.
*Indicates change is significantly different from group that makes no switch at 5%; ** indicates change is significantly different from group that makes no switch at 1%.

Fixed-Effects Analysis

We now turn to a formal longitudinal analysis using individual fixed effects; the results are summarized in Table 6. We estimate a series of regressions, similar to those in our cross-sectional analysis, except our standard set of regressors no longer needs to include variables that remain constant over time, namely, the indicators for female, Hispanic, and African American. The first specification for the entire sample, the one with standard controls (Table 6, column (1)), suggests that those in licensed occupations earn 0.033 log points more than their unlicensed counterparts. The identification of this effect comes from the switches of individuals from an unlicensed job to a licensing-covered job, or vice versa. Through our sample criteria—in particular, the requirement that individuals cannot be enrolled in

Table 6. Impact of Licensing and Union Coverage on Wages, Fixed-Effects Estimates

	(1)	*(2)*	*(3)*
Licensed	0.033**	0.015	0.019*
	(0.008)	(0.009)	(0.009)
	[0.048]	[0.022]	[0.028]
Union	0.125**	0.128**	0.132**
	(0.006)	(0.006)	(0.006)
R^2	0.305	0.315	0.321
Number of observations	130,587	130,587	130,587
Standard controls[a]	Yes	Yes	Yes
Occupational controls[b]	None	One-digit	Two-digit

Notes: Licensing definition: II-Whole. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. The upper bound of licensing appears in brackets. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Column (1) includes indicators for years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year. Columns (2) and (3) add controls for occupations: column (2) major-occupation dummies; column (3), two-digit- occupation dummies.

[a]Standard controls include demographic and human capital variables (controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.

[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.

*Indicates significant at 5%; ** significant at 1%.

school—we have tried to avoid cases in which individuals were working in one unlicensed occupation while finishing training to switch to a different, licensed occupation. In such cases, individuals might receive a wage increase, not as a result of licensing but simply because they finished training or moved to a different occupation. As additional tests, we also try specifications in which we include occupation controls, for both major and two-digit occupations. Inclusion of major-occupation controls cuts the coefficient to a statistically insignificant 0.015 log points, but moving to the next level of occupation detail actually increases the returns to licensing slightly to 0.019 points, which is significant at the 5% level.

As mentioned earlier, the attenuation bias of measurement error is more pronounced in the presence of fixed effects (Griliches and Hausman 1986). Thus, we are interested in the bounds when measurement error is taken into account.

The results for union premiums in the fixed-effects models are quite different from those for licensing returns. Irrespective of which occupational controls are included, workers with union coverage are estimated to have earnings exceeding those without coverage by 0.12 to 0.13 log points, all else remaining equal. Thus, the results fall somewhere between those from the nonparametric longitudinal approach and those from the cross-sectional approaches.

Robustness Checks

In this section, we engage in four exercises to check the robustness of our results in the previous section. First, we rerun the results using our alternative definitions of licensing. Second, we bound the licensing parameters, taking measurement error into account. Third, we rerun the results using just the 2002 to 2008 period, when measurement error is apt to be less of an issue because we do not need to recode the statute data to 1970 codes. Also, because this span is more recent, our data on licensing coverage, accessed in 2012, are more likely to be applicable. Finally, we broaden our search for the impact of licensing by considering aspects of nonwage compensation.

Alternative Definitions of Licensing

We now see whether our results, both from the cross-sectional analyses (Table 4) and the longitudinal analyses (Table 6), are robust to our alternative definitions of licensing. The licensing definition we have used thus far (II-Whole) has the lowest incidence of employment in occupations requiring licenses (Table 1) because it excludes occupations in which only part of the occupation requires a license.

How do the results for the other two definitions compare to those for II-Whole? In the cross-sectional results for the entire sample (Table 7, column (1)), when licensing is the sole regressor, it has a coefficient of 0.159 log points and 0.283 log points under the other two definitions. An inverse relationship tends to exist between the restrictiveness of the definition and the impact. Thus, the results we have seen already are near the upper edge of this range, and this will be the case for all specifications. The coefficients are reduced by more than one-half with the inclusion of the standard controls (Table 7, column (2)), with the returns to licensing coverage now being 0.068 log points and 0.123 log points. The addition of major-occupation dummies (column (3)) leads to a further significant reduction in the return to licensing in percentage terms, with the coefficients now being 0.020 log points and 0.088 log points. As we have already seen, the replacement of controls for major occupations with those for two-digit occupations (column (4)) does not have a substantial additional influence.

For union coverage, the results are quite robust across the three definitions of licensing. The coefficient on that variable is always statistically significant and is consistently 0.16 log points for the second specification

Table 7. Impact of Licensing and Union Coverage on Wages, Cross-Sectional Estimates for Alternative Licensing Definitions

	(1)	(2)	(3)	(4)
A. Licensing definition: II-Partial				
Licensed	0.159**	0.068**	0.020**	0.020**
	(0.016)	(0.008)	(0.007)	(0.007)
Union		0.159**	0.183**	0.195**
		(0.008)	(0.008)	(0.008)
R^2	0.020	0.428	0.459	0.472
B. Licensing definition: I-Partial				
Licensed	0.283**	0.123**	0.088**	0.096**
	(0.019)	(0.014)	(0.012)	(0.011)
Union		0.157**	0.183**	0.195**
		(0.008)	(0.008)	(0.008)
R^2	0.029	0.429	0.461	0.473
Number of obeservations	131,043	131,043	131,043	131,043
Standard controls[a]	No	Yes	Yes	Yes
Occupational controls[b]	None	None	One-digit	Two-digit

Notes: Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Column (1) controls for licensing. Column (2) adds the standard set of regressors (indicators for female, Hispanic and African American, years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year). Columns (3) and (4) add controls for occupations: column (3), major-occupation dummies; column (4), two-digit occupation dummies.

[a]Standard controls include demographic and human capital variables (indicators for female, Hispanic and African American, as well as controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.

[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.

*Indicates significant at 5%; ** significant at 1%.

(column (2)), 0.183 log points with major-occupation dummies (column (3)), and 0.195 log points with two-digit-occupation controls (column (4)).

With the fixed-effects approach (Table 8), the parameters tend to be fairly robust across the three licensing definitions, with those for II-Whole occurring near the upper end of a narrow range. The first specification for the entire sample, the one with standard controls (column (1)), suggests that those in licensed occupations earn between 0.026 log points and 0.043 log points more than their unlicensed counterparts. The inclusion

Table 8. Impact of Licensing and Union Coverage on Wages, Fixed-Effects Estimates for Alternative Licensing Definitions

	(1)	*(2)*	*(3)*
A. Licensing definition: II-Partial			
Licensed	0.026**	0.010*	0.014**
	(0.004)	(0.004)	(0.005)
Union	0.126**	0.128**	0.132**
	(0.006)	(0.006)	(0.006)
R^2	0.305	0.315	0.321
B. Licensing definition: I-Partial			
Licensed	0.043**	0.030**	0.039**
	(0.008)	(0.008)	(0.008)
Union	0.125**	0.127**	0.132**
	(0.006)	(0.006)	(0.006)
R^2	0.305	0.315	0.321
Number of observations	130,587	130,587	130,587
Standard controls[a]	Yes	Yes	Yes
Occupational controls[b]	None	One-digit	Two-digit

Notes: Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Column (1) includes indicators for years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year. Columns (2) and (3) add controls for occupations: column (2), major-occupation dummies; column (3), two-digit-occupation dummies.

[a]Standard controls include demographic and human capital variables (controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.

[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.

*Indicates significant at 5%; ** significant at 1%.

of major-occupation controls (column (2)) tends to lower the return to licensing by about 0.015 log points, a small reduction in an absolute sense but more substantial in terms of percentage. Moving to the next level of occupation detail (column (3)) tends to have less effect, but, if anything, the direction is toward increasing the returns to licensing. Union premiums tend to be quite robust to licensing definition.

Bounding the Licensing Parameters

Bollinger (1996, 2001) extended the work of Aigner (1973) and others to show that bounds can be placed on the parameters of binary regressors, such as the licensing variables, and how such bounds can be tightened when auxiliary information is available on misclassification rates. These bounds are estimated on the assumptions that only the variable of interest is mismeasured and the measurement error is independent of the other regressors. Although such conditions may not hold exactly—for instance, measurement error probably exists in the union coverage variable as well—we think that the bounds that can be calculated are still informative.

In general, two types of misclassification are possible when dealing with binary variables. Let p equal the probability of reporting the individual as licensed when that person is not and q equal the probability of reporting the individual as not licensed when the person actually has a license. Bounds can be computed without knowledge of the values of p and q, but Bollinger showed that information on these misclassification rates can enable us to narrow the bounds.

The PDII 2008 (conducted by Westat and analyzed by Kleiner and Krueger 2013) provides a source for such information. Individuals were first asked: "Do you have a license or certification issued by a federal, state, or local government agency to do your job?" To distinguish between those who had a license and those who had a certification, those who responded "yes" were then asked: "Would someone who does not have a license or certificate be legally allowed to do your job?" Those who responded "no" to the second question after having said "yes" to the first question were coded as licensed, and the remaining individuals were not. We can then consider this variable as providing a true measure of licensing attainment.

Using the occupation codes and states of residence in the Westat survey, we can then recode individuals as being licensed or not based on the three licensing definitions that we have used for the NLSY79. For each of these definitions, we can calculate a p and a q. Using formulas from Bollinger (1996, 2001), we can then develop bounds for the licensing coefficients. The lower bounds are the actual coefficients, under the assumption that the measurement error is zero. The assumption for the upper bounds is that measurement error is the maximum, given the estimated p and q.

For licensing definition II-Whole, p (the estimated probability of incorrectly coding an individual as licensed) is 0.034. That is, our methodology codes as licensed only about 3% of those who were actually unlicensed. The other misclassification rate, q, at 0.170, indicates that about one-sixth of those we code as licensed are not actually licensed. Using both these error rates, we calculate an upper bound for the licensing coefficient for each of the cross-sectional models in Table 4 and fixed-effects models in Table 6, which we show in brackets in those tables.

For all the models, the upper bound is roughly 50% higher than the lower bound estimated by the regression. Thus, despite the same relative impact, whether accounting for measurement error makes a substantial absolute difference depends on which model one believes to be most appropriate. If, for example, one views a cross-sectional model with one- or two-digit controls as most appropriate, then the upper bounds allow for returns to licensing as high as 0.11 log points. For the fixed-effects models, the upper bound tends to be much lower because of the lower base.

Limiting the Samples to 2002 to 2008

An alternative way of assessing the impact of measurement error is to limit the sample to observations from 2002 to 2008. We are likely to have less measurement error in our licensing variable during this subperiod for two reasons. First, this span is more recent, so our data on licensing coverage are more likely to be applicable. Second, we do not have to recode the occupations in our licensing data because the NLSY79 and the statutory licensing data were both SOC-based during this period.

A comparison of the cross-sectional licensing coefficients in Table 9 for 2002 to 2008 with those in Table 4 for the entire period reveals that, with controls (column (2)), they are quite similar. In contrast, bigger differences are apparent in the comparison with the fixed-effects coefficients in Table 6. For the more recent period alone, the licensing coefficient is never significant. Thus, evidence from the most recent period does not support the hypothesis that measurement error in the 1979 to 2000 data is lowering the return to licensing.

Nonwage Compensation

One possibility we have not considered thus far is that at least part of the payoff to licensing requirements comes in the form of nonwage compensation, something that has long been known to be the case for labor unions (e.g., Freeman and Medoff 1984; Lewis 1986). During each interview, the NLSY79 has usually collected information on access to the two most important fringe benefits: employer-provided health insurance and retirement plans.[22] With our wage samples, we estimate linear probability models of access to these two benefits, using the same specification as in our other cross-sectional models.[23]

The results, shown in Table 10, confirm those in the earlier literature indicating that any estimates of the union wage gap are understatements of the union compensation gap. Those covered by a collective bargaining agreement are 12 to 13 percentage points more likely to have access to employer-provided health insurance. The union fringe-benefit effect is even greater for retirement plans, with the difference being 17 to 19 percentage points.

The situation is quite different in the case of licensing. Without controls, those who are in occupations requiring licenses are about 6% more likely to have access to employer-provided health plans and about 10% more likely to have access to an employer-run retirement plan. The difference is completely erased, however, as soon as any controls are put into the regression.

[22]Information did not tend to be collected from some of the self-employed and those in unincorporated businesses. We tried the analysis two ways, including everyone but having a dummy variable for self-employed and excluding the self-employed; we did not find the results to be sensitive.

[23]We estimated linear probability models rather than logits or probits for ease of convergence and of correcting for two-way clustering.

Table 9. Impact of Licensing and Union Coverage on Wages, Cross-Sectional and Fixed-Effects Estimates, 2002–2008

	Cross-Sectional				*Fixed-Effects*		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
Licensed	0.213**	0.101**	0.070**	0.056**	0.009	0.007	0.023
	(0.027)	(0.024)	(0.023)	(0.020)	(0.027)	(0.029)	(0.028)
Union		0.107**	0.148**	0.171**	0.064**	0.064**	0.068**
		(0.014)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)
R^2	0.015	0.372	0.418	0.446	0.058	0.066	0.077
Number of observations	21,401	21,401	21,401	21,401	20,329	20,329	20,329
Standard controls[a]	No	Yes	Yes	Yes	Yes	Yes	Yes
Occupational controls[b]	None	None	One-digit	Two-digit	None	One-digit	Two-digit

Notes: Licensing definition: II-Whole. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Column (1) controls for licensing. Column (2) adds the standard set of regressors (indicators for female, Hispanic and African American, years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year). Column (5) includes indicators for years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year. Columns (3), (4), (6), and (7) add controls for occupations: columns (3) and (6), major-occupation dummies; columns (4) and (7), two-digit occupation dummies.

[a]Standard controls include demographic and human capital variables (indicators for female, Hispanic and African American, as well as controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.

[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.

*Indicates significant at 5%; ** significant at 1%.

Table 10. Impact of Licensing and Union Coverage on Access to Health and Retirement Benefits, Linear Probability Models

	Health benefits				*Retirement benefits*			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Licensed	0.064**	–0.010	–0.010	–0.008	0.104**	–0.010	–0.018	–0.019
	(0.011)	(0.008)	(0.009)	(0.010)	(0.014)	(0.011)	(0.012)	(0.013)
Union		0.116**	0.127**	0.127**		0.172**	0.186**	0.188**
		(0.006)	(0.006)	(0.006)		(0.009)	(0.009)	(0.009)
R^2	0.002	0.214	0.228	0.237	0.005	0.237	0.249	0.258
Number of observations	110,720	110,720	110,720	110,720	73,258	73,258	73,258	73,258
Standard controls[a]	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occupational controls[b]	None	None	One-digit	Two-digit		None	One-digit	Two-digit

Notes: Licensing definition: II-Whole. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Standard errors appear in parentheses. Standard errors take account of complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011). Columns (1) and (5) control for licensing. Columns (2) and (6) add the standard set of regressors (indicators for female, Hispanic and African American, years of schooling, potential experience, and potential experience squared; indicators for union coverage, government employment, self-employment, and part-time status; and sets of dummy variables for major industry, state of residence, and year). Columns (3), (4), (7), and (8) add controls for occupations: columns (3) and (7), major-occupation dummies; columns (4) and (8), two-digit occupation dummies.

[a]Standard controls include demographic and human capital variables (indicators for female, Hispanic and African American, as well as controls for years of schooling, potential experience, and potential experience squared), indicators for union coverage, government employment, and self-employment, part-time status, and sets of dummy variables for major industry, state of residence, and year.

[b]The 1970 codes have 12 one-digit occupational categories, and the 2000 codes have 10 categories; the 1970 codes have 44 two-digit categories, and the 2000 codes have 22 categories.

*Indicates significant at 5%; ** significant at 1%.

The Effects of Licensing and Union Coverage on Wage Dispersion

Up to now, our sole focus has been on the mean impacts of licensing and union coverage, primarily on wage levels but also on the likelihood of receiving benefits. In this section, we change gears and look at the effects of these institutions on wage dispersion. That unions are associated with lower wage inequality is well known (Freeman 1982; Card 1996), and in fact, the reduction of dispersion is often a stated objective in wage bargaining (Freeman and Medoff 1984).[24] Much less is known about the effects of licensing on wage inequality, but neither professional associations nor state officials who monitor licensed jobs express as a goal the reduction of dispersion in the regulated occupations (Kleiner 2006). Nonetheless, we test for a relationship between both licensing and union coverage and wage dispersion using a technique attributable to Card (1996) in the case of unions, which was also used by Kleiner and Krueger (2010, 2013) for both licensing and unions.

In Table 11, panel A, we start with the more familiar case of union coverage. We first estimate a log wage regression on the nonunion sample using the standard controls (minus union coverage but plus a licensing variable) and dummy variables for two-digit occupations. Using the resulting coefficients, we predict the wages for the entire sample and divide the sample into quartiles based on these predicted wages. We then run a log wage regression on the whole sample using the standard controls and dummy variables for two-digit occupations, and we compute the predicted wage (conditional wage) and the residual squared (squared error). The means of these two are calculated by quartile and for the sample as a whole. Of greatest interest in Table 11, panel A, is that we confirm the finding that unions are associated with less dispersion, with the mean squared error being a statistically significant 0.024 higher for the nonunion sample than for the union sample. The nonunion mean squared error is higher in the second, third, and fourth quartiles. In the first quartile, the union sample has a greater dispersion, but the difference is not significant at the 5% level.

Turning to the impact of licensing, we see that the licensed sector actually has higher dispersion overall, although the difference is not statistically significant. In the first quartile, the mean squared error in the licensed sector is 0.069 higher than in the unlicensed sector, and this difference is statistically significant; in the third quartile, it is 0.019 lower, with the difference again being statistically significant. None of the differences in dispersion between the licensed and unlicensed sectors reported in Kleiner and Krueger (2013) were statistically significant, but their sample was much smaller than the one used here.

[24]In addition, union standardization of wage rates leads both directly and indirectly to a compression of wages, as described by Abowd and Farber (1982). The resulting lowering of returns to skill in the union sector makes union jobs less attractive to the most able; in contrast, the less able will queue for such opportunities and employers will try to hire the most-skilled in the queue.

Table 11. Impact of Licensing and Union Coverage on Wage Dispersion

A. Nonunion wage distribution

	Predicted nonunion wage quartile[a]				
	(1)	*(2)*	*(3)*	*(4)*	*Total sample*
Conditional mean ln(Wage)[b]					
Nonunion	2.205	2.508	2.737	3.108	2.641
Union	2.428	2.718	2.947	3.276	2.838
Total	2.239	2.547	2.780	3.132	2.675
Union – Nonunion	0.223	0.210	0.210	0.167	0.197
p value	0.000	0.000	0.000	0.000	0.000
Conditional mean squared error ln(Wage)[b]					
Nonunion	0.116	0.137	0.159	0.183	0.149
Union	0.127	0.127	0.117	0.131	0.125
Total	0.118	0.135	0.151	0.175	0.145
Union – Nonunion	0.011	−0.010	−0.042	−0.052	−0.024
p value	0.061	0.039	0.000	0.000	0.000
Number of observations					
Nonunion	33,630	27,333	23,898	21,896	106,757
Union	6,573	6,935	6,669	4,109	24,286
Total	40,203	34,268	30,567	26,005	131,043

B. Unlicensed wage distribution

	Predicted unlicensed wage quartile[c]				
	(1)	*(2)*	*(3)*	*(4)*	*Total sample*
Conditional mean ln(Wage)[b]					
Unlicensed	2.225	2.537	2.772	3.121	2.646
Licensed	2.327	2.622	2.875	3.244	2.926
Total	2.229	2.543	2.784	3.143	2.675
Licensed – unlicensed	0.103	0.085	0.103	0.123	0.279
p value	0.000	0.000	0.000	0.000	0.000
Conditional mean squared error ln(Wage)[b]					
Unlicensed	0.115	0.137	0.155	0.172	0.144
Licensed	0.184	0.146	0.135	0.162	0.154
Total	0.118	0.138	0.152	0.171	0.145
Licensed – unlicensed	0.069	0.009	−0.019	−0.010	0.010
p value	0.000	0.268	0.011	0.261	0.072
Number of observations					
Unlicensed	37,990	31,835	27,398	21,866	119,089
Licensed	1,661	2,363	3,351	4,589	11,954
Total	39,641	34,198	30,749	26,455	131,043

Notes: Licensing definition: II-Whole. Includes all years of NLSY79 from 1979 to the present, except for 1994, when union questions were not asked of everyone in the sample, and 2010, when the variable indicating class of work was not available. To be included in the entire sample in any given year, individuals must have been working for pay and have a wage greater than one-half the real value of the minimum wage in January 1, 1981 ($3.35 in 1981 dollars) and less than $75 per hour (in 2010 dollars); not have been enrolled in school as of May of the survey year; have had valid state-of-residence data and be a resident of 1 of the 50 states; and have had valid occupation data. In addition, none of the variables used in the regression analysis may be missing. Weighted by NLSY79 longitudinal weights. Estimations take into account the complex survey design of NLSY79 and common-group effects for detailed occupation by state cell (Moulton 1990; Cameron et al. 2011).

[a]Nonunion wage quartile is determined based on the predicted wage from a regression with these controls on the sample of respondents without union coverage.

[b]Conditional mean log wage and mean squared error of log wage result from averages of the predicted values from a regression that includes the standard controls plus dummy variables for two-digit occupations.

[c]Unlicensed wage quartile is determined based on the predicted wage from a regression with these controls on the sample of respondents who were unlicensed.

Discussion

Using a number of approaches, we have estimated wage premia for both licensing and union coverage. Whereas our estimates for the impact of union coverage are well within the ranges in the prior literature, those for licensing, particularly those from the fixed-effects estimation, tend to be somewhat lower. We can propose several possible explanations for this discrepancy. As noted, measurement error is probably dampening our estimates because the upper bounds of our cross-sectional estimates, taking into account measurement error, are closer to previous estimates. Our estimates are also an average for an extended period, 1979 to 2008, whereas other studies focused on one particular year. During this time period, the age distribution of our sample tends to be younger than that in a single cross section, so possibly returns to licensing increase as a cohort ages. Finally, with our data we are able to consider only licenses awarded by the state, thus ignoring those provided by the federal and local governments.

One overarching issue that separates the two institutions—licensing and unions—we have examined is coverage versus membership. For example, the effect of actually having a license may be much larger than that of just being in an occupation in which a license is required for some or many in that occupation. This difference may be bigger than the effect of being covered by a collective bargaining agreement compared to being a union member. We calculated the impact of membership versus coverage in the PDII for occupational licensing, using no occupation controls and two-digit-occupation controls.[25] The results show a difference of between 14% and 18% for having an occupational license. Further, the estimates show a negative influence for a worker in an occupation that is licensed who has not attained a license. The estimates for the difference between union membership and coverage are much smaller, averaging between 5% and 12% (Budd and Na 2000). Thus, part of the differences in the wage gaps for licensing and unionization that we find may be attributable to the larger premium for attaining a license relative to just working in an occupation covered by a licensing law compared to these effects for union membership relative to union coverage.

One other potential source of differences is that unionization's peak was in the 1950s and this institution has had a longer period in which to influence wages than occupational licensing, which has seen its largest growth in the 1970s to the present. Several studies have shown that occupational licensing takes longer to influence public policy and for lower-quality workers in the occupation who have not met current requirements to retire or leave the occupation and decrease the labor supply (Kleiner and Vorotnikov 2012; Kleiner 2013; Thornton and Timmons 2013). Consequently, the longer history of unionization in the labor market may also be a source of the wage gap. Finally, unions can engage in concerted activities at the firm level to reallocate the firms' resources away from capital and toward unions (Hirsch 1991).

[25]Results available on request.

Conclusion

We have conducted a number of analyses using both cross-sectional and longitudinal approaches to measure the impact of two important labor market institutions—licensing and unionization—on wage determination in the United States. Using these different approaches, our estimates of the economic returns to union coverage are greater than those for licensing coverage. Moreover, unions also reduce wage inequality, in contrast to occupational licensing.

We have engaged in a number of exercises to understand why the preponderance of our evidence suggests a generally modest return to licensing coverage in contrast to past research (usually cross-sectional in nature), which tended to find a larger one. Measurement error, although certainly an important issue, is not a full explanation, and we have found no evidence of an economic return to licensing in the form of fringe benefits. Obtaining an occupational license may matter more for wage determination than just being in an occupation covered by a licensing law. Further research using the kind of analysis we have used here, using longitudinal data and considering both membership and coverage, will help labor economists and policymakers better understand the roles of these two institutions, both of which state they protect workers and make consumers better off, on labor market outcomes.

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