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Beyond traditional academic degrees: The labor market returns to occupational credentials in the United States

Abstract

Occupational credentials provide an additional—and, at times, alternative—path other than traditional academic degrees for individuals to increase productivity and demonstrate their abilities and qualifications to employers. In the United States, these credentials typically take the form of licenses and certifications. Although a critical part of the workforce landscape, the literature on the returns to credentials is inadequate, with prior research typically relying on Ordinary Least Squares (OLS) regressions which do not sufficiently control for selection. Using questions that identify credential receipt from the 2015 and 2016 United States' Current Population Surveys, we construct an instrumental variable of local peer influence using the within-labor market credential rate of individuals sharing the same sociodemographic characteristics, while controlling for the same group's average wages and a suite of demographic and geographic controls. We use this instrument in a marginal treatment effects estimator, which allows for estimation of the average treatment effect and determines the direction of selection, and we estimate the effects of credentials on labor market outcomes. We find large, meaningful returns in the form of increased probability of individual employment, an effect which is concentrated primarily among women. The effect of having a credential on log wages is higher for those in the sub-baccalaureate labor market, suggesting the potential role of occupational credentials as an alternative path to marketable human capital and a signal of skills in the absence of a bachelor's degree.

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1 Introduction

There has been an unprecedented expansion of the higher education system in the United States over the past three decades, fueled in part by the labor market's demand for workers with education and training beyond a high school diploma. A defining feature of this expansion is the development and proliferation of occupational credentials via non-traditional postsecondary pathways. However, these credentials exhibit significant heterogeneity along several dimensions: in the occupational requirements for specific credentials, in the ways that students choose what credentials to pursue, and in the ways that employers evaluate potential hires based on their possession of these credentials. With such heterogeneity, it remains unclear whether the acquisition of such credentials has a payoff exceeding the efforts and costs for students when they enter the workforce, and whether the magnitude of that payoff varies according to the type of traditional academic degree with which the occupational credential is "paired."

This paper examines labor market outcomes associated with receipt of the two most common types of occupational credentials in the United States: licenses and certifications. Licenses are credentials awarded by a governmental licensing agency, typically at the state level in the United States (our area of study), based on predetermined criteria that may include some combination of degree attainment, assessments, apprenticeship programs, or work experience. Examples include cosmetology licenses, teaching licenses, pharmacist licenses, and heating, ventilation, and air conditioning (HVAC) repair licenses. Certifications are credentials typically awarded by a non-governmental certification body to individuals who demonstrate that they have acquired the designated knowledge, skills, and abilities to perform a specific job or task. Examples include information technology certifications (e.g., network support, programming, etc.) and project management professional certifications. One key difference between a certification and a license is that a license conveys a legal authority to work in an occupation, whereas a certification is not lawfully required in order to work in the field of the certification (although it may be required by employers). Over the years, occupational licensing has become a more central feature of the labor market with 26% of occupations requiring a license in 2012, up from 17% of occupations requiring licenses in 1983 (Redbird, 2017). Most certifications and licenses – particularly those aimed at workers in the sub-baccalaureate labor market – eschew traditional liberal arts coursework and seat time in favor of the development and demonstration of occupation-specific competencies.

The goal of our study is to assess whether occupational credentials accrue distinct individual-level labor market benefits in the form of higher employment rates and wages, and whether these returns vary depending upon the type of traditional academic degree with which it is paired and the gender of the credential-holder. We hypothesize that holding a credential will yield strong labor market returns as a signal of human capital and potential productivity. We further expect the returns to be higher for licenses than for certifications because licenses impose a form of "occupational closure" where certain tasks in the economy can only be performed legally by a select set of workers (Weeden, 2002). This closure allows for tighter control over supply and in turn creates a form of monopolization of certain parts of the economy that distinctly benefits those in possession of the license when demand for licensed labor is high.

Despite the important role credentials play in sorting workers into occupations, the research base on the economics of credentials is still in its infancy. Historically, occupational

credential attainment has been imprecisely and/or inconsistently measured in large-scale, nationally representative surveys used to study education and labor market outcomes. Hence, there are few national-level studies in the United States that examine the outcomes of occupational credential-holders across all segments of the economy. We aim to bolster this nascent body of research by analyzing data from the 2015 and 2016 Current Population Surveys (CPS), one of the first national surveys to include questions that permit the identification of sample members with certifications and licenses. In addition to improved data, we employ the method of marginal treatment effects (MTEs) with instrumental variables (IVs), which permits (with some assumptions) the identification of a continuum of treatment effects and summary statistics along that continuum, including the average treatment effect on the treated (ATT) and average treatment effect (ATE), which are key for structuring policy incentives. This exercise is similar to Carneiro et al. (2011), who use MTE to estimate the returns to education.

Our study makes three key contributions to the literature. First, in order to examine the relationship between occupational credentials and labor market outcomes, we develop a novel "local peer influence" IV: a leave-one-out estimator of the proportion of individuals in the same local demographic group (race by gender by education level by age) in the same Core-Based Statistical Area (CBSA) that have an occupational credential, all while controlling for race, gender, education level, and age of the individual, the same local demographic group's average wage, local market labor force participation, and local unemployment rates as independent covariates in the outcome models. We validate this instrument by estimating the return to an associate degree using the same type of instrument, and contextualizing our point estimates with those from prior research. Second, we leverage this instrument to estimate the effect of licenses and certifications on individual employment and wages, contributing to the literature that has previously identified these premia from cross-sectional and fixed effects regressions. Third, we document substantial heterogeneity in the returns to occupational credentials along two dimensions central to occupational stratification in the United States: education level and gender. In what follows, we first review past research on the labor market returns to occupational credentials and develop specific hypotheses. Next, we outline our empirical model and discuss how we use the CPS to estimate it. We then present our results and conclude with a summary of our findings.

2 Background

2.1 Past research on occupational licenses

Despite their growing popularity, social science's understanding is still evolving regarding the role that occupational credentials play in preparing students for the labor force, in the production of human capital more broadly, and in how employers interpret these credentials as evidence of competencies when making hiring and salary decisions. In the United States, research efforts have been led in part by the Interagency Working Group on Expanded Measures of Enrollment and Attainment (GEMEnA), a federally commissioned group tasked with developing and validating measures of the participation in and credentialing of education and training for work, including metrics that measure the attainment of occupational credentials. Prior to GEMEnA, federal surveys in the United States had disparate approaches for asking sample members about

occupational credentials, with some asking about them in survey modules focused on educational attainment and school enrollment, and others asking about them in survey modules focused on job training. Without standardized, systematic metrics in federal surveys, it was not possible to reliably study credentials across occupations at the national level. By creating these new "gold standard" metrics, GEMEnA has laid the foundation for social scientists to embark on new research in the areas of educational attainment and workforce development.

Pre-GEMEnA attempts at estimating the labor market returns to licenses and certifications yielded mixed results. Kleiner *et al*. analyzed an array of cross-sectional nationally representative surveys and found that wages were between 10% and 18% higher among those with licenses when compared to those without (Kleiner and Krueger, 2010; Kleiner and Krueger, 2013; Kleiner and Vorotnikov, 2017). In these surveys, the estimated returns to certifications were substantially smaller (Kleiner and Krueger, 2013; Kleiner and Vorotnikov, 2017). In contrast, however, research using the Education Longitudinal Study of 2002, which tracked a nationally representative cohort of high school graduates from the class of 2004, identified an earnings premium of between 14% and 25% from holding a certification among young adults (Albert, 2017).

Lacking data that included direct measures of occupational credentials, Redbird (2017) pooled data from the 1983–2012 CPS (prior to its implementation of GEMEnA's measures in 2015) and used state laws regarding licensure requirements for specific occupations to determine whether or not workers in states that required licenses for their occupations earned more than their counterparts holding the same occupation in states that did not require licenses. She found no association between state licensure laws and wages. The treatment effect identified under these conditions is a very specific one: the returns to having a license because the state requires one. There may, however, be strong returns to obtaining a license or certification in a state or in an occupation where they are not required, as the receipt of the credential may serve to distinguish the human capital of credential-holders in hiring or promoting processes in those states or industries.

One of the first surveys to incorporate GEMEnA's measures was the 2012 Survey of Income and Program Participation (SIPP), which is a nationally representative household-based survey collected by the U.S. Census Bureau. Once these new metrics were added to the SIPP, it was estimated that 21.6% of adults in the country held a currently active certification or license, with rates of receipt higher among those with more advanced traditional academic degrees such (such as bachelor's degrees) than those with high school diplomas and associate degrees (Ewert and Kominski, 2014). Using this data, Gittleman *et al*. (2018) found that adults with licenses were more likely to be employed, and if employed, had 7% higher wages than their peers without licenses. Gittleman and Kleiner (2016) used the National Longitudinal Study of Youth to identify individuals who switch into or out of occupations that require licenses in their state of residence, and from that, estimate a fixed effects model of the return to switching into a license-required occupation. They found the wage growth from such a switch to be between 2% and 7%. Finally, Ingram (2019) used data from the CPS, which also included the occupational credential questions per the guidance of GEMEnA, to estimate a propensity score model of the licensure earnings premium. He additionally leveraged state variation in licensure rates to estimate a model using metropolitan statistical areas (MSAs) spanning state borders, estimating wage returns of between 4% and 8%.

Taken together, these studies highlight the potential for occupational credentials to improve labor market outcomes of workers in the United States. While informative, these studies have a number of limitations. The analyses conducted by Kleiner *et al*. used cross-sectional data with low response rates, and so had a limited ability to account for selection and may be affected by non-response bias. Redbird (2017) and Albert's (2017) analyses used data from nationally representative surveys with larger samples and higher response rates but were conducted prior to the development of the GEMEnA measures, and in the case of Redbird's (2017) study, direct measures of licensure were not available, and were thus inferred. Gittleman *et al*.'s (2018) analysis benefits from the strong survey properties of the SIPP and the inclusion of the GEMEnA measures, but used cross-sectional Ordinary Least Squares (OLS) regressions which did not control for selection or omitted variable bias. Consequently, their estimated earnings benefits likely reflect some dimensions of positive selection into occupational credential programs. Gittleman and Kleiner (2016) meanwhile use the National Longitudinal Survey of Youth (NLSY) and are able to control for time-invariant omitted variables through individual fixed effects, but, like Redbird (2017), do not observe actual licensure status (instead inferring it from their occupation and state, and the laws for that state and occupation). As a consequence, they were unable to identify returns to licenses in states that do not require them.

Lastly, Ingram's (2019) propensity score matching analysis of the CPS was able to take advantage of a large nationally representative survey with high response rates and the inclusion of the GEMEnA measures. While an important contribution, matching estimators are only able to match based on observed characteristics, and it may be that there are differences between those with credentials and those without credentials based on *unobserved* characteristics (e.g., ability, motivation, etc.). The magnitude of the bias that may arise from selection on unobserved factors is unknown; if large, it would lead to greater worries regarding Ingram's estimates, while if small there would be less of a concern. Additionally, propensity score estimators require a common support, and thus constrain estimation to narrow and perhaps unique segments of the sample where there are available matches on observed characteristics. This provides an ATT estimate. This is certainly of interest, and in this paper we present it as one estimate from our model. However, ATT limits generalizability to the broader population (which would be captured from the ATE, which our method additionally estimates). Our paper offers an alternative strategy to estimating the returns of occupational credentials that circumvents these limitations.

In our analysis, we build on this growing body of research by analyzing data from the 2015 and 2016 CPS, which includes a large nationally representative sample and is characterized by high response rates, and employ the occupational credential questions per the guidance of GEMEnA. To limit potential bias owing to selection, constraints, omitted variables, and measurement error, we use a local peer influence instrument via the within-CBSA credential rates of local individuals sharing the same sociodemographic characteristics as instruments, and we include the inverse mills ratios (IMRs) in the second stage of Heckman regressions that predict wages. In using the first 2 years in which the GEMEnA measures were included on the CPS and incorporating IVs to attenuate possible selection bias in estimating the effects of occupational credentials on labor market outcomes, our study improves upon past research that attempts to understand how the provision of licenses and certifications can directly benefit workers.

2.2 **Contingent effects of traditional academic degrees**

A distinctive quality of licenses and certifications is that they can serve as "capstones" on top of traditional academic degrees, which in turn collectively signal occupation-specific qualifications to prospective employers. The value of these signals likely varies depending on the level of quality of educational skills facilitated through traditional academic degree programs, which are more clearly understood markers of human capital. As mentioned earlier, licenses and certifications are less prevalent among those in the sub-baccalaureate labor market than among those with a bachelor's degree (Ewert and Kominski, 2014). Additionally, bachelor's degrees convey a more comprehensive set of skills and capabilities than associate degrees or high school diplomas. Therefore, we hypothesize that occupational credentials serve to differentiate high-quality sub-baccalaureate job applicants, more so than for those with bachelor's degrees, which would result in potentially larger returns to these credentials.

To illustrate, consider two hypothetical recent college graduates. The first has an associate degree in business administration and is considering a job as an administrative assistant in a marketing consulting firm. While there are no licenses required to be an administrative assistant, the job applicant might opt to acquire a computing certification (e.g., a Microsoft certification or a Cisco certification) to enhance their hiring prospects. The second has a bachelor's degree in business administration and is seeking a job as a portfolio manager at the same marketing consulting firm. Similar to the first applicant, this second applicant has acquired a computing certification for an entry-level job they held while working their way through college. In the situation of the associate degree holder, the certification may serve to differentiate the applicant from the rest of the pool of low-skill workers aiming for the administrative assistant position. In the situation of the bachelor's degree holder seeking a portfolio manager position, the certification is likely less relevant to the employer than their bachelor's degree. Therefore, the returns to the associate degree holder's certification should be higher than the returns to the bachelor's degree holder's certification, holding industry/occupation and education constant.

2.3 **Contingent effects of gender**

In addition to heterogeneity in returns to occupational credentials by the attainment of traditional academic degrees, we also explore heterogeneity by gender. Educational attainment has been increasing among women, in tandem with a college earnings premium that is larger for women than for men (DiPrete and Buchmann, 2006). Despite this growth, sizeable wage and employment gaps by gender remain (e.g., Goldin and Rouse, 2000; Blau and Kahn, 2017). In particular, women face substantial discrimination in the hiring process in part because employers believe female applicants are more committed to family than their jobs (e.g., Blau and Kahn, 2017; Correll *et al*., 2007; Coate and Loury, 1993) and in part because employers believe female applicants are less capable of performing the tasks required for the job (Coffman *et al*., 2021; Reuben *et al*., 2014). Note that presumptions about job commitment appear to extend to single women during their childbearing years (e.g., as in Petit, 2007). As our context is the United States, we primarily describe studies based in the United States that identify employers' reluctance to hire women and mothers. However, we note that this is not a

United States-specific phenomenon (e.g., for France, see Coudin *et al*, 2018; for Canada, see Javdani and McGee, 2019), although an audit study in the Netherlands found no evidence of gender or parenthood discrimination (Mari and Luijkx, 2020).

It is possible that occupational credentials on women's resumes could attenuate these sources of discrimination by signaling their commitment to their careers (countering the stereotype of early exits by potential mothers) and enhanced workplace competencies (countering the stereotype of lower capability). The evidence to date suggests this might be the case. For example, Blair and Chung's (2018) analysis of the SIPP documents the potential of license acquisition by women to reduce gender wage gaps. Similarly, Law and Marks (2009) find that, historically, occupational licensure led to increased employment in skilled and licensed fields for female workers. Therefore, we hypothesize that occupational credentials will bolster the labor market prospects of women more than for men.

3 Methods

The central objective of our analysis is to estimate the returns to occupational licenses and certifications. We examine two outcomes: the probability of being employed conditional on being in the labor force, and log hourly wages conditional on being employed. We focus on hourly wages instead of annual earnings as the former are more applicable to sub-baccalaureate workers – i.e., one of our key subpopulations of interest.

3.1 The IV

Prior evaluations of the returns to credentials have relied on regressing labor market outcomes on credential status as well as a broad set of individual controls, including education level and in some cases, individual fixed effects. However, these approaches may be biased, for all the same reasons prior literature has noted that similar regressions of returns to traditional academic degrees are known to be biased (Card 2001). These reasons include the omitted variable bias of not observing and thus failing to control for factors that select individuals into license programs (such as ability, interests, career goals, etc.), bias from selection on heterogeneity in the anticipated returns to license, credit constraints, and so forth. Measurement error of employment and earnings would further bias results. This necessitates an approach that can tease out the true returns to credentials either across the distribution (such as expressed through MTEs) or some average of the population, such as for the entire group (ATE), for those that get credentials (ATT), and for those that do not get credentials [Average Treatment Effect on the Untreated (ATU)], or the treatment effect for those affected by the instrument [the Local Average Treatment Effect (LATE)].

An IV method is one approach we can use to estimate these parameters. As discussed, the existing literature has not, to date, incorporated an IV when estimating the returns to occupational credentials. However, the literature on returns to formal education, such as bachelor's degree attainment, have used IVs extensively. Some commonly used IVs include the distance between home and colleges (Card, 1995; Doyle and Skinner, 2016), changes in tuition costs and financial aid availability (Velez *et al*., 2019), and changes in mandatory schooling age

thresholds (van Huellen and Qin, 2019; Balestra and Backes-Gellner, 2017; Oreopolous, 2006); see Card (2001) for a review of this literature. For occupational credentials, however, these previously used instruments are less useful. Credentials can be acquired in several unobserved locations, including some through online learning, rendering geographic distances less relevant. Tuition costs for these programs vary tremendously across states and over time, but there is not a readily available cost database for the universe of these programs, nor documentation for what credential each person has in our data, let alone where they acquired it. Additionally, there are often no mandatory age requirements for credentials, and no requirements of necessary training for groups (that is, that individuals must attend training classes, whether or not they want to work in that area).

Lacking guidance of previously used and well-established IVs from the returns-to-schooling literature that could be applied to returns to credentials, we introduce an instrument that is "local peer influence" based, working on the assumption that peer groups that have higher rates of credentialing may increase the propensity of individuals in that peer group to pursue and acquire credentials. The use of this instrument is motivated by research which shows that net of sociodemographic and academic characteristics, peer groups influence academic achievement (Calvó-Armengol *et al*., 2009; Hanushek *et al*., 2003) and college enrollment (Fletcher, 2012, 2015). In our study, local peer groups are defined as the people that live in the same CBSA and are the same gender and race/ethnicity (American Indian, Asian, Black, Hispanic, White, or other, where all racial groupings are for the non-Hispanic), have the same educational attainment, and are within the age band of 5 years younger to 15 years older.[1] Our sample has 358 unique CBSAs, with several local peer groups within each CBSA by race/ethnicity, gender, educational attainment, and age), leading to thousands of unique peer groups (which we define as demographic groups within a CBSA). The average number of other individuals in a peer group is around 240. To illustrate the operationalization of our instrument, take for example, a 44-year-old Hispanic man in the Columbus, Ohio CBSA with an associate degree. For this individual, we calculate a leave-one-out estimator of the proportion of Hispanic men aged between 39 years and 59 years living in Columbus with an associate degree that have a license.

3.2 **Outcome model specification**

Eqs (1) and (2) present the regression specifications we use as our second-stage regressions for the two outcomes, the probability of being employed (emp_{ijst}) conditional on being in the labor force and log hourly wages ($lnwage_{ijst}$) conditional on being employed.

$$emp_{ijst} = \alpha + \beta Cred_i + X_i\gamma + \rho_1 PeerWage_{ij} + \rho_2 LocalEarn_{ij} + \phi unemp_{jt} + \lambda lfpart_{jt} + \psi_s + \theta_t + \varepsilon_{ijst} \quad (1)$$

$$\begin{aligned} lnwage_{ijst} = {} & \alpha + \beta Cred_i + X_i\gamma + \rho_1 PeerWage_{ij} + \rho_2 LocalEarn_{ij} + \lambda_1 potexp_i + \lambda_2 potexp_i^2 \\ & + \phi_1 unemp_{jt} + \phi_2 lfpart_{jt} + \psi_s + \theta_t + \kappa IMR_{ijst} + \varepsilon_{ijst} \end{aligned} \quad (2)$$

1 We use the non-symmetric band for age on the assumption that most individuals look to their peers for signals of appropriate behaviors, with greater weight toward those older than themselves who are further along in their schooling and careers. We tested several other age bracket options: a narrow band (2 years younger to 5 years older), a broad band (10 years younger to 30 years older), and no age restriction. We selected the age band we did as it minimized the mean squared predicted error of our model (using the first stage regression including other covariates).

The primary regressor of interest is $Cred_i$, an indicator for holding a credential (separately, licenses or certifications). In addition, employment and wages are functions of individual characteristics including race/ethnicity, gender, educational attainment, and age (X_i). The outcomes are also functions of local labor market conditions. Therefore, we control for the county's unemployment rate ($unemp_{jt}$) and the labor force participation rate ($lfpart_{jt}$), and include state fixed effects (ψ_s). It is important to control for these measures of labor market conditions, as these are correlated with decisions to credential and the outcomes, as well as the IVs themselves (Cameron and Heckman, 1998, 2001). We also control for time (i.e., month and year of survey) fixed effects (θ_t), and for log wage, we include a quadratic in potential labor market experience ($potexp_i$) and the inverse mills ratio (IMR_{ijst}) so as to adjust for the selection into being employed. For the log wage regression, we use a Heckman selection mechanism by including the IMR for the probability of employment. For the IMR, our excluded instruments are the triple interactions among gender, marital status, and having dependents. We hypothesize that there are differences in employment along the intersections of these demographic variables that are not fully explained by the subgroups alone. For example, reservation wages (impacting both labor force participation and wages conditional on employment) may be higher for married women with dependents than for married non-parents, given current cultural norms regarding childcare. As described above, we instrument credential status using the local peer influence instrument, included in the first stage.

We may still worry that even after controlling for this array of potential confounds, there remains a direct impact of a higher-credentialed local peer group on an individual's labor market outcomes, which would violate the exclusion restriction of the instrument: a group motivated enough to pursue credentialing may be strong in other ways that improves outcomes, even after controlling for the direct differences in group wages through X_i. To address this concern, we include as additional control variables a leave-one-out estimator for the local peer group's average wages (allowing for zeros for non-employment) and the across-demographic local average wages ($PeerWage_{ij}$ and $LocalEarn_{ij}$, respectively). We argue that these control for remaining direct impacts of the peer group on the outcomes as well as the strength of the local labor market, such that any residual impact of the local peer group credential rate on an individual's own credentialing probability is the remaining pathway in which the local peer group can impact an individual's labor market outcomes, after controlling for all other variables.

For smaller regions and smaller peer groups, we adjust the definition of the instrument ($PeerCred_{ij}$) and peer wages measure ($PeerWage_{ij}$) to use the local rate aggregated *across* demographic groups, rather than within, still limited to the CBSA. We perform this adjustment when the sample size on which to estimate the local peer credential rate is fewer than 30 observations across the 2 years of data, which occurs for 13.7% of our observations. Additionally, we note that in the CPS, the CBSA is the MSA for areas connected to an MSA; for an individual living outside any MSA, their CBSA is functionally a state identifier that excludes all MSAs in the state.

The first stage equations model credential status as a function of all other covariates in Eqs (1) and (2) (for each outcome respectively), as well as the local peer influence measure (our IV), using a probit model. We recognize that the use of peer credentialing as an instrument is novel in the returns to education literature, yet the returns to credentialing literature is relatively nascent and with divergent estimates. For a validation check of our instrument, we

construct the parallel local peer influence instrument for having an associate degree among those with less than a bachelor's degree, and estimate identical models examining the returns to an associate degree, a well-developed literature. We are thus able to compare the estimated returns to an associate degree using our instrument and our data set to the estimated returns to associate degrees published in well-established studies. As detailed later, our estimated returns to an associate degree with our instrument in the CPS compares very favorably to the estimates already established in the existing literature, supporting the use of our instrument for predicting credential receipt.

As with any IV, two key conditions must be met for valid identification of the effect of credentials on labor market outcomes. First, the instrument must be *relevant*, with a strong observed correlation between the local peer influence measure and the credential status of the individual. Second, the instrument must be uncorrelated with the error term and the exclusion restriction must hold, in that there is no impact of variation in the local peer influence measure on labor market outcomes *except through the receipt of a credential*. The latter assumption is a canonically untestable assumption. Instead, we must provide a convincing defense of its validity.

We consider potential violations of the exclusion restriction, and we argue that our specification addresses these issues. We first frame potential violations in the peer effects literature, following the logic and guidelines put forth by Manski (1993). There, Manski argues that a linear-in-means peer effects specification (as we implicitly have in the first stage of the regression) combines three effects: the endogenous effect, the contextual effect, and the correlated effect. As we go through these, we emphasize though that we do not need to isolate the causal pathway (the "endogenous effect" in Manski) for our IV to remain valid; we can have all three effects present, as long as the exclusion restriction is not violated. This combination of effects only affects the interpretation, not the validity, of our results: we are able to estimate the effect on occupational credential receipt on labor market outcomes regardless of knowing whether it is peer influence directly or another mechanism correlated with peer influence.

First is the endogenous effect, which is the desired causal impact of peers' outcomes on an individual. This does not violate the exclusion restriction: it is our main hypothesized pathway. Second is the contextual effect, which is the impact of peers' mean characteristics on an individual's outcomes. This may cause an issue; an individual with peers who have higher credential rates may also have peers who have higher baseline employment probabilities, earnings, income, consumption, and other relevant factors. These contextual effects may also impact the probability of credentialing (which only helps strengthen the first stage and is not problematic) but may also violate the exclusion restriction through these correlations with outcomes that do not act directly through credentialing.

We do two things to address these issues. First, we include indicators for the demographic characteristics we use to define peers (e.g., race/ethnicity, gender, educational attainment, and age), which account for those levels of contextual effects. Second, we also include in the regression (as independent variables) the peers' average wages and peers' employment rate. These serve to control for the most important sources of heterogeneity that contribute to the violation of the exclusion restriction. For example, the peers of an individual that have higher credential rates may have other kinds of motivation, networking prowess, and drive that improve labor market outcomes. By controlling for those, we constrict the potential ways in which peers'

unobserved qualities, as correlated with credential rates, may impact individual employment rates and wages. Thus, we would expect that not controlling for these peer labor market outcomes would lead to higher estimated impacts of credentialing on a person's labor market outcomes. While not presented in this paper, we tested it both ways and indeed found that including these controls somewhat attenuates the effects, meaning our preferred (with controls included) estimates are potentially understated.

The third peer effect pathway is the correlated effect, which is that the person and their peers may be in the same context and environment, which may drive credentialing probabilities (not problematic, strengthens the first stage) as well as the labor market outcomes (problematic, would violate the exclusion restriction). Including the peers' average wages, peers' average employment rate, local unemployment rates, local labor force participation rates, and state fixed effects are important again to address this concern. For example, individuals in a peer group with a higher credential rate in an area with a high labor force participation rate and in a state with a more advanced vocational education system may have better connections to the labor market overall, or may have heightened access to education and training opportunities in their area. However, these would also impact the peers in the same way, and by separating out the peers' labor market outcomes, we control for these types of differences and absorb those types of potential violation of the exclusion restriction.

Additionally, we note that by looking at peers both geographically and demographically in the model, we are able to leverage identification along both pathways in order to separate out the peer impact. For example, for concerns regarding violations of the exclusion restriction that may occur due to common geographical contexts, we note that our effect is in part identified by comparing individuals in the same CBSA but from different demographic groups, which then accounts for such factors. Note that we do not include CBSA fixed effects in the model, but state fixed effects, to allow for comparison of peer groups between CBSAs within the state. We additionally control for CBSA-level labor conditions such as the overall unemployment rate, as well as the unemployment rates of one's peer group. For concerns regarding violations of the exclusion restriction that may occur from demographic group commonalities—e.g., young Black men facing certain labor market hurdles across the country that are not specific to one area—we rely on the identification of the net effect after including those demographic group controls at the national level. We are thus comparing within a demographic group and between CBSAs within the same state to leverage remaining variation in the credential rate between these groups. Together, the inclusion of peer group labor market outcomes as additional controls along with state fixed effects, local labor market conditions, and demographic group indicators, provides us with a reasonable claim that the remaining variation in the credentialing rate of peers is likely to only impact an individual's labor market outcomes through the change in the probability that the individual is also credentialed.

Given our parametric formulation of the outcome variables with covariates, our interpretation of the estimates of LATE fall under the limitations noted by Blandhol *et al.* (2022). They find such models will estimate averages of not only compliers (the typical interpretation of LATE), but also always and never takers, some of which may have negative weights. Thus, we must exercise caution when interpreting IV estimators, as they are unlikely to be the LATE of compliers. The MTE approach not only yields a LATE-type estimator from the IV, but also the ATT and ATE given the distribution of the instrument in the population, which are our

primary focus. This comes at the price of additional parametric assumptions but relaxes the burden of interpretation of LATE noted by Blandhol *et al.* (2022).

3.3 **MTE modeling framework**

We use the instruments in the MTE estimation framework summarized in Cornelissen *et al.* (2016) (see also Carneiro *et al.*, 2011). We apply the parametric Roy model of MTE, which allows us to estimate additional parameters other than LATE, by leveraging the continuous nature of our IV. Intuitively, the MTE estimates several LATEs along the entire distribution of the continuous IV. This allows for repeated estimates of a LATE at different levels of what the literature calls the (unobserved) distaste for treatment. These MTEs can then be aggregated up as weighted averages along that distribution to estimate, for example, the ATE by adjusting to the sample population level given the distribution of the IV in the sample. This represents a major advantage of the MTE estimator: LATE is not likely to be highly policy-relevant in our case (and may not even be LATE, per Blandhol *et al.* 2022), given our instrument, as policymakers likely care about the returns not merely for individuals that would be motivated to pursue a credential only with sufficient peer support, but rather for the population more generally. Using the MTE model, we are able to estimate the ATE, ATT, ATU, and LATE of occupational credentials, and map out the returns across the MTE curve as a function of the distaste for treatment. As described in Cornelissen *et al.* (2016), the MTE estimator is given by:

$$MTE\left(X_i = x, U_{Di} = p\right) = \frac{\partial E\left(Y_i | X_i = x, P\left(Z_i\right) = p\right)}{\partial p} \tag{3}$$

where U_{Di} is the percentile of the unobserved distaste for treatment, Z_i is our continuous IV, and p is the probability of receiving a credential. We present the MTE curves as a function of U_{Di} for the average observable characteristics X_i. The estimates are weighted averages of the MTE across certain populations (e.g., across the treated group for the ATT). The support of Z_i varies by the credential of interest. Figure A1 in the Supplementary Appendix presents the histograms of Z_i for each of the two cases (licensure and certifications), for the treated and control groups. In both cases, there is full support of the treated group's distribution among the control group. Anticipating the first-stage findings below, the distribution for the treated group is shifted to the right of the distribution for the control group. Additionally, the licensure distribution has a wider support, ranging from between 0 to 0.6 (with some outliers above 0.6). The certification distribution has a narrower support, ranging from 0 to just above 0.1, again with some outliers above 0.1.

3.4 **Decomposition methodology**

We are additionally interested in decomposing the net returns to wages, allowing for the effect to be a function of not only the returns to wages conditional on working, but also the returns driven by changes in the likelihood of employment. Letting w be hourly wages and emp be the employment status, and restricting all to individuals within the labor force, we note that by the law of total probability and the fact that non-workers have zero wages,

$$E\left[w|X\right] = E\left[w|X, emp = 1\right]\Pr\left(emp = 1|X\right) \tag{4}$$

The overall difference in wages between those that have a credential (*Cred* = 1) versus those that do not (*Cred* = 0) can be decomposed as:

$$\begin{aligned} E[w|X,Cred=1]-E[w|X,Cred=0] &= E[w|X,emp=1,Cred=1]\Pr(emp=1|X,Cred=1) \\ &\quad - E[w|X,emp=1,Cred=0]\Pr(emp=1|X,Cred=0) \\ &= \beta_W \Pr(emp=1|X,Cred=1)+\beta_E E[w|X,emp=1,Cred=0] \end{aligned} \tag{5}$$

where

$$\begin{aligned} \beta_W &= E[w|X,emp=1,Cred=1]-E[w|X,emp=1,Cred=10][w|X,emp=1,Cred=1] \\ \beta_E &= \Pr(emp=1|X,Cred=1)-\Pr(emp=1|X,Cred=0) \end{aligned} \tag{6}$$

The total returns are the sum of two elements: β_W, the wage returns conditional on working (the typically estimated return), or the intensive margin of the effect; and β_E, the wage returns to employment given being in the labor force, or the extensive margin of the effect. We estimate each of the four elements of Eq. (5) to construct the decomposition.

Given the data-driven construction of the instruments, analysis being conducted across multiple stages, use of IMR within the MTE framework, and the decomposition being a function of several parameters from separate regressions with different samples, we bootstrap all of the standard errors. We block-bootstrap at the CBSA level with 500 bootstraps to account for within-labor market intraclass correlation that would otherwise bias the standard errors.

4 Data

For our analysis, we pool the 2015 and 2016 CPSs, which contained a set of questions used to determine occupational credentialing developed by GEMEnA. We limit the sample to individuals between ages 18 and 65 – the working population – that that are not enrolled in school.[2] Tables 1 and 2 present the characteristics and distribution of the overall sample, stratified by educational attainment and credential status.

In the CPS, sample members are first asked: "Do you have a currently active professional certification or a state or industry license? Do not include business licenses, such as a liquor license or vending license." If they respond yes, they are then asked: "Were any of your certifications or licenses issued by the federal, state, or local government?" If they respond no, they are considered to only have a certification. If they respond yes, they are considered to have a license. Receiving a license and receiving a certification are not mutually exclusive, and hence there are four ways to classify workers: those with a license but without a certification, those with a certification but without a license, those with both a license and certification, and those with neither. Given the wording of the questions, we cannot identify all four groups separately; specifically, we cannot identify those with licenses but no certifications separately from those with licenses and certifications. We also cannot determine if an individual holds multiple licenses or multiple certifications. As the MTE framework allows only one endogenous variable, we separately estimate: (a) the returns to licenses compared to no licenses and no certifications (Column 2 versus Column 3 in Tables 1 and 2); as well as (b) the returns to certifications

2 We do not limit age when constructing the instrumental variables, so as to have a measure of the degree to which older peers have credentials among those older than age 50.

Table 1 Sample Characteristics, Sub-Baccalaureate

	Certification-holders	License-holders	Non-credential holders	All
IVs				
Local peer group mean certification	0.028	*NA*	0.021	0.021
	(0.019)		(0.017)	(0.017)
Local peer group mean license	*NA*	0.153	0.130	0.133
		(0.058)	(0.059)	(0.059)
Selected Covariates				
Local peer group mean wages	11.746	11.193	10.220	10.383
	(3.929)	(3.755)	(3.808)	(3.823)
Local group mean wages	8.639	8.570	8.521	8.530
	(1.181)	(1.155)	(1.152)	(1.153)
Local unemployment rate	0.050	0.050	0.051	0.050
	(0.014)	(0.012)	(0.013)	(0.013)
Local labor force participation rate	0.832	0.832	0.827	0.828
	(0.055)	(0.055)	(0.055)	(0.055)
Potential experience	25.162	26.130	26.484	26.410
	(12.316)	(12.334)	(13.794)	(13.577)
Male	0.593	0.512	0.495	0.499
	(0.491)	(0.500)	(0.500)	(0.500)
Married	0.571	0.589	0.502	0.515
	(0.495)	(0.492)	(0.500)	(0.500)
Any dependents	0.380	0.377	0.311	0.321
	(0.485)	(0.485)	(0.463)	(0.467)
N	5,863	40,689	250,618	297,170
Percent	2.00%	13.70%	84.30%	100%

Note: Standard deviations in parentheses.

IV, instrumental variable.

with no licenses compared to those with no licenses and no certifications (Column 1 versus Column 3 in Tables 1 and 2).

Tables 1 and 2 also present the demographic composition of each credential group, including gender (male, female), marital status (married, single), race/ethnicity (White, Black, Asian, Hispanic, American Indian), birth cohort (millennial, generation X, baby boomer), and formal educational attainment (less than high school, high school graduate, associate degree, bachelor's degree, graduate degree). Birth cohorts are defined as follows: millennials were born between 1981 and 1997, generation X'ers were born between 1964 and 1980, and baby boomers were born between 1951 and 1963 (truncated as we only consider individuals through age 65).

Among those with an associate degree or less, men have higher licensure rates than women, but the reverse is true among those with a bachelor's degree or higher. In both samples, married respondents and those with dependents are disproportionately more likely to hold a credential than single respondents and respondents without dependents. We also note that the overall credentialing rate for both licenses and certifications is higher for those with bachelor's degrees than for those at the sub-baccalaureate level. This is particularly true for licenses; note that several occupations that require more than a bachelor's degree also require a license (e.g., lawyers, teachers, physicians, pharmacists, etc.).

Table 2 Sample Characteristics, Bachelor's Degree or More

	Certification-holders	License-holders	Non-credential holders	All
Instrumental variables				
Local peer group mean certification	0.054	*NA*	0.048	0.049
	(0.027)		(0.026)	(0.026)
Local peer group mean license	*NA*	0.335	0.308	0.317
		(0.085)	(0.084)	(0.085)
Selected Covariates				
Local peer group mean wages	23.887	22.261	23.190	22.926
	(6.748)	(6.402)	(6.699)	(6.626)
Local group mean wages	21.480	20.721	21.355	21.163
	(3.432)	(3.403)	(3.515)	(3.491)
Local unemployment rate	0.048	0.049	0.049	0.049
	(0.011)	(0.011)	(0.011)	(0.011)
Local labor force participation rate	0.842	0.838	0.838	0.838
	(0.051)	(0.053)	(0.051)	(0.052)
Potential experience	22.708	23.136	22.088	22.435
	(11.145)	(11.548)	(12.294)	(12.038)
Male	0.522	0.397	0.484	0.458
	(0.500)	(0.489)	(0.500)	(0.498)
Married	0.673	0.698	0.625	0.649
	(0.469)	(0.459)	(0.484)	(0.477)
Any dependents	0.421	0.421	0.360	0.381
	(0.494)	(0.494)	(0.480)	(0.486)
N	5,098	45,408	95,722	146,228
Percent	3.50%	31.10%	65.50%	100%

Note: Standard deviations in parentheses.

Figure 1 presents the averages for the two outcomes we evaluate in this paper. Employment is a categorical indicator based on an individual's employment status in the week prior to the administration of the survey. Wages are expressed as a continuous hourly wage rate (or implicit hourly wage rate for salaried workers) for the respondent's primary job.[3]

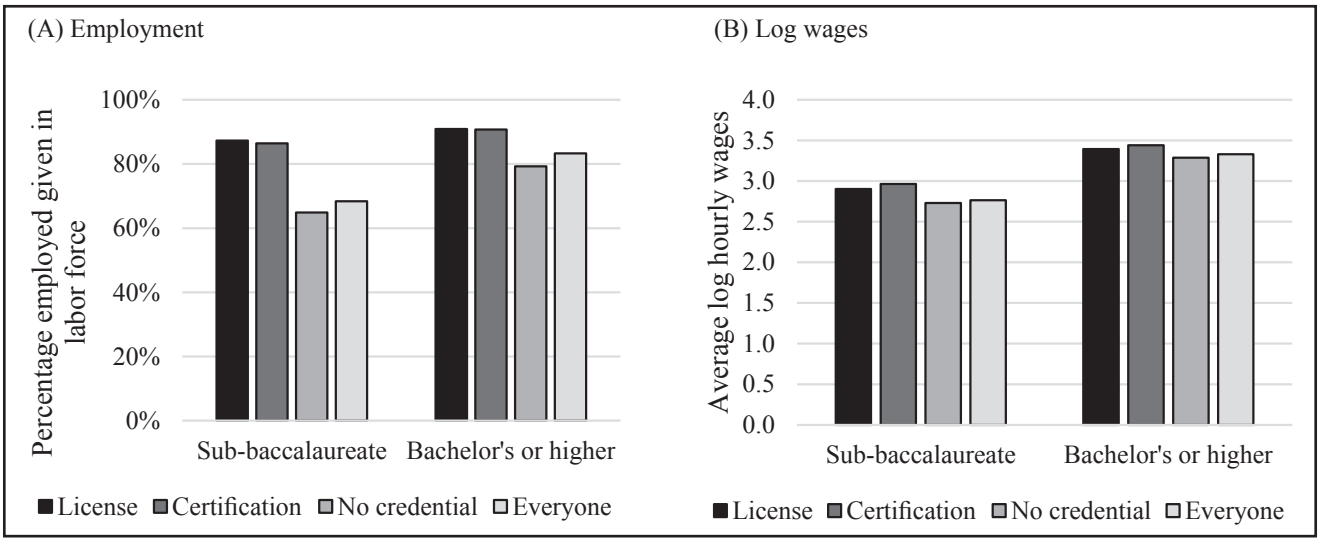
5 Results

5.1 Main results

Table 3 presents the first-stage regression results of the effect of the proportion of peers with a license on having an occupational credential. The *base controls* add in state, month of survey and year of survey fixed effects, the local peer group's mean wages, and other demographic variables, but excluding race/ethnicity, gender, educational attainment, and age (the demographic characteristics that define peer groups), which are added in the final "All controls" column. The *All controls* column shows the importance of controlling for these demographic variables independently. The coefficients in the first stage remain highly significant and large in

3 The CPS Merged Outgoing Rotation Groups do not contain any measure of self-employment income. Self-employed workers, about 11% of license-holders in our age range and about 9% of certification-holders, are thus included in analyses of employment only.

Figure 1 Average labor market outcomes by credential status and level of education.



Note: Authors' calculations from the CPS 2015 and 2016 (pooled). Certification refers to individuals with a certification but no license, while license refers to individuals with a license, whether or not they have a certification. Outcomes are not covariate-adjusted, but are weighted using CPS survey weights. CPS, current population survey.

Table 3 First-stage regressions predicting credential receipt

		License			Certifications		
		No controls	**Base controls**	**All controls**	**No controls**	**Base controls**	**All controls**
Sub-baccalaureate	Coef.	0.798***	0.659***	0.425***	0.489***	0.239***	0.198***
	Std. Error	0.012	0.019	0.023	0.023	0.029	0.03
	F-stat.	5,504.5	1,982.7	192.6	802.2	353.4	45
	Mean	0.14	0.14	0.14	0.023	0.023	0.023
	N	291,307	291,307	291,307	256,481	256,481	256,481
Bachelor's degree or higher	Coef.	0.810***	0.615***	0.422***	0.404***	0.232***	0.207***
	Std. Error	0.021	0.024	0.026	0.03	0.036	0.036
	F-stat.	3,160.8	646.5	199.4	227.1	49.4	21.2
	Mean	0.322	0.322	0.322	0.051	0.051	0.051
	N	141,130	141,130	141,130	100,819	100,819	100,819

Note: Each cell coefficient comes from a separate regression. Regressions labelled "base controls" include state, month, and year fixed effects, and average local (CBSA) peer wages. "All controls" specifications additionally include birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators, marital status, having dependents at home, and controls for local labor conditions (employment rate, average wages, labor force participation in the CBSA). Std. Error = Standard Error from block-bootstrapping, F. stat = F-statistic, Mean is the outcome mean for the regression sample, and N = sample size of regression. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

CBSA, core-based statistical area.

magnitude but decrease as more controls are added. For example, among those in the sub-baccalaureate group, going from a local peer group with a 12.9% licensure rate (the mean peer group average) to one with a 22.9% licensure rate is associated with a 0.425 (4.25 percentage

point) increase in the likelihood of that individual obtaining a license – a sizeable increase. The instruments are strong, with F-statistics often in the hundreds, and never below 20.[4]

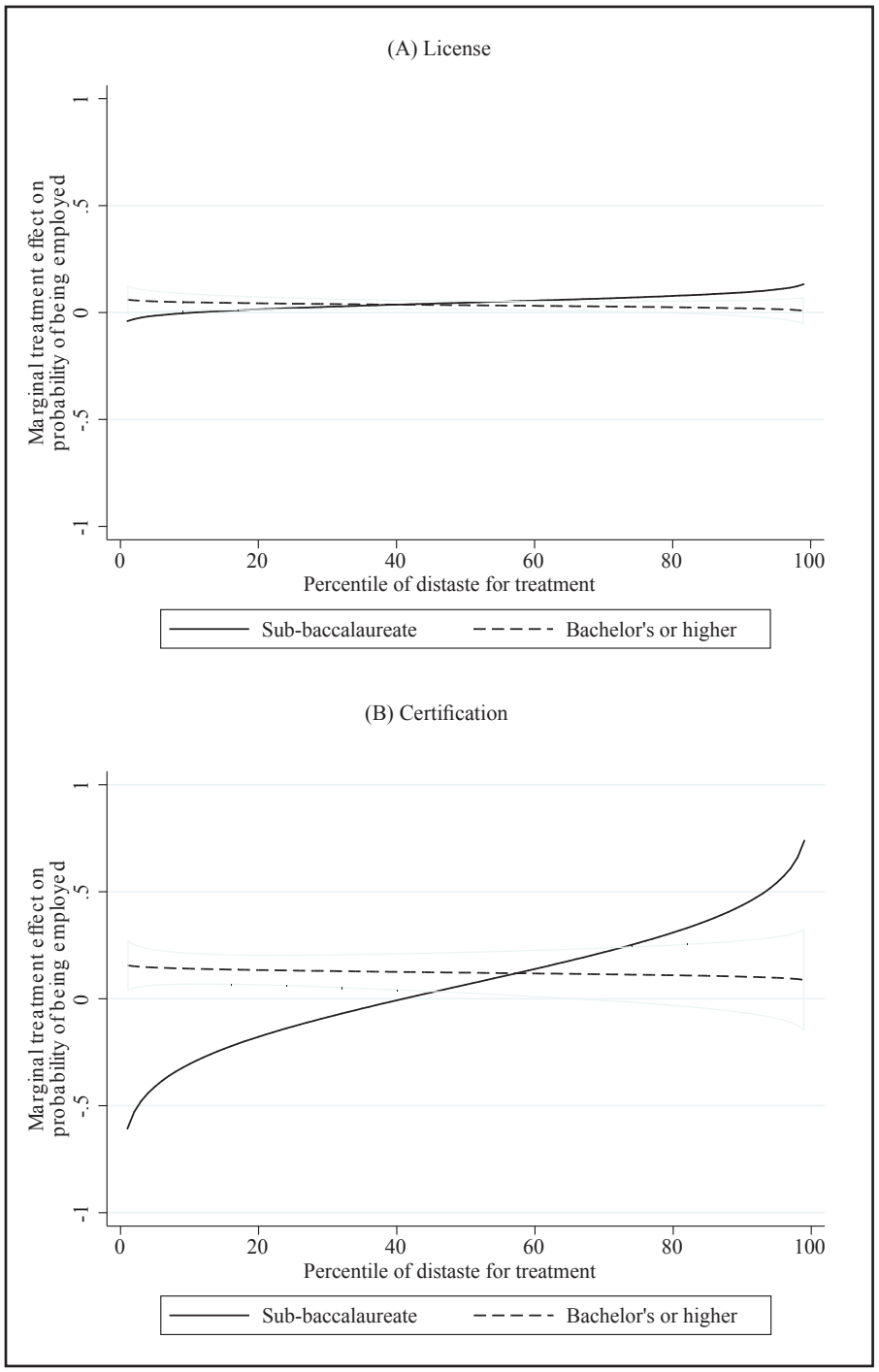
We next move to the second-stage results, starting with the returns to employment conditional on being in the labor force as the outcome. Figure 2 presents the MTE returns curves by level of education. These plots provide a visual depiction of selection into treatment via the instrument. These curves are plotted over the distribution of "resistance to treatment" – in our case, how *unlikely* someone is to obtain a credential by virtue of having a highly credentialed network. If the most positive returns are concentrated with those who easily comply with the treatment—who are very likely to obtain a credential when their peers are credentialed—then this is a case of positive selection. Positive selection is reflected in a downward slope of the MTE curve, where the most positive effects occur low in that resistance distribution. In contrast, if the graph slopes upward, this is indicative of negative selection. An upward slope means that the strongest returns are concentrated among those least likely to obtain a credential in the way our instrument suggests (i.e., influenced by one's peers in a highly credentialed peer network).

For licenses, we find small, generally positive effects on employment (the curves tending to have positive values). The sub-baccalaureate curve slopes upwards, consistent with negative selection into treatment – those with the highest distaste for treatment (the far right of the x-axis) have the most to gain in outcomes, but are least likely to get the treatment due to that distaste. This negative selection into treatment results in a LATE estimate [via two-stage least squares (2SLS) or MTE] that is lower than the ATE, because restricting the estimate to only compliers omits the non-compliers with potentially more positive effects. For baccalaureate license-holding, the slope is basically flat, indicating an absence of strong selection on distaste for treatment (note the similarity between LATE and ATE). Returns are strongest for those with the greatest distaste for treatment, which includes those least moved by our instrument.

For certifications, we see much stronger differences between sub-baccalaureate and baccalaureate populations. The graph suggests negative returns to employment for sub-baccalaureate workers most incentivized by peer credentialing (with the negative selection evidenced by a strong upward slope and an ATE much larger and more positive than LATE), while for the baccalaureate population, there are positive returns for all workers but particularly for those with little resistance for treatment (positive selection, slight downward slope, LATE larger than ATE). Negative selection into treatment has been previously identified in the returns to education literature – for example, see Brand and Xie (2010), who find that non-college attenders stand to gain more from college attendance than those who do pursue a degree. Rationalizing this finding in a general population requires either belief in barriers that are systematically imposed on those who stand to benefit most (for example, credit constraints that do not allow them to take time away from work required to attain a credential, despite being poised to benefit the most from credential receipt) or misinformation concentrated among the same group.

4 We find additional evidence in support of the instruments by exploring CBSA-level measures of racial segregation. To do so, we use a percentile transformation of the divergence index created by the University of California at Berkeley's Othering and Belonging Institute (Menendian *et al*., 2021). In local communities that are more racially segregated, we would hypothesize that our instrument is a stronger predictor of individual credential status – that we are more likely to have correctly represented an estimate of their peer group along racial–ethnic lines (in addition to the other factors). We find evidence of this. In regressions of the endogenous individual credential status on the instrument (not shown), the measure of local segregation, and their interaction, the interaction was positive (although relatively small in magnitude) in all four cases (sub-baccalaureate vs. BA or higher, license vs. certification), and statistically significant in three of those cases.

Figure 2 MTEs of credential-holding for the probability of being employed, conditional on being in the labor force.



Note: Shaded region represents 95% confidence interval from block-bootstrapping. Results come from estimating MTE model of the marginal impact of credentialing across distaste for treatment on the outcome of probability of being employed. MTEs, marginal treatment effects.

Note that in our case, this is only required in reference to those on the margin of pursuing a credential based on peer influence – the existence of individuals who are easily persuaded by peer influence but stand to gain little (or even lose) from a credential, along with their opposites. We also find positive employment effects on the untreated population among bachelor's degree holders for both credential types.

Table 4 Effects of credential-holding on employment by level of education

	License		Certification	
	Sub-baccalaureate	**Bachelor's or higher**	**Sub-baccalaureate**	**Bachelor's or higher**
OLS	0.020***	0.012***	0.011***	0.004*
	(0.001)	(0.001)	(0.003)	(0.002)
ATT	−0.012	0.044**	−0.450***	0.134***
	(0.023)	(0.018)	(0.062)	(0.045)
ATE	0.149***	0.039**	0.372***	0.022
	(0.031)	(0.015)	(0.122)	(0.060)
N	210,006	120,014	178,619	82,854
Mean	0.944	0.976	0.939	0.972

Note: Each column comes from a separate regression. Regressions also include state, year, and month fixed effects, birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators, indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, local labor force participation rate, and local peer group wages. Block-bootstrapped standard errors in parentheses. Mean is the outcome mean for the regression sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

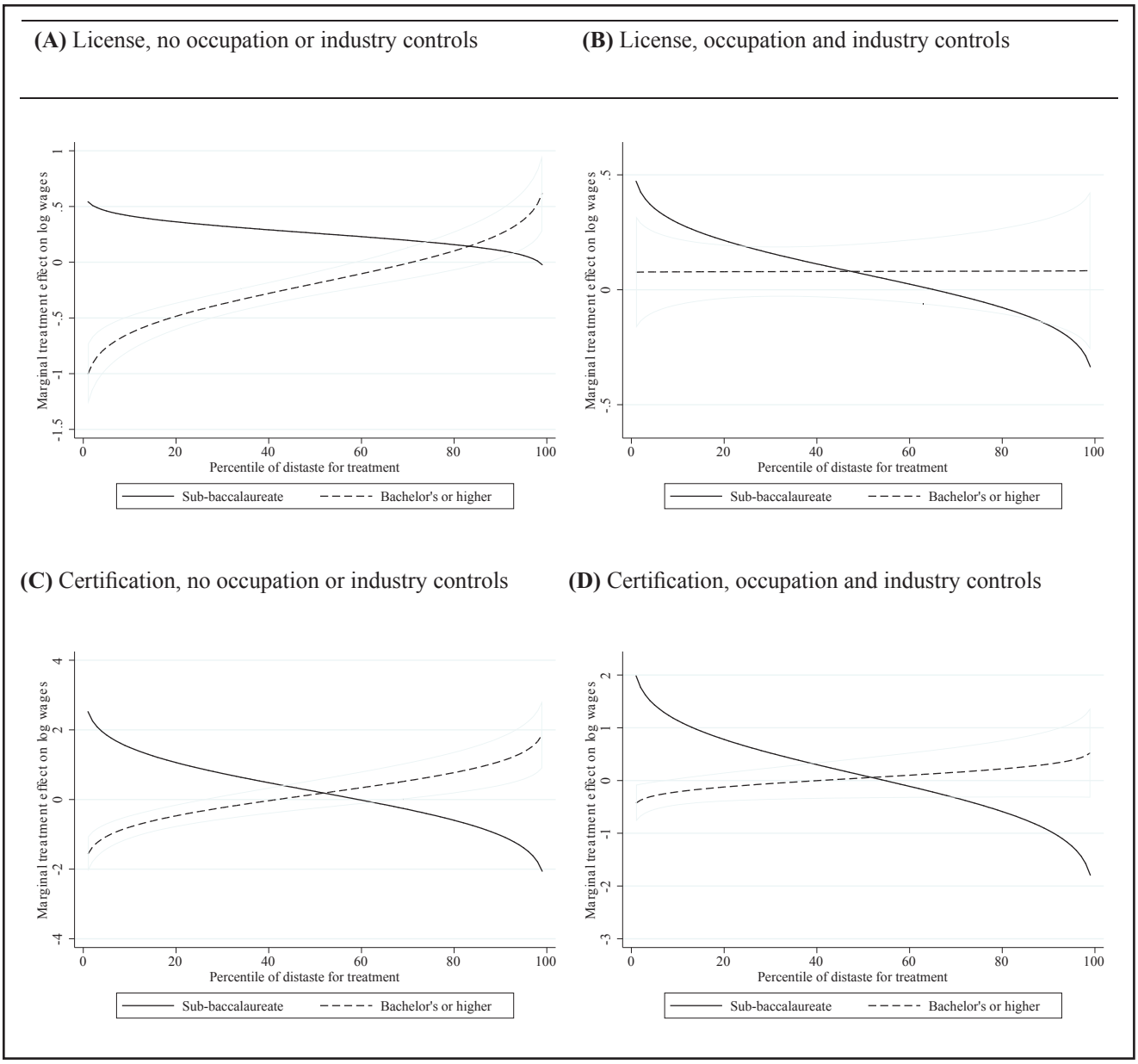
ATT, average treatment effect on the treated; ATE, average treatment effect.

Table 4 presents OLS, ATT, and ATE estimates from the second-stage regression predicting employment, conditional on being in the labor force. We are most interested in the ATT and ATE estimates as they provide insight into how credentials benefit workers in the aggregate. Table A1 in the Supplementary Appendix replicates this table, but also includes 2SLS estimate as well as the MTE-based LATE and Average Treatment Effect for the Untreated (ATUT) estimates. We identify, on average, positive employment effects of licensure as well as positive effects of certification for sub-baccalaureate workers. The ATE across models is positive in all four models, and significant in three of the four. Of particular importance, the ATE for licenses among sub-baccalaureate workers is a 15% increased probability of employment compared to 4% for workers with bachelor's degrees. The ATE for certifications among sub-baccalaureate workers is a 37% increased probability of employment compared to 2% for workers with bachelor's degrees.

Figure 3 presents the returns to log wages, both with and without occupation and industry (at the two-digit level for Standard Occupational Classification (SOC)- and North American Industry Classification System (NAICS)-based codes, respectively) fixed effects. The sub-baccalaureate population has positive selection in all four models, indicated by the downward-sloping curve, while workers with bachelor's degrees or higher have generally negative selection. The negative selection for the bachelor's or higher models may reflect the presence both of low-earning credentialed jobs that require bachelor's degrees, such as teaching, social work, and nursing, as well as several high-earning non-credentialed jobs for individuals with at least bachelor's degrees, including business management and several STEM occupations. This is supported by the shallower slopes of the MTE curves once we control for occupation and industry.

Table 5 presents OLS, ATT, and ATE estimates across the models for log wages. Table A2 in the Supplementary Appendix replicates this table, but also includes LATE, 2SLS, and ATUT

Figure 3 MTEs of occupational credential-holding for log wages, conditional on being employed.



Note: Shaded region represents 95% confidence interval from block-bootstrapping. Results come from estimating MTE model of the marginal impact of credentialing across distaste for treatment on the outcome of probability of being employed. MTEs, marginal treatment effects.

estimates. For both credential types and education levels, the ATE are closer to zero and do not maintain statistical significance when occupation and industry fixed effects are included, compared to when they are excluded. Without occupation and industry fixed effects, part of the identified treatment effect may include credentialed individual's increased ability to transition into higher-paying occupations compared to a non-credentialed individual. This arguably represents a more complete treatment effect, reflecting another dimension of credentials' returns (entry into higher-paying occupations).

While the model that includes industry and occupation fixed effects only identifies the increase in wages within industries and occupations (that is, not accounting for the credential's

Table 5 Effects of credential-holding on log wages by level of education

	License				Certification			
	Sub-baccalaureate		At least bachelor's		Sub-baccalaureate		At least bachelor's	
	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls
OLS	0.095***	0.072***	0.079***	0.063***	0.138***	0.090***	0.099***	0.063***
	(0.004)	(0.004)	(0.006)	(0.005)	(0.008)	(0.008)	(0.008)	(0.008)
ATT	0.422***	0.329***	−0.539***	0.121*	1.889***	1.435***	−1.146***	−0.287**
	(0.067)	(0.045)	(0.070)	(0.066)	(0.144)	(0.108)	(0.191)	(0.129)
ATE	0.260***	0.069	−0.192***	0.080	0.232	0.095	0.152	0.049
	(0.065)	(0.063)	(0.054)	(0.062)	(0.180)	(0.180)	(0.211)	(0.196)
N	177,819	177,819	104,586	104,586	152,228	152,228	72,261	72,261
Mean	2.759	2.759	3.325	3.325	2.737	2.737	3.297	3.297

Note: Each column comes from a separate regression. Additional covariates in all columns include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators (e.g., professional degree, bachelor's degree, some college), indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, IMR for employment, local labor force participation rate, and local peer group wages. Block-bootstrapped standard errors in parentheses. Mean is the outcome mean for the regression sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

ATT, average treatment effect on the treated; ATE, average treatment effect; IMR, inverse mills ratio.

impact on their ability to transition to higher-paying industries and occupations), the model that includes the fixed effects may better isolate the effect of credential-holding in differentiating candidates in the same industry pursuing the same job. The ATEs are largest for sub-baccalaureate licensed workers at around 0.26, or a 26% wage increase. Our estimate is somewhat larger than prior cross-sectional estimates of a 10%–18% increase (c.f. Kleiner and Krueger, 2010; Kleiner and Krueger, 2013; Kleiner and Vorotnikov, 2017). Yet the estimates with occupation and industry fixed effects are smaller than those found in the cross-sectional literature (and do not rise to statistical significance), consistent with the possibility of positive selection bias in prior estimates. However, the ATE is not significant for certifications, where the variance in quality and labor market returns is likely much higher than for licenses, leading to noisier estimates. This is true for certified sub-baccalaureate workers as well as for workers with a bachelor's degree.

Interestingly, the ATE for licensed workers with a bachelor's degree or higher is −0.19 (19% wage decrease) when not controlling for occupation or industry. We speculate that the negative ATE reflects the presence of several low-paying licensed jobs among this population (e.g., teachers, social workers) and high-paying jobs that are not licensed (e.g., management, software developers). This interpretation is supported by the fact that the same model, once occupation and industry fixed effects are added, produces a positive, albeit insignificant, coefficient. Note that in prior cross-sectional work, Kleiner and Vorotnikov (2017) find a significant certification earnings premium of approximately 9%, very similar to our (insignificant) estimate with our occupation and industry fixed effects.

We recognize that the estimated sub-baccalaureate returns to certifications (as measured by ATT) are quite large in magnitude. We suggest caution in interpreting these results,

especially given how much rarer certification is in the sample (with no license); only 2.3% of the sample holds one (see Table 3). However, the results remain suggestive of potentially substantial returns to occupational certifications.

Additionally, while not reported in Table 5, the selection into paid employment modeled by the coefficient on the Inverse Mill's Ratio is consistently negative and statistically significant, ranging between −0.265 and −0.167. This may partly be explained by our not having employment measures for self-employed workers (see Footnote 4). This negative selection into paid employment is consistent with many findings in the literature [see, for example, Bollinger and Hirsch (2013)].

We next turn to the decomposition of the returns on hourly wages, with parameter estimates for the OLS and the ATE from the MTE model shown in Table 6. Here we use hourly wages in place of log wages for ease of interpretation. Note that this decomposition is for the entire sample (that is, not only for treated, untreated, or for some local treatment group); a derivation of the decomposition for such a subgroup would require additional assumptions and is left for future work. Thus, we only calculate and present the OLS and ATE estimates here.

The estimates in Table 6 show that for sub-baccalaureate licenses, approximately three quarters of the total effect comes from the extensive margin (increased likelihood of working) and one quarter from the intensive margin (higher wages). For bachelor's or higher, the only positive and significant effect is for licenses on the extensive margin: workers are more likely to be employed because of their license, but have no subsequent pay increase of significance. Note that this is the model which includes occupation and industry fixed effects, which diminishes the effect of credential receipt on wages.

Table 6 Decomposed and total effects of credentials on hourly wages by level of education

		License			Certification		
		Extensive	**Intensive**	**Total**	**Extensive**	**Intensive**	**Total**
Sub-baccalaureate	OLS	0.174***	0.044	0.218***	0.103***	0.074	0.177**
		(0.037)	(0.044)	(0.062)	(0.038)	(0.066)	(0.072)
	ATE	0.397***	0.013	0.410**	0.608	−0.460**	0.145
		(0.152)	(0.069)	(0.165)	(0.561)	(0.218)	(0.607)
	Mean			18.35			17.93
At least bachelors	OLS	0.267***	0.003	0.269***	0.101*	−0.016	0.085
		(0.049)	(0.038)	(0.062)	(0.058)	(0.054)	(0.078)
	ATE	0.768***	−0.003	0.765***	2.944**	0.024	2.968**
		(0.277)	(0.069)	(0.280)	(1.146)	(0.178)	(1.171)
	Mean			33.34			32.57

Note: The extensive margin is the effect on overall wages driven by changes in the likelihood of being in labor force and probability of being employed, and the intensive margin is the effect driven by changes in wages conditional on working. Each coefficient comes from a separate regression. Additional covariates include state, month, and year fixed effects, birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators, indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, IMR for employment, local labor force participation rate, and local peer group wages. Block-bootstrapped standard errors in parentheses. Mean is the outcome mean for the regression sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

ATT, average treatment effect on the treated; ATE, average treatment effect; IMR, inverse mills ratio.

5.2 **Instrument validation with associate degree return**

We are able to construct a parallel local peer group instrument for the returns to an associate degree for the sub-baccalaureate population. Specifically, we estimate the fraction of people in the same demographic group in the CBSA that have an associate degree and repeat the MTE analysis for those with an associate degree or less. This serves two valuable purposes for our study. First, we can compare our estimate of the returns to education for traditional academic degrees already established in the literature. This allows a validation check of our instrument choice and MTE approach. Second, it allows us to compare for the same sample the returns to an associate degree to the returns to a license or to a certification, as a potential alternative educational pathway.

The OLS, ATT, and ATE estimates are presented in Table 7. For ease of comparison, we include the license and certification results from Table 5. Table A3 in the Supplementary Appendix replicates this table, but also includes LATE, 2SLS, and ATUT estimates. We estimate the return to an associate degree of 0.16 using OLS and 0.25 for ATE. Lang and Weinstein (2013), using cross-sectional OLS, estimate a return to an associate degree that ranges 0.10 to 0.18 depending on the major, and Dadgar and Trimble (2015) use individual fixed effects and find estimates between 0.02 and 0.09, putting our OLS estimate on the higher end (likely because we are unable to include individual fixed effects). More causal estimates put the return around 0.15 log points per year (see Oreopoulos and Petronijevic, 2013) for about a 30-log point increase, putting our ATE estimate of the 2-year degree slightly smaller than that indicated in this literature. This serves to validate our IV choice and modeling approach, both in range of estimates from the OLS versus the ATE estimate and the increase in coefficients in moving from OLS to ATE.

Table 7 Comparison of log wage returns to associate degree using our IV to returns to certification and licenses for the sub-baccalaureate population

	License	**Certification**	**Associate degree**
OLS	0.095***	0.138***	0.164***
	(0.004)	(0.008)	(0.004)
ATT	0.422***	1.889***	0.121***
	(0.067)	(0.144)	(0.023)
ATE	0.260***	0.232	0.251***
	(0.065)	(0.180)	(0.036)
N	182,381	182,381	182,381
Mean	2.764	2.764	2.764

Note: Each column comes from a separate regression. Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators, indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, IMR for employment, local labor force participation rate, and local peer group wages. Block-bootstrapped standard errors in parentheses. Mean is the outcome mean for the regression sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

ATT, average treatment effect on the treated; ATE, average treatment effect; IV, instrumental variable; IMR, inverse mills ratio.

Table 8 Effects of credential-holding on employment by gender and level of education

	License		Certification	
	Sub-baccalaureate	**Bachelor's or higher**	**Sub-baccalaureate**	**Bachelor's or higher**
Men				
OLS	0.019***	0.011***	0.011***	0.003
	(0.002)	(0.002)	(0.004)	(0.003)
ATT	−0.126***	0.048	−0.426***	0.061
	(0.032)	(0.031)	(0.083)	(0.066)
ATE	0.025	0.018	0.078	0.234***
	(0.027)	(0.019)	(0.071)	(0.082)
N	115,193	58,417	98,991	43,804
Mean	0.943	0.977	0.938	0.973
Women				
OLS	0.022***	0.013***	0.011**	0.005
	(0.002)	(0.001)	(0.005)	(0.003)
ATT	0.267***	0.031	−0.295***	0.246***
	(0.042)	(0.025)	(0.098)	(0.080)
ATE	0.149***	0.039**	0.372***	0.022
	(0.031)	(0.015)	(0.122)	(0.060)
N	94,813	61,597	79,628	39,050
Mean	0.945	0.976	0.940	0.970

Note: Each column by gender comes from a separate regression. Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation X, and baby boomers), race–ethnicity indicators, highest education indicators, indicators for marital status and having dependents, local unemployment rate, local average wages, local labor force participation rate, and local peer group wages. Block-bootstrapped standard errors in parentheses. Mean is the outcome mean for the regression sample. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

ATT, average treatment effect on the treated; ATE, average treatment effect.

Table 7 also shows that the ATE is very similar between the return to license, return to certification, and return to associate degree, all around 25%. Of course, certain credentials will require formal schooling such as would happen through the acquisition of an associate degree. Nonetheless, these results are suggestive of the potential for occupational credentials to serve as viable alternative educational pathways leading to higher-paying jobs.

5.3 **Returns by gender**

Table 8 presents OLS, ATT, and ATE estimates from models predicting employment and Table 9 presents OLS, ATT, and ATE estimates from models predicting wages. These tables are parallel to Tables 4 and 5, but with the models now stratified by gender. Tables A4 and A5 in the Supplementary Appendix replicate these tables, but also include LATE, 2SLS, and ATUT estimates. As discussed, credentials could potentially signal both career commitment and capability (countering bases for gender-based hiring discrimination identified in papers such as Correll *et al*, 2007; Petit 2007; Coffman *et al*., 2021). We hypothesize that through this

Table 9 Effects of credential-holding on log wages by gender and level of education

	License				Certification			
	Sub-baccalaureate		At least bachelor's		Sub-baccalaureate		At least bachelor's	
	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls
Men								
OLS	0.087***	0.072***	0.018**	0.027***	0.145***	0.087***	0.081***	0.052***
	(0.005)	(0.005)	(0.009)	(0.008)	(0.009)	(0.008)	(0.012)	(0.012)
ATT	0.353***	0.073	−0.558***	0.132	1.662***	1.216***	−1.105***	−0.428**
	(0.099)	(0.066)	(0.113)	(0.091)	(0.183)	(0.144)	(0.254)	(0.179)
ATE	0.131	0.166*	0.193	0.314***	−0.615**	−0.419	0.542	0.886***
	(0.103)	(0.097)	(0.122)	(0.114)	(0.258)	(0.267)	(0.357)	(0.329)
LATE	0.271***	0.089	−0.197**	0.174**	1.580***	1.222***	−0.916***	−0.259
	(0.084)	(0.062)	(0.097)	(0.087)	(0.191)	(0.153)	(0.252)	(0.178)
2SLS	0.049	0.103	−0.348***	−0.193	0.579***	0.479**	−0.525*	−0.997***
	(0.092)	(0.092)	(0.110)	(0.144)	(0.220)	(0.205)	(0.277)	(0.262)
ATUT	0.088	0.184*	0.489***	0.386**	−0.695***	−0.477*	0.644*	0.969***
	(0.119)	(0.112)	(0.162)	(0.152)	(0.267)	(0.276)	(0.377)	(0.349)
N	95,138	95,138	49,653	49,653	82,510	82,510	37,841	37,841
Mean	2.848	2.848	3.422	3.422	2.825	2.825	3.409	3.409
Women								
OLS	0.096***	0.064***	0.128***	0.092***	0.119***	0.087***	0.121***	0.079***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.017)	(0.015)	(0.013)	(0.012)
ATT	0.025	0.440***	−0.330***	0.288***	0.989***	1.284***	−1.174***	0.128
	(0.097)	(0.059)	(0.096)	(0.087)	(0.240)	(0.164)	(0.286)	(0.191)
ATE	−0.086	−0.739***	−0.179***	0.047	−0.030	−1.660***	−0.210	−0.560**
	(0.100)	(0.103)	(0.063)	(0.059)	(0.340)	(0.353)	(0.279)	(0.268)
LATE	−0.018	0.016	−0.212***	0.099*	1.193***	1.358***	−1.104***	−0.018
	(0.077)	(0.055)	(0.065)	(0.058)	(0.287)	(0.207)	(0.263)	(0.177)
2SLS	−0.095	−0.124	−0.342***	−0.542***	−0.898***	−0.836***	0.025	−0.510*
	(0.086)	(0.107)	(0.074)	(0.107)	(0.301)	(0.290)	(0.312)	(0.287)
ATUT	−0.110	−0.998***	−0.075	−0.119	−0.057	−1.739***	−0.153	−0.602**
	(0.120)	(0.129)	(0.086)	(0.086)	(0.347)	(0.362)	(0.293)	(0.283)
N	82,681	82,681	54,933	54,933	69,718	69,718	34,421	34,421
Mean	2.657	2.657	3.237	3.237	2.633	2.633	3.174	3.174

Notes: Each column by gender group comes from a separate regression. Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators, indicators for gender and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, inverse mills ratio for employment, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

mechanism, occupational credentials will bolster the labor market prospects of women, more so than for men. This is partially supported in our analysis. As shown in Table 8, we find that our employment effects are almost entirely driven by women for three of the four cases, with significant increases in employment for female sub-baccalaureate workers holding licenses or certifications as well as for female baccalaureate workers holding licenses. In contrast, the only significant employment effect for men is concentrated among certification-holders with at least a bachelor's degree.

For wages, we see the opposite story (Table 9). The only positive and significant wage effects accrue to men – specifically, to men with licenses (in either education group), and to men with a certification (among those with a bachelor's degree or higher). For women, we actually find evidence of *negative* returns to credentials, and these negative effects generally are not eliminated by accounting for industry and occupation. Together, these results suggest that for women, occupational credentialing matters in terms of hiring but not necessarily in terms of wages. It is plausible that female credential-holders are better-compensated along other non-wage dimensions not captured in our analysis.

6 Conclusion

Occupational credentials, whether licenses or certifications, provide an additional and at times alternative path for individuals to increase productivity as well as signal their ability, qualifications, and career commitment to employers. This may be particularly important for workers in the sub-baccalaureate labor market and for women. To date, the evidence of the returns to licenses and certifications is limited, with much of the prior research relying on cross-sectional OLS regressions. Using data on credential receipt from the 2015 and the 2016 CPSs and constructing a new IV of the within-CBSA credential rate of individual's demographic peer groups, we identify the effect of licenses and certifications on labor market outcomes. We use our instrument to estimate the distribution of treatment effects through a MTE estimator. Given the large difference in what form these licenses and credentials take depending on the education level of the credential holder, we conduct all analyses separately for sub-baccalaureate and bachelor's degree populations.

Our analysis yields a number of findings of note. First, we find large, meaningful returns to credential-holding for the probability of employment conditional on being in the labor force. OLS models significantly underestimate the employment returns to credentials for sub-baccalaureate workers due to the negative selection into having a credential. However, the estimated ATEs for sub-baccalaureate workers are larger and more consistently significant for both licenses and certifications. Specifically, the ATE for licenses among sub-baccalaureate workers is a 15% increased probability of employment compared to 4% for workers with bachelor's degrees. The ATE for certifications among sub-baccalaureate workers is very large, a 37% increased probability of employment compared to 2% for workers with bachelor's degrees. This often-overlooked return to credentials also plays an important role in overall wages, accounting for the majority of wage increases in our decomposition, compared to wage increases given employment, when controlling for occupation and industry fixed effects. This suggests that occupational credentials act as an important signal to employers in the hiring process, especially for those with less than a bachelor's degree – which is not altogether

surprising as those lacking a bachelor's degrees often need to differentiate themselves from other job applicants and workers in terms of the types of knowledge, skills, and abilities they can bring to employers.

Second, we find that compensation for the acquisition of an occupational credential can be substantial. If we take a back-of-the-envelope weighted average of our effects for all education groups to mirror the literature (approximately two-thirds of the sub-baccalaureate estimate plus one-third of the bachelor's or higher estimate), we find an estimate of around an 11% return to a license across education groups, reduced to 7% when occupation and industry fixed effects are included. These estimates are by and large consistent with those observed in other studies (Albert, 2017; Gittleman *et al*., 2018; Ingram, 2019; Kleiner and Krueger, 2013). However, this average conceals a large disparity in the return to a license for sub-baccalaureate workers (26%) and those with a bachelor's degree or higher (–19%). Additionally, these returns are from the models which do not include occupation and industry fixed effects; once those are included, the returns converge to around 8% for both education groups. We interpret the findings to suggest that for sub-baccalaureate workers, while there is a within-industry/occupation increase in wages from having a license (around 8%), the majority of the return to wages comes from the ability to transition into occupations and industries that have higher wages. The reverse is true for those with a bachelor's degree or higher, where there is wage loss from licenses leading to jobs in lower-paying industries and occupations, such as teaching, the effect of which disappears when looking at wage changes within occupation/industry. These nuances are novel contributions to the literature, and reveal complexities that should be considered when evaluating the role of occupational credentials as sorting mechanisms in the labor market.

Third, we find that occupational credentials shape labor market outcomes differently for women then for men. Our identified employment effects are concentrated among women while our identified wage effects are concentrated among men. The former highlights how occupational credentials can serve as a meaningful signal of women's human capital when they are seeking employment. However, the latter suggests that despite their utility during the hiring process, occupational credentials do not attenuate long-standing gender disparities in earnings, which is in contrast to the findings identified by Blair and Chung (2018).

Despite the strengths of our research design, findings from this study should be considered in light of its limitations. Instrumental variables rely on limiting the variation in the endogenous variable. While this limiting removes the bias when the assumptions hold, it nonetheless reduces the power of the analysis. Also, IVs are typically only able to estimate the LATE for those impacted by the instrument (and even then, new research has critiqued the ability of 2SLS to do even that; see Blandhol *et al*. 2022). In our case we are able to identify the ATE, but this is based on the assumption that the MTE model is correct, including in our case the parametric form of the estimated returns curve with respect to distaste for treatment.

Additionally, we have data limitations, as the CPS questions do not allow us to distinguish between those with licenses but no certifications separately from those with licenses and certifications. It also does not reveal the precise credential obtained, when it was obtained, nor the total number of credentials held. This limits the conclusions that we can draw from the analysis. Also, while the CPS is a commonly used and expertly designed survey to examine labor market outcomes, there is always the potential for measurement error from self-reported

employment and earnings outcomes (Bollinger 1998). If uncorrelated with having a credential, this measurement error would likely attenuate the results towards zero. Instrumenting offers a commonly used strategy for addressing measurement error, and may be one of the reasons we find larger results in general from the MTE model than from OLS.

Our analysis is also limited in estimating proxies for peer networks using demographic characteristics instead of having data on sample members' actual peers. Further, we construct peer groups at the CBSA level as the CPS further does not allow us to estimate the peer credential rate proxy at finer geographic levels, which with a sufficient number of observations could strengthen the instrument and the first stage estimation. This presents a benefit of including the GEMEnA questions in a much larger survey such as the American Community Survey, which would allow for a more refined geographic level of analysis. Lastly, our analysis uses data from 2015 and 2016; these were strong years for the United States economy, and the returns may be smaller than in less robust economic years with slack labor markets. Future research which makes use of recent CPS waves collected during the SARS-CoV-2 (COVID-19) pandemic will be able to build off our findings and explore the value of occupational credentials during turbulent spells in the economy.

While our study is focused on the American context, research on postsecondary occupational credentials undertaken in Finland (Bockerman *et al*., 2019), Japan (Morikowa, 2018), and Switzerland (Oswald-Egg and Renold, 2021) find similar results. Compared with the United States, Finland and Switzerland have more expansive vocational training as part of their national education systems and more structured and easily accessible apprenticeship programs. Japan's education system is more similar to the United States, with a historical orientation toward traditional academic programs. One might expect the returns to occupational credentials to be greater in countries like the United States and Japan where such credentials are less common and thus serve to differentiate the skills of workers – in particular those in the subbaccalaureate labor market. That multiple studies confirm the benefit of occupational credentials in different countries with different educational systems suggests their value is not contingent on the existing vocational training infrastructure.

In closing, although our study does not evaluate a particular policy initiative or credentialing program, our findings do have implications for policy makers, educational leaders, and other practitioners interested in improving school-to-work transitions. In demonstrating the value of occupational credentials, our research suggests that secondary and postsecondary programs which position students for licenses and certifications is one way to support the subsequent employment of their graduates. This could entail including coursework and apprenticeship experiences in traditional academic programs such that students, by virtue of exposure to both, would not stop with obtaining an associate or bachelor's degree but also simultaneously earn a license or certification; such an arrangement would also better facilitate and pay for the requisite testing for credentialing. Through these measures, academic programs would be more accessible and would be more aligned with the credential requirements, which in turn should expedite employment. Furthermore, that we see the largest benefits from credentials accrue to sub-baccalaureate workers and to women, which suggests that a more integrated vocational system that explicitly incorporates the fulfillment of requirements for credentials could potentially diminish long-standing educational and gender disparities in the labor market, provided credentials remain a distinctive, separating signal. Future research

which identifies the mechanisms that undergird the relationships we identify here, as well as the educational and labor market contexts which might amplify or attenuate these relationships, can help inform the direction of occupational credentialing programs so that they can optimally prepare workers for a changing economy.

Declarations

Availability of data and material

The datasets used for this analysis are available from the National Bureau of Economic Research. https://www.nber.org/research/data/current-population-survey-cps-supplements

Competing interests

The authors declare that they have no competing interests.

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Authors' contributions

All three authors contributed to the study concept and design. Bozick undertook the initial data cleaning and the literature review. Baird and Zaber conducted all analyses and wrote up the results. All three authors contributed to the development and editing of the final document.

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Appendix

Table A1 Effects of credential-holding on employment by level of education

	License		Certification	
	Sub-baccalaureate	**Bachelor's or higher**	**Sub-baccalaureate**	**Bachelor's or higher**
OLS	0.020***	0.012***	0.011***	0.004*
	(0.001)	(0.001)	(0.003)	(0.002)
ATT	−0.012	0.044**	−0.450***	0.134***
	(0.023)	(0.018)	(0.062)	(0.045)
ATE	0.149***	0.039**	0.372***	0.022
	(0.031)	(0.015)	(0.122)	(0.060)
LATE	0.017	0.036***	−0.469***	0.152***
	(0.018)	(0.012)	(0.067)	(0.041)
2SLS	0.053**	0.030**	−0.359***	0.194***
	(0.021)	(0.014)	(0.077)	(0.045)
ATUT	0.058***	0.028**	0.082	0.121**
	(0.019)	(0.013)	(0.055)	(0.053)
N	210,006	120,014	178,619	82,854
Mean	0.944	0.976	0.939	0.972

Notes: Each column comes from a separate regression. Regressions also include state, year, and month fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators, indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

Mean = sample outcome mean.

***$p < 0.1$.

Table A2 Effects of credential-holding on log wages by level of education

	License				Certification			
	Sub-baccalaureate		At least bachelor's		Sub-baccalaureate		At least bachelor's	
	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls
OLS	0.095***	0.072***	0.079***	0.063***	0.138***	0.090***	0.099***	0.063***
	(0.004)	(0.004)	(0.006)	(0.005)	(0.008)	(0.008)	(0.008)	(0.008)
ATT	0.422***	0.329***	−0.539***	0.121*	1.889***	1.435***	−1.146***	−0.287**
	(0.067)	(0.045)	(0.070)	(0.066)	(0.144)	(0.108)	(0.191)	(0.129)
ATE	0.260***	0.069	−0.192***	0.080	0.232	0.095	0.152	0.049
	(0.065)	(0.063)	(0.054)	(0.062)	(0.180)	(0.180)	(0.211)	(0.196)
LATE	0.372***	0.241***	−0.334***	0.067	2.011***	1.583***	−1.003***	−0.257**
	(0.060)	(0.046)	(0.052)	(0.055)	(0.159)	(0.125)	(0.178)	(0.122)
2SLS	0.383***	0.453***	−0.489***	−0.644***	1.785***	1.511***	−0.147	−0.424**
	(0.062)	(0.068)	(0.059)	(0.086)	(0.178)	(0.172)	(0.204)	(0.187)
ATUT	0.227***	0.015	−0.006	0.057	0.181	0.053	0.231	0.069
	(0.073)	(0.072)	(0.075)	(0.086)	(0.184)	(0.185)	(0.222)	(0.206)
N	177,819	177,819	104,586	104,586	152,228	152,228	72,261	72,261
Mean	2.759	2.759	3.325	3.325	2.737	2.737	3.297	3.297

Notes: Each column comes from a separate regression. Additional covariates in all columns include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators (e.g., professional degree, bachelor's degree, some college), indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, inverse mills ratio for employment, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A3 Comparison of log wage returns to associate degree using our instrumental variable to returns to certification and licenses for the sub-baccalaureate population

	License	Certification	Associate degree
OLS	0.095***	0.138***	0.164***
	(0.004)	(0.008)	(0.004)
ATT	0.422***	1.889***	0.121***
	(0.067)	(0.144)	(0.023)
ATE	0.260***	0.232	0.251***
	(0.065)	(0.180)	(0.036)
LATE	0.372***	2.011***	0.424***
	(0.060)	(0.159)	(0.030)
2SLS	0.383***	1.785***	1.250***
	(0.062)	(0.178)	(0.077)
ATUT	0.227***	0.181	0.281***
	(0.073)	(0.184)	(0.043)
N	182,381	182,381	182,381
Mean	2.764	2.764	2.764

Notes: Each column comes from a separate regression Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators, indicators for gender, marital status, and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, inverse mills ratio for employment, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A4 Effects of credential-holding on employment by gender and level of education

	License		Certification	
	Sub-bac	**BA+**	**Sub-bac**	**BA+**
Men				
OLS	0.019***	0.011***	0.011***	0.003
	(0.002)	(0.002)	(0.004)	(0.003)
ATT	−0.126***	0.048	−0.426***	0.061
	(0.032)	(0.031)	(0.083)	(0.066)
ATE	0.025	0.018	0.078	0.234***
	(0.027)	(0.019)	(0.071)	(0.082)
LATE	−0.070***	0.029	−0.429***	0.110*
	(0.025)	(0.022)	(0.085)	(0.064)
2SLS	−0.000	0.020	−0.301***	−0.049
	(0.030)	(0.024)	(0.101)	(0.065)
ATUT	0.056*	0.006	0.095	0.244***
	(0.032)	(0.022)	(0.073)	(0.087)
N	115,193	58,417	98,991	43,804
Mean	0.943	0.977	0.938	0.973
Women				
OLS	0.022***	0.013***	0.011**	0.005
	(0.002)	(0.001)	(0.005)	(0.003)
ATT	0.267***	0.031	−0.295***	0.246***
	(0.042)	(0.025)	(0.098)	(0.080)
ATE	0.149***	0.039**	0.372***	0.022
	(0.031)	(0.015)	(0.122)	(0.060)
LATE	0.242***	0.038**	−0.304***	0.252***
	(0.033)	(0.016)	(0.116)	(0.071)
2SLS	0.235***	0.033*	−0.434***	0.501***
	(0.035)	(0.018)	(0.128)	(0.081)
ATUT	0.123***	0.044**	0.390***	0.009
	(0.037)	(0.019)	(0.125)	(0.064)
N	94,813	61,597	79,628	39,050
Mean	0.945	0.976	0.940	0.970

Notes: Each column by gender comes from a separate regression. Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators, indicators for marital status and having dependents, local unemployment rate, local average wages, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A5 Effects of credential-holding on log wages by gender and level of education

	License				Certification			
	Sub-baccalaureate		At least bachelor's		Sub-baccalaureate		At least bachelor's	
	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls
Men								
OLS	0.087***	0.072***	0.018**	0.027***	0.145***	0.087***	0.081***	0.052***
	(0.005)	(0.005)	(0.009)	(0.008)	(0.009)	(0.008)	(0.012)	(0.012)
ATT	0.353***	0.073	−0.558***	0.132	1.662***	1.216***	−1.105***	−0.428**
	(0.099)	(0.066)	(0.113)	(0.091)	(0.183)	(0.144)	(0.254)	(0.179)
ATE	0.131	0.166*	0.193	0.314***	−0.615**	−0.419	0.542	0.886***
	(0.103)	(0.097)	(0.122)	(0.114)	(0.258)	(0.267)	(0.357)	(0.329)
LATE	0.271***	0.089	−0.197**	0.174**	1.580***	1.222***	−0.916***	−0.259
	(0.084)	(0.062)	(0.097)	(0.087)	(0.191)	(0.153)	(0.252)	(0.178)
2SLS	0.049	0.103	−0.348***	−0.193	0.579***	0.479**	−0.525*	−0.997***
	(0.092)	(0.092)	(0.110)	(0.144)	(0.220)	(0.205)	(0.277)	(0.262)
ATUT	0.088	0.184*	0.489***	0.386**	−0.695***	−0.477*	0.644*	0.969***
	(0.119)	(0.112)	(0.162)	(0.152)	(0.267)	(0.276)	(0.377)	(0.349)
N	95,138	95,138	49,653	49,653	82,510	82,510	37,841	37,841
Mean	2.848	2.848	3.422	3.422	2.825	2.825	3.409	3.409

(*Continued*)

Table A5 Continued

	License				Certification			
	Sub-baccalaureate		At least bachelor's		Sub-baccalaureate		At least bachelor's	
	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls	No occupation or industry controls	Occupation and industry controls
Women								
OLS	0.096***	0.064***	0.128***	0.092***	0.119***	0.087***	0.121***	0.079***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.017)	(0.015)	(0.013)	(0.012)
ATT	0.025	0.440***	−0.330***	0.288***	0.989***	1.284***	−1.174***	0.128
	(0.097)	(0.059)	(0.096)	(0.087)	(0.240)	(0.164)	(0.286)	(0.191)
ATE	−0.086	−0.739***	−0.179***	0.047	−0.030	−1.660***	−0.210	−0.560**
	(0.100)	(0.103)	(0.063)	(0.059)	(0.340)	(0.353)	(0.279)	(0.268)
LATE	−0.018	0.016	−0.212***	0.099*	1.193***	1.358***	−1.104***	−0.018
	(0.077)	(0.055)	(0.065)	(0.058)	(0.287)	(0.207)	(0.263)	(0.177)
2SLS	−0.095	−0.124	−0.342***	−0.542***	−0.898***	−0.836***	0.025	−0.510*
	(0.086)	(0.107)	(0.074)	(0.107)	(0.301)	(0.290)	(0.312)	(0.287)
ATUT	−0.110	−0.998***	−0.075	−0.119	−0.057	−1.739***	−0.153	−0.602**
	(0.120)	(0.129)	(0.086)	(0.086)	(0.347)	(0.362)	(0.293)	(0.283)
N	82,681	82,681	54,933	54,933	69,718	69,718	34,421	34,421
Mean	2.657	2.657	3.237	3.237	2.633	2.633	3.174	3.174

Note: Each column by gender group comes from a separate regression. Additional covariates include state fixed effects, month fixed effects, year fixed effects, birth cohort indicators (e.g., millennials, generation-X, and baby boomers), race-ethnicity indicators, highest education indicators, indicators for gender and having dependents, local unemployment rate, local average wages, potential experience as a quadratic, inverse mills ratio for employment, local labor force participation rate, and local peer group wages.

Block-bootstrapped standard errors in parentheses.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Figure A1 Marginal treatment effects of occupational credential-holding for log wages, conditional on being employed. **(A)** Licenses and **(B)** Certifications.

