Three Essays in Applied Microeconomics: Medicaid Expansion, CeMENT Coauthorship Networks, and Occupational Licensing

By

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ABSTRACT

This dissertation includes three essays in applied microeconomics, which is a fundamental outward-looking branch of economics that applies both economics theories and methodologies to actual questions of individual behavior and societal outcomes. The three essays are focusing on real world topics of the ACA's Medicaid expansion, female economists' collaboration networks and occupations licensing. In the first essay, using the restricted NHANES data from 2007 to 2014, effects of the ACA's Medicaid expansion on three public health measures are examined by comparing expansion states with non-expansion states. The results show that the Medicaid expansion in 2014 decreases the systolic blood pressure and increases the usage of cholesterol lowering medication, however, no significant effects on diabetes measures. It is also confirmed that the ACA's Medicaid expansion increases the total health and Medicaid coverage. In the second essay, a unique randomized control trial of CEMENT workshop is examined to investigate its effect on female economists' collaboration networks. The CEMENT workshop provides a particular opportunity to observe female economists' career accomplishments and research productivity in the program. The results show that the participating female economics scholars publish about one more paper and have about 0.5 more numbers of unique coauthors on average, comparing to the control group. The CEMENT workshop helps the treated female economists improve their research productivity and expand the magnitude of their collaboration networks. The last essay studies the effects of occupational licensing on non-U.S. citizen's labor market outcomes, using the monthly CPS Job Certification data from 2015 to 2019. Non-U.S. citizens are found to be less likely to have job certificates or licenses. Compared to licensed U.S. natives, non-U.S. citizens are still suffering from a wage penalty even if with job certificates or licenses.

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For the second chapter, I would like to thank Carlos Zambrana and Patricia Oslund for assisting with Web of Science searches. We are grateful to the many women who volunteered their time to participate in the CEMENT mentoring experiment. This research was funded by National Science Foundation Grant SES-1547054. Any errors are our own responsibility.

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INTRODUCTION

This dissertation is the collection of three papers in applied microeconomics.

The first chapter is on the ACA's Medicaid expansion in 2014. The objective of this chapter is to examine the effects of the ACA's Medicaid expansion in 2014 on health outcome with a difference-in-differences approach, using restricted geotagged NHANES data from 2007 to 2014. Consistent with previous studies, we find that the ACA's Medicaid expansion increased the Medicaid coverage in 2014 in expanded states. We also find that the ACA's Medicaid expansion in 2014 is associated with a decrease in systolic blood pressure of 4.125 mmHg (3.4%) and an increase in the usage of cholesterol lowering medication of 5.2 percentage points. In constrast, no significant effects of diabetes measures or medications are found. With the still hot going questions and arguments of the trend of government's attitudes, as well as the public's attention, this study results shed light on how ACA's Medicaid expansion improve health.

The second chapter is a unique study of female economists' collaboration networks, using the CEMENT data. Women has long been underrepresented in academic ranks in the economics profession. Female economics scholars are not only less likely to get promoted in the academia but also have lower research productivity. Many researchers argue that a lack of professional networks among female economists contributes to this underrepresentation. The Committee on the Status of Women in the Economics Profession (CSWEP) of the American Economics Association (AEA) established the CEMENT mentoring workshop to support women in research careers. CEMENT is a randomized control trial that contributes to an exogenous change in professional networks. As collaboration is measured by coauthorship networks in academic research area, CEMENT provides a unique data set to monitor female economists in treated groups to examine whether CEMENT workshop affects the number of publications and coauthors of the treated women. Our results show that treated women publish about one more paper. However, once we control for coauthors, the treatment effect is no longer significant, indicating that the collaboration networks help with research productivity. We further find that the CEMENT program increases the number of coauthors for the treated female economics scholars.

The third chapter investigates the effect of occupational licensing on labor market outcomes, using the new CPS Job Certification data. Researchers have examined the wage premium of working with an occupational license, yet fewer studies have looked into the occupational licensing effect in the groups of non-U.S. citizens. In this chapter, I re-examine the wage premium of occupational licensing, and extend it to the labor market effects of occupational licensing of non-U.S. citizens, using the monthly personal data from the IPUMS-CPS with the newly added data on job certifications and licensing from October 2017 to January 2019. Preliminary results show that non-U.S. citizens experiences a wage penalty and are less likely to obtain job certifications and licenses. Furthermore, even with a license, non-U.S. citizens are still earning less, compared to U.S. natives.

The three essays in this dissertation contribute to the ongoing literature in applied microeconomics. The first chapter contributes to the understanding of the Medicaid expansion and enriches the literature on the improvement of health in both policy studies and health affairs. The second chapter is a unique study of the collaboration networks of female economists, using the unique randomized control trial of CEMENT workshop to deal with the endogeneity of networks. The third chapter contributes to the research studies of the effect of occupational licensing on immigrants in the United States.

ESSAY 1

DOES THE ACA'S MEDICAID EXPANSION IMPROVE HEALTH?

I. Introduction

While Medicaid before 2014 provided health coverage to millions of Americans, many low-income adults were ineligible. These individuals could not afford to manage chronic conditions due to a lack of health insurance and the resulting unaffordable out-of-pocket medical costs, effectively reducing access to health care services. The Affordable Care Act was intended to close this coverage gap by expanding Medicaid to adults with income of or below 138% of the federal poverty level, making most low-income adults largely eligible.

While the Affordable Care Act was targeted at giving more Americans access to affordable and quality health insurance, it also aimed at reducing the growth of the health care costs. Early studies have already established that the Medicaid reimbursements was positively correlated with the physician treatment of Medicaid patients (Showalter 1997). Under the ACA, expansion states would receive more assistance on Medicaid program, which would have a positive impact on prenatal care (Mukerjee and Quinn 2008) and substantial long-term benefits (Miller and Wherry 2016).

Prior to 2014 ACA's Medicaid expansion, several papers studied the effects of earlier expansions. The 2008 Oregon expansion of traditional Medicaid showed that Medicaid eligibility leaded to greater health care utilization, lower out-of-pocket medical expenditures, fewer medical bills, and better self-reported health (Finkelstein et al. 2012); yet it seemed to have no statistically significant effect on hypertension, high cholesterol levels, or high hemoglobin levels (Baicker et al. 2013). Better adult health conditions were also found to be associated with mother's prenatal coverage under Medicaid in their early life (Miller and Wherry 2016). Finally, the prior Medicaid expansions also reduced the Supplemental Security Income (SSI) participation (Burns and Dague 2016), as previously one needed to get onto SSI to get Medicaid.

Despite the intention of the Affordable Care Act, the Supreme Court in 2012 permitted individual states to choose not to expand Medicaid and to decline substantial federal funding. This partial expansion[1] has generated an abundance of research to evaluate the impacts of the Medicaid expansion on numerous aspects. Relative to non-expansion states, expansion states had increases in health insurance coverage for low-income adults (Black and Cohen 2015; Blumberg et al. 2016) and decreases in the uninsured rate (Benitez et al. 2016; Sommers et al. 2016). However, multiple other papers found minimal effects on labor supply, measured by employment status, labor force participation, or hours worked (Kaestner et al. 2015; Hamersma and Unel 2015; Leung and Mas 2016; Bradley et al. 2016; Gooptu et al. 2016). There was at least some evidence, though, for a reduction in hours worked for people who working with lower educational attainment (Asako et al. 2016), and a trade-off between full-time and part-time (Aslim 2016). Finally, consistent with the work from the Oregon expansion (Finkelstein et al. 2012), the ACA Medicaid expansion also improved multiple economic outcomes. This included significant reductions in the number of unpaid bills and the amount of debt (Hu et al. 2016), an increase of personal credit score and a decrease in the probability of bankruptcy (Caswell 2016), and a reduction in federal disability program participation (Chatterji and Li 2016).

On the hospital side, the Medicaid expansion has caused a significant increase in Medicaid admissions, as well as a significant decrease in admissions covered by other commercial insurance.

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[1] 27 states expanded Medicaid in 2014, including the District of Columbia. Five more states expanded Medicaid after 2014, which is beyond the scope of this study.

(Hempstead and Cantor 2016). Uncompensated care costs have also decreased (Blavin and Holahan 2016), while Medicaid discharges and hospital revenues have been increased (Nikpay 2016). At the individual level, many papers pointed to improvements of access to health care, such as physician visit, certain dental visit, overnight hospital stays, and breast exam (Wherry and Miller 2016; Simon et al. 2016; Sommers et al. 2016), and utilization of health services (Decker 2016). However, a few papers did not find significant effects on health care access and utilization (Shartzer et al. 2015; Sommers et al. 2016). To our knowledge, however, only one paper has examined health outcomes of Medicaid expansion and found an increase in diagnoses of diabetes by 5.2 percentage points and diagnoses of high cholesterol by 5.7 percentage points (Wherry and Miller 2016).

The primary reason for this lack of research on the effect of Medicaid expansion on health outcomes is data limitations. Our paper is the first[2] to use professionally gathered actual health data to study the impact of the Medicaid expansion in 2014. The National Health and Nutrition Examination Survey (NHANES) microdata contains the results of a health examination, as well as interviews and questionnaires. While this data has not previously been used to study the ACA Medicaid Expansion, one paper has used this data cross-sectionally to examine the correlation between health insurance and the health outcomes using NHANES 1999-2012, showing that health insurance coverage was associated with significant lower Hemoglobin A1c, total cholesterol and systolic blood pressure (Hogan et al. 2015).

In our paper, we seek to be the first to examine the impacts of the ACA Medicaid expansion in 2014 on direct health measures, including both the objective measures and the self-reported

[2] The first version of the paper was complete in 2016. Early version of this paper can be found on SSRN: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2817082

diagnoses of diabetes, high blood pressure, and high cholesterol, using the NHANES data. We exploit the effects of the state-variation resulting from the partial Medicaid expansion, using a difference-in-differences model. With the consistent finding of Medicaid expansion increasing health insurance coverage, we expect to see improvements in health outcomes in expansion states in three aspects: (1) reductions in the diagnosis of high blood pressure, high cholesterol and diabetes; (2) an increasing in prescript medication for hypertension, hypercholesterolemia and diabetes; and (3) improvements in examination results of blood pressure, cholesterol level and Hemoglobin A1c. Therefore, our paper would improve our understanding of how public policy decisions regarding health coverage impact health outcomes.

II. Data

As mentioned above, the primary data used in this analysis come from a public and restricted data of the National Health and Nutrition Examination Survey (NHANES) for the years 1999-2014. The NHANES data is designed to directly and objectively assess the health and nutrition conditions of American adults and children. This survey is a unique data set with both interviews and physical examinations, which provides both professional and self-reported diagnostic health information of individuals in our analysis, as well as demographic and geographic information.

As described above, the NHANES collects information on both objective and self-reported health outcomes. Objective health outcomes come from the NHANES examination and laboratory tests, while self-reported health information is from individual interview and questionnaire. In our analysis, we focus on three objective health outcomes: total cholesterol, systolic blood pressure, and hemoglobin A1c, as per Hogan et al. (2015)'s work with the NHANES on the correlation between Medicaid and health. All of these also have the benefit of not needing to be measured while fasting (Sidhu et al. 2012), allowing us to use the larger examination subsample as opposed to the smaller fasting subsample.

In addition to these examination variables, there are also variables for self-reported information from the interviews. This includes three related variables for each condition. One is whether a physician has diagnosed the individual with the condition associated with that variable (i.e., hypercholesterolemia, hypertension, and diabetes, respectively). Another is whether a physician has ever told the individual to take medication for that condition. Finally, the third is whether the individual is currently taking the prescribed medicine for that condition. To be noted that the survey questions change as of the diagnosis of diabetes that there are two diagnosis variables for diabetes (diabetes and borderline diabetes). Only one question asked for the prescribed medication for diabetes. Additionally, the data set also contains variables on a variety of demographic information on each individual, as well information on health insurance.

We supplement the NHANES data with public data from BLS's Local Area Unemployment Statistics). This allows us to control for the seasonally unadjusted unemployment rate at the county-month level, as unemployment affects both Medicaid eligibility (through income) and health outcomes (e.g., Ruhm 2000; Cutler et al. 2016).

Finally, the restricted version of the NHANES that we used for this project contains several non-public variables, including the dates of the examination and interview, the state and county of residence of the respondent, and the annual survey weight (as opposed to the public biennial ones). This allows us to match each observation to whether he or she lived in a treated or control state and whether the expansion was in effect (as described below), and match with the county-level unemployment rate.

III. Methodology

Our primary source of variation in this paper is whether a surveyed individual living in a state that was substantially affected by the ACA's Medicaid expansion. We use a difference-in-difference (DID) research design, comparing changes in outcomes in the group of treated states to the same changes in the control states. Following the classification strategy by Kaestner et al. (2015), we group all 51 states (and DC) by both their expansion status in 2014 and the implementation of expansions similar to the ACA prior to 2014, as these prior expansions would reduce the impact of the ACA. Such classification is more reasonable since early expansion experience would influence the differences in the impacts of ACA before and after the expansion. The treated states fall into one of two categories:

- Treated states that expanded Medicaid in 2014 and had a partial or limited prior expansion similar to the ACA: AZ, CA, CO, CT, HI, IA, IL, MD, MN, NJ, OR RI, and WA.
- Treated states that expanded Medicaid in 2014 and had no prior expansion similar to ACA: AR, KY, MI, NH, NV, NM, ND, OH, and WV.[3]

Noted that we include both MI and NH in the treated group, although both states did not expand Medicaid from January of 2014. Michigan expanded Medicaid in April of 2014, which is an early month that the Medicaid expanded for 9 months of the year. While New Hampshire expanded Medicaid in August of 2014, it is a small state with smaller population than other states

[3] Note that Michigan expanded Medicaid in April of 2014 and New Hampshire expanded in August of 2014. Following Kasetner et al. (2015), we consider both of them to be treated, as Michigan was an expansion state for most of the year and New Hampshire is a smaller state with so few observations that it is unlikely to matter.

so that the later year expansion is not likely to affect our analysis results. Besides, we have month information from the NHANES data that we can get control of the expansion month.

The control group includes two other groups of states. First, obviously, it includes all non-expansion states in 2014.[4] Secondly, it also includes states had partial expansions prior to 2014. There are other five Medicaid expansion states (DE, DC, MA, NY, and VT) that are excluded from our analysis. Because these states had full/comprehensive prior expanding experience similar to ACA of 2014, we expect to see little to no change in these states after 2014. The control states are therefore fall into one of the following two categories:

- Non-Medicaid expansion states that had no prior expanding experience: AL, AK, FL, GA, ID, KS, LA, MS, MO, MT, NE, NC, OK, PA, SC, SD, TX, UT, VA, and WY.
- Non-Medicaid expansion states that had limited/partial prior expanding experience: IN, ME, TN, and WI.

We limit the sample to non-disabled adult age 19 to 64 with high school education or less who live in above treated and control states. This is preferable to define the sample by income, yet the Medicaid expansion is targeted at people with income of or less than 138% of Federal Poverty Level (FPL) and so income is endogenous.[5] Educational attainment, on the other hand, is strongly correlated with income but not directly related to Medicaid eligibility and so provides a reasonable “intent to treat” subsample. Such sample stratification is used in Kaestner et al. (2015). In addition to this sample, we also include a separate sample with college graduates as a comparison placebo group. Furthermore, we constrain our data to those with non-missing values for the key outcome

[4] In addition, several states, including Indiana, Pennsylvania, Alaska, Montana and Louisiana, expanded Medicaid after 2014. As 2015 is beyond the scope of this paper, we have considered those states non expansion states here.
[5] Using income to constrain the sample not only results in endogeneity but also limits the sample size to be even smaller due to the missing value of data information.

variables: systolic blood pressure, total cholesterol, medication for high cholesterol and diabetes. This allow us to have a more consistent sample size through all variables of interests in the analysis. With the above classification, we estimate the effect of ACA Medicaid expansion on health outcomes using the following regression models. In the following equations, *i* is for an individual in the survey, *c* for the county that individual lives in, *s* for the state that individual lives in, and *t* for the time of the interview.

$$\begin{aligned} y_{icst} &= \alpha + \gamma Expansion_s + \delta Implemented_t + \sigma(Expansion_s * Implemented_t) \\ &\quad + \mathbf{X}_{icst}\boldsymbol{\beta} + \mathbf{time}_t + \mathbf{geography}_{cs} + \varepsilon_{icst} \end{aligned}$$

y are the outcomes of interest, including, cholesterol, blood pressure, and diabetes. **X** is a vector individual level demographic controls, including age, gender, racial dummy indicators, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. It also includes the county-time-level unemployment rate control. **time** is a vector of year and month fixed effects (to control for national level differences by year and also for seasonality), and **geography** a vector of state fixed effects (to control for time invariant level differences by geography). σ is the primary coefficient of interest.

Implemented will equal one if the year (of interview or examination, depending on the outcome) is 2014 and zero otherwise. The sample will be limited to those age 19-64 (to avoid confounding the results with programs for children or the elderly), and educational attainment of a high school diploma or less (per Kaestner et al. 2015 and Hu et al. 2016). The years 2007-2013 will be used as control years to be compared to 2014. The regression is weighted using the restricted annual interview or examination weight corresponding to source of the outcome variable.

IV. Results

A. Summary Statistics

Descriptive statistics are presented in Tables 1. There are 5024 observations in the years 2007-2014 which have restricted state and county identifier, among which only 200 observations are in treated states in 2014. This unfortunately feature of the data results in two large consequences for our analysis: first, since many of our variables of interest have missing values for some observations, we are limited in what we can investigate. Second, our results will be noisy and so while we believe we have come to plausible conclusions, they are less robust than we would like.

Table 2 shows the means of the primary variables from NHANES that we use for each of the four groups of observations in Table 1. From the summary statistics, we can conclude two facts. First, there are some substantial 2014 differences between the treatment and control states. This is not surprising as the decision to expand Medicaid was not random and rather was the result of a partisan political process. These differences necessitate the individual demographic controls that are included in the regressions below. Second, as mentioned above, with the concern of non-blank values, we keep our sample to be close to non-missing sample. Even within our constrained sample, we still encountered missing values of control variables. To address it, we coded up variables with missing values accordingly to try to get a relatively bigger sample size. The Medicaid variable's high missing value rate is particularly unfortunate, resulting in a still-small sample size (especially the treated sample), as it makes it impossible to either have Medicaid as an outcome variable or do a two-stage analysis. The regression below are therefore reduced form "intent to treat" estimates.

B. Sensitivity Analysis

Before the Difference-in-Differences analysis, we include a sensitivity analysis. First, we present an event study of differences in trends on key insurance variables (Figure 1-1 and Figure 1-2) and objective health measures of our main sample (Figure 2-1, 2-2, and 2-3). Second, we conduct pre-trend analysis on the same insurance variables and objective health measures. Results are shown in Table 3. Although the pre-trend coefficient on Hemoglobin A1c is significant at 10% level, all insignificant pre-trend coefficients prove the pre-trend common assumption for our Difference-in-Difference method that no effect happened before 2014.

C. Difference-in-Differences Analysis

Main regression results are shown in Table 4 to 6, one for each medical condition of high cholesterol, high blood pressure, and diabetes. Each contains the results of separate regression on two different samples. The “HS Sample” is our main regression sample, and the “COL Sample” is our placebo sample with college education. All regressions are controlled for state fixed effect, and time (year and month) fixed effects.

Table 4 shows the results for cholesterol measures. The four coefficients take a consistent story, although only one is statistically significant. 5.2 percentage points more people were told to take cholesterol lowering medicine, and potentially more people were diagnosed with high cholesterol. Negative coefficient on objective measure of total cholesterol, indicating a possible outcome of the increasing medication to lower cholesterol level. Thus, Medicaid expansion in 2014 is found to be associated in the increase in cholesterol lowering medication.

Table 5 shows the analogous results for blood pressure measures. Medicaid expansion in 2014 is found to be associated with a reduction of 4.125 mmHg in systolic blood pressure, which on a pre-2014 state mean of 118.9mmHg is a 3.4% decrease. However, this is likely not the result of changes in the medication to treat hypertension, as we found statistically insignificant results for the diagnosis and medication for hypertension.

Table 6 gives the regression result for Hemoglobin A1c, diagnosis and medication for diabetes. Noted that there is a change to the diagnosis variables that there are two of them. One is the diagnosis of diabetes, and another is the diagnosis of borderline diabetes. In both conditions, prescribed medication could be given as treatment (which likely to depend on doctor's preference and patients' condition). However, none of the results are more than marginally statistically significant.

In addition, low income sample, as in previous studies, are used as an alternate specification. We did the same regression on low income sample and results are presented in Appendix Table 1. We have quite consistent point estimates but lose some power and as a result statistical significance.

Analysis on Medicaid coverage is presented in Table 7. Consistent with other studies, we found a 3.8 percentage increase in Medicaid coverage for our main sample in expansion states in 2014. Noted that for low income sample, there is a 5.3 percentage increase in the Medicaid coverage in 2014 in treated states, compare to control states.

V. Discussion

The results from the tables above are that the Medicaid expansion increased individual's propensity to be cholesterol lowering prescriptions, which had the dual effect of lowering total cholesterol and lowering blood pressure. There was no statistically significant increase in the share of individuals diagnosed with high blood pressure or diabetes nor any changes in medication for those conditions.

The cholesterol results are somewhat puzzling given a strong increase in the share of individuals taking medicine but a statistically insignificant increase in the share of individuals diagnosed with high cholesterol. However, there is medical literature suggesting that individuals are often prescribed cholesterol lowering medication despite having only borderline high cholesterol or having other general symptom of metabolic disease (Grundy 2014). And it could be possible that people who had been told to take medication only began taking it after they gained the Medicaid coverage after the Medicaid expansion in 2014 (or earlier depending on states).

Additionally, the blood pressure results appear puzzling as there is a drop of blood pressure despite no change in medication. One potential explanation is that a common side effect of cholesterol lowering medication is lowered blood pressure (Golomb et al. 2008). Therefore, it is plausible that the substantial increase in cholesterol lower prescriptions is also lowering blood pressure.

As mentioned in other studies, dietary and lifestyle can have different effects on health improvement. The eating habit and the exercise frequency are two of the factors that can largely affect one's health conditions, which could possibly improve the three health measures we spotted in our analysis. However, these factors are not the key concern of our study as we only focused on the effect of the ACA's Medicaid expansion on health outcomes.

VI. Conclusion

In this paper, we examine whether the ACA Medicaid expansion in 2014 improve health using NHANES data for objectively collected and self-reported health measures, 2007-2014. We limit the sample to be non-disabled adults age 19 to 64 with high school education or less, living in treated and control states. With the concern endogeneity of income to Medicaid, we use education to stratify the sample. Estimates of Medicaid expansion on health outcomes shows that the ACA Medicaid expansion is associated with a 5.2 percentage point increase in cholesterol lowering medication usage, and a decrease of 4.125 mmHg of systolic blood pressure, likely a side effect of the cholesterol lowering medication.

Overall, this suggests that the Medicaid expansion did result in improved health measures for the affected individuals.

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TABLES FOR ESSAY 1

Table 1: Summary Statistics: Number of Observation with Low Educational Attainment[6]

	2007-2013	**2014**	**Total**
Treated states	2137	200	2337
Control states	2358	329	2687
Total	4495	529	5024

[6] The fact that the interview and examination dates may not be the same for a particular observation can result in the two straddling New Year's Day, such that one but not the other may be before 2007 or after 2014. This table uses the interview date and so these counts may be slightly different for examination variables.

Table 2: Summary Statistics of NHANES, 2007-2014

Variable \ Group / Year	Treatment Group/States: Pre-expansion (2007-2013) Mean	N	Treatment Group/States: After-expansion (2014) Mean	N	Control Group/States: Pre-expansion (2007-2013) Mean	N	Control Group/States: After-expansion (2014) Mean	N	p-value for Treatment vs. Control in pre-period
Panel 1: Demographics									
Age	40.57	2137	38.9	200	41.7	2358	38.72	329	0.85
U.S. Citizen	0.785	2137	0.549	200	0.873	2358	0.89	329	0.00
Male	0.522	2137	0.514	200	0.534	2358	0.563	329	0.00
Non-Hispanic White	0.533	2137	0.337	200	0.602	2358	0.614	329	0.00
Non-Hispanic Black	0.103	2137	0.0712	200	0.146	2358	0.155	329	0.00
Hispanic	0.306	2137	0.54	200	0.214	2358	0.188	329	0.00
Married	0.52	2127	0.459	195	0.544	2352	0.558	318	0.31
Pregnant	0.0102	2137	0.0054	200	0.017	2358	0.013	329	0.56
Panel 2: Insurance Coverage									
Have Health Insurance	0.617	2137	0.637	200	0.612	2358	0.684	329	0.00
Medicaid	0.0717	2137	0.12	200	0.045	2358	0.0702	329	0.00
Panel 3: Cholesterol, Diagnosis and Medication of Hypercholesterolemia (High Cholesterol)									
Total Cholesterol (mm/dL)	196.9	2119	189.2	200	196.9	2358	186.4	329	0.07
Diagnosis of Hypercholesterolemia	0.244	2137	0.246	198	0.24	2355	0.222	328	0.00
Told to Take Medicine for Hypercholesterolemia	0.133	2137	0.125	200	0.146	2358	0.105	329	0.05
Now Taking Medicine for Hypercholesterolemia	0.0717	1903	0.0682	173	0.119	2150	0.0798	296	0.94
Panel 4: Systolic Blood Pressure, Diagnosis and Medication for Hypertension (High Blood Pressure)									
Systolic Blood Pressure (mmHg)	118.9	2119	116.7	200	120.8	2358	122.1	329	0.00
Diagnosis of Hypertension	0.222	2134	0.226	200	0.236	2355	0.257	329	0.00
Told to Take Medicine for Hypertension	0.169	2134	0.148	200	0.192	2355	0.183	329	0.00
Now Taking Medicine for Hypertension	0.14	2023	0.106	188	0.16	2251	0.155	307	0.00
Panel 5: Hemoglobin A1c, Diagnosis and Medication for Diabetes									
Hemoglobin A1c (%)	5.585	2116	5.657	200	5.618	2358	5.514	329	0.64
Diagnosis of Diabetes	0.066	2135	0.0713	200	0.0647	2357	0.0423	329	0.87
Diagnosis of Borderline Diabetes	0.0203	2135	0.0221	200	0.0218	2357	0.0187	329	0.37
Now Taking Medicine for Diabetes	0.0478	2137	0.0686	200	0.0595	2358	0.0299	329	0.95

Table 3: Pre-trend Analysis on Health Insurance Variables and Objective Health Measures, 2007-2013

Panel 1: Pre-trend Analysis on Health Insurance Variables

	With health insurance coverage	With Medicaid coverage	No health insurance coverage
Treatment*T	0.004	0.005	-0.004
	(0.006)	(0.004)	(0.006)

Panel 2: Pre-trend Analysis on Objective Health Measures

	Systolic Blood Pressure	Total Cholesterol	Hemoglobin A1c
Treatment*T	0.616	-0.421	0.036*
	(0.425)	(0.433)	(0.02)

Table 4: Difference-in-Differences Estimates of Effect of ACA Medicaid Expansion on Cholesterol, 2007-2014

	Variables	Total Cholesterol	Diagnosis of High Cholesterol	Told to Take Prescribed Medicine for High Cholesterol	Now Taking Prescribed Medicine for High Cholesterol
HS Sample	DID Coefficient	-4.609	0.042	0.052***	0.024
		(3.805)	(0.033)	(0.016)	(0.025)
COL Sample	DID Coefficient	-1.656	0.048	-0.073	-0.029
		(4.895)	(0.044)	(0.043)	(0.035)
State FE		Yes	Yes	Yes	Yes
Time FE		Yes	Yes	Yes	Yes
Demographic Control		Yes	Yes	Yes	Yes

Notes: HS = High school diploma or less. COL = College Graduate. Robust standard errors clustered at the state level are in parentheses. Regressions are weighted using the restricted annual weights for either the interview or exam sample. Individual level demographic controls include age, gender, race, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. Year, month, and state fixed effects are included, as is the seasonally unadjusted monthly county unemployment rate. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Difference-in-Differences Estimates of Effect of ACA Medicaid Expansion on Blood Pressure, 2007-2014

	Variables	Systolic Blood Pressure	Diagnosis of Hypertension	Told to Take Prescribed Medicine for Hypertension	Now Taking Prescribed Medicine for Hypertension
HS Sample	DID Coefficient	-4.125***	-0.006	-0.004	-0.015
		(1.326)	(0.036)	(0.022)	(0.023)
COL Sample	DID Coefficient	-1.429	-0.090	-0.024	-0.001
		(3.023)	(0.115)	(0.089)	(0.098)
State FE		Yes	Yes	Yes	Yes
Time FE		Yes	Yes	Yes	Yes
Demographic Control		Yes	Yes	Yes	Yes

Notes: HS = High school diploma or less. COL = College Graduate. Robust standard errors clustered at the state level are in parentheses. Regressions are weighted using the restricted annual weights for either the interview or exam sample. Individual level demographic controls include age, gender, race, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. Year, month, and state fixed effects are included, as is the seasonally unadjusted monthly county unemployment rate. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Difference-in-Differences Estimates of Effect of ACA Medicaid Expansion on Diabetes, 2007-2014

	Variables	Hemoglobin A1c	Diagnosis of Diabetes	Diagnosis of Borderline Diabetes	Medication for Diabetes
HS Sample	DID Coefficient	0.065	0.008	0.016	0.025
		(0.088)	(0.026)	(0.019)	(0.016)
COL Sample	DID Coefficient	0.110	-0.025	0.023*	-0.006
		(0.098)	(0.028)	(0.010)	(0.033)
State FE		Yes	Yes	Yes	Yes
Time FE		Yes	Yes	Yes	Yes
Demographic Control		Yes	Yes	Yes	Yes

Notes: HS = High school diploma or less. COL = College Graduate. Robust standard errors clustered at the state level are in parentheses. Regressions are weighted using the restricted annual weights for either the interview or exam sample. Individual level demographic controls include age, gender, race, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. Year, month, and state fixed effects are included, as is the seasonally unadjusted monthly county unemployment rate. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Difference-in-Differences Estimates of Effect of ACA Medicaid Expansion on Health Insurance and Medicaid, 2007-2014

	Variables	Health Insurance	Medicaid
HS Sample	DID Coefficient	-0.006	0.038**
		(0.067)	(0.016)
COL Sample	DID Coefficient	-0.019	-0.000
		(0.043)	(0.007)
Low Income Sample	DID Coefficient	-0.028	0.053**
		(0.059)	(0.025)
State FE		Yes	Yes
Time FE		Yes	Yes
Demographic Control		Yes	Yes

Notes: HS = High school diploma or less. COL = College Graduate. Robust standard errors clustered at the state level are in parentheses. Regressions are weighted using the restricted annual weights for either the interview or exam sample. Individual level demographic controls include age, gender, race, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. Year, month, and state fixed effects are included, as is the seasonally unadjusted monthly county unemployment rate. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

FIGURES FOR ESSAY 1

Figure 1-1: differences in trends on health insurance coverage, 2007-2014

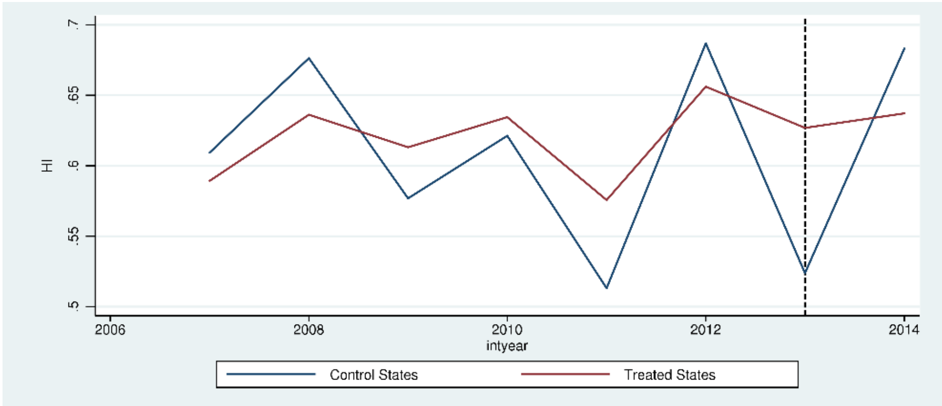


Figure 1-2: differences in trends on Medicaid coverage, 2007-2014

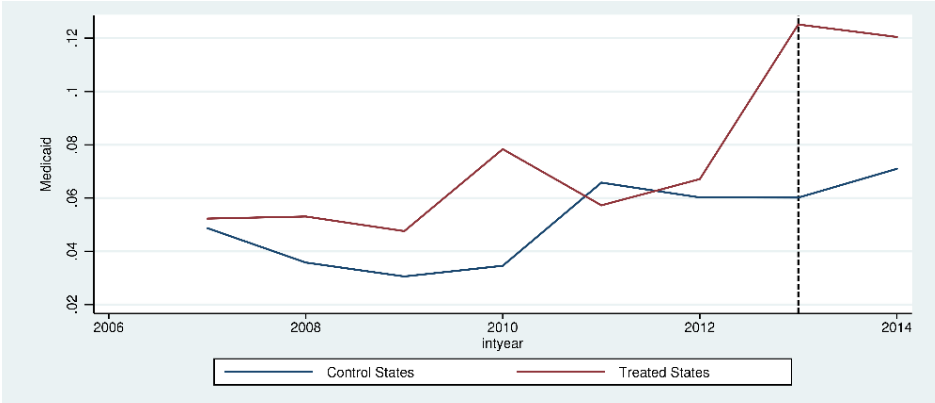


Figure 2-1: differences in trends on objective systolic blood pressure (mmHg), 2007-2014

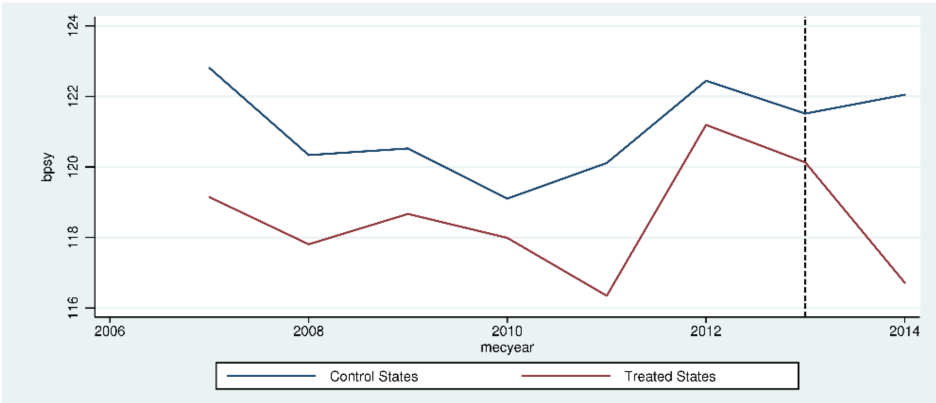


Figure 2-2: differences in trends on objective total cholesterol (mg/dL), 2007-2014

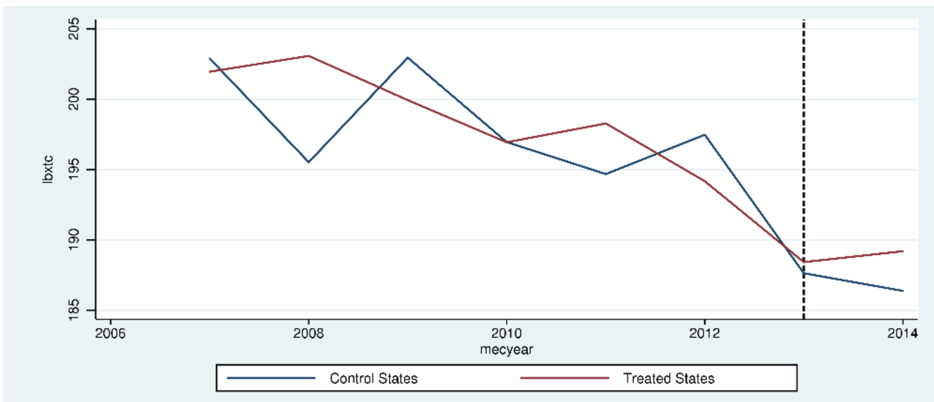
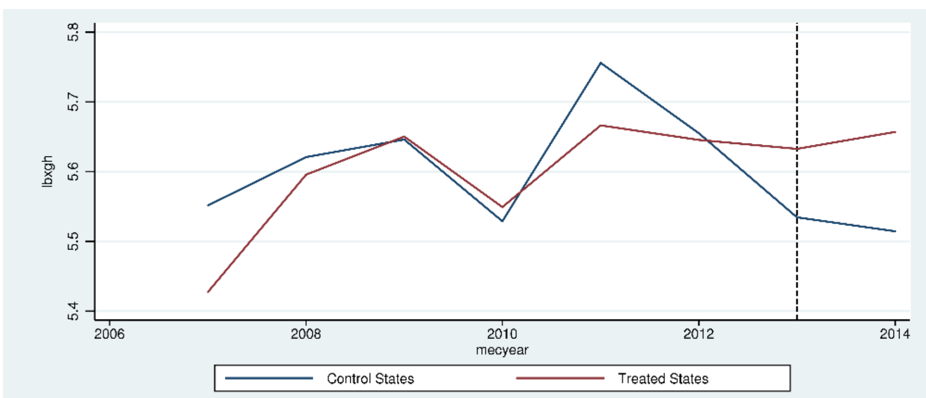


Figure 2-3: differences in trends on objective hemoglobin A1c (%), 2007-2014



APPENDIX FOR ESSAY 1

Appendix Table 1: Difference-in-Differences Estimates of Effect of ACA Medicaid Expansion on Health Outcomes, Low Income Sample (FPL<= 138%), 2007-2014

Panel A: Blood Pressure Measures				
	Systolic Blood Pressure	Diagnosis of Hypertension	Told to Take Prescribed Medicine for Hypertension	Now Taking Prescribed Medicine for Hypertension
DID Coefficient	-2.028	0.044	0.081**	0.060
	(1.277)	(0.044)	(0.035)	(0.043)
Panel B: Cholesterol Measures				
	Total Cholesterol	Diagnosis of High Cholesterol	Told to Take Prescribed Medicine for High Cholesterol	Now Taking Prescribed Medicine for High Cholesterol
DID Coefficient	-1.879	-0.019	-0.003	0.011
	(3.213)	(0.023)	(0.035)	(0.026)
Panel C: Diabetes Measures				
	Hemoglobin A1c	Diagnosis of Diabetes	Diagnosis of Borderline Diabetes	Now Taking Prescribed Medicine for Diabetes
DID Coefficient	-0.037	0.006	-0.014	0.008
	(0.098)	(0.032)	(0.015)	(0.021)
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Demographic Control	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the state level are in parentheses. Regressions are weighted using the restricted annual weights for either the interview or exam sample. Individual level demographic controls include age, gender, race, family income poverty ratio, marital status, family size, household size, citizenship status, and pregnancy status. Year, month, and state fixed effects are included, as is the seasonally unadjusted monthly county unemployment rate. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Data Appendix

The data used in this paper comes from three sources:

1. Kaestner et al. (2015)'s classification strategy of states into Medicaid Expansion treatment and control groups. Available at http://www.nber.org/papers/w21836

2. Restricted geocoded NHANES data, accessed at a Census Research Data Center (RDC). Proposal information for accessing restricted data is available at http://www.cdc.gov/rdc/b3prosal/pp300.htm. The public data subset is available at http://wwwn.cdc.gov/nchs/nhanes/search/nhanes_continuous.aspx

3. Local unemployment rates from the BLS, available publicly at http://www.bls.gov/lau/

ESSAY 2

COLLABORATION NETWORKS AMONG FEMALE ECONOMISTS: AN EXAMINATION OF COAUTHORSHIP USING CEMENT DATA

I. Introduction

The “leaky pipeline” at virtually every rank of academic economics indicates that women are less likely to progress up the career ladder than men. Women in the economics field are less likely to get tenure (Ginther and Kahn 2004, Ginther and Kahn 2015) when compared to their male colleagues. Many studies have investigated the reasons for women’s underrepresentation in the economic profession. Research productivity is cited to be one of the major reasons for the gender difference in the economic academia (Conley, Önder and Torgler 2016, Conley and Önder 2014). Among all the related factors, increasing importance of co-authorship in economic publications has been noted (Hamermesh 2012). Studies have shown that collaborations among economists are positively related to the overall productivity of both men and women (Cainelli et al 2015). However, women have fewer coauthors in economics partly because they tend to coauthor with other women (McDowell, Singell and Stater 2006). More recent studies have addressed the gender difference in coauthorship networks and its impact on researcher’s productivity (Ductor 2015, Hsieh et al 2018), although identifying the causal effects of professional networks is difficult because networks are inherently endogenous. In this study, we investigate the impact of the CEMENT mentoring randomized controlled trial on coauthorship networks. Our results show that the CEMENT workshop expanded coauthorship networks among treated women.

The American Economic Association (AEA) Committee on the Status of Women in the Economics Profession (CSWEP) has monitored the representation of female economists since the 1990s (Lundberg 2018). With the support of the National Science Foundation (NSF) and the AEA, CSWEP established the CEMENT mentoring program to support junior female economists.[7] The National Workshop (now the Workshop for Faculty in Doctoral Programs) focuses on junior female economists employed at institutions where research accomplishments weigh heavily in the promotion decision. It was originally designed as a randomized controlled trial, and was held every other year from 2004-2014. Based on an interim evaluation (Blau et al 2010), the AEA funded the workshop every year starting in 2015. Interim results have shown that the CEMENT program increased the number of publications, publications in top journals, and the number of federal grants for treated cohorts (Blau et al 2010). An updated evaluation also shows that the CEMENT workshop increases publications (Currie, Ginther, Blau and Croson 2018). However, neither evaluation has investigated the mechanism contributing to the improvement in publication outcomes.

In this paper, we use the CEMENT workshop as an exogenous source of variation in the coauthorship networks of treated women. We measure the magnitude of research networks by the total number of co-authors of journal publications gathered from publications on Web of Science. Then we investigate whether the growth of the networks is achieved in the treated group to the control group by comparing the number of authors over time, and we find that the mentoring treatment increases publications, but once we control for the number of coauthors, the treatment effect is no longer significant. We further examine the treatment effect on the magnitude of

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[7] CSWEP runs two mentoring workshops. The CEMENT Workshop for Faculty in Doctoral Programs (https://www.aeaweb.org/about-aea/committees/cswep/programs/CEMENT-mentoring-workshops#doctoral) was originally a randomized controlled trial and is the source of data for this paper. The workshop is held immediately following the ASSA meetings. The CEMENT Workshop for Non-Doctoral programs (https://www.aeaweb.org/about-aea/committees/cswep/programs/CEMENT-mentoring-workshops#nondoctoral) is held in conjunction with a regional economic association meeting (in 2020 with the Western Economic Association Meetings). The Non-Doctoral workshop was not designed as a randomized controe Illed trial and does not emphasize the same professional development topics as the Doctoral Workshop.

networks and our estimates show that CEMENT treatment did increase the number of unique coauthors by almost two. This suggests that the mentoring conference expanded the networks of treated women and resulted in a higher number of publications.

II. The Importance of Collaboration in Research Productivity

The underrepresentation of women in academic careers has long been studied, and women are especially underrepresented in math-intensive science fields in academia (Long 2001, National Academies 2005). There is a large body of literature of women's disadvantage in science since the 1990s and the gender gap has narrowed in many science, technology, engineering, and mathematics (STEM) fields (Ceci et al 2014, Ginther, Kahn and McCloskey 2016, Kahn and Ginther 2018). However, women's representation among PhD students and faculty has stagnated since the turn of the century. Ginther and Kahn (2004, 2014) show that women are significantly less likely to be promoted to tenure than men. Ginther and Kahn (2014) evaluated the gender differences in various career stages in the social sciences, and found that gender differences in the likelihood of receiving tenure could not be explained by observable characteristics as in other social science fields, and the gap was ever larger for single and childless women.

Various factors have been examined as explanations for the underrepresentation of women in academic careers in math-intensive STEM fields (Ceci et al 2014, Kahn and Ginther 2018). Women's lower research productivity is always cited as one primary reason in explaining women's disadvantage in academic careers. Conley, Önder and Torgler(2016) found that the research productivity is positively related with the availability of academic jobs for both genders. Women's responsibility in childbearing and caregiving negatively affect their research productivity (Joecks, Pull and Backes-Gellner 2014, Krapf et al 2014). At work, women devote more time in teaching and other non-research obligations (Bellas and Toutkoushian 1999, Harter, Becker and Watts

2010, Manchester and Barbezat 2013). Lacking research resources – grants and knowledge sharing – is another reason (Duch et al 2012). Moreover, Taylor, Fender and Burke (2006) addressed the importance of coauthorship, conference presentations and peer effects to the gender difference in research productivity, showing the importance of collaboration and research networks to women's research productivity.

Coauthorship is increasingly important for research productivity in economics Hamermesh (2012). Several researchers have examined coauthorship and its impact on academic research productivity (Laband and Tollison 2000). Ductor (2015) shows that for journals listed in EconLit, the proportion of more-than-one-authored papers increased from 24.7% in 1970s to 62.7% in 2011. Hamermesh (2013) examines the changes in the top economics journals in six decades and addressed the growing importance of coauthorship in economic paper published as well. Cainelli et al (2012, 2015) studied a group of Italian economists and their research outcomes, and found that one's research productivity was correlated with the "propensity to collaborate, his/her 'international' connections and the stability of his/her collaborative behavior." Although many researchers have shown a positive association between coauthorship and research productivity, establishing the causal effect of coauthorship is difficult due to the endogeneity of collaboration networks. Lee and Bozeman (2005) instrumented coauthorship using a scale for the location of co-authors. Common research interest between coauthors has also been used as instrument to handle the endogeneity of coauthorship networks (Ductor 2015).

A limited number of papers have studied women's collaboration networks. McDowell, Singell and Stater (2006) showed that female economists were less likely to coauthor than their male colleagues, indicating that lacking professional research networks might explain women's lower research productivity. McNeely and Schintler (2010) summarized the importance of gender disparities rooted from early-life attitude and choices of towards math-intensive subject led to women's underrepresentation later in the academia, which contributed to rather smaller professional networks for women to collaborate with each other that ultimately resulting in lower female publication rate.. Lacking role models in the research area for women scholars could also lead to lower research productivities and hence promotions (Blau et al 2010). However, women's smaller coauthorship networks may be a rational choice because Sarsons (2017) found that women were given less credit for coauthored work than men. In addition, Hengel (2017) found that women economists may be held to a higher standard for publications than men.

Although women publish fewer research papers, the average number of citations to those papers is the same as men's (Ceci et al 2014, Ginther and Kahn 2004). De Leon and McQuillan (2019) found that the cancellation of the 2012 American Political Science Association Annual meeting reduced the number of citations to papers that were scheduled to be presented. The citation effects were larger for less prominent authors. Card et al (2018) found that female authored papers in economics receive 25% more citations than observably similar male-authored publications. Similar to Hengel (2017), they argue that women's publications are held to higher standard than male-authored papers by referees.

Given the importance of collaboration to research productivity, we hypothesize that the CEMENT workshop provides a positive shock to collaboration networks. Since the CEMENT workshop is a randomized controlled trial, it may result in an exogenous change to professional networks for those who are treated, giving us an opportunity to examine the coauthorship networks of CEMENT participants compared to the control group. We discuss the CEMENT mentoring trial and related data below.

III. The CEMENT Randomized Controlled Trial

The CEMENT Mentoring Workshop for Faculty in Doctoral Programs was designed to provide role models (senior female economists) and peers in one's research field. The workshop is held immediately after the ASSA meetings and lasts two days. It is designed to provide tacit knowledge on managing one's academic career. Between 40-50 junior faculty attend and are divided into groups of 4-5 women in the same field. Two senior female economists in the same field are assigned to each group.

Prior to the conference, each woman circulates a research paper that will be read and discussed by the group. During small group sessions, each paper is given an hour where all group members provide comments and feedback on the work. In between group sessions, plenary sessions made up of a panel of senior mentors are held. These panel discussions focused on the topics of research and publishing, getting grants, networking strategies, teaching, the tenure process, and work-life balance. Thus, the CEMENT intervention focused on strategies for publishing research as well as providing comments on a specific paper. In addition, the networking strategy session focused on how to increase professional exposure which would also be associated with increasing one's network.

Approximately 80 people applied to each workshop. Applications were screened by completeness and research intensity of an applicant's current institution (those at teaching-focused

universities were re-directed to the Non-Doctoral Workshop). Applicants were assigned to groups based on research field (e.g. Labor, Macroeconomics, Health, Development, etc.). Within each field, applicants were randomly assigned to treatment or control groups, and more applicants were treated than not (e.g. if there were eight applicants in a field, five were selected as treatments). All applicants were told that there were more applications than slots available in the workshop, and several women applied for multiple workshops. Several women applied for multiple workshops in the eight cohorts, and we use information from the last application to determine whether the woman was "ever treated." If a woman was part of a control group in an earlier cohort and then became treated, she was dropped from the control group.

IV. Data on Publications and Careers

We collected the Curriculum Vitae (CVs) and Web of Science publications of applicants to the CEMENT workshop. Our Web of Science queries for publications were based on five years prior to the doctorate through the third quarter of 2018. We collected data on the first eight CEMENT cohorts from 2004—2016. We did not include the most-recent cohorts because it will take time for potential new collaborations to result in additional publications. In total, there are 592 people in the data. Table 1 lists the cohort year and number of treatment and control groups. For all eight cohorts of female economists, we have their basic information – last name, first name, institutions and email address and their cohort. We used this information to track them on the Internet to find their most recent CVs. We searched for the person by their full name and screened the searching results by the institution and field. We were looking for the most recent CVs (updated CVs in 2017) for each person. For most people who is working in the academy in 2017, we can find either their most recent CVs. From the CVs, we are collecting demographic information, education history, employment history, publications and grants (if listed in the CVs). For people whose CVs are not found through online search, we then turn to other sources of information, such as LinkedIn, academic webpages, or personal webpages. If still we cannot find the person, then we searched for any additional information that could possibly identify the person we are looking for, such as institution/company webpage, ResearchGate, Google Scholar, etc. After we successfully identify the person on the Internet, we entered the data information that we could possibly find in the Access Database file, which is designed specifically for the CEMENT data. In this part of data collection, we mainly focused on the demographic information, education history and employment history, because such information is easier to be confirmed through the above online sources. We ranked the quality of the PhD granting departments and of the person's current employer using rankings based on Kalaitzidakis, Mamuneas, and Stegnos (2003) because it includes international universities.[8]

Once we had information on affiliations, we collected publication data from Clarivate Analytics Web of Science (https://clarivate.com/products/web-of-science/). Web of Science (WOS) is an online subscription-based scientific citation indexing service, giving access to multiple databases, which include most highly-cited economics journals. Although publication data is collected on CVs, we were unable to find recent CVs for several participants. In addition, hand-entry of publications from CVs resulted in significant coding errors. Publications from other sources (such as Google Scholar) contain many working papers and papers that are not published in refereed journals. In contrast, WOS provides consistent measures of publications. However, WOS authors are not-disambiguated thus searchers for people with common names may have false positive publication records. In addition, the number of economics journals indexed in WOS has changed over time.[9] The appendix provides information on how we queried WOS and validated publications.

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[8] The top 10 departments were Harvard University, University of Chicago, Massachusetts Institute of Technology, Northwestern University, University of Pennsylvania, Yale University, Princeton University, Stanford University, University of California- Berkeley and New York University.

[9] According to the Journal Citation Reports the number of Economics journals indexed in WOS increased from 166 in 2002 to 191 in 2007, 333 in 2012 and 353 in 2017 (the latest year available). Some of this was from the creation of new journals (e.g. the American Economic Journals). In addition, WOS expands the number of journals covered.

Our WOS publication data contain information on year of publication, journal, affiliations, coauthor names, and citations. We ranked journals based on quality. Top tier journals are the American Economic Review, the Journal of Political Economy, the Quarterly Journal of Economics, and Econometrica. Rank 2 journals are considered top field journals, and Rank 3 publications are all journals indexed in WOS that are not categorized as Rank 1 or Rank 2. Table 2 lists the titles of Rank 1 and Rank 2 Journals and the number of publications by those in the treatment and control groups in our sample.

V. Results

We begin our analysis with graphical depictions of the data. Figure 1 shows the average publications by year since PhD in our sample. As expected very few economists publish before completing their doctorate and then the number of publications grows to over .8 publications per year between 5 and 9 years since doctorate. Productivity peaks at 7 years past PhD (coinciding with the typical tenure clock) and then trends downward. Figure 2 shows that female economists in our sample are increasingly likely to coauthor. The average number of authors on publications was two in the early years of our sample but increased to almost three by the end.

In order to test the validity of our experiment, Table 3 tests for significant differences in observable characteristics for the treatment and control group. Our t-tests of mean differences indicate no significant differences in observable characteristics between the treatment and control groups.

Figure 3 repeats the analysis of Figure 1 but in this case, by treatment status. The treatment and control groups track each other in terms of publications per year through five years past PhD and then the treatment group has a higher number of publications per year than the control group. Figure 4 repeats this analysis by year since treatment. The treatment group had a slightly higher average publications during the year of the treatment, and then the two averages coincide until year seven, when the treatment group has higher average publications.

Since Figures 1 and 3 show that average publications differ by years since PhD, and our sample includes people who received their doctorates between 1995 and 2015, it is important to control for career stage and when individuals in the sample received the treatment. We begin by replicating analysis from Currie, Ginther, Blau and Croson (2018) on the effect of the CEMENT treatment on the total number of publications indexed in WOS in Table 4. Model 1 includes an indicator for treatment only, the coefficient is positive, but marginally significant ($p<.11$). When we include controls for years since PhD and treatment cohort, the results indicate that treated women have 1.1 additional publications compared to the controls. Model 3 adds controls for ranking of the doctorate program and first job. Treated women have one more publication than the controls ($p<.06$).

Table 5 repeats this analysis, but instead uses the count of number of unique coauthors. Models 1 through 3 show that each additional coauthor leads to over .5 publications. Models 4 through 6 add controls for both coauthors and treatment. The treatment effect falls in magnitude and is no longer statistically significant. However, the coauthor coefficient is of the same magnitude and remains statistically significant.

Given that the number of coauthors eliminates the estimated treatment effect, we examine whether treatment is associated with more coauthors in Table 6. Models 1 and 2 in Table 6 indicate that treated women have between 1.5 and 1.7 more coauthors than those in the control group. Model 3 adds controls for the rank of the doctorate program and the rank of the first job. Women with jobs at top 10 institutions have 5 more coauthors, and women at top 11-20 institutions have

6 more coauthors. Despite these significant effects, the treated women still have 1.5 more coauthors than those in the control group.

Finally, we examine the number of high-quality publications those in the top journals (QJE, JPE, AER and Econometrica) and top field journals listed in Table 2. Model 2 in Table 7 indicates that women in the treatment group have .5 more high quality publications than the control group. However, Model 3 indicates that these publications are explained by the rank of the doctorate and first job. Models 7 through 9 control for both treatment and the number of coauthors. The treatment effect is no longer significant. However, each additional coauthor is associated with about .1 more high quality publications.

VI. Conclusion

Women continue to be underrepresented in the economics profession. In particular, researchers have found that women publish fewer papers than men (Ginther and Kahn 2004), have more difficulty publishing their work (Hengel 2017 and Card et al 2018), and are given less credit for coauthored work (Sarsons 2017). The CEMENT randomized controlled mentoring trial has shown that mentoring increases the number and quality of publications (Blau, Currie, Croson and Ginther 2010, Currie, Ginther, Blau and Croson 2018). This paper examines whether the increase in publications is the result of expanded coauthoring networks.

We found that the mentoring treatment increases publications, but once we control for the number of coauthors, the treatment effect is no longer significant. Estimates show that CEMENT treatment did increase the number of unique coauthors by almost two. This suggests that the mentoring conference expanded the networks of treated women and resulted in a higher number of publications.

Although these preliminary results suggest that the CEMENT workshop expanded treated women's professional networks, more work remains. We are in the process of identifying the number of direct collaborations that resulted from the mentoring workshop. In addition, we plan to use citations to publications to determine whether the quality of the published research increased.

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TABLES FOR ESSAY 2

Table 1: Summary Statistics of Cohorts

Cohort	Cohort Year	Number of Observation		
		Treated	Control	Total
1	2004	45	34	79
2	2006	38	28	66
3	2008	45	26	71
4	2010	34	21	55
5	2012	40	52	92
6	2014	41	45	86
7	2015	40	38	78
8	2016	40	25	65

Table 2: List and the Number of Rank 1 and Rank 2 Journal Publications

Rank 1 Journals	Number of Publications		
	Treated	Control	Total
American Economic Review	60	33	93
Econometrica	10	2	12
Journal of Political Economy	13	10	23
Quarterly Journal of Economics	40	14	54
	Rank 1 Total		182
Rank 2 Journals			
American Economic Journal: Applied economics	42	31	73
American Economic Journal: Economic Policy	32	36	68
American Economic Journal: Macroeconomics	11	6	17
American Economic Journal: Microeconomics	2	7	9
American Economic Review Papers & Proceedings	116	84	200
Brookings Papers on Economic Activity	11	11	22
Economic Journal	23	9	32
Economics Letters	52	46	98
Economic Theory	8	4	12
Econometric Theory	8	0	8
European Economic Review	22	24	46
Games and Economic Behavior	22	20	42
International Economic Review	19	10	29
Journal of Applied Econometrics	17	7	24
Journal of Business & Economic Statistics	16	3	19
Journal of Development Economics	43	27	70
Journal of Economic Dynamics & Control	8	13	21
Journal of Economic History	16	17	33
Journal of Economic Literature	11	9	20
Journal of Economic Perspectives	13	7	20
Journal of Economic Theory	9	5	14
Journal of Econometrics	39	13	52
Journal of Environmental Economics and Management	11	16	27
Journal of Financial Economics	10	11	21
Journal of Health Economics	63	50	113
Journal of Human Resources	55	39	94
Journal of International Economics	20	38	58

Journal of Labor Economics	25	16	41
Journal of Monetary Economics	12	13	25
Journal of Public Economics	69	59	128
JAMA-Journal of the American Medical Association	4	0	4
New England Journal of Medicine	28	2	30
Oxford Bulletin of Economics and Statistics	6	0	6
Proceedings of the National Academy of Science of the United States of America	10	9	19
RAND Journal of Economics	7	3	10
Review of Economics and Statistics	56	36	92
Review of Economic Studies	22	7	29
Scandinavian Journal of Economics	4	3	7
Science	8	7	15
	Rank 2 Total		1648

Table 3: Test of Balance for Covariates

	Treat	Control	T-test	p-value
Foreign PhD	0.071	0.109	1.446	0.149
Top 10 PhD	0.320	0.288	-0.745	0.457
Top 11-20 PhD	0.255	0.228	-0.663	0.508
Top 21-40 PhD	0.189	0.207	0.465	0.642
Top 41-100 PhD	0.134	0.141	0.244	0.807
Top 101+ PhD	0.102	0.136	1.133	0.258
First Job Top 10	0.112	0.114	0.080	0.937
First Job Top 11-20	0.090	0.060	-1.214	0.225
First Job Top 21-40	0.087	0.060	-1.102	0.271
First Job Top 41-100	0.158	0.147	-0.348	0.728
First Job 100+	0.457	0.484	0.588	0.556
Academic First Job	0.888	0.842	-1.482	0.139
PhD Year	2007.450	2007.495	0.105	0.917

Table 4: Effect of CEMENT Treatment on Publications

	(1)	(2)	(3)
VARIABLES	Publications	Publications	Publications
Treatment	0.908	1.034*	0.964*
	(0.599)	(0.547)	(0.542)
Top 10 PhD			-0.645
			(0.726)
Top 11-20 PhD			-0.157
			(0.737)
Top 21-40 PhD			0.999
			(0.779)
First Job Top 10			2.931***
			(0.859)
First Job Top 11-20			2.331**
			(0.979)
First Job Top 21-40			0.967
			(0.995)
Cohort	No	Yes	Yes
Years Since PHD	No	Yes	Yes
Observations	506	506	506
R-squared	0.005	0.250	0.277

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of Coauthors on Number of Publications

VARIABLES	(1) Publications	(2) Publications	(3) Publications	(4) Publications	(5) Publications	(6) Publications
Unique Coauthors	0.549***	0.497***	0.499***	0.548***	0.496***	0.498***
	(0.018)	(0.018)	(0.019)	(0.018)	(0.018)	(0.019)
treatment				0.147	0.249	0.263
				(0.356)	(0.345)	(0.346)
Top 10 PhD			-0.229			-0.237
			(0.462)			(0.463)
Top 11-20 PhD			-0.085			-0.098
			(0.469)			(0.469)
Top 21-40 PhD			0.262			0.259
			(0.497)			(0.497)
First Job Top 10			0.530			0.530
			(0.555)			(0.555)
First Job Top 11-20			-0.790			-0.808
			(0.635)			(0.635)
First Job Top 21-40			0.686			0.668
			(0.633)			(0.634)
Cohort	No	Yes	Yes	No	Yes	Yes
Years Since PhD	No	Yes	Yes	No	Yes	Yes
Observations	506	506	506	506	506	506
R-squared	0.651	0.704	0.707	0.651	0.704	0.707

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of Treatment on Number of Coauthors

VARIABLES	(1) total number of unique coauthor of wos journal	(2) total number of unique coauthor of wos journal	(3) total number of unique coauthor of wos journal
treatment	1.387	1.582*	1.408*
	(0.881)	(0.858)	(0.840)
Top 10 PhD			-0.821
			(1.125)
Top 11-20 PhD			-0.117
			(1.142)
Top 21-40 PhD			1.487
			(1.208)
First Job Top 10			4.825***
			(1.332)
First Job Top 11-20			6.310***
			(1.518)
First Job Top 21-40			0.601
			(1.542)
Cohort	No	Yes	Yes
Years Since PhD	No	Yes	Yes
Observations	506	506	506
R-squared	0.005	0.148	0.197

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of Treatment and Coauthors on Rank 1 and 2 Publications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	highpub	highpub	highpub	highpub	highpub	highpub	highpub	highpub	highpub
treatment	0.391	0.503*	0.425				0.228	0.354	0.309
	(0.282)	(0.277)	(0.261)				(0.263)	(0.266)	(0.252)
Unique Coauthors				0.119***	0.096***	0.084***	0.118***	0.094***	0.082***
				(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Top 10 PhD			1.217***			1.293***			1.284***
			(0.349)			(0.337)			(0.337)
Top 11-20 PhD			0.463			0.488			0.472
			(0.354)			(0.342)			(0.342)
Top 21-40 PhD			0.329			0.210			0.207
			(0.374)			(0.362)			(0.362)
First Job Top 10			2.530***			2.133***			2.133***
			(0.413)			(0.404)			(0.404)
First Job Top 11-20			1.008**			0.510			0.489
			(0.471)			(0.462)			(0.462)
First Job Top 21-40			0.766			0.738			0.717
			(0.478)			(0.461)			(0.461)
Cohort	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Years Since PhD	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	506	506	506	506	506	506	506	506	506
R-squared	0.004	0.132	0.248	0.137	0.203	0.299	0.138	0.206	0.301

Standard errors in parentheses

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

FIGURES FOR ESSAY 2

Figure 1: Average Publications by Year Since PhD

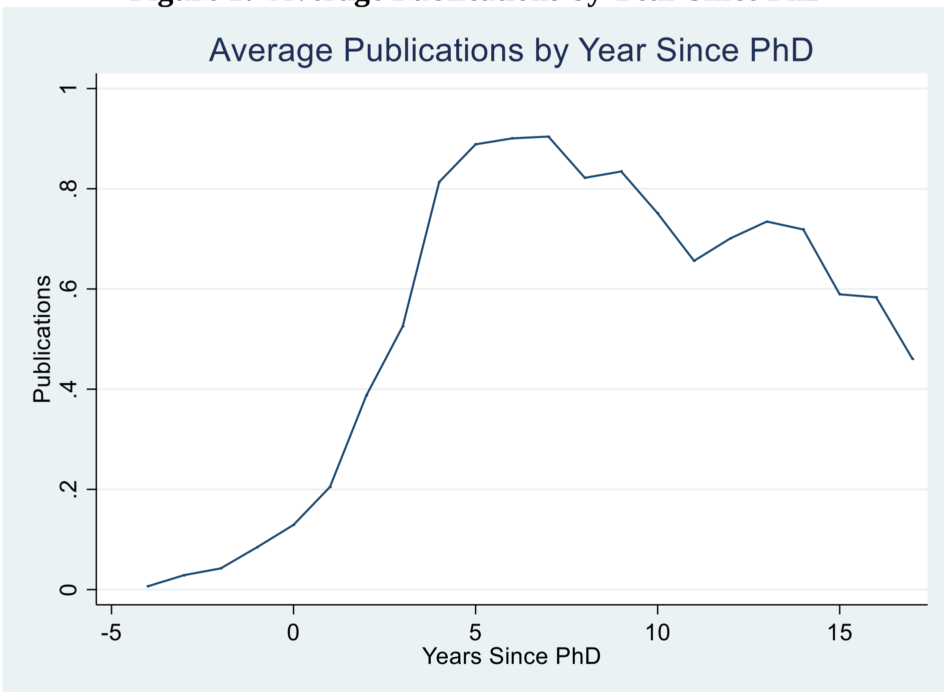


Figure 2: Average Authors on Publications by Year

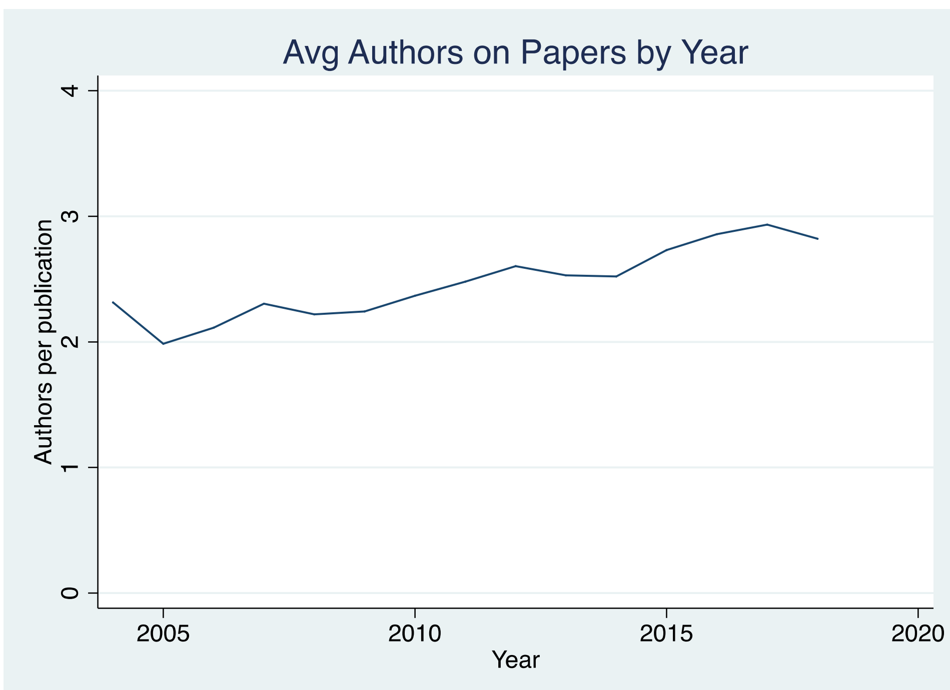


Figure 3: Average Publications by Year Since PhD and Treatment

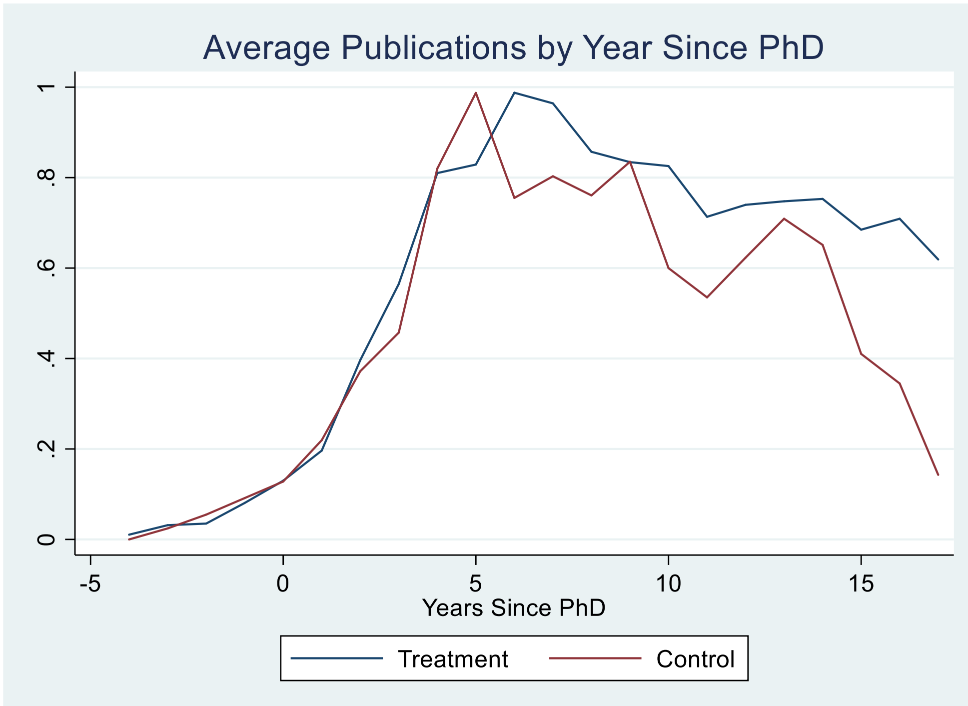
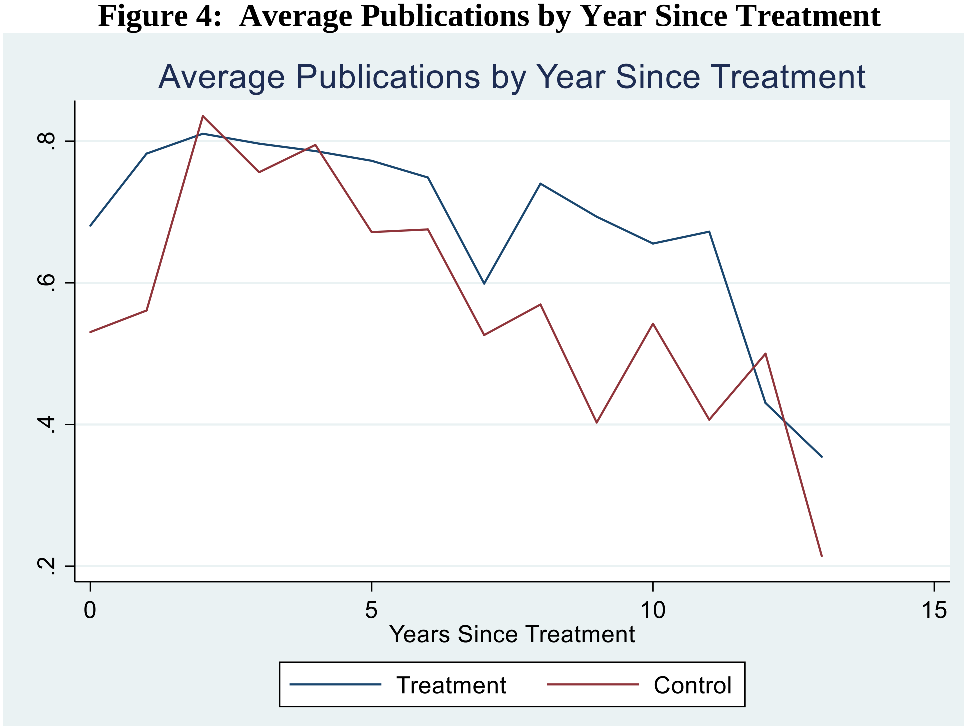


Figure 4: Average Publications by Year Since Treatment



APPENDIX FOR ESSAY 2

Appendix: Web of Science Search Process

We started by creating a list of search queries for everyone in the CEMENT data. Each search query includes names, publication year intervals and affiliations. For names, we use both full names and name initials, because publications indexed in WOS prior to 2008 included only last names and initials for first and middle name. We limit the publication year intervals to be from 5 years prior to PhD year to 2018. The affiliations are the job institutions or affiliations of each person. Such information could be found on the job information collected in in the CVs. Institution names must be changed according to the Web of Science organization enhanced index.

For example, we have a female in the data named Katherine R. McDonald, who graduated in the 2008 from University of Kansas, and she worked in the University of Chicago from 2009 to 2013 and switched to Federal Reserve Bank of Boston after 2013. Then the search query for her would be:

(AU = "McDonald, Katherine" or AU = McDonald, KR or AU = "McDonald, K") and (PY = (2003-2018)) and (OG = "University of Kansas" or OG = "University of Chicago" or OG = "Federal Reserve Bank – Boston")

The search with the name "McDonald, KR" would results in all the records with the full name "McDonald, Katherine R." For people without a middle name (or middle name is not found), the search using last name and first name initials would result in records with same last name but not same first name. For example, paper published by McDonald, Kathy or McDonald, Kevin are likely to be found. Economics papers published before 2006 and Health economics paper published in health journals are more likely to be published in name initials.

After searching and downloading the result files from WOS for each person, we put the data together and hand-validated publications using information from the CVs. We use author's full names, field, institutions and journals to find the false records.

There are several potential limitations to our data. First, the data collection is based on online searches. We are not able to find all the information for everyone in the data, making it less consistent in data colleting. Second, because of the query limitations in WOS, we cannot simply use the WOS search results only. The search queries vary by each person. Some searches returned out with zero record which required updating. The screening process may have introduced coding errors. Most of the CV searches were done in 2017, however publications were found in WOS through the third quarter of 2018.

ESSAY 3

THE IMPACT OF OCCUPATIONAL LICENSING AND NON-U.S. CITIZEN: AN ANALYSIS USING CPS JOB CERTIFICATE DATA

I. Introduction

Occupational regulations are widely adopted by states that approve minimum standards or credentials for work in order to practice in certain occupations. The regulations range from less restrictive (required to register with government agencies), having an exclusive right to a title (known as certifications or certificates)[10], to very restrictive (known as licensure, where it is illegal to practice without a license)[11] (Gittleman, Klen and Kleiner, 2015). In the United States, occupational licensing is a fast-growing form of occupational regulation. Back in the early 1950s, only 5 percent of the labor force was covered by licensing laws. Through the 2000s, the percentage increased to 20% and today, the coverage is closed to 30% of the U.S labor force (Blair and Chung, 2018).

Occupational licensing, or job certifications, require individual workers to pass certain credentials to practice in certain occupations. Such credentials include requirements on education, special trainings, and/or to pass certification tests. Theoretically, such occupational regulations would restrict the labor supply and hence drive up the wage payments. These regulations are aimed at protecting the licensed practitioner as well as maintaining the quality of labor and service in the occupations. Thus, occupational licensing sets barriers to the initial entry, and thereby reduces the

[10] A certification or certificate is always a state-grant title (occupational "right-to-title") which protect persons meeting predetermined standards. People without a certification may also be able to perform in the occupation but cannot use the title.

[11] Licensing refers to the "right-to-practice" which is protected by the law. It is illegal to practice in the occupation without a licensure.

labor supply but increases the wage in certain occupations. Empirical studies have examined the economic effects in various aspects of occupational licensing.

Early studies have looked into the labor market influences of occupational regulations (Friedman and Kuznets, 1945). More recently, with the rapid growing of licensure coverage, the primary focus of research studies is to examine the wage premium. Kleiner (2000) looked into the wage differences in selected licensed and non-licensed occupations, and concluded a wage advantage for the licensed occupations. More recently, an approximately 15% wage premium is found to be associated with occupational licensing (Kleiner and Krueger, 2009). Later, Kleiner and Krueger (2013) found a slightly higher occupational licensing wage premium of 18%. Similarly, Gittleman et al (2015) re-confirmed a licensing wage premium, and found that licensed workers were more likely to receive retirement or pension plan offers, which is a contradictory to the primary findings of Gittleman and Kleiner one year later (Gittleman and Kleiner, 2016)[12].

However, labor market outcomes are not restricted to earnings. Employment is equally important. Law and Marks (2009) conducted a quasi-experimental research design to estimate the labor supply effects of licensing for the minorities and found that licensing laws did not harm the female and black workers but helped in occupations where worker's quality was hard to measure.[13] On the contrary, Blair and Chung (2018) found that occupational licensing reduced equilibrium labor supply using a boundary discontinuity design and found that licensing reduced relative labor supply of white men by 15.2% and black men by 18.9%, but no significant effect for women. From the perspective of certain occupations, a wage premium of 16.2% was found for licensed massage

[12] Gittleman and Kleiner (2016) found no evidence to support the better access to retirement or pension plan offers for licensed worker.

[13] They found that the introduction of licensing laws increased the representation of blacks among teachers, physicians and practical nurses, and of women among engineers and pharmacists. But reductions in the representation were found for black barbers and female teachers.

therapists, together with a reduced labor supply (Thornton and Timmons, 2013). Kleiner and Park (2010) explored the occupational licensing in dentists and dental hygienists, and found a 10% wage premium and 6% increase in employment growth for licensed hygienists in states allowing independent work. However, opposite effects were found for licensed dentists. Later, Kleiner et al (2016) found a 5% wage premium for licensed nurses but no significant employment effects. Another economic effect of occupational licensing is the reduction of mobility of human capital (Kleiner and Wheelan, 2010; Peterson, Pandya and Leblang, 2014; Mulholland and Young, 2016), which has been well established.

In addition to the studies stated above, few studies have examined the effects of occupational licensing on immigrants. Although some have studied the effects on the minorities (Law and Marks, 2009; Blair and Chung, 2018), most are focused on female and underrepresented minorities. Federman et al (2006) studied the entry of Vietnamese into manicuring occupation (which normally requires a licensure) across countries, and found that with the increase in the required amount of training, a reduction of the number of Vietnamese manicurists were found as well as the number of new entries (Federman, Harrington and Krynski, 2006). Two other studies examined the occupational licensing on immigrants in Canada. Banerjee and Phan (2014) showed that occupational licensing requirements in the destination country largely affected the entry into regulated professions. Gomez et al (2015) investigated the effect of occupational licensing on immigrant labor market outcomes in Canada. They estimated the earning premium for immigrants and non-immigrants in regulated occupations and found that immigrants – compare to non-immigrants – were earning more but were less likely to work in regulated occupations. Similarly, in Australia, Tani (2018) examined the occupational licensing as an additional selection hurdle for a large selective immigration program and reconfirmed a licensing wage premium as well as a reduction of over-education and occupational downgrade of people working in licensed jobs.[14]

As a national and state policy issue, the lack of a comprehensive database of occupational licensing or job certifications is one major difficulty in examining this topic. In this study, I continue with the question of the economic effects of occupational licensing, and focus on the population of non-U.S. citizens, using the most recent CPS data on job certifications and licenses at an individual level. I re-examine the effects of occupational licensing on earnings and hours worked per week, and also investigated the effects for non-U.S citizens. My results re-confirm a wage penalty of being a non-U.S. citizen and a wage premium of having a job certificate(and license)[15]. Non-U.S. citizens are found to be less likely to have job certificates (and licenses). Furthermore, I look at a sample of licensed people only, and non-U.S. citizens still suffer from a wage disadvantage. However, the effects on earnings vary across occupational groups.

The rest of the paper is organized as follows. In Section II, I describe the data and the models used in the study. In Section III, I present and interpret the main regression results. In the last section, I conclude with a discussion.

II. Data and Model

A. Job Certificate Data

In January 2015, three questions on job certifications and licenses, based on the work of the Interagency Working Group on Expanded Measures of Enrollment and Attainment

[14] Another finding of this paper is a substantial skill wastage happened after settlement. Not every immigrant continues working in a licensed occupation after settlement. An under-use of migrants' human capital was noticed as a result of a tighter connection between employment and immigration policy.
[15] In CPS data, questions are asked whether have a professional certification or state or industry license. In this study, job certificate and license are treated the same as occupational licensing.

(GEMEnA), were introduced to the CPS. The questions are asked of all household members who are age 15 and older. The three questions are:

1. Do you have a currently active professional certification or a state or industry license?

If answer "Yes" for question (1), then the two following questions are asked:

2. Were/Are any of your certifications or licenses issued by the federal, state, or local government?
3. Was/Is your certification or license required for any of your jobs (*job/main job/job from which you are on layoff/job at which you last worked)*?

A primary advantage of the CPS Job Certification and Licensing data is that it is a microdata asked of individuals. Most of the occupational licensing data used previously are based on occupations where licenses vary by states. Such data are focused on whether occupational licensing or certification is required for certain occupations in each state. Since people's occupations are not used to identify whether they have a certification or license, the CPS provides better data to measure the impact of occupational licensing. This is because occupational regulations may vary across states and occupations. Thus, it is not sufficient to measure the presence of a certification or license based on occupation or job title. The CPS provides an opportunity to investigate the effect of occupational licensing on individual outcomes.

However, some disadvantages of the data need to be stated here. First, certifications and licenses are defined together as "credentials that demonstrate a level of skill or knowledge need to perform a specific type of job".[16] There is no way to distinguish between a job certificate and an occupational license. Second, in question (3), "required" means either legally required or required

[16] Business licenses or non-time-limited licenses are not included.

by employer. It is not clear on the jobs that asked for required certifications or license. This variable could be misleading. Thus, in the analysis, I use occupation to supplement the first two job certificates (or license) variables, and the third question is not used as a determinate of whether a certificate (or license) is required for the job.

B. Sample Selection

Data used in this paper is gathered from IPUMS-CPS.[17] Although CPS job certification and licensing data was introduced in January 2015, variables for all three questions were not available on IPUMS-CPS until October 2017.[18] Thus, I select monthly personal data using IPUMS-CPS from October 2017 to January 2019.[19] The sample was limited to individual age 16 to 65 who are currently employed and not in the armed forces. Table 1-1 shows summary statistics of the full data set. Nearly 23 percent of the total sample reported to have active job certificate and license. Compared to 24 percentage number of licensed U.S. citizens, only 10 percent of non-U.S. citizens have job certificate and license. Then I restricted the data to the sampled people with active job certificates only, and Table 1-2 shows the summary statistics. The majority of the licensed workers are U.S. citizens, and only 3.4% are non-U.S. citizens. Consistent with the full sample, female and married people are more likely to have job certificates. The weighted average mean values on the bottom panel shows that licensed U.S. citizens are earning a bit more than licensed non-U.S. citizen, however, the mean values are not significant different.

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[17] **Sarah Flood, Miriam King, Renae Rodgers, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 6.0 [dataset]. Minneapolis, MN: IPUMS, 2018. https://doi.org/10.18128/D030.V6.0.**

[18] Data in the year of 2015 and 2016 could be extract from the US Census Bureau's DataWeb FTP page. Due to programming error, the third question was not asked in May and June 2015 of household in their first and fifth interviews.

[19] Data used in this study was retrieved from IPUMS-CPS in March 2019, when job certifications and licensing data were updated to January 2019. More monthly data will be available later.

C. Models

I start with an analysis by examining the labor market effects of non-U.S citizens and job certification (and license). The dependent variables are log of hourly wage, log of weekly earnings and hours worked per week. Both hourly wage and weekly earnings are CPI adjusted. The model is as below:

$$Y = \alpha + \beta_1 \text{ non-U.S citizen} + \beta_2 \text{ Job Certificate and License} + \gamma X + \mu$$

In the model, non-U.S citizen equals to 1 if the person is not a U.S. citizen. For job certificate and license indicators, I use two variables according to question (1) and question (2). Question (1) (variable 1) provide an indicator of whether people have an active job certificate or license. Question (2) (variable 2) is an indicator whether people have an active government-issued job certificate or license.[20] X is a set of control variables that influence the labor market outcomes, including gender, age, marital status, year of immigration, educational attainment, working status (full time or part time) and union coverage. In addition, I control for time variables (year and month). All standard errors are robust. The specific weight is used for analysis with CPS earning variables.[21]

To further examine the foreign citizen with certificate, I restrict the sample to people working with active certificate and license only, and then estimate the wage and labor supply effect on non-U.S. citizen. Regression results are presented in the following section.

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[20] Question 2 (variable 2) fit the definition of occupational license. It can be treated as a license variable, different from job certification.

[21] In CPS, an earning weight is a personal level weight used in any analysis including hourly wage, weekly earnings and hours worked per week. All regression programs are conducted using STATA with the earnings weight.

III. Results

Table 2 and Table 3 present the estimation results of the effects of non-U.S citizen and the two job certificate (and license) variables on labor market outcomes. Consistent with previous findings, a wage penalty is found for non-U.S citizens: compared to U.S citizens, non-U.S. citizens earn 8.6 percent less in hourly wage and 7.7 percent less in weekly earnings. No significant effects of occupational licensing were found on labor supply. The results also confirm the occupational licensing wage premium. Having an active job certificate (and license) increases the hourly wage by 6.8 percent and weekly earnings by 8.6 percent (Table 1). Having a government-issued job certificate (and license) has a slightly smaller wage premium: 5.5 percent increase in hourly wage and 7.2 percent increase in weekly earnings (Table 2). Both job certificate (and license) variables significantly increase the hours worked per week by approximate 1 hour on average.

Then I restrict the sample to people with active job certificate (and license) only[22] and estimate the effects of wage and labor supply on non-U.S. citizens. Table 4 presents supportive evidence for foreign citizen wage penalty. For people with active job certificates (and licenses), non-U.S. citizens earns 9.9 percent less in hourly wage and 11.9 percent less in weekly earning than licensed U.S. citizens. Also, non-U.S. citizens are working 0.68 hours less per week (at 5% significant level).Thus, the non-U.S. citizen wage penalty remains in the sample of individuals holding a job certificate or license.

In addition to the above analysis, I estimate a probit model to see whether non-U.S. citizens are less likely to obtain job certificates (and licenses) in the United States. Results are presented in

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[22] Sample that having a government-issued job certificates (and licenses) is a subset of the sample having active job certificates (and licenses). In order to have a bigger sample size of licensed people, I limit the sample by having active job certificates (and licenses) only.

Table 5. All significant and negative coefficients tell that non-U.S. citizens are less likely to obtain job certificates (and licenses). I later run a marginal effect on each citizen variable, and result shows that, keeping everything else constant, getting job certificates is more likely to be judged as low in the non-U.S. citizen groups than in the U.S. citizen population.

Since regulations are different across occupations, I take a closer look at the earnings effects of the job certificates (and license) in 10 major occupation groups[23]. I re-estimate the models of wage premium on two earnings variables over full sample and limited sample across 10 occupation groups. Table 6 shows the estimate results on hourly wage, and Table 7 presents the results on weekly earnings. Both Table 6 and Table 7 have three panels. Panel 1 shows the effects of non-U.S. citizens and having active job certificates using full sample. Panel 2 shows the effects of non-U.S. citizens and having government-issued job certificates using full sample. Panel 3 shows the effects of non-U.S. citizens using limited sample of having active job certificates only. From both tables, with full sample, non-U.S. citizens' wage penalty is significant in most occupation groups except for high-skill industries – Business and Finance, Management and Professional – as well as Farming Fishing and Forestry. The other occupations are considered low-skill occupations, in which the wage penalty for non-U.S. citizens is consistent in Panel 1 and Panel 2 of both tables. The wage premium of having job certificates (and licenses) is even more obvious in the full sample panels (Panel 1 and Panel 2), except for people working in the Farming, Fishing and Forestry. However, when limiting the sample to licensed people only, the significant wage penalty for being a non-U.S. citizen is missing, except for Service and Production industries (Panel 3).

[23] The occupation groups are classified according to the CPS occupation classification code 2010.

IV. Conclusion

The CPS job certification and licensing data provide a microdata of occupational licensing on an individual base. Different from previous studies, now we can observe whether an individual have a job certificate (and license) or not. Using the recent CPS data with job certificate and license, I re-examine the effect of occupational licensing on wage earnings, and an occupational wage premium is re-confirmed. Non-U.S. citizens are suffering from a wage penalty, compare to U.S. natives. And non-U.S. citizens are less likely to obtain job certificates and licenses. Even among licensed workers, the wage penalty for non-U.S. citizens persist.

Across different occupations, the foreign citizen wage penalty is found in low-skilled occupations, and the licensing wage premium is confirmed in almost all occupations, except for Fishing, Farming and Forestry. Since the licensure requirements are different across occupations, some occupations, certain occupation groups may have more strict requirement on licensing, compare to other occupations in which licenses and certificates are less important that education and skills. For high-skilled occupations, such as Financial, Business and Professional related jobs, a certificate or license may not be required such that working experience, certain skills and educational credential are more important. On the contrary, for low-skilled occupations, such as Barbers, Manicurist/Pedicurist and Massage Therapist, a license is always required as a credential to enter the market. Moreover, the population groups may vary across occupations. For examples, there are more Vietnamese working as Manicurist/Pedicurist compare to Americans. But when limiting the sample to people with job certificates only, there are fewer supportive evidences for wage penalty for non-U.S. citizens. Well, licensed immigrants in Service industry are still receiving a significant wage penalty.

Since CPS job certification and licensing data are asked for individual but not available for states, further research could merge in state occupational licensing data by occupation information, which will be devoted to understanding the effects clustered on states. Furthermore, occupational licensing effects vary across occupation, which can be further investigated.

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TABLES FOR ESSAY 3

Table 1-1: Summary Statistics of Employed Workers age 16 to 65 by Licensing Status and Characteristics, CPS monthly data from Oct. 2017 to Jan. 2019

Variables	Total	% Have Active Job Certificate and License	% Have Government Issued Job Certificate and License
Total	965,506	0.2290	0.2078
Gender			
Male	501,357	0.2045	0.1806
Female	464,149	0.2554	0.2373
Age Group			
16-24	114,758	0.0915	0.0826
25-44	436,158	0.2414	0.2184
45-65	414,590	0.2539	0.2314
Marital Status			
Married	535,599	0.2638	0.2405
Not Married	429,907	0.1856	0.1672
Citizenship			
US Citizen	890,978	0.2397	0.2181
Non-US Citizen	74,528	0.1001	0.0876
Work Status			
Usually Full Time	805,731	0.2410	0.2183
Usually Part Time	159,775	0.1684	0.1552
Education Level			
Less than HS	75,935	0.0624	0.0564
HS or GED	252,792	0.1316	0.1186
Some College	168,121	0.1769	0.1577
Associate	106,575	0.3115	0.2842
Bachelor	232,590	0.2643	0.2368
Master	94,660	0.3994	0.3665
Professional	14,793	0.7242	0.6927
Doctorate	20,035	0.5050	0.4783
Union Status			
Union Coverage	25,045	0.3837	0.3625

Table 1-2: Summary Statistics of Employed Workers age 16 to 65 by Licensing Status and Characteristics, CPS monthly data from Oct. 2017 to Jan. 2019: Limited to People with Active Job Certificate (or License) only

Variables	Total	U.S. Citizen	Non-U.S. Citizen
Total	221,067	213,610	7,457
Gender			
Male	102,515	98,645	3,870
Female	118,552	114,965	3,587
Marital Status			
Married	141,291	136,277	5,014
Not Married	79,776	77,333	2,443
Work Status			
Usually Full Time	194,165	187,670	6,495
Usually Part Time	26,902	25,940	962
Education Level			
Less than HS	4,741	4,054	687
HS or GED	33,274	31,895	1,379
Some College	29,748	28,923	825
Associate	33,197	32,498	669
Bachelor	61,469	59,641	1,828
Master	37,807	36,551	1,256
Professional	10,713	10,336	337
Doctorate	10,118	9,712	406
Mean Value			
Hourly Wage	28.045	28.132	26.044
Weekly Earnings	1171.39	1175.784	1071.477
Hours Worked per Week	41.767	41.794	41.141

Table 2: Effects of non-U.S Citizen and Job Certificate (and License) on Labor Market Outcomes, Controlling for Year and Month

VARIABLES	Hourly Wage	Weekly Earning	Hours Worked per Week
Non-US Citizen	-0.086***	-0.077***	-0.098
	(0.01)	(0.01)	(0.07)
Have Active Job Certificate	0.068***	0.086***	1.160***
	(0.00)	(0.00)	(0.05)
Demographic			
Female	-0.172***	-0.226***	-1.874***
	(0.00)	(0.00)	(0.04)
Age	0.008***	0.009***	0.036***
	(0.00)	(0.00)	(0.00)
Married	0.119***	0.128***	0.280***
	(0.01)	(0.01)	(0.09)
Year of Immigration	-0.000	-0.000*	-0.014***
	(0.00)	(0.00)	(0.00)
Education			
High school or GED	0.177***	0.208***	0.279
	(0.02)	(0.03)	(0.41)
Some college	0.243***	0.271***	0.338
	(0.02)	(0.03)	(0.41)
Associate degree	0.325***	0.355***	0.404
	(0.02)	(0.03)	(0.41)
Bachelor's degree	0.623***	0.656***	0.729
	(0.02)	(0.03)	(0.41)
Master's degree	0.773***	0.796***	1.130**
	(0.02)	(0.03)	(0.41)
Professional degree	0.860***	0.964***	4.417***
	(0.03)	(0.03)	(0.47)
Doctorate degree	0.881***	0.950***	2.897***
	(0.03)	(0.03)	(0.44)
Usually Part Time	-0.283***	-0.972***	-19.098***
	(0.00)	(0.01)	(0.06)
Union membership	0.066***	0.082***	-0.042
	(0.00)	(0.00)	(0.06)
R-sqr	0.334	0.47	0.507
Obs	198830	202969	192175

* p<0.05, ** p<0.01, *** p<0.001

Table 3: Effects of Non-U.S Citizen and Government-Issued Job Certificate (and License) on Labor Market Outcomes, Controlling for Year and Month

VARIABLES	Hourly Wage	Weekly Earning	Hours Worked per Week
Non-US Citizen	-0.087***	-0.078***	-0.105
	(0.01)	(0.01)	(0.07)
Have Active Gov-Issued Job Certificate	0.055***	0.072***	1.075***
	(0.00)	(0.00)	(0.05)
Demographic			
Female	-0.172***	-0.226***	-1.881***
	(0.00)	(0.00)	(0.04)
Age	0.008***	0.009***	0.036***
	(0.00)	(0.00)	(0.00)
Married	0.120***	0.129***	0.284***
	(0.00)	(0.00)	(0.04)
Year of Immigration	-0.000	-0.000*	-0.014***
	(0.00)	(0.00)	(0.00)
Education			
High school or GED	0.179***	0.210***	0.293
	(0.02)	(0.03)	(0.41)
Some college	0.245***	0.274***	0.364
	(0.02)	(0.03)	(0.41)
Associate degree	0.329***	0.361***	0.442
	(0.02)	(0.03)	(0.41)
Bachelor's degree	0.627***	0.661***	0.764
	(0.02)	(0.03)	(0.41)
Master's degree	0.779***	0.803***	1.176**
	(0.02)	(0.03)	(0.41)
Professional degree	0.870***	0.976***	4.469***
	(0.03)	(0.03)	(0.47)
Doctorate degree	0.888***	0.958***	2.942***
	(0.03)	(0.03)	(0.44)
Usually Part Time	-0.284***	-0.972***	-19.105***
	(0.00)	(0.01)	(0.06)
Union membership	0.068***	0.084***	-0.043
	(0.00)	(0.00)	(0.06)
R-sqr	0.333	0.47	0.507
Obs	198830	202969	192175

* p<0.05, ** p<0.01, *** p<0.001

Table 4: Effects of Non-U.S. Citizens on Labor Market Outcomes, controlling for year and month, limited sample for people with active job certificates (and license) only

Variables	Hourly Wage	Weekly Earning	Hours Worked per Week
Non-U.S. Citizen	-0.099***	-0.119***	-0.680**
	(0.02)	(0.02)	(0.24)
Demographic			
Female	-0.172***	-0.244***	-2.706***
	(0.01)	(0.01)	(0.09)
Age	0.007***	0.007***	0.014***
	(0.00)	(0.00)	(0.00)
Married	0.092***	0.086***	-0.077
	(0.01)	(0.01)	(0.09)
Year of Immigration	0.000	0.000	-0.021***
	(0.00)	(0.00)	(0.01)
Education			
High school or GED	0.382***	0.373**	-1.015
	(0.07)	(0.12)	(2.91)
Some college but n~e	0.431***	0.414***	-0.747
	(0.07)	(0.12)	(2.89)
Associate degree	0.554***	0.518***	-1.287
	(0.07)	(0.12)	(2.89)
Bachelor's degree	0.816***	0.777***	-1.174
	(0.07)	(0.12)	(2.89)
Master's degree	0.924***	0.888***	-0.436
	(0.07)	(0.12)	(2.89)
Professional degree	1.092***	1.152***	3.499
	(0.07)	(0.12)	(2.90)
Doctorate degree	1.075***	1.102***	1.91
	(0.07)	(0.12)	(2.90)
Usually Part Time	-0.157***	-0.808***	-18.661***
	(0.01)	(0.01)	(0.15)
Union membership	0.054***	0.076***	0.380**
	(0.01)	(0.01)	(0.12)
R-sqr	0.234	0.328	0.367
Obs	47661	49049	46329

* p<0.05, ** p<0.01, *** p<0.001

Table 5: Probit of Job Certificate (and License) being a Non-U.S. Citizen

Variables	Have Active Job Certificate		Have Gov-Issued Job Certificate	
Non-U.S. Citizen	-0.566***	-0.385***	-0.582***	-0.398***
	(0.01)	(0.02)	(0.01)	(0.02)
Demographic				
Female		0.185***		0.226***
		(0.01)		(0.01)
Age		0.005***		0.005***
		(0.00)		(0.00)
Married		0.145***		0.146***
		(0.01)		(0.01)
Year of Immigration		-0.003***		-0.003***
		(0.00)		(0.00)
Education				
High school or GED		1.008***		0.938***
		(0.21)		(0.21)
Some college		1.223***		1.133***
		(0.21)		(0.21)
Associate degree		1.637***		1.544***
		(0.21)		(0.21)
Bachelor's degree		1.506***		1.409***
		(0.21)		(0.21)
Master's degree		1.868***		1.767***
		(0.21)		(0.21)
Professional degree		2.819***		2.718***
		(0.21)		(0.21)
Doctorate degree		2.154***		2.073***
		(0.21)		(0.21)
Usually Part Time		-0.157***		-0.142***
		(0.0`)		(0.01)
Union membership		0.450***		0.482***
		(0.01)		(0.01)
Obs	886298	203294	886298	203294

* p<0.05, ** p<0.01, *** p<0.001

Table 6: Effects of Non-U.S. Citizen and Job Certificate on Hourly Wage by Occupation Groups

	Major Occupation Classification Groups									
Variable	Management, Business and Financial	Professional	Service	Sales and Related	Office and Administrative Support	Farming, Fishing and Forestry	Construction and Extraction	Installation, Maintenance and Repair	Production	Transportation and Material Moving
Panel 1: full sample										
Non-U.S. Citizen	0.014	0.039**	-0.044***	-0.095***	-0.096***	-0.015	-0.103***	-0.100***	-0.099***	-0.077***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)
Active Job Certificate	0.034***	0.036***	0.135***	0.104***	0.044***	0.017	0.102***	0.067***	0.078***	0.116***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
Panel 2: full sample										
Non-U.S. Citizen	0.013	0.037*	-0.045***	-0.095***	-0.095***	-0.016	-0.104**	-0.101***	-0.100**	-0.078***
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)
Active Gov-Issued Job Certificate	0.013	0.027***	0.139***	0.104**	0.035**	-0.025	0.095***	0.062***	0.072***	0.117***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.05)	(0.01)	(0.01)	(0.01)	(0.01)
Panel 3: limited sample to active job certificates only										
Non-U.S. Citizen	0.016	-0.045	-0.093***	-0.131	-0.111	-0.083	-0.152**	0.002	-0.199*	-0.060
	(0.04)	(0.04)	(0.03)	(0.09)	(0.06)	(0.18)	(0.06)	(0.07)	(0.08)	(0.05)

* p<0.05, ** p<0.01, *** p<0.001

Table 7: Effects of Non-U.S. Citizen and Job Certificate on Weekly Earnings by Occupation Groups

	Major Occupation Classification Groups									
Variable	Management, Business and Financial	Professional	Service	Sales and Related	Office and Administrative Support	Farming, Fishing and Forestry	Construction and Extraction	Installation, Maintenance and Repair	Production	Transportation and Material Moving
Panel 1: full sample										
Non-U.S. Citizen	0.001 (0.02)	0.034* (0.02)	-0.012 (0.01)	-0.095*** (0.02)	-0.083*** (0.02)	0.024 (0.04)	-0.124** (0.02)	-0.114*** (0.03)	-0.115*** (0.02)	-0.087*** (0.02)
Active Job Certificate	0.053*** (0.01)	0.059*** (0.01)	0.144*** (0.01)	0.133*** (0.02)	0.065*** (0.01)	0.028 (0.06)	0.131*** (0.02)	0.108*** (0.02)	0.104*** (0.02)	0.203*** (0.01)
Panel 2: full sample										
Non-U.S. Citizen	-0.000 (0.02)	0.032 (0.02)	-0.012 (0.01)	-0.095*** (0.02)	-0.082*** (0.02)	0.022 (0.04)	-0.126*** (0.02)	-0.117*** (0.03)	-0.117*** (0.02)	-0.089*** (0.02)
Active Gov-Issued Job Certificate	0.032*** (0.01)	0.048*** (0.01)	0.148*** (0.01)	0.130*** (0.02)	0.049** (0.02)	-0.013 (0.06)	0.125*** (0.02)	0.090*** (0.02)	0.091*** (0.02)	0.201*** (0.01)
Panel 3: limited sample to active job certificates only										
Non-U.S. Citizen	-0.031 (0.04)	-0.057 (0.04)	-0.124*** (0.03)	-0.200* (0.10)	-0.126 (0.07)	-0.131 (0.20)	-0.115 (0.07)	0.029 (0.09)	-0.237* (0.09)	-0.107 (0.06)

* p<0.05, ** p<0.01, *** p<0.00