

LAW AND LABOR MARKETS: THREE ESSAYS ON INDIVIDUAL DECISION
MAKING

by

Darwyn Deyo
A Dissertation
Submitted to the
Graduate Faculty
of
George Mason University
in Partial Fulfillment of
The Requirements for the Degree
of
Doctor of Philosophy
Economics

Committee:

_____ Director

_____ Department Chairperson

_____ Program Director

_____ Dean, College of Humanities
and Social Sciences

Date: _____ Spring Semester 2017
George Mason University
Fairfax, VA

Law And Labor Markets: Three Essays On Individual Decision Making

A Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy at George Mason University

by

Darwynn Deyo
Master of Arts
George Mason University, 2014
Bachelor of Science
Saint Mary's College of California, 2008

Director: Thomas Stratmann, Professor
Department of Economics

Spring Semester 2017
George Mason University
Fairfax, VA

Copyright 2017 Darwyn Deyo
All Rights Reserved

DEDICATION

I dedicate this dissertation to the individuals in this study – they did a lot of the work.

ACKNOWLEDGEMENTS

I would like to thank my family and friends who supported me through the doctoral process and believed in me along the way. I would also like to thank my advisers and committee members who guided me through the dissertation and helped me produce strong research. Thank you for staying with me through the end.

TABLE OF CONTENTS

	Page
List of Tables	vi
List of Figures	vii
List of Abbreviations	viii
Abstract	ix
Introduction.....	1
Chapter 1 – Licensing and Service Quality	4
Data	8
Empirical Model.....	15
The Effect of Licensing on Quality.....	21
Conclusion.....	31
Chapter 2 – Minimum Wages and Commercial Street Crime	36
Empirical Model.....	39
Data	43
Results	49
Conclusion.....	59
Chapter 3 – Supreme Court Decisions and Entrepreneurs.....	61
Background	64
Empirical Model.....	70
Data	72
Results	80
Conclusion.....	83
Appendix Supplemental Tables	85
References.....	94

LIST OF TABLES

Table	Page
Table 1.1 Summary Statistics	9
Table 1.2 Variable Descriptions	10
Table 1.3 Percent Distribution of Ratings by Industry	11
Table 1.4 Pearson Correlation Matrix of Any Licensing and All Yelp Ratings.....	13
Table 1.5 Mean, Standard Deviation, and Frequencies for Ratings	15
Table 1.6 Results for Licensing and Competition	22
Table 1.7 Results for Separate Border Samples with Any Licensing.....	23
Table 1.8 Results Comparing Licensing and Quality	24
Table 1.9 Results for the Effect of Any Licensing on Quality	27
Table 1.10 Mean Comparisons and T-Tests for Unlicensed and Licensed Businesses....	28
Table 1.11 Equality of Distribution Tests Between Yelp and the SBO.....	29
Table 1.12 Tests for Significant Differences in Ratings Between States	31
Table 2.1 Summary Statistics	44
Table 2.2 Variable Descriptions	45
Table 2.3 Timeline of Minimum Wage Changes by State.....	46
Table 2.4 Mean Comparisons and T-Tests for Commercial Street and Traffic Crimes ...	49
Table 2.5 Results for Separate Crime Samples.....	50
Table 2.6 Results for the Effect of a Minimum Wage Change on the Number of Commercial Street Crimes	51
Table 2.7 Results for the Effect of a Minimum Wage Change on the Number of Crimes	52
Table 2.8 Results for the Effect of a Minimum Wage Change on the Percent of Commercial Street Crime	54
Table 2.9 Results for the Effect of a Minimum Wage Change on the Percent of Crimes	55
Table 2.10 Test for Omitted Variable Bias With Full and Restricted Models	57
Table 2.11 Instrumental Variable Test Using the Share of State Democrats	58
Table 3.1 Variable Descriptions	73
Table 3.2 Summary Statistics	75
Table 3.3 Growth in Employment Over Time	76
Table 3.4 Economic Activity Case Types in Study	77
Table 3.5 Mean Comparisons and T-Tests for Treatment and Control States.....	80
Table 3.6 Results for Separate Treatment and Control Samples	81
Table 3.7 Results for the Effect of Court Decisions on the Supply of Entrepreneurs	82

LIST OF FIGURES

Figure	Page
Figure 1.1 Confidence Interval for Ratings and Licensing	25
Figure 1.2 Box Plot of Licensing and Ratings by Border Status	26
Figure 1.3 States Which Changed Licensing During the Study Period	30
Figure 1.4 Percent Distribution of Yelp Ratings	33
Figure 2.1 Two-Period Model of Legal and Illegal Labor Markets	40
Figure 2.2 Commercial Street and Traffic Crime Trends	47
Figure 3.1 States in the Treatment Group	78

LIST OF ABBREVIATIONS

Yelp Application Programming Interface.....	API
U.S. Census Survey of Business Owners.....	SBO
North American Industry Classification System.....	NAICS
Federal Bureau of Investigation’s Uniform Crime Reports.....	UCR
Value of marginal product.....	VMP
Patient Protection and Affordable Care Act.....	ACA
Integrated Public Use Microdata Series.....	IPUMS
Gross domestic product.....	GDP

ABSTRACT

LAW AND LABOR MARKETS: THREE ESSAYS ON INDIVIDUAL DECISION MAKING

Darwyn Deyo, Ph.D.

George Mason University, 2017

Dissertation Director: Dr. Thomas Stratmann

How do individuals change their labor choices in response to new labor laws? The introduction of new laws and regulations may lead individuals to decrease their labor supply, or to change the quality and type of labor they supply. Rational and utility-maximizing choices may also lead to unexpected outcomes for lawmakers and individual suppliers. These empirical essays use identification for causal inference and models of individual decision making to analyze how labor market laws influence individuals' labor choices.

INTRODUCTION

Laws and regulation govern almost every aspect of modern market economies. Individuals are influenced by laws they are not even aware of through complex market responses. Prices change and individuals opt in and out of markets in attempts to optimize their behavior. Although many laws and regulations have a stated aim or goal, there are often unintended consequences to these laws. More importantly, there are often unexpected consequences to these laws and regulations. Interventions that attempt to protect public health and safety may do the opposite. Efforts to increase incomes may cause problems that individuals solve with creative and unorthodox approaches. I focus here on how laws and regulations for labor markets can produce unintended and unexpected results using models of individual decision making.

I first consider occupational licensing. A common justification for licensing is consumer protection, that is, quality and safety. I employ a unique dataset of individual Yelp business ratings to estimate the relationship between licensing and quality for four occupations. I first test whether licensing impacts competition for these occupations using a negative binomial model and find a negative relationship between more licensing and the number of firms. I design a difference-in-differences model with state fixed effects, using business location near state borders as a treatment group and state requirements for licensing as the treatment. Yelp ratings are used as the measure of service quality. I find

that requiring any licensing for an occupation significantly lowers quality. This result is robust to all the individual licensing types except minimum age requirements. I also find evidence for increasing returns from licensing for licensing fees and education and training.

Another popular labor market regulation is state and federal attempts to increase the minimum wage. However, low-skilled groups who face strict budget constraints and are unemployed after an increase in the minimum wage may shift to illegal labor markets in order to generate income. In particular, crimes with an income-producing component, such as prostitution, drug sales, or theft, may be particularly viable second-best alternatives for these individuals. I employ individual-level arrest data and state-level changes in the minimum wage for Chicago and New York City to estimate the effect of minimum wage increases on the number and percent of income-based crimes. I also develop the term “commercial street crime” to include all crimes that can generate income, departing from the conventional division of property crimes from violent crimes. I find a positive relationship between increases in the state minimum wage and increases in commercial street crime, as well as increases in different types of commercial street crime. This result is robust to several specification tests.

Finally, I consider how courts influence labor markets through direct and indirect effects. The Supreme Court of the United States, as the highest judicial authority in the country, has the final say on whether a law is constitutional or not. Thus there is a high opportunity cost when the Court makes a decision that limits individuals in that it cannot be reversed by any entity other than the Court. In particular, court decisions can change

the incentives and costs faced by entrepreneurs. Using U.S. Census data and a database of Supreme Court cases, and employing linear regression, I find that from 1950 to 2000, precedent-altering Supreme Court cases increased the labor supply of entrepreneurs but decreased the share of entrepreneurs in the labor force. This suggests that while precedent-altering court cases increase the supply of entrepreneurs, they do so at a decreasing rate.

All three of these studies illustrate how individuals respond in creative ways to laws and regulations, and oftentimes these responses produce outcomes that lawmakers would hope to avoid. However, all of these outcomes are measurable and predictable through models of individual decision making. Market laws and regulations, therefore, are best considered as a double-edged sword to both the individuals who avoid them and the lawmakers whose policies result in unintended consequences.

CHAPTER 1 – LICENSING AND SERVICE QUALITY

In the United States, occupational licensing varies widely from state to state. As of 2013, one-third of the workforce was licensed compared with 5 percent of the workforce in 1950 (Kleiner and Krueger 2013). In July 2015, the White House issued a report on the rise of occupational licensing in recent decades and the potential for licensing to have a negative impact on consumers and the labor force (The White House 2015). Although conventional wisdom suggests that occupational licensing protects public health and safety, empirical research on the impact of licensing on quality remains an important field of study.

Previous research has found that occupational licensing creates market power and monopoly effects, including increased prices, a fall in the labor supply, and increased profits for licensed providers (Dorsey 1983; Hogan 1983; Klein 1998; Kleiner 2000). Suppliers face less competition in the marketplace with licensing, and as licensing often represents sunk costs in the form of entry fees and education or training, it may represent significant barriers to entry, as defined in Baumol & Willig (1981). Licensing has been found to increase massage therapists' earnings by over 16 percent and barber earnings between 11 and 22 percent, while midwife licensing has been found to increase prices by as much as 44 percent (Adams, III, Ekelund, Jr., and Jackson 2003; Timmons and Thornton 2010; Thornton and Timmons 2013). In addition to reducing the number of

suppliers and increasing both prices and profits, licensing may also result in lower quality as suppliers face less competition, especially if consumers face asymmetric information or if reputational effects are weak (Shapiro 1982; Shapiro 1983; Shapiro 1986).

Legislatures and courts often cite product quality and consumer safety rationales when passing and upholding occupational licensing (*Thomas v. Collins* (323 U.S. 516) [1945]; *Meadows v. Odom* (03-960-B-2) [2003]; *Vong v. Sansom* (037208) [2009]; Theiss 2011). In the 1889 case of *Dent v. West Virginia* (129 U.S. 114 [1889]), the first Supreme Court decision to address occupational licensing, Justice Stephen Field held that “the power of the state to provide for the general welfare of its people authorizes it to prescribe all such regulations as, in its judgment, will secure or tend to secure them against the consequences of ignorance and incapacity as well as of deception and fraud.” If occupational licensing increases quality, the net effect of licensing may be positive. However, the growth of licensing laws has been linked to political competition, higher demographic stability, greater labor heterogeneity, and government demand (J. K. Smith 1982; Faith and Tollison 1983). None of these factors illustrate a clear link between licensing and protecting quality. Therefore, it is possible that the argument that licensing increases quality is an *ex post* rationale for existing regulation.

Other research suggests that licensing may not increase quality. As a medical profession, dentistry is a commonly licensed or certified profession but more stringent licensing has not been found to improve dental outcomes (Kleiner and Kudrle 2000). Licensing may also not affect quality in the same way for all income-groups. Child care licensing reduced the number of providers in low-income neighborhoods while increasing

the quality of providers in high-income neighborhoods (Hotz and Xiao 2011). Licensing may also change the distribution of quality without changing the average level of quality, either by shifting the distribution of quality or by changing the tails of the quality distribution. Teacher certification tests, for example, widen the distribution of quality as measured by student test scores, and the gains from licensing primarily accrue to high-income areas (Larsen 2015).

Although the impact of licensing on quality has been studied for professional occupations such as dentistry and teaching, research on the impact of licensing on quality for occupations such as barbers and manicurists is more limited (Carpenter II 2011; Hotz and Xiao 2011; Carpenter, II 2012). There is also no theoretical reason to expect licensing to affect quality in the same way for all skill groups. Further, there is significant variance in which occupations are licensed and by how much (Carpenter II et al. 2012). The variance in the extent and stringency of occupational licensing allows me to study the intensive effects of licensing on quality using Yelp ratings for individual businesses.

Yelp ratings have been found to work well as a substitute for other reputational effects and to accurately reflect quality as measured by other sources (Bardach et al. 2013; Luca 2016; Ranard et al. 2016). There is also evidence that businesses change their behavior in response to public ratings databases, suggesting consumers believe ratings accurately measure quality (Bardach et al. 2013; Gergaud, Storchmann, and Verardi 2015; Luca 2016; Ranard et al. 2016). Yelp ratings have even been used to improve hospital care as a supplement to traditional patient surveys, with Yelp ratings reporting on quality for more hospitals and subjects than traditional surveys (Ranard et al. 2016). Yelp

is also partnering with local health departments to improve visibility and information about safety ratings for food trucks (Booth 2014). The Yelp data therefore allow me to address questions about the impact of state occupational licensing on quality. Further, if Yelp ratings represent perceived quality, both consumers and suppliers would have the same imperfect information from Yelp. The ratings may also represent expected quality, such that consumers respond to the Yelp ratings as if they represented true quality and suppliers respond as if the ratings represented the true quality of their competitors (Luca 2016).

In this study, I focus on the impact of licensing on quality for four occupations: barbers, cosmetologists, manicurists, and massage therapists. These occupations were selected due to the competitive features of the markets, aside from barriers to entry created by licensing. The four occupations are labor-intensive and common in informal markets in the developing world, suggesting very low capital constraints for start-ups (Tshuma and Jari 2013). There is also frequent overlap across the occupations, as manicure parlors may also offer massage therapy services, and so on. However, I formally test if licensing affects competitiveness in these occupations before proceeding to the quality and licensing analysis. I then employ a difference-in-differences analysis with state fixed effects to estimate the relationship between licensing and quality. I identify businesses near state borders as the treatment group and state requirements for licensing as the treatment in their respective models. I then use the Yelp ratings and a database of state licensing requirements to estimate the effect of licensing on quality.

I have two main findings of statistical significance. First, there is less competition in states with higher licensing fees and education and training requirements, and there is more competition in states with more licensing exams, higher minimum school grade requirements, and higher minimum age requirements. I also find evidence for increasing and decreasing returns from licensing to competition by type of licensing requirement.

Secondly, I also find that higher licensing fees and education and training requirements are associated with lower quality, and higher minimum age requirements are associated with higher quality. I also find that requiring any licensing for an occupation lowers quality about a quarter of a star. I find that requiring any licensing fees, any education or training, any licensing exams, or any minimum school grade also lowers quality. I find no effect from requiring any minimum age.

Data

Panel data come from individual Yelp ratings from businesses located in the 50 states and the District of Columbia for barbers, cosmetologists, manicurists, and massage therapists. I collected the data from Yelp in May 2015 using a web scraper that pulled the information for individual businesses in a given city and state using the Yelp Application Programming Interface (API). Summary statistics for the variables in this study are included in Table 1.1.

Table 1.1 Summary Statistics

Variable	Mean	S.D.	Min	Max
Rating	4.06	1.09	1	5
Fees	\$125	\$76.11	\$0	\$775
Education	338	150	0	884
Exams	2	0.64	0	4
Grade	10	5.18	0	12
Age	17	7.31	0	23
Licensed	0.91	0.29	0	1
Border	0	0.34	0	1
Firm Count	38,700	35,925	82	91,665

Note: Licensing variables from the IJ License to Work report (Carpenter II et al. 2012). A median age less than 16 indicates that more states do not have any minimum age licensing requirements.

The original sample consisted of ratings from 189,624 businesses. The individual business rating is used as the measure of quality. Average business ratings are built from individual reviews, potentially dating from Yelp’s launch in October 2004 to the data collection in May 2015. It is not possible to disaggregate the average business ratings over time, precluding time-series analysis. The final sample includes 54,602 businesses in the four occupations outlined above. North Dakota and South Dakota are not included in the final sample due to the minimum 10 reviews restriction. Descriptions for each variable in are included in Table 1.2.

Table 1.2 Variable Descriptions

Variable	Description
Rating	Individual business ratings, measured on a ½ point scale from 1 to 5
Fees	Logged fees paid in \$, including application fees, processing fees, and licensing fees, for the initial license only
Education	Total days of education or training required, where hours were converted to days, years were multiplied by 365. Educational degrees were converted to the number of days it took to complete the degree.
Exams	The number of written and practical licensing exams
Grade	The minimum school grade
Age	The minimum age required
Licensed	Binary indicator for whether a state requires any licensing, where if a state requires fees, education or training, exams, or minimum school grade or ages, Licensed = 1
Border	Binary indicator for whether a city is within one hour of another state (=1 if yes, =0 if no)
D*LIC	The difference-in-differences variables associated with each licensing type, where Border = 0 and Any Licensing by type = 1 (Model 3).
Firm Count	The number of businesses by occupation by state according to the 2012 Survey of Business Owners

Note: Licensing variables from the IJ License to Work report (Carpenter II et al. 2012).

The Yelp data include the individual business's Yelp rating, the number of Yelp reviewers, and the address, city, and state of the business. The business rating is the average of all individual ratings left for the business and is measured on a half-point scale from 1 to 5 (Table 1.3). I used the number of reviewers to restrict the sample to business ratings with at least 10 reviewers to account for the possibility of business owners leaving themselves ratings that positively bias the sample of ratings, although this risk is expected to be small (Luca 2016).

Table 1.3 Percent Distribution of Ratings by Industry

Yelp Rating	All	Barbers	Hair	Nails	Massage
1	0.05	0.06	0.02	0.1	0.07
1.5	0.45	0.32	0.15	1.01	0.39
2	1.55	1.1	0.67	3.23	1.33
2.5	4.33	3.54	2.24	8.5	3.49
3	8.44	9.74	5.23	14.57	7.69
3.5	15.28	22.53	10.63	22.72	16.29
4	21.93	39.38	19.4	25.4	21.65
4.5	28.79	23.33	35.07	18.46	26.25
5	19.18	0.06	26.59	6.01	22.84

Note: Calculated from study sample of ratings with at least 10 reviewers. Hair is the cosmetology occupation, and Nails is the manicurist occupation.

Occupational licensing types vary by state and firms in the sample potentially face five different types of licensing requirements. Operators must pay an entry fee; they must complete a certain number of education or other training days; they must pass a given number of practical and/or written licensing exams; they must complete a minimum school grade; and/or they must be a certain age before they can work in the market.¹ The License to Work Report details occupational licensing requirements for 102 occupations that require some skill but have not traditionally been licensed (Carpenter II et al. 2012). These requirements can vary broadly across states and occupations. For example, California required barbers to pay \$125 in licensing fees, complete 350 days of education and training, and pass two exams. New York required barbers to pay \$60 in licensing fees, complete 884 days of education and training, and pass three exams. In comparison,

¹ The days of education and training may include on-the-job training, internships, apprenticeships, or other forms of nontraditional education (Carpenter II et al. 2012).

California did not license massage therapists at all, while New York required massage therapists to pay \$115 in licensing fees, complete 233 days of education and training, and pass one exam. Given the extent and variance of licensing requirements across the United States, I focus here on the intensive effect of licensing levels, as well as the impact of requiring a type of licensing.

Barbers cut and shave hair and beards. Cosmetologists shampoo, cut, color, and style hair, as well as provide other hair-styling services. Manicurists clean, shape, and decorate nails. Massage therapists massage customers (Carpenter II et al. 2012). At the time of the licensing study, barber licensing applied in 49 states and the District of Columbia (Carpenter II et al. 2012). On average barbers are required to pay \$130 in fees, complete 416 days of education and training, and pass two exams. Cosmetology licensing applied in all 50 states and the District of Columbia. On average cosmetologists are required to pay \$142 in fees, complete 372 days of education and training, and pass two exams. Manicurist licensing applied in 49 states and the District of Columbia. On average manicurists are required to pay \$91 in fees, complete 87 days of education and training, and pass two exams. Massage therapist licensing applied in 38 states and the District of Columbia. On average, massage therapists are required to pay \$181 in fees, complete 139 days of education and training, and pass one exam.

I joined the Yelp ratings data with the licensing database for the four occupations in this study. The Yelp rating for individual businesses is used as the dependent variable for the primary models of interest. I use four types of licensing requirement variables as the independent variables. I first use the value associated with licensing requirements; for

example, fees for massage therapists in Arizona are \$189. Licensing fees and the days of education and training are logged. I then created a vector of licensing variables by type equal to the squared value of licensing requirements to account for increasing or decreasing returns from licensing. Next, I created a binary indicator variable by licensing type for whether a state requires any licensing for that occupation for the five licensing types. For example, any fees = 1 if a state requires any licensing fees. I also created a binary indicator variable for whether a state requires any licensing at all for an occupation. For example, if a state requires any one of the five licensing types, any licensing = 1. The Pearson correlations between licensing types and the Yelp ratings are listed in Table 1.4. Requiring licensing fees is highly correlated with requiring education and training and licensing exams. Requiring any education or training is also highly correlated with requiring licensing exams.

Table 1.4 Pearson Correlation Matrix of Any Licensing and All Yelp Ratings

	Fees	Education	Exams	Grade	Age	Rating
Fees	1.00					
Education	0.85	1.00				
Exams	0.82	0.88	1.00			
Grade	0.57	0.49	0.46	1.00		
Age	0.38	0.34	0.32	0.36	1.00	
Rating	0.02	-0.03	-0.04	0.05	0.30	1.00

Note: No states require a minimum age for manicurists. All Pearson correlations are significant at the 99 percent confidence level.

The number of businesses by industry and state from the 2012 U.S. Census Survey of Business Owners (SBO) is used as the dependent variable when considering the effect of licensing on competition to address the possibility that the higher occupational earnings associated with licensing reflect higher quality, increased demand, or a reduction in the lemons problem (Akerlof 1970). It is also possible that markets are monopolistic due to reasons other than licensing. The number of firms therefore includes a measure of relative market competition as used in Bloom, Propper, Seiler, & Van Reenen (2015). I also include the number of firms in the full model to control for how competition may influence quality.

I identified businesses that are on state borders using the Yelp city, which allowed me to identify the effect licensing has on businesses in border cities compared with non-border cities. Usefully, firms in border cities face a similar mix of consumers and quality expectations but different licensing requirements. Using a binary indicator variable, I identify and set equal to 1 those businesses in cities within one hour's drive of a state border. All other businesses are identified as non-border and are set equal to 0, including those in cities on international borders. I then created the difference-in-differences treatment and treatment group interaction variables, where the interaction variable = 1 if the any licensing variables = 1 and border = 0. Although I do not have any price information for the businesses in the sample, I included state fixed effects to account for state differences including income, population density, and other unobservable variables that may influence the results.

Table 1.5 Mean, Standard Deviation, and Frequencies for Ratings

	Non-Border	Border
Unlicensed	4.16 (0.72) 4,893	4.12 (0.78) 275
Licensed	4.07 (0.76) 42,379	3.95 (0.76) 7055

Note: Standard deviations are listed below means, and frequencies are listed below standard deviations, for the sample of Yelp ratings with 10 reviews or more.

I compare the mean values, standard deviation, and frequency of ratings for firms in the study for each of the licensing variables in Table 1.5. Importantly, there appear to be small differences in unlicensed non-border and border firms and larger differences between licensed non-border and border firms. Firms licensed and on the border appear to have lower ratings than firms that are unlicensed and on the border. I find this result for overall licensing, for education and training, for licensing exams, and for minimum school grade requirements, although the difference for the last category is not significant.

Empirical Model

If occupational licensing creates monopoly power and allows suppliers to increase prices in order to generate monopoly profits, suppliers may also increase profits by lowering or not investing in quality, especially for markets with lower consumer willingness to pay or weak reputational effects. Although licensing requirements are expected to remove the bottom tail of the provider distribution and thus increase quality, the monopoly effects from licensing on quality may instead lower average quality (Shapiro 1982). Although fixed costs will not create monopoly effects, licensing

requirements are often sunk costs, which may lower quality. Practitioners pay a licensing fee that they cannot recover, they complete days of education or training, they pass a number of exams, and/or they meet a minimum grade or age requirement (Baumol and Willig 1981).² The direction of the effect on quality may only be determined empirically.

Research also suggests that the impact of licensing on quality may be ambiguous, or that licensing may change the distribution of quality, especially if the requirements deter skilled workers (Besanko, Donnenfeld, and White 1987; Larsen 2015). The potential for low-quality products is increased in the case of asymmetric information, and in the case of weak reputational effects. As low quality from monopoly effects may occur without licensing, I focus on occupations that are competitive aside from the barriers to entry created by licensing. I also include a measure of competition in the model using the number of firms for each occupation and state. I then employ a difference-in-differences analysis with state fixed effects to compare the quality of licensed firms with that of unlicensed firms.

The difference-in-differences analysis departs from the traditional method of time and place. The treatment in the analysis comes from a state requiring licensing for an occupation. The treatment is exogenous to a single firm, which cannot independently influence whether a state requires licensing or which types of licensing a state requires. The choice of quality by a firm, therefore, is a response to the presence of licensing (Shapiro 1986). The treatment and control groups in the analysis are determined by a firm's location near the state border. If the city in which a firm is located is within an

² Licensing requirements for the days of education or training may not represent sunk costs if suppliers can amortize the cost of their education or training and pass the cost on to consumers.

hour's drive of the state border, the firm is on a state border and falls into the control group.³ Otherwise, the firm is not on the state border, and falls into the treatment group.

I use firms on state borders as the control group as customers have the option to drive across the state border for services and therefore competition may be stronger in these markets. Firms in the treatment group are far enough away from the state border that the impact of licensing should dominate the effect of any cross-border competition.

Therefore, the interaction variable compares the licensing coefficient for firms that are licensed and operate far from a state border with firms that are licensed and operate near a state border, firms that are unlicensed and operate far from a state border, and firms that are unlicensed and operate near on a state border. The coefficient for the treatment group should therefore measure the impact of licensing on quality relative to competitive and unlicensed markets. I also separately test the effect of licensing on quality for border and non-border businesses in order to test the parallel trends assumption of the difference-in-differences model. Although I am concerned about how the quality for a given occupation in a state may influence licensing regulation, I do not think that quality in a given state will influence the licensing requirements in a state that it borders. Therefore, the use of the border variable should effectively identify the direction of causality.

I first developed a negative binomial regression model to estimate the impact of licensing on competition, by each type of licensing. The model uses the level of licensing as the independent variables of interest and is estimated at the state level. Fees and the days of education and training are logged.

³ City border status was determined using Google Maps.

$$(1) \quad \text{FIRMS}_{os} = \alpha + \beta_1 \text{LICLEV}_{os} + \beta_2 \text{LICLEV2}_{os} + \varepsilon_i,$$

where FIRMS_{os} is the number of firms by occupation and state using the SBO data. LICLEV is a vector of the values for the five licensing types by occupation and state. LICLEV2 is a vector of the squared values for the five licensing types by occupation and state to account for increasing or decreasing returns from licensing.

I then employ a linear regression model with state fixed effects to measure differences in quality from licensing. The model uses the level of licensing as the independent variable of interest and estimates rating differences within states and across occupations. Coefficients are the weighted average across occupations and states. I then exploit variation in the level and type of licensing. Fees and the days of education and training are logged.

$$(2) \quad \text{YELP}_{ios} = \alpha + \beta_1 \text{LICLEV}_{os} + \beta_2 \text{LICLEV2}_{os} + \beta_3 \text{BORDER}_s + \beta_4 Z_s + \beta_5 \text{FIRMS}_{os} + \varepsilon_i,$$

where YELP_{ios} is the individual Yelp rating for an individual business in an occupation and state.

The last model employs the OLS difference-in-differences state fixed effects model. This model uses a binary indicator for whether a state has any licensing, by type of licensing and occupation; a binary indicator that identifies whether a business is located near a state border; and the interaction between each licensing indicator variable and the border variable. As cities on state borders include businesses with various mixes of licensing by occupation and type, and consumers who are not bound by state lines when making purchases, the interaction variable captures how licensing affects the quality of businesses that face licensing and less competition than businesses that do not

face licensing and more competition. The coefficients estimate the impact of licensing within states and across occupations. I then exploit variation in ratings across licensing requirements and the border proximity to estimate the impact of licensing on quality.

$$(3) \text{YELP}_{ios} = \alpha + \beta_1 \text{ANYLIC}_{os} + \beta_2 \text{BORDER}_s + \beta_3 D^*LIC + \beta_4 Z_s + \beta_5 \text{FIRMS}_{os} + \varepsilon_i,$$

where *ANYLIC* is the vector of licensing. *ANYLIC* measures the treatment group in the analysis. For example, Massachusetts requires 733 days of education and training for barbers, and so *ANYEDU* for education and training = 1 for barber businesses in that state. However, Massachusetts does not require a minimum grade for barbers, so *ANYGRADE* for the minimum grade = 0 for barber businesses in that state. If a business is located within an hour's drive of the state border, *BORDER* = 1, and = 0 otherwise. *BORDER* is the treatment group in the analysis. *D*LIC* is the vector of interaction variables by licensing types, where *D*LIC* = 1 for a business if *ANYLIC* = 1 and *BORDER* = 0. I also conduct joint estimation of the licensing requirements with this model. Coefficients are estimated at the individual business level by state and licensing type and reflect the weighted average across occupations and states. All standard errors are robust and clustered at the state level.

The distribution of Yelp ratings is clustered around the top of the scale, which runs contrary to conventional wisdom about people being more likely to leave negative reviews on Yelp. I assume the Yelp rating (YELP_{ios}) follows a cardinal scale such that the distances between each rating along the distribution are equal. Specifically, reviewers may believe that the difference between a rating of one star and two stars is equal to the difference between three and four stars. I run these regressions on the full sample of all

occupations pooled together. In a sensitivity test, I also run all the regressions on separate samples for each individual occupation.

I considered several potential issues with the Yelp data, especially as this is the only study using this unique dataset. First, it is possible that selection bias exists in the Yelp data and that the distribution of businesses by states in the sample does not match the true distribution of businesses by states. To address this possibility, I compared the distribution of Yelp businesses by occupation and state with the distribution of their corresponding North American Industry Classification System (NAICS) industries in the 2012 SBO, also by state. Barbers in the sample were matched to NAICS industry code 812111 (barber shops); cosmetologists were matched to NAICS industry code 812112 (beauty salons); manicurists were matched to NAICS industry code 812113 (nail salons); and massage therapists were matched to NAICS industry code 621340 (offices of physical, occupational and speech therapists, and audiologists), based on the NAICS industry descriptions. I tested for selection bias in each occupation by comparing the Yelp distributions with the SBO distributions. I used the Aspin-Welch two-sample t-test with unequal variance, as well as a two-sample Kolmogorov-Smirnov test for equality of distributions in the number of businesses by state and occupation. I used the full sample of all Yelp ratings for this analysis as I am interested in the overall distribution of businesses and not the value of the ratings.

Yelp was launched in October 2004 and ratings potentially include reviews left over the entire time period from October 2004 to May 2015, when I collected the data. Fourteen states changed their licensing requirements during this period, as determined

from news articles and WestLaw, an online legal research service. I therefore considered how states may change their licensing in response to quality and therefore how states that changed their licensing requirements may differ from states that did not. I conducted a robustness test for whether states that changed their licensing requirements during the study period had significantly different Yelp ratings than states that did not change their licensing requirements. These changes were primarily to the level of the licensing requirements, for example, increasing licensing fees or the days of education and training. I tested for significant differences in ratings for states that changed their licensing requirements and those that did not using two-sample t-tests for each occupation, after first testing for whether the variances were equal for these two groups in each occupation. I used the sample of Yelp ratings with 10 or more reviews for this analysis as I was interested in differences in ratings between states that did and did not change their licensing requirements.

The Effect of Licensing on Quality

I find that licensing occupations impacts quality across occupations within states significantly, although the effect and direction varies by type of licensing. I also find that licensing lowers competition, as measured by the number of firms by occupation. Sensitivity tests were run for each occupation, and the results are included in the Appendix. A positive sign in the results means that licensing increases quality, as measured by Yelp ratings. A negative sign means that licensing lowers quality.

Table 1.6 Results for Licensing and Competition

SBO Firm Count	(1)
Fees	-12.240** (5.133)
Education	-6.320*** (2.338)
Exams	0.909*** (0.273)
Grade	0.163** (0.069)
Age	0.168** (0.072)
Fees Squared	6.067** (2.554)
Education Squared	3.262*** (1.160)
Exams Squared	-0.250*** (0.077)
Grade Squared	-0.011* (0.006)
Age Squared	-0.010** (0.004)
Observations	319

Note: Model 1 reports the results for the negative binomial model.
Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$;
*** $P < .01$.

The marginal effect of licensing on competition for the four occupations in the study is statistically significant for all five licensing types (Table 1.6). Increasing licensing fees and the days of education and training is associated with less competition. Increasing the number of licensing exams, the minimum school grade, and the minimum age is associated with more competition. I also find evidence for increasing returns to licensing requirements for licensing fees and the days of education and training, and evidence for decreasing returns to licensing from licensing exams, the minimum school grade, and the minimum age.

I separately test the sample of border businesses and the sample of non-border businesses using Model 2 to examine whether the results are significantly different (Table 1.7). I find that the results for the licensing variables of interest are statistically similar between the two samples, although there are some differences in magnitude. The results reflect that state licensing requirements within a state will be the same for both border and non-border firms, and that the variation in licensing comes from the mix of businesses on state borders and the level and type of licensing by state.

Table 1.7 Results for Separate Border Samples with Any Licensing

Yelp Rating	Border	Non-Border
Fees	0.226* (0.134)	0.131* (0.071)
Education	-0.132 (0.274)	-0.262 (0.179)
Exams	-0.210 (0.152)	-0.288* (0.148)
Grade	-0.095 (0.090)	-0.042 (0.055)
Age	0.322*** (0.037)	0.520*** (0.092)
SBO Firm Count	0.000*** (0.000)	0.000* (0.000)
Observations	7,330	47,272
Groups	43	44
R-Squared	0.098	0.134

Note: Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

The marginal effect of licensing on quality, as measured by the level of licensing, is statistically significant for licensing fees, the days of education and training, and minimum age requirements (Table 1.8).

Table 1.8 Results Comparing Licensing and Quality

Yelp Rating	(2)
Fees	-8.869*** (2.758)
Education	-10.540*** (3.716)
Exams	-0.112 (0.109)
Grade	0.024 (0.027)
Age	0.045** (0.019)
Fees Squared	4.437*** (1.378)
Education Squared	5.283*** (1.856)
Exams Squared	0.008 (0.035)
Grade Squared	-0.003 (0.002)
Age Squared	-0.002 (0.001)
Border	-0.001 (0.027)
SBO Firm Count	0.000 (0.000)
Observations	54,602
Groups	49
R-Squared	0.137

Note: Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$;
*** $P < .01$.

Requiring any licensing fees is associated with lower quality, but there is evidence for increasing returns to quality from licensing. It is possible that requiring licensing fees does work as theorized; that is, the bottom of the distribution is cut out by requiring a monetary commitment from the potential supplier. Increasing the days of education or training is also associated with lower quality and I find evidence for increasing returns to education and training requirements. Increasing the number of licensing exams or the minimum school grade is not significantly associated with quality, and I find no evidence of increasing or decreasing returns to quality. Higher minimum age requirements are associated with higher quality, but I find no evidence of increasing or decreasing returns to quality.

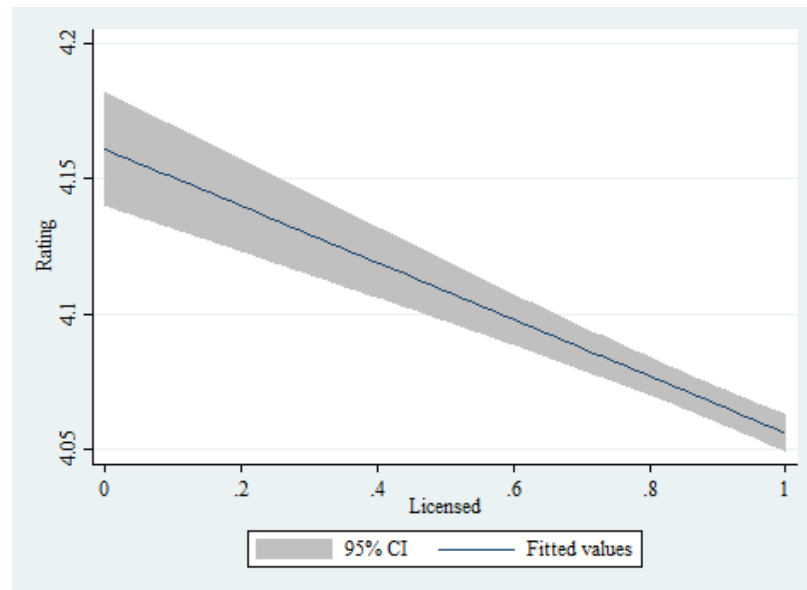


Figure 1.1 Confidence Interval for Ratings and Licensing

Note: Sample of Yelp ratings with 10 or more reviews.

Figure 1.1 illustrates the 95 percent confidence interval for ratings and licensing, defined as a state requiring any licensing for a given occupation. There is a clear negative relationship between more licensing and ratings, such that having more licensing produces lower ratings. The rating scale is restricted to the 4 stars and higher in this figure for purposes of scale. The box plot in Figure 1.2 also illustrates the range of Yelp ratings by licensed and border status. As illustrated in Table 1.3, the median value of Yelp ratings is about 4 stars, but the box plot illustrates there is still a wide range in the distribution of ratings between firms and their border and licensed status.

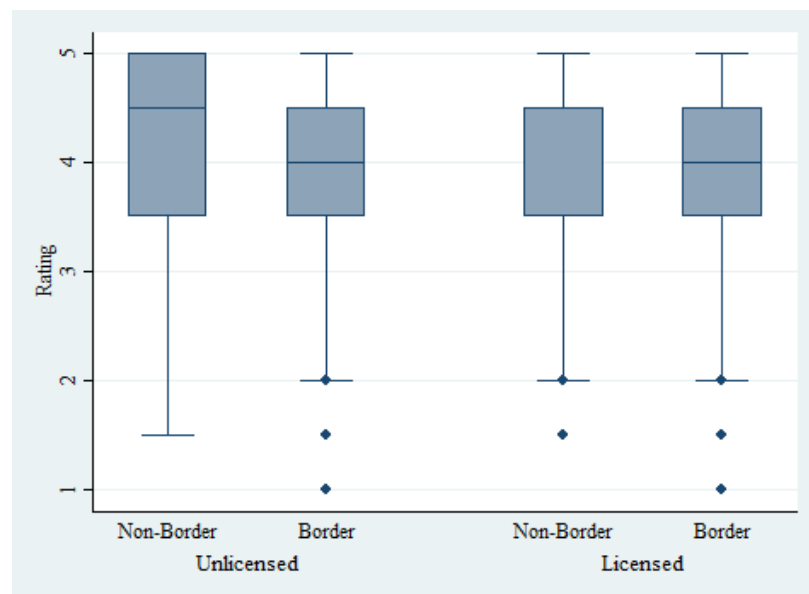


Figure 1.2 Box Plot of Licensing and Ratings by Border Status
 Note: Sample of Yelp ratings with 10 or more reviews.

Licensing as measured by states having any licensing for an occupation is statistically significant and negative when states require any licensing or when states require licensing

fees, education and training, licensing exams, or minimum school grade requirements (Table 1.9).

Table 1.9 Results for the Effect of Any Licensing on Quality

Yelp Rating	(3a)	(3b)	(3c)	(3d)	(3e)	(3f)	(3g)
Fees	0.130 (0.172)						0.197 (0.145)
D*Fees	-0.383* (0.193)						-0.055 (0.106)
Education		-0.169** (0.082)					-0.185 (0.274)
D*Education		-0.220** (0.096)					-0.077 (0.147)
Exams			-0.182*** (0.044)				-0.148 (0.159)
D*Exams			-0.233*** (0.049)				-0.145 (0.087)
Grade				-0.100 (0.101)			-0.057 (0.081)
D*Grade				-0.168* (0.097)			0.008 (0.054)
Age					0.402*** (0.030)		0.367*** (0.044)
D*Age					0.101 (0.071)		0.146 (0.091)
Licensed						-0.175** (0.082)	
D*Licensed						-0.257*** (0.081)	
Border	-0.335* (0.168)	-0.207** (0.099)	-0.209*** (0.051)	-0.057 (0.045)	0.039 (0.038)	-0.245*** (0.087)	-0.196* (0.100)
SBO Firm Count	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000* (0.000)
Observations	54,602	54,602	54,602	54,602	54,602	54,602	54,602
Groups	49	49	49	49	49	49	49
R-Squared	0.063	0.070	0.073	0.065	0.107	0.072	0.129

Note: Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Requiring any licensing lowers quality about a quarter of a star. The treatment variable of licensing and the identifying variation are significant and negative for all licensing types when estimated separately, except for minimum age requirements, although the joint effect of licensing on quality appears to be null. A null joint effect from licensing is relevant when considering the correlation between licensing types among states.

I conducted several robustness and specification tests on the results. I considered whether there are significant differences between licensed and unlicensed firms (Table 1.10). I employed the Aspin-Welch two-sided t-test with unequal variance to compare the mean values of quality and other outcome variables for licensed and unlicensed firms, as determined by whether a firm is in a state that requires licensing for that occupation. The test reveals significant differences between the two groups for all variables, but most importantly for Yelp ratings.

Table 1.10 Mean Comparisons and T-Tests for Unlicensed and Licensed Businesses

	Mean	Unlicensed	Licensed	T-Test
Ratings	4.07 (0.76)	4.16	4.06	9.86
Border	0.13 (0.34) (14.02)	0.05	0.14	-25.60
SBO Firm Count	38,700 (35,926)	8,221	41,887	-200.00
N	54,602	5,168	49,434	

Note: Mean, standard deviation, and confidence t-test for all variables by state border location.

I also tested for the possibility of selection bias in the Yelp data such that the distribution of businesses by states in the sample did not match the true distribution of businesses by states.

Table 1.11 Equality of Distribution Tests Between Yelp and the SBO

	Two-Sample T-Test	Two Sample Kolmogorov-Smirnov
Barbers	0.990	0.728
Cosmetology	1.000	0.187
Manicurists	0.989	0.877
Massage	0.998	0.408

Note: P-values are reported for the two-sample t-test and the Kolmogorov-Smirnov test for equality of distribution. The t-tests were calculated allowing for unequal variances between the two samples, and the Kolmogorov-Smirnov tests were calculated using exact p-values. The Yelp sample is for all Yelp results, not just those with 10 or more reviews.

I first used the Aspin-Welch two-sample t-test for whether the Yelp distributions were significantly different from the SBO distributions, assuming unequal variances, and found no significant differences for any of the four occupations (Table 1.11). To check the results, I also used a two-sample Kolmogorov-Smirnov test for equality of distributions, for each occupation. I found no significant difference between the business distributions for any of the occupations.

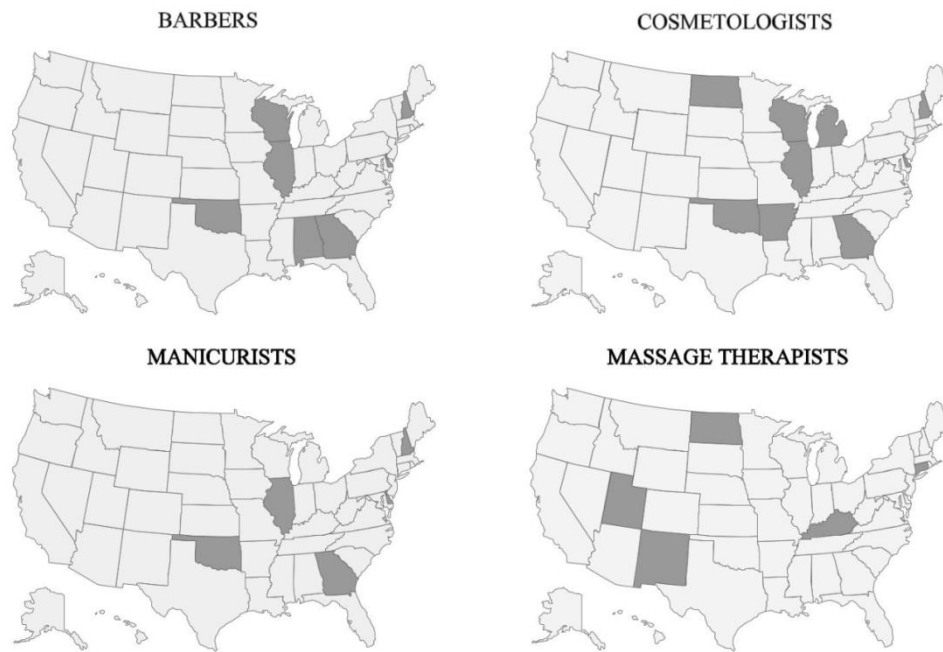


Figure 1.3 States Which Changed Licensing During the Study Period

Note: Collected from WestLaw and online news reports for 2004-May 2015. If a state changed any one of the five licensing types, it is classified as having changed its licensing regulation and shaded.

In addition, fourteen states changed licensing requirements during the study period (Figure 1.3). I therefore identified states which changed their licensing requirements for each occupation, and tested whether states which changed their licensing requirements had significantly different ratings from states which did not (Table 1.12). I first used a Satterthwaite test for equality of variance for ratings between states that did and did not change their requirements. Ratings for barbers, cosmetologists, and massage therapists did not have significantly unequal variance between these two state groups, while manicurist ratings did exhibit unequal variance. I then used a two-sample t-test to determine whether ratings between states which did and did not change their licensing requirements were significantly different, by occupation. The t-test assumed

equal variance except for manicurist ratings, which uses the Aspin-Welch two-sample t-test assuming unequal variance. I found no significant differences for ratings between the state groups over the study period, suggesting that states are not changing their licensing in response to changes in quality in the sample study period.

Table 1.12 Tests for Significant Differences in Ratings Between States

	Satterthwaite Test	Two-Sample T-Test
Barbers	0.239	0.184
Cosmetologists	0.812	0.254
Manicurists	0.003	0.817
Massage Therapists	0.393	0.331

Note: P-values are reported for the Satterthwaite equality of variance and the two-sample t-test. The sample uses Yelp ratings with 10 or more reviews.

Conclusion

I find that requiring any licensing lowers quality. I also find that higher licensing fees and education and training requirements are not associated with higher quality. This suggests that licensing does not increase product quality as measured by consumer reviews (Adams, III, Jackson, and Ekelund, Jr. 2002; Federman, Harrington, and Krynski 2006; Kleiner 2006). Notably, firms have an incentive to invest in education and training, as firms that invest in human capital outperform firms that do not (Bosma et al. 2004; Dimov and Shepherd 2005). Firms may invest in education and training and internalize the gains from education and training without a licensing requirement. It is also possible that the positive effect of requiring any kind of education and training licensing may be

mitigated by monopoly effects (Adams, III, Jackson, and Ekelund, Jr. 2002). I find support for this finding in testimony that shows what is actually included in some schools' curricula (U.S. District Court Eastern District of Missouri 2015). In this case, an owner of a barber and cosmetology academy stated the curriculum at the academy included 245 hours of required fitness education, including an indeterminate number of hours on how to stand properly. Importantly, individuals who have not met a minimum school grade, such as high school graduation, will often be unable to meet licensing requirements for days of education due to trade school enrollment prerequisites. It is also possible that the minimum school grade requirement is linked to the licensing requirement for the days of education and that removing the minimum school grade requirement would increase quality that is currently attributed to the days of education and training requirement.

This study cannot control for unobservable changes over time. The ratings for individual businesses are an average of all ratings, which could have potentially been left any time between October 2004 and May 2015. I also cannot determine whether there are unlicensed producers on Yelp, although there are strong incentives for producers outside the legal market to avoid publicity, or whether businesses choose to operate in a licensed or an unlicensed state based on the quality they expect to produce (Balko 2014; Sibilla 2016). However, I know of no evidence that suggests sellers strategically choose quality first and then choose whether to operate in a licensed or unlicensed state. In fact, as a single business cannot directly influence the choice of licensing, it is more likely that the

businesses choose quality after facing a set of exogenous constraints such as licensing requirements

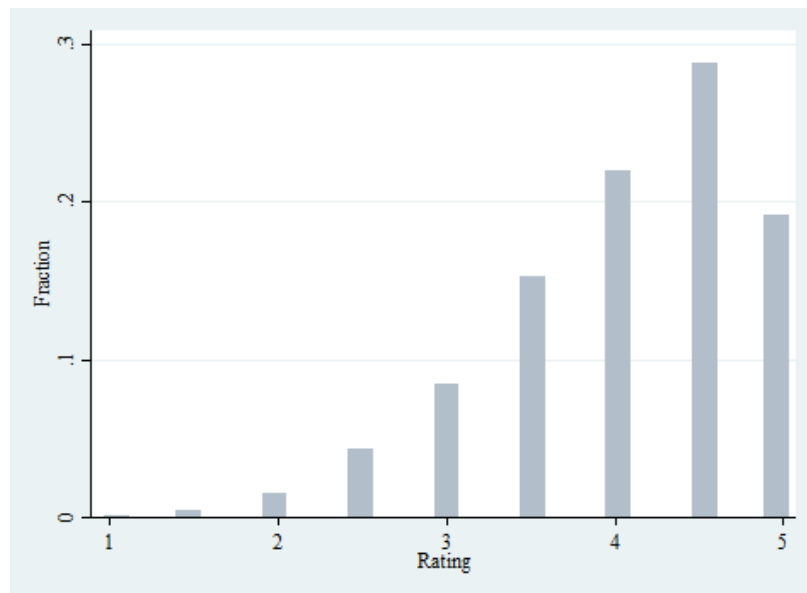


Figure 1.4 Percent Distribution of Yelp Ratings

Note: Sample of Yelp ratings with 10 or more reviews.

It is also worth noting that ratings of four stars and above represent a large proportion of the sample (Figure 1.4). Ratings of 4 stars and above represent 62.8 percent of the sample for barbers; 81.1 percent of the sample for cosmetologists; 49.9 percent of the sample for manicurists; 70.7 percent of the sample for massage therapists; and 69.9 percent overall. It is possible that people do not like to give negative reviews, or even that Yelp removes reviews (Handy 2012). The proportion of high ratings suggests that if the results are biased, it is from an overestimation of the benefits of licensing requirements. The high ratings for businesses may also illustrate how licensing removes the bottom of

the distribution. I also cannot control for how reviewers are different from non-reviewers with respect to quality but it is reasonable to assume that these differences do not vary across states or licensing. Further, the state fixed effects may capture any within-state variation that would significantly change reviewer traits. However, to the degree that reviewers do not represent the full population, the results would reflect how licensing influences ratings but not necessarily quality.

Self-service by consumers can also lead to a fall in real quality that is not captured in the Yelp ratings (S. L. Carroll and Gaston 1983). The Cadillac effect, where licensing increases prices such that some consumers exit the market, is not incompatible with licensing lowering ratings. This study uses the competitive outcome as the comparison group, and high prices may exist with low quality in the monopolistic framework. The effect of licensing lowering ratings may be exacerbated as consumers turn to self-provision and do-it-yourself projects in response to higher prices, changing the distribution of the consumer market. Consumers facing higher prices from licensing may also expect higher quality in return and be relatively unsatisfied.

The only costs I can capture in these models are the upfront costs of licensing. Significant other costs exist from licensing, such as reduced availability or longer wait time, which cannot be captured in the analysis. On the other hand, the costs of compliance with licensing are large compared to the magnitude of the impact of licensing. For example, in the sample, a total of \$2 million was hypothetically paid to state licensing authorities by barbers; \$7.9 million by cosmetologists; \$2.8 million by

manicurists; and \$2.5 million by massage therapists⁴. It is also possible that reputational effects are weaker with more licensing. If licensing signals quality without increasing it, I would expect a negative impact on quality as consumers rely on licensing instead of alternative reputational signals.

Despite conventional wisdom that occupational licensing ensures product quality and protects the public interest, there has been little research on the impact of licensing on quality for occupations such as the ones in this study. The results suggest that licensing can significantly lower quality for these occupations. Four out of five common licensing types significantly lower quality for businesses that are licensed and face less competition, while the minimum age requirement has no effect on quality. Given that some types of licensing are associated with less competition for these occupations and that licensing lowers quality, legislatures and courts should consider alternative methods of protecting the public health and safety.

⁴ These costs were calculated by multiplying the number of businesses in the full sample collected from Yelp by state with the licensing fees in those states.

CHAPTER 2 – MINIMUM WAGES AND COMMERCIAL STREET CRIME

In recent years, there has been a movement toward significantly increasing minimum wages at the state level. At the federal level, then President Obama issued an executive order in 2014 to increase the minimum wage for federal employees. Although the literature demonstrates unemployment effects for low-skilled workers, relatively less is known about the secondary effects of increasing the minimum wage (Card et al. 1994; Sabia, Burkhauser, and Hansen 2012; Zavodny 2000). Individuals who cannot find legal employment may seek employment in alternative labor markets. The literature examining these secondary effects has primarily focused on how increases in the minimum wage change the labor-leisure tradeoff and whether the income or substitution effect dominates between minimum wage increases and the opportunity cost of committing crime. However, literature on the employment effects from increasing the minimum wage highlights the fact that low-skilled and high-risk individuals are more vulnerable to changes in the labor market, and young women, single mothers, and minorities may be even more vulnerable (Ahn, Arcidiacono, and Wessels 2011a; Currie and Fallick 1996a; Kolker 2013). Therefore, there are significant implications for what happens to these groups if they are priced out of the legal labor market.

One potential outcome from increasing the minimum wage is that low-skilled and high-risk individuals may seek employment in the illegal labor market. Research on the

impact of wage mandates, such as the minimum wage or living wages, has been focused on how increasing the minimum wage impacts urban and youth crime (Beauchamp and Chan 2014; Fernandez, Holman, and Pepper 2014a; Hashimoto 1987; Thompson 2009). These studies have produced conflicting results over whether wage mandates increase crime. Crime is often divided into the two categories of property crime and violent crime (Jacob and Lefgren 2003; Raphael and Winter-Ebmer 2001; Fernandez, Holman, and Pepper 2014b). These studies also use the Federal Bureau of Investigation's Uniform Crime Reports (UCR) methodology for crime, which divides most crimes into property or violent crime. For example, burglary is listed under property crime while robbery is listed under violent crime, even though the functional difference between the two categories is that the criminal uses a gun in the latter. However, some violent crimes such as robbery may generate illegal income. Stealing someone's wallet or television may provide temporary funds for an individual without other labor prospects, although repeated thefts also bring increased contact with the police authorities (Becker 1974). Using the UCR crime categories may therefore not be the best way to capture the substitution effects from the minimum wage increase for individuals treating illegal activity as an alternative labor market.

Previous studies have found that with minimum wage increases, firms will lay off the least productive workers, where the value of the individual's marginal product (VMP_L) is greater than the minimum wage (Gould, Weinberg, and Mustard 2002). However, although income and substitution effects will influence whether individuals seek income through illegal activity, it is unclear which effect dominates (Ahn, Arcidiacono, and

Wessels 2011b). Individuals with jobs and higher incomes will substitute away from commercial street crime, while individuals without jobs may substitute toward commercial street crime. Even if labor in these two markets are not considered perfect substitutes, the net effect of a higher minimum wage on crime is still unclear when considering an individual's tradeoffs and opportunity cost of entering the illegal labor market.

In this study, I identify a set of crimes that provide a repeated stream of payments to the individual and are directly linked to an employment category. These include lifestyle crimes, such as prostitution, the sale or shipment of prohibited drugs, theft, and a general list of other income-generating crimes such as pandering or fraud. Theft of higher-value items with a defined market, such as cars or car parts, would also offer reliable repeated payments to the criminal. I term these "commercial street crimes" and include any crimes that provide income, such as prostitution, drug sales, pandering, counterfeit sales, burglary, or robbery, as compared with non-income-producing crimes, such as murder or arson. Commercial street crime is a useful analysis group because it captures crimes across many categories. I also contribute to the literature by focusing on the types of crimes most likely to be committed by low-skilled workers who are priced out of the legal market for labor (Gardecki and Neumark 1998; Neumark and Wascher 2006; Ahn, Arcidiacono, and Wessels 2011b; Currie and Fallick 1996b).

I also add to the literature by considering what happens to the labor surplus after an increase in the minimum wage and by using an alternative data source that may better represent changes in the illegal labor market. I find that arrests for commercial street

crime go up 14 percent after an increase in the state minimum wage. Arrests for drug sales go up 18 percent, arrests for theft go up 12 percent, and arrests for other commercial street crimes go up 24 percent. There were 351,813 arrests for commercial street crimes in Chicago and New York City in the base year of the study, suggesting that an increase in the minimum wage would result in an additional 49,254 arrests for commercial street crimes, compared with no increase in the minimum wage and conditional on other labor market factors.

Empirical Model

I start with a two-period model where Period 1 takes place before an increase in the state minimum wage and Period 2 takes place after the increase in the state minimum wage. In Period 2, firms and labor will maximize their respective incomes. In this model, I have three agents: firms, labor, and police. In Period 1, firms freely contract for labor within a competitive labor market at the equilibrium wage rate. In this study I consider the competitive labor market wage rate to be the competitive legal labor market wage rate (W_{LEG}). In Period 2, the state minimum wage increases and firms reallocate their labor selection based on labor productivity. Firms will change their aggregate labor selection based on who is hired on the margin due to the value of their marginal product (VMP_L) and based on the overall mix of labor and capital in production. Labor is not treated as homogenous as differences in human capital determine whether individuals retain or gain employment in Period 2. Individuals with the lowest VMP of labor will not be employed in Period 2. The least skilled, least educated individuals will then be unemployed. Individuals not employed in the legal labor market will face a choice of whether to enter

the illegal labor market for the competitive illegal labor market wage rate W_{ILL} (Figure 2.1).

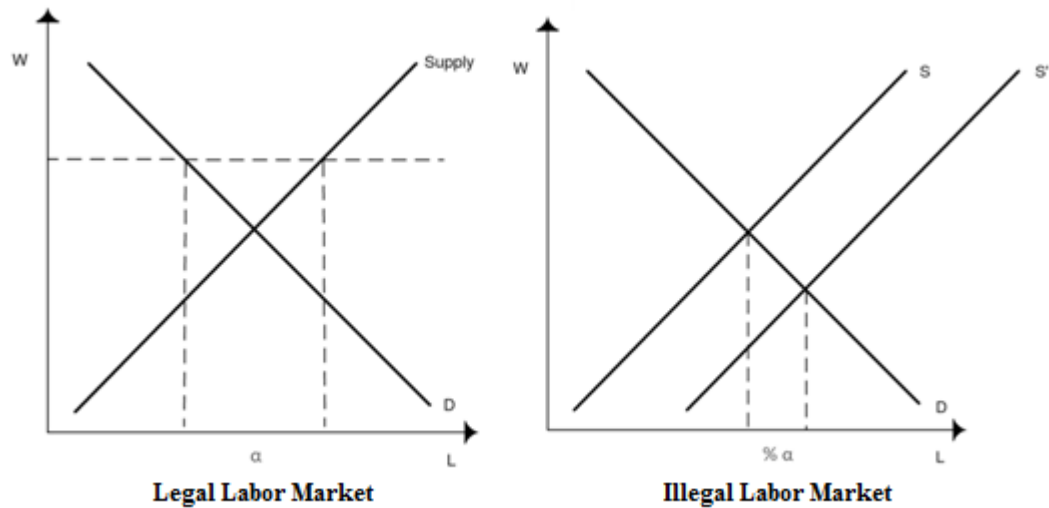


Figure 2.1 Two-Period Model of Legal and Illegal Labor Markets

In general, if the wage W_{ILL} an individual is offered in the illegal labor market exceeds their $E[W_{LEG}]$ in the legal market, the offer of the illegal labor will dominate, conditional on the individual's human capital and risk preference. The individual's human capital will determine their opportunity cost of committing commercial street crimes and how much the illegal labor market will substitute for the legal labor market. Individuals also choose the share of their labor that is supplied to the illegal labor market and the legal labor market, where α is the labor surplus in the legal market and $\% \alpha$ is the share of labor supplied to the illegal labor market, where α is bounded between 0 and 1. Over time, the individual's opportunity cost of participating in the illegal labor market

may change due to changes in the labor force and other macroeconomic factors. The elasticity of the individual's labor supply may also change over time as other labor opportunities arise. Police will increase the probability of getting caught in the illegal labor market and will change the expected value of income from commercial street crime.

I employ a linear regression model with neighborhood and month fixed effects to estimate the effect of increasing the minimum wage on commercial street crime. I estimate this model using four different specifications.

$$(1) \quad LNCRIME_{ist} = \alpha + \beta_1 CRIME_{is} + \beta_2 MINWAGE_{st} + \beta_3 INTERACTION_{ist} + \beta_4 MONTHS_{st} + \beta_5 POLICE_{st} + \beta_6 LABOR_{st} + \varepsilon_i,$$

where *LNCRIME* is the logged number of crimes by crime type, city neighborhood, and unique month; *CRIME* is a binary indicator = 1 if the individual committed a commercial street crime, with separate regressions for each crime type; *MINWAGE* is a binary indicator = 1 if the state increased the minimum wage in that year, and = 0 otherwise; *INTERACTION* = 1 if *CRIME* = 1 and *MINWAGE* = 1 for each observation; *MONTHS* is the number of months since an increase in the state minimum wage, where *MONTHS* = 0 in the month where a state increases the minimum wage; *POLICE* is the number of police officers in the city where crimes take place; and *LABOR* is the percent change in the labor force participation rate by month and city.

$$(2) \quad LNCRIME_{ist} = \alpha + \beta_1 CRIME_{is} + \beta_2 LNWAGE_{st} + \beta_3 INTERACTION_{ist} + \beta_4 MONTHS_{st} + \beta_5 POLICE_{st} + \beta_6 LABOR_{st} + \varepsilon_i,$$

where *LNWAGE* is the logged minimum wage by city neighborhood and year.

$$(3) \quad PERCENT_{ist} = \alpha + \beta_1 CRIME_{is} + \beta_2 MINWAGE_{st} + \beta_3 INTERACTION_{ist} + \beta_4 MONTHS_{st} + \beta_5 POLICE_{st} + \beta_6 LABOR_{st} + \varepsilon_i,$$

where *PERCENT* is the percent of crimes by crime type, city neighborhood, and unique month.

$$(4) \quad PERCENT_{ist} = \alpha + \beta_1 CRIME_{is} + \beta_2 LNWAGE_{st} + \beta_3 INTERACTION_{ist} + \beta_4 MONTHS_{st} + \beta_5 POLICE_{st} + \beta_6 LABOR_{st} + \varepsilon_i,$$

I group the commercial street crimes into separate samples where the treatment group includes commercial street crimes and the control group includes traffic crimes. Regressions are run separately for each sample with fixed effects by neighborhood and unique month in the sample. I also include two-way clustering by both city neighborhood and unique month. Individual-city regressions are also run separately and reported in the Appendix.

I employ individual arrest data for Chicago and New York City (City of Chicago 2015; New York City Police Department 2015). As all individuals in the data were arrested, a control group for income-producing crime was necessary. Traffic crimes, such as speeding, running a red light, or driving while under the influence, are also included in the datasets. Traffic crimes are an ideal control group because they are non-commercial street crimes, they are relatively unresponsive to changes in the minimum wage, they are not usually considered criminal activity, and they contain a broad range of arrestee demographics. It is also useful to compare commercial street crimes with traffic crimes as individuals arrested for these crimes should have more similar non-violent risk preferences. Comparing commercial street crimes with homicide, for example, may pool

individuals with very different risk preferences. The arrest data include the type of crime the individual was arrested for, demographic information for the arrestee, the location of the crime and the arrest, and several other identifying variables.

The state minimum wages in this study exceed the federal minimum wage during the sample period. I do not consider city-level prevailing wage laws in this study, which apply more commonly to government contract positions and not the low-skilled labor I consider in this study. Individual-city-level crimes are then compared with state-level changes in the minimum wage.

Data

I employ individual-level arrest data from the Chicago Police Department and stop-and-frisk and arrest data from the New York Police Department, which includes information on the crime associated with each arrest, the location of the arrest, and the date of their arrest. I pool these samples as I am interested in the observation of crime rather than arrests for crime. Crime data is aggregated to the level of neighborhoods by unique month in the study. I consider both the number of crimes by neighborhood and unique month as well as the percent of commercial street crimes by neighborhood and unique month. Table 2.1 lists the summary statistics for the variables in this study.

Table 2.1 Summary Statistics

Variable	Mean	S.D.	Min	Max
Number of Crimes				
<i>Commercial</i>	4.99	1.52	0.00	7.45
<i>Traffic</i>	1.26	1.69	0.00	5.41
Percent of Crimes				
<i>Commercial</i>	0.01	0.03	0.00	0.39
<i>Traffic</i>	0.04	0.06	0.00	1.00
Minimum Wage Indicator	0.50	0.50	0	1
Logged Minimum Wage	2.09	0.04	2.01	2.17
Months Since Minimum Wage Change	14.50	10.53	0	35
Logged Police Officers	10.24	0.42	9.48	10.48
Labor Force Percent Change	0.001	0.004	-0.010	0.010
Share of Democrats	0.61	0.04	0.54	0.66

Note: Study includes individual indicators for each crime type with each sample including 50 percent traffic crimes by neighborhood and unique months and 50 percent commercial street crimes by crime type. N = 9,792 for the number of crimes samples and N = 9,404 for the percent of crimes samples.

I use crime observations from Chicago and New York City to conduct a within-city, across-time analysis. The Chicago data come from publicly available Chicago Police Department arrest data for 2006-2009, and the New York City data come from the New York City Stop and Frisk database for 2006-2009. State minimum wage data come from the U.S. Department of Labor. Definitions for each variable in are included in Table 2.2.

Table 2.2 Variable Descriptions

Variable	Definition
Number of Crimes	Number of commercial or traffic crimes for each unique month and city neighborhood, logged.
Percent of Crimes	Percent of commercial or traffic crimes relative to the total number of crimes, for each unique month and city neighborhood
Minimum Wage Indicator	A binary arrest indicator = 1 if the state increased its minimum wage that year, and = 0 otherwise.
Logged Minimum Wage	The state minimum wage by year
Crime Indicator	A binary indicator = 1 for commercial street crimes and = 0 for traffic crimes
Crime*Minimum Wage	A binary indicator = 1 if the crime indicator = 1 and the minimum wage indicator = 1, and = 0 otherwise
Months Since Minimum Wage Change	The number of months since a minimum wage increase
Logged Police Officers	The number of police officers by city and year, logged
Labor Force Percent Change	Percent change in the labor force by city and year
Share of Democrats	The percent of Democrats in the state legislature by state and year

Note: Crime data reflect the total number of commercial street and traffic crimes. The individual crime types of prostitution, drug sales, theft, and other commercial street crimes are paired with traffic crimes in the same way as the overall sample.

Importantly, the cities in this study do not increase their minimum wages during the study period. I conduct a balanced panel using 25 local neighborhood identifiers for Chicago using the city districts identified in the original data, 77 local neighborhood identifiers for New York City using the police precincts identified in the original data, 48 unique months, two crime groups in each analysis, and the comparison of the two cities. There are 9,792 observations in each panel using the number of crimes, and 9,404 observations in each panel using the percent of commercial street crime. There are fewer observations in the panel using the percent of commercial street crimes due to some observations being dropped with a base of zero.

Table 2.3 Timeline of Minimum Wage Changes by State

Year	Illinois		New York	
	Wage	Change Indicator	Wage	Change Indicator
2006	6.50	0	6.75	1
2007	6.50	0	7.15	1
2008	7.50	1	7.15	0
2009	7.75	1	7.15	0

Note: A change indicator = 1 indicates the state increased the minimum wage that year. The Illinois minimum wage listed applies to employers of four or more.

The data from Chicago and New York City provide a clean natural experiment. Table 2.3 illustrates that in 2006 and 2007, Illinois did not increase its minimum wage but New York did increase its minimum wage. Then, in 2008 and 2009, Illinois increased its minimum wage but New York did not. This provides an opportunity for a city-to-city difference-in-differences analysis for changes in the minimum wage using four years of arrest data. Trends for commercial street and traffic crimes for both cities are illustrated in Figure 2.2.

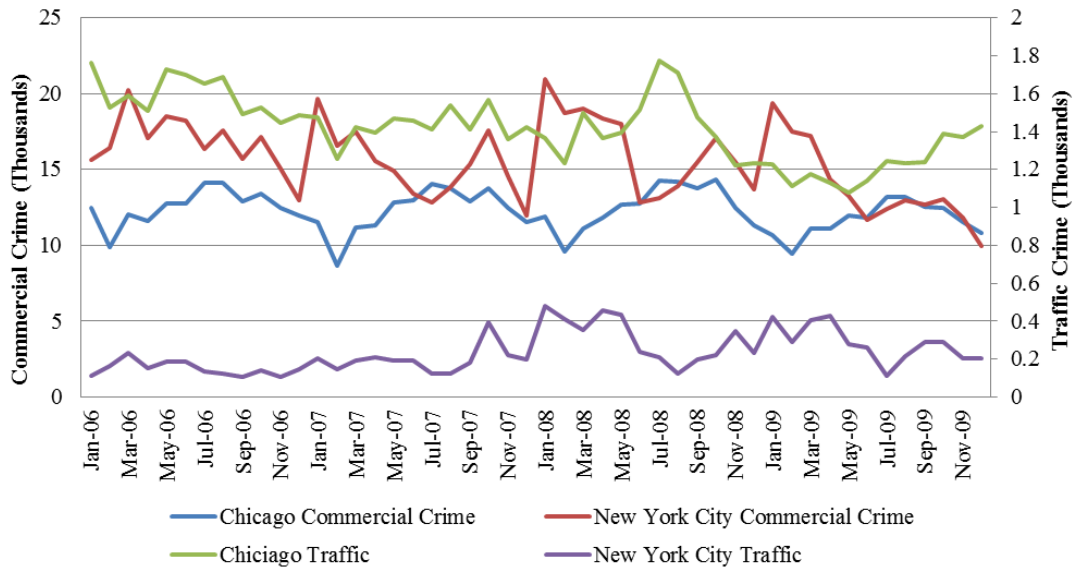


Figure 2.2 Commercial Street and Traffic Crime Trends

Note: Data come from the Chicago and New York City arrest data.

I create a binary indicator for each crime in the study, where the commercial street crime = 1 as the treatment group and the traffic crime = 0 as the control group. I include an indicator variable for a minimum wage increase = 1 for a given year when a state increases the minimum wage. If a state increases the minimum wage in Year 2 and Year 4 but not in Year 3, the minimum wage variable = 1 for Year 2 and Year 4, and = 0 for Year 3. Historical state minimum wage data comes from the U.S. Department of Labor. As a robustness test, I also include the logged minimum wage level to account for the magnitude of a minimum wage increase. I then create an interaction variable between the crime indicator variable and the minimum wage variable, where the interaction = 1 when the crime indicator = 1 and the minimum wage indicator variable = 1.

I also include a linear time variable for the number of months since an increase in the state minimum wage. When New York increased its minimum wage in 2006, for example, the number of months since a minimum wage increase = 0, and the month afterward = 1, and so forth. This accounts for the passage of time associated with changes in the minimum wage, over which time the labor supply for illegal markets may become more elastic. A positive sign suggests that labor substitutes into illegal labor markets as demand in legal labor markets becomes more inelastic, while a negative sign suggests that labor substitutes out of illegal labor markets as demand in legal labor markets becomes more elastic. I also include the total number of police officers in Chicago and New York City by year, which comes from the UCR. Although the direction of the effect between more police and crime is difficult to identify, I only attempt to control for the effect of more police. Finally, I also include the monthly percent change in the labor force participation rate for each city, which comes from the Bureau of Labor Statistics Local Area Unemployment Statistics. A positive sign here suggests that as the legal labor force participation grows, the illegal labor force participation should fall.

As previously discussed, I also include fixed effects by each city's local neighborhood and unique month in the study. As crime is local, I can expect that crime occurs near the criminal's home based on the sociological distance decay function (Brantingham and Brantingham 1995; O'Leary 2011). This allows me to use local neighborhood fixed effects for the arrest location to control for local neighborhood differences within cities. Finally, I include the share of Democratic state legislatures by

state and year in an instrumental variable robustness test. Data on state legislator parties comes from Ballotpedia.

Results

Table 2.4 reports the results for the t–test comparisons between the treatment group of commercial street crimes and the control group of traffic crimes, using the individual number of crimes. There are significant differences between the treatment and control groups for the minimum wage variables of interest. However, it is necessary to consider the separate interactions between the comparison groups and the minimum wage increase between cities over time. I report the OLS marginal estimates for all results.

Table 2.4 Mean Comparisons and T-Tests for Commercial Street and Traffic Crimes

Variable	Mean	Commercial	Traffic	T-Test
Commercial Street Crimes	3.124 (0.025)	4.986 (0.022)	1.262 (0.024)	-110
Commercial Percent	0.514 (0.005)	0.992 (0.000)	0.035 (0.001)	-1000
Minimum Wage Indicator	0.504 (0.000)	0.505 (0.000)	0.469 (0.002)	-18.63
Logged Minimum Wage	2.084 (0.000)	2.084 (0.000)	2.080 (0.000)	-21.73

Note: Standard errors are listed in parentheses.

I first establish that the treatment and control groups are comparable by separating the treatment group of commercial street crimes from the control group of traffic crimes

(Table 2.5). I find different signs and magnitudes for the estimates for all variables except the labor force participation rate.

Table 2.5 Results for Separate Crime Samples

Number of Crimes	Commercial	Traffic
Minimum Wage Indicator	-0.05 (0.04)	-0.15*** (0.05)
Months Since Minimum Wage Increase	-0.02*** (0.00)	0.01*** (0.00)
Police Officers	11.51*** (4.39)	-21.97*** (4.80)
Labor Force Percent Change	-13.63*** (3.64)	-13.58*** (3.99)
Observations	4,896	4,896
Groups	3,748	3,748
R-Squared	0.869	0.868

Note: Results reported using Model 1. Robust standard errors are in parentheses. * P < .10; ** P < .05; *** P < .01.

I report the results for the number of commercial street crimes in Table 2.6. Increasing the minimum wage increases arrests for commercial street crime by 14 percent. Crime is increasing in both models, and while the binary minimum wage indicator is negatively associated with commercial street crime, the logged minimum wage is positively associated with crime. The number of months since a minimum wage increase is only significant in the model with the logged minimum wage. This suggests an elastic illegal labor market supply such that over time, individuals shift their labor into illegal labor markets as legal labor market opportunities or other opportunities become less available. Policing has a negative but insignificant effect in the model with the binary

minimum wage indicator but has a positive and significant effect in the model with the logged minimum wage. The percent change in the labor force participation rate is negatively associated with the number of commercial street crimes, such that as the legal labor market supply grows the illegal labor market supply falls.

Table 2.6 Results for the Effect of a Minimum Wage Change on the Number of Commercial Street Crimes

Number of Crimes	(1a)	(2a)
Crime Indicator	3.65***	3.28**
	(0.03)	(1.33)
Minimum Wage Indicator	-0.17***	
	(0.04)	
Logged Minimum Wage		8.94***
		(3.07)
Crime * Minimum Wage	0.14***	0.22
	(0.05)	(0.64)
Months Since Minimum Wage Increase	-0.00	0.02***
	(0.00)	(0.01)
Police Officers	-5.23	12.64**
	(3.57)	(5.47)
Labor Force Percent Change	-13.61***	-13.61***
	(2.98)	(2.98)
Observations	9,792	9,792
Groups	3,748	3,748
R-Squared	0.866	0.866

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

I report the results for the number of individual crime types in Table 2.7.

Table 2.7 Results for the Effect of a Minimum Wage Change on the Number of Crimes

	Prostitution		Drug Sales		Theft		Other	
	(1b)	(2b)	(1c)	(2c)	(1d)	(2d)	(1e)	(2e)
Crime Indicator	-0.39*** (0.03)	2.04** (1.03)	0.91*** (0.03)	4.35*** (1.40)	3.54*** (0.03)	2.63** (1.33)	-0.16*** (0.02)	2.79*** (0.73)
Minimum Wage Indicator	-0.21*** (0.05)		-0.30*** (0.05)		-0.16*** (0.04)		-0.07** (0.04)	
Logged Minimum Wage Level		21.07*** (3.83)		20.24*** (3.50)		8.86*** (3.08)		-3.76 (3.03)
Crime * Minimum Wage	-0.02 (0.04)	-1.17** (0.49)	0.18*** (0.05)	-1.61** (0.67)	0.12*** (0.05)	0.46 (0.64)	0.24*** (0.03)	-1.36*** (0.35)
Months Since Minimum Wage Increase	0.01*** (0.00)	0.06*** (0.01)	-0.00 (0.00)	0.041*** (0.01)	-0.00 (0.00)	0.02*** (0.01)	0.01*** (0.00)	-0.00 (0.01)
Police Officers Labor Force Percent Change	-25.36*** (4.42)	15.10** (6.70)	-10.10** (4.06)	28.27*** (6.26)	-4.76 (3.57)	13.20** (5.47)	-7.36** (3.43)	-16.13*** (5.24)
	-11.26*** (3.71)	-11.26*** (3.71)	-11.65*** (3.37)	-11.65*** (3.37)	-14.25*** (2.30)	-14.25*** (3.00)	-9.73*** (2.82)	-9.73*** (2.82)
Observations	9,792	9,792	9,792	9,792	9,792	9,792	9,792	9,792
Groups	3,748	3,748	3,748	3,748	3,748	3,748	3,748	3,748
R-Squared	0.755	0.755	0.621	0.621	0.860	0.860	0.791	0.789

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

I find that increasing the minimum wage increases arrests for drug sales by 18 percent, arrests for theft by 12 percent, and arrests for other commercial street crimes by 24 percent. I also find that the number of months since a minimum wage increase is positive and significant for all four crime types, although the model using the binary minimum wage indicator is not significant for drug sales or theft. I find effects similar to the full commercial street crime sample for police officers and the labor force participation rate

I report the results for the percent of commercial street crimes in Table 2.8. In this case, increasing the minimum wage would increase the percent of arrests for commercial street crime by 0.01 percentage points. The percent of commercial street crime is increasing in both models, and while the binary minimum wage indicator is negatively associated with the percent of commercial street crime, the logged minimum wage is positively associated with the percent of crime.

Table 2.8 Results for the Effect of a Minimum Wage Change on the Percent of Commercial Street Crime

Percent of Crimes	(3a)	(4a)
Crime Indicator	0.95***	0.83***
	(0.00)	(0.07)
Minimum Wage Indicator	-0.01***	
	(0.00)	
Logged Minimum Wage		0.74***
		(0.17)
Crime * Minimum Wage	0.01***	0.06*
	(0.00)	(0.04)
Months Since Minimum Wage Increase	0.01	0.00***
	(0.17)	(0.00)
Police Officers	0.00***	0.63**
	(0.00)	(0.30)
Labor Force Percent Change	-0.90***	0.01
	(0.20)	(0.17)
Observations	9,404	9,404
Groups	3,748	3,748
R-Squared	0.993	0.993

Note: Model 3 reports the results using a binary indicator for a minimum wage increase. Model 4 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

The percent change in the labor force participation rate is negatively associated with the number of commercial street crimes in the model with the binary minimum wage indicator and positive in the model with the logged minimum wage. I report the results for the percent of individual crime types in Table 2.9.

Table 2.9 Results for the Effect of a Minimum Wage Change on the Percent of Crimes

	Prostitution		Drug Sales		Theft		Other	
	(3b)	(4b)	(3c)	(4c)	(3d)	(4d)	(3e)	(4e)
Crime Indicator	-0.03*** (0.00)	0.04 (0.05)	0.04*** (0.00)	0.14 (0.13)	0.85*** (0.00)	0.24* (0.13)	-0.02*** (0.00)	0.09* (0.06)
Minimum Wage Indicator	-0.02*** (0.00)		-0.02*** (0.00)		-0.00 (0.01)		-0.01*** (0.00)	
Logged Minimum Wage Level		1.19*** (0.17)		1.29*** (0.36)		0.62 (0.41)		0.09 (0.20)
Crime * Minimum Wage	0.01*** (0.00)	-0.03 (0.03)	0.02*** (0.00)	-0.05 (0.06)	-0.01** (0.00)	0.29*** (0.06)	0.01*** (0.00)	-0.05* (0.03)
Months Since Minimum Wage Increase	0.00*** (0.00)	0.00*** (0.00)	0.00 (0.00)	0.00*** (0.00)	0.00 (0.00)	0.00** (0.00)	0.00*** (0.00)	0.00* (0.00)
Police Officers	-1.67*** (0.21)	0.64** (0.31)	-1.15*** (0.34)	1.34** (0.57)	-0.68* (0.41)	0.82 (0.65)	-0.77*** (0.22)	-0.64* (0.35)
Labor Force Percent Change	0.21 (0.17)	0.21 (0.17)	0.34 (0.37)	0.34 (0.37)	-0.54 (0.42)	-0.54 (0.42)	0.38** (0.19)	0.38** (0.19)
Observations	9,404	9,404	9,404	9,404	9,404	9,404	9,404	9,404
Groups	3,748	3,748	3,748	3,748	3,748	3,748	3,748	3,748
R-Squared	0.480	0.480	0.340	0.338	0.966	0.966	0.397	0.397

Note: Model 3 reports the results using a binary indicator for a minimum wage increase. Model 4 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

I find that increasing the minimum wage increases the percent of arrests for prostitution by 0.01 percentage points, the percent of arrests for drug sales by 0.02 percentage points, and the percent of arrests for other commercial street crime by 0.01 percentage points. I find that increasing the minimum wage decreases the percent of arrests for theft by 0.01 percentage points. I find larger effects compared to the full commercial street crime sample for police officers and insignificant effects for the labor force participation rate except for other commercial street crimes.

I also address the possibility that unobservable variables may be driving the results using the selection on unobservable variables test (Altonji, Elder, and Taber 2005; Bellows and Miguel 2009; Nunn and Wantchekon 2011). I use a restricted model that does not include the police (Table 2.10). I do not find evidence that unobservable variables are driving the results, especially for the variables of interest. In fact, the estimates for the interaction term are the same for both the full and restricted models. Further, in several cases the test produces a null value as the estimates for the variables of interest are the same in both the full and restricted models. This suggests the results are not being driven by omitted variable bias. I also conduct a linear trends test to determine whether the results are simply the result of changes over time and do not find support for this hypothesis.

Table 2.10 Test for Omitted Variable Bias With Full and Restricted Models

Number of Crimes	β^F	β^R	Ratio
Crime Indicator	3.65*** (0.03)	3.65*** (0.03)	.
Minimum Wage Indicator	-0.17*** (0.04)	-0.15*** (0.04)	7
Crime * Minimum Wage	0.14*** (0.05)	0.14*** (0.05)	.
Months Since Minimum Wage Increase	-0.00 (0.00)	-0.01*** (0.00)	1
Police Officers	-5.23 (3.57)		2
Labor Force Percent Change	-13.61*** (2.98)	-13.88*** (2.98)	50
Observations	9,792	9,792	
Groups	3,748	3,748	
R-Squared	0.866	0.866	

Note: Results reported using Model 1 using a binary indicator for a minimum wage increase. A ratio of 7 suggests that unobservable variables would have to explain 7 times as much as observable variables to change the result for the variable of interest. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Although minimum wage changes occur at the state level and crimes are measured at the city neighborhood level, it is possible that state governments increase the state minimum wage in response to city crime, particularly when crime occurs in major metropolitan cities. I therefore include an instrumental variable test using the total share of Democratic state legislators in the Illinois and New York state legislatures by city and year. Although Democratic state legislators are more likely to support minimum wage increases and could increase the state minimum wage in response to crime rates, it is unlikely that the share of state Democrats would affect an individual's choice to commit a crime (*The Washington Times* 2016; Bolton 2013; Campbell 2017; *NBC 13* 2015).

I first conduct a Durbin-Wu-Hausman test for endogeneity and find support for including the instrumental variable test. The results for these regressions are reported in Table 2.11, which uses the primary model of the binary minimum wage indicator and the number of commercial street crimes. This model omits the police as it is collinear with the share of state Democrats. The instrumental variable test supports the initial results, as the signs and magnitudes of the variables of significance are consistent with the primary OLS analysis.

Table 2.11 Instrumental Variable Test Using the Share of State Democrats

	First Stage - Minimum Wage	IV – Number of Crimes
Crime Indicator	-0.04*** (0.00)	3.65*** (0.03)
Minimum Wage Indicator	.	-0.20*** (0.05)
Crime * Minimum Wage	0.08*** (0.00)	0.15*** (0.04)
Months Since Minimum Wage Increase	-0.05*** (0.00)	-0.01*** (0.00)
Labor Force Percent Change	-2.80** (1.26)	-13.87*** (2.98)
Observations	9,792	9,792
Groups	9,792	3,748
R-Squared	0.960	0.865

Note: First stage results use the residual of the minimum wage indicator. Results reported using Model 1. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

I also report the results for the individual cities of Chicago and New York City in the Appendix.

Conclusion

The results of this study suggest that an increase in the minimum wage substitutes labor into some illegal markets. This also suggests that labor is not homogenous to illegality. Unemployed individuals may work in the shadow economy, off the books, or depend on other social and welfare networks, including private savings. This study illustrates that some labor may also shift into illegal labor markets for commercial street crime.

It is possible that sample selection from false arrests is influencing the study results, such that more individuals are falsely arrested for crimes after an increase in the minimum wage. Although police officers may have an incentive to engage in false arrests, the incentives associated with police making false arrests should not be influenced by changes in the minimum wage as police officers are not paid at the minimum wage. Similarly, although conviction rates likely do not include the full sample of arrests, it is unlikely that conviction rates for a subset of crimes would change after an increase in the minimum wage. If conviction rates are significantly different from arrest totals, I would expect the conviction rates to be related to other new legislation that was enacted at the same time as the minimum wage increase and would therefore treat both commercial street crimes and traffic crimes in the same way.

The capital intensity of the market may also determine which illegal labor market individuals enter, so the demand for labor in complex illegal markets may be more

inelastic than the demand for labor in illegal markets with low barriers to entry. There are also oligopolies and local monopsonies within cities in both legal and illegal labor markets. Although labor in monopsonistic markets may not shift into illegal markets, monopsony labor is not the representative low-skill, high-risk labor supply most susceptible to unemployment from the minimum wage. Other related factors, such as changes in demand, may also influence the magnitude of the effect from the minimum wage but the selection effect test suggests that omitted or unobservable variables are not driving the primary results.

I find a positive and significant relationship between an increase in a state's minimum wage and local commercial street crimes, including separate analyses for different types of commercial street crimes. I also find that the amount of time since a minimum wage increase influences the number of commercial street crimes, illustrating an elastic labor supply response. Given the theoretical implications of a labor surplus following a minimum wage increase, these results are particularly illuminating for considering how individuals in the labor surplus respond to unemployment.

CHAPTER 3 – SUPREME COURT DECISIONS AND ENTREPRENEURS

The Supreme Court of the United States, as the highest judicial authority in the country, has the final say on whether a law is constitutional. Thus there is a high opportunity cost when the Court makes a decision that limits individuals in that it cannot be reversed by an entity other than the Court. Although the U.S. Congress can pass new laws to legislate around Court rulings, the Court can in turn find that new law to be unconstitutional based on the new precedent as soon as a party brings a suit. The speed with which individuals and groups can bring suits against new laws has been recently illustrated by the flurry of lawsuits surrounding the 2010 Patient Protection and Affordable Care Act (Savage 2014). Therefore, when the Supreme Court rules on an issue involving economic activity, it represents a form of market planning. National and state legislatures also engage in more traditional market planning by passing laws that restrict or redirect economic activity, but the effects of Supreme Court cases are of particular interest given the national scope and application of Court decisions.

There is a long history of courts engaging in market planning which can change incentives for entrepreneurs. In the 1889 case of *Dent v. West Virginia*, Justice Stephen Field held that “the power of the state to provide for the general welfare of its people authorizes it to prescribe all such regulations as, in its judgment, will secure or tend to secure them against the consequences of ignorance and incapacity as well as of deception

and fraud.” The 1938 Supreme Court case of *United States v. Carolene Products Co.* further changed the precedent for how the regulation of economic activity would be treated in courts and legislatures. Importantly, Footnote Four in that case provides the legal foundation for the rational basis test that courts use when assessing the validity of legislation or regulation (Abbot 2013; Gilman 2004; Mellor 2012). The term rational basis is a misnomer as it instructs courts to assume there is a rational basis for legislation or regulation, even if the legislature or regulatory agency never provided a rational basis for the law when it was introduced. Instead, courts can simply defer to the legislation if a rational basis can be theorized (The Notorious RBT (Rational Basis Test)). More recently, in 1955 the Supreme Court sanctioned state requirements such as licensing in *Williamson v. Lee Optical*. These state mandates continue to affect entrepreneurs starting new businesses more than they affect established businesses that are grandfathered into new policies (*Thomas v. Collins* 1945, *Meadows v. Odom* 2003, *Vong v. Sansom* 2009, Theiss 2011).

If courts engage in market planning, they can potentially influence the labor supply. Entrepreneurs play an essential role in a modern market-based economy: they innovate; they create new industries, as happened in Silicon Valley in the 1990s; they employ many individuals; and they create wealth, accounting for 55 percent of all jobs overall and for 66 percent of all new jobs since the 1970s (U.S. Small Business Administration 2017). Entrepreneurship and self-employment also provide opportunities for minorities and immigrants to create wealth outside of traditional firms and institutions (Hamilton 2000; Heilman and Chen 2003; Fairlie 2005). There are many kinds of

entrepreneurs but in this study I allow the supply of entrepreneurs to be represented by the supply of self-identified employers and the self-employed, in order to allow the labor supply to represent the largest baseline over the period sample. However, despite the importance of entrepreneurs, the U.S. labor supply began to shift toward employment at firms and participation in union organizations during the last half of the twentieth century (G. R. Carroll and Mosakowski 1987; Kleiner and Krueger 2013). It is therefore worth considering the role that courts played in this transition and what incentives entrepreneurs, and specifically productive entrepreneurs, face from court decisions (Baumol 1990).

When a court changes the precedent for the regulation of economic activity, the entrepreneur must re-assess their production function. Courts may reduce uncertainty by clarifying law, or they may increase uncertainty about what new laws may be upheld in the future. Any new laws or regulations that are upheld represent a mix of compliance and transaction costs for business owners who must either comply with the new laws, operate in violation of the law or regulation with the attendant risk of being caught and penalized, or shut down their business. The shape of the business owner's cost curves may change, particularly for labor regulation that changes the compensation package they offer or must provide to their employees. Under one set of court decisions, entrepreneurs may be confident that they may operate with clarity about the regulation mix they will face. Under a different set of court decisions, entrepreneurs may face uncertainty about the costs and regulations they will have to face, which may precipitate a shut-down

decision for the business and result in the entrepreneur becoming an employee or exiting the labor force.

Given the role of entrepreneurs and small businesses in innovation and economic growth, and the influence that courts can exert over markets, I hypothesize that court decisions can significantly influence the growth of entrepreneurs. The direction of the effect will depend in large part on the content of each court case but I argue that federal court decisions are not arbitrary factors unimportant to the individual entrepreneur's choice set or production function. This study therefore adds to the literature by directly considering how individual entrepreneurs respond to court decisions using Census employment data and a database of Supreme Court cases. I find that while the labor supply of entrepreneurs increases in response to court decisions, the share of the labor supply from entrepreneurs falls, suggesting a U-shaped effect where the labor supply of entrepreneurs is increasing at a decreasing rate.

Background

As precedent-altering court cases may influence the labor supply of entrepreneurs, it is worthwhile to consider the content of and potential effects from a selection of landmark Supreme Court cases regarding the regulation of economic activity. Although these cases do not represent a comprehensive list of cases, they do highlight the scope and issues involved in the regulation of economic activity. State and local courts could also affect the choice set and production function of entrepreneurs in their jurisdiction, but I focus here on Supreme Court cases as these would have the broadest national effect.

Dent v. West Virginia (1889) was the first Supreme Court case that dealt with occupational licensing but it also created a legal precedent for the regulation of economic activity by states. The case involved the licensing of medicine, which the Court upheld on the grounds of protecting the public health and safety. The majority opinion states that “it is undoubtedly the right of every citizen of the United States to follow any lawful calling, business, or profession he may choose.... But there is no arbitrary deprivation of such right where its exercise is not permitted because of a failure to comply with conditions imposed by the state for the protection of society.” The case therefore highlights that there are issues on which the state may restrict and regulate economic activity. Potential doctors in the nineteenth century and entrepreneurs in other occupations would have accounted for the possibility of their occupations being regulated more in the future. In this case, the actual intent of the decision was to restrict the supply of doctors, and therefore by design, this precedent-altering case would have reduced the supply of entrepreneurs.

Lochner v. New York (1905) was a landmark case holding that liberty of contract was implicit in the Due Process Clause of the Fourteenth Amendment in the U.S. Constitution. This case also includes the first reference to the rational basis test in Justice Oliver Wendell Holmes’ minority opinion, although the test would not become common practice for several decades. In the case, the Supreme Court reviewed a New York State law that capped the number of hours a baker could work each day at 10 and each week at 60. The Justices held that the law was unnecessary to protect the health and safety of bakers and that the law regulated the terms of employment in violation of the Fourteenth

Amendment. The majority opinion states that the law is “unreasonable, unnecessary and arbitrary interference with the right and liberty of the individual to contract.” As the regulation of hours was established law, *Lochner v. New York* altered the precedent of economic regulation and signaled to entrepreneurs that they could freely contract with their employees on the number of hours worked. It also began a period of court decisions that restricted the scope of economic regulation, known as the Lochner Era. In this study, the supply of entrepreneurs would have increased in response to the Court’s decision.

The *West Coast Hotel Co. v. Parrish* (1937) case marked the end of Lochner Era Supreme Court decisions regarding employment contracts. The Supreme Court here upheld the constitutionality of minimum wage laws and found that the liberty to contract could be restrained or regulated if adopted for the health, safety, morals, and welfare of the people. The minimum wage in this question applied to women and minors and was brought by a maid employed by the West Coast Hotel. The Court also found the state had a special interest in protecting women against employment contracts and that women needed to be protected by the state. The majority opinion includes language consistent with the rational basis test stating that “the legislature is primarily the judge of the necessity of such an enactment, that every possible presumption is in favor of its validity, and that, though the court may hold views inconsistent with the wisdom of the law, it may not be annulled unless palpably in excess of legislative power.” As this decision altered three decades of labor regulation and the right to freely contract with labor, entrepreneurs would have anticipated reduced flexibility in their hiring decisions. In this study, the case would have resulted in a fall in the labor supply of entrepreneurs.

United States v. Carolene Products Co. (1938) is the landmark case that included guidelines for whether a court should use the rational basis test in what has become known as Footnote Four. The Supreme Court considered the question of whether filled, or tinned, milk was real milk and could therefore be marketed as such to the public across state lines. This case therefore falls into the category of commercial speech, which is one of the issues included in the study sample. The Justices here determined that “‘filled milk’ means any milk cream, or skimmed milk ... to which has been added, or which has been blended or compounded with, any fat or oil other than milk fat, so that the resulting product is in imitation or semblance of milk, cream, or skimmed milk,” and therefore it was “an adulterated article of food, injurious to the public health, and its sale constitutes a fraud upon the public.” The *Carolene Products* decision made it unlawful to “ship or deliver for shipment in interstate or foreign commerce, any filled milk.” The decision did not prohibit the intrastate sale of filled milk but dramatically reduced the size of the market. The decision of this case would have been a signal to entrepreneurs that courts could significantly restrict what products they could sell, where they could sell them, and to whom. This case would have reduced the expected value of any new business venture and thus would have reduced the supply of entrepreneurs.

However, Footnote Four in the case created an even stronger signal to entrepreneurs that they would face more regulatory hurdles and restrictions in the future. Footnote Four states that

There may be narrower scope for operation of the presumption of constitutionality when legislation appears on its face to be within a specific prohibition of the Constitution, such as those of the first ten amendments, which are deemed equally specific when held to be embraced within the Fourteenth.... It is unnecessary to

consider now whether legislation which restricts those political processes which can ordinarily be expected to bring about repeal of undesirable legislation, is to be subjected to more exacting judicial scrutiny under the general prohibitions of the Fourteenth Amendment than are most other types of legislation.... Nor need we inquire whether similar considerations enter into the review of statutes directed at particular religious...or national...or racial minorities...whether prejudice against discrete and insular minorities may be a special condition, which tends seriously to curtail the operation of those political processes ordinarily to be relied upon to protect minorities, and which may call for a correspondingly more searching judicial inquiry.

Footnote Four thus held that some rights were more important than others. It specifically lists the right to vote, restraints on the dissemination of information, interferences with political organizations, and the prohibition of peaceable assembly as important rights that courts should protect. The right to contract, trade, and other commercial activities were left off the list, which is noteworthy given that *United States v. Carolene Products Co.* dealt with commercial activity. Therefore, Footnote Four would also have significantly reduced the future expected labor supply of entrepreneurs.

In 1955, the Supreme Court heard the case of *Williamson v. Lee Optical of Oklahoma, Inc.* which dealt with the licensing of opticians. In 1954, a case was brought before a United States District Court in Oklahoma challenging the state's requirements for optician licensing (*Lee Optical of Oklahoma v. Williamson*). The Lee Optical business, which sold eyeglasses, did not employ any optometrists or ophthalmologists. The state of Oklahoma had passed legislation stating only licensed opticians could sell eyeglasses. Although the district court struck down the state's licensing requirement, the state appealed to the U.S. Supreme Court, which upheld the state's occupational licensing law.

This case also marks the first time that Footnote Four from *United States v. Carolene Products Co.* was cited in a Supreme Court case, establishing a precedent. The Court's majority opinion stated that the law did not violate the Due Process Clause of the Fourteenth Amendment because the state had an interest in protecting the public health and safety. Once again, the Court's decision signaled to entrepreneurs that the state could regulate their economic activity. This case is also relevant as a more recent expansion of occupational licensing to a broader class of occupations. Occupational licensing now covers one-third of the U.S. workforce, including many entrepreneurs and immigrant business owners (Kleiner and Kudrle 2000; Meyer 2016; Whaley 2016).

In 2010, President Obama signed the Patient Protection and Affordable Care Act (ACA) into law. This law included a provision that individuals must purchase health insurance for themselves or pay a penalty. Businesses with 50 or more employees also had to provide health insurance to their employees. Several challenges to the ACA emerged with *National Federation of Independent Business v. Sebelius* moving to the U.S. Supreme Court. In the majority decision upholding the law, Chief Justice John Roberts upheld the health insurance penalty as a tax writing that "it is abundantly clear the Constitution does not guarantee that individuals may avoid taxation through inactivity. A capitation, after all, is a tax that everyone must pay simply for existing, and capitations are expressly contemplated by the Constitution. The Court today holds that our Constitution protects us from federal regulation under the Commerce Clause so long as I abstain from the regulated activity. But from its creation, the Constitution has made no such promise with respect to taxes." The Court therefore created a precedent not only

that businesses were required to provide health insurance to their employees, but that the failure to purchase a consumer good could be penalized with a tax. As current and future entrepreneurs would take these costs into account, including whether it was profitable to remain in business, the Court decision would have reduced the labor supply of entrepreneurs.

Empirical Model

I employ a two-period model to estimate the effect of Supreme Court decisions on the labor supply of entrepreneurs. Period 1 takes place before a Supreme Court decision that alters a legal precedent and Period 2 takes place after the decision. Each state has a unique Period 1 and Period 2 in the sample. In Period 2, the Court changes how economic activity is regulated in the state. The direction of the effect on employment will depend on the content of each case. However, altering an existing precedent will increase short-term uncertainty as entrepreneurs determine compliance with the new precedent and decide whether to operate under the new precedent. The court decision may also potentially reduce uncertainty as new laws are clarified and upheld. The precedent alteration may change transaction costs for entrepreneurs and compliance costs may be significant (Crews 2013). For example, although the ACA was signed into law in 2010, many business owners expected the Supreme Court to strike down key portions of the law and thus held off on ensuring compliance with the new law. Once the Supreme Court upheld the ACA, business owners could begin the process of deciding how to comply, including decisions on whether to shut down their businesses. A Supreme Court decision

may therefore influence individuals to switch from being an entrepreneur to alternative forms of employment, such as being an employee or exiting the labor market.

I employ a linear regression model with state and Census-year fixed effects to estimate the effect of Supreme Court decisions on entrepreneurship.

$$(1) \quad \text{LNENT}_{ist} = \alpha + \beta_1 \text{TREATED}_{is} + \beta_2 \text{TREATMENT}_{st-n} + \beta_3 \text{INTERACTION}_{ist} + \beta_4 \text{LFP}_{st} + \beta_5 \text{LNGDP}_{st} + \varepsilon_i,$$

where LNENT is the logged number of entrepreneurs by state and Census year; TREATED is a binary indicator = 1 if a precedent-altering case originated from that state, and = 0 otherwise; TREATMENT is a binary indicator = 1 if a precedent-altering case occurred in a given ten-year Census period by state, and = 0 otherwise; INTERACTION = 1 if TREATED = 1 and TREATMENT = 1 by state and Census year; LFP is the labor force participation rate for the Census year, and LNGDP is the logged ten-year average national GDP by state and Census year.

In a separate analysis I use the percent of entrepreneurs relative to the total labor reported in the Census as the outcome variable.

$$(2) \quad \text{PERCENT}_{ist} = \alpha + \beta_1 \text{TREATED}_{is} + \beta_2 \text{TREATMENT}_{st-n} + \beta_3 \text{INTERACTION}_{ist} + \beta_4 \text{LFP}_{st} + \beta_5 \text{LNGDP}_{st} + \varepsilon_i,$$

where PERCENT is the percent of entrepreneurs relative to the total labor supply, by state and Census year.

I include fixed effects by state and Census year to control for unobservable state and time variation, which may influence which states originated cases and when precedent-altering cases occur. Fixed effects by state and Census year also control for state-level

factors that are likely correlated with the employment figures. I include robust standard errors clustered by state level.

I use states from which precedent-altering Supreme Court cases originated as the treatment group and the precedent-altering case decision year as the treatment. Although all states would be subject to the precedent-altering Supreme Court decision, I use the originating state as the treatment group given that a precedent was challenged in those states, precipitating change. States that do not originate precedent-altering cases may already be in compliance with the Court's decision given the variation in the U.S. federal system. The treatment of the precedent alteration, however, will affect all states at the same time, and thus the interaction between the terms allows me to consider how entrepreneurship changes in response to Supreme Court decisions.

Data

Table 3.1 lists the variable descriptions for this study. I employ individual employment data from the U.S. Census for 1950 to 2000 to measure the labor supply of entrepreneurs over time. The data are collected and organized by the Integrated Public Use Microdata Series (IPUMS) project. I use Supreme Court case data from the U.S. Supreme Court Database (Ruggles et al. 2015; Spaeth et al. 2016). The IPUMS data include detailed self-identified Census information on individuals' employment type, industry, status as the head of household, participation in the labor force, and type of occupation. The IPUMS data also include the individuals' self-identified status as being self-employed, an employer, a private-sector wage employee, a government employee, or a member of the U.S. Army. I use these descriptive variables to restrict the dataset to the

labor supply of the self-employed and employers as the measure of entrepreneurs. I then aggregate the data to the state and Census-year level in order to analyze trends in entrepreneurship over time. The IPUMS employment data are linked to the Supreme Court case data in a balanced panel of 304 observations. The sample includes the 50 U.S. states and the District of Columbia for six Census periods from 1950 to 2000; however, Hawaii and Alaska are not included in the analysis from 1950 as they were not yet part of the United States.

Table 3.1 Variable Descriptions

Variable	Description
Number of Entrepreneurs	The logged number of the self-employed and employers, aggregated by state and Census year
Percent of Entrepreneurs	The percent of the self-employed and employers relative to the total, by state and Census year, on a scale of 0 to 1
Treated State	= 1 if a precedent-altering Supreme Court case on economic activity originated from that state and = 0 otherwise.
Treatment	= 1 by state and Census year if a Supreme Court case on economic activity overturned precedent and = 0 otherwise
Treated State * Treatment	The interaction between Treated and Treatment where Interaction = 1 if a Treated State = 1 and the Treatment year = 1 and = 0 otherwise
Labor Force Participation	The labor force participation rate, on a scale of 0 to 100
National GDP	The logged ten-year average national GDP by Census year, chained in billions of 2009\$

Note: Entrepreneurs reflect the number of individuals self-reporting as self-employed or as employers in each Census year.

Table 3.2 lists summary statistics for the variables in this study. The IPUMS dataset includes individual observations of individual employment by Census year from 1950 to 2000. I use the logged number of the self-employed and employers from the U.S. Census as the measure of entrepreneurship and outcome variable. Although many times of entrepreneurs exist, I include individuals who self-identify as employers or as self-employed as entrepreneurs. Although some self-employers may actually be employees and reflect legal labor designations more than entrepreneurship, this does not influence the primary question of the study. In this case, even being self-employed and attached to a business would fall under similar labor regulations as individuals who are self-employed as true entrepreneurs.

Individuals were identified as the head of a household, whether they were part of the labor force, and whether they were in non-farm sectors. I restricted the data to heads of households in the non-farm labor force in order to compare similar observations over time, given that this labor group in 1950 would differ in kind and degree from this group in 2000 and matching types mitigates unobservable variable bias (J. P. Smith and Ward 1985a). Individual employment was then aggregated by state and Census year in order to estimate the impact of court decisions on state-level entrepreneurship over time. Court decisions that alter the regulatory landscape for economic activity would affect the employment decision of individuals who are self-employed or employers similarly in that these individuals must consider compliance costs, transaction costs, and the decision of whether to continue being self-employed or an employer, become an employee, or exit the labor market.

Table 3.2 Summary Statistics

Variable	Mean	S.D.	Min	Max
Number of Entrepreneurs	9.85	1.53	5.94	13.47
Percent of Entrepreneurs	0.07	0.02	0.03	0.13
Treated State * Treatment	0.10	0.30	0	1
Labor Force Participation	62.76	3.25	59.2	67.1
National GDP	8.41	0.58	7.55	9.23

Note: N = 304.

I also calculate the share of the labor supply that is self-employed or an employer in order to compare the overall growth in entrepreneurship with the relative share of entrepreneurship. In order to calculate the share of the labor supply that is self-employed or an employer in the IPUMS data, I first aggregated the total labor supply from the IPUMS data that also met the criteria of being the head of a household, in the labor force, and in a non-farm sector. I then divided the variable for the aggregate number of self-employed and employers by the total labor supply.

Table 3.3 lists the labor supply and percent share over time for entrepreneurs compared to the total labor supply. It is of particular interest that although the total number of entrepreneurs follows a rising trend over the study sample period, the share of entrepreneurs falls significantly from 1950 to 1980, a period during which courts often ruled in favor of regulating economic activity under the Warren and Burger Courts. There is notably a large jump in the reporting from 1970 to 1980 but period fixed effects should account for this.

Table 3.3 Growth in Employment Over Time

Census Year	Entrepreneurs	Total	% of Entrepreneurs
1950	458,560	6,477,342	7.1%
1960	463,160	8,136,324	5.7%
1970	630,227	10,127,765	6.2%
1980	3,972,239	62,008,765	6.4%
1990	4,934,627	67,983,360	7.3%
2000	5,598,370	76,202,911	7.3%

Note: Data analyzed from IPUMS (Ruggles et al. 2015).

The U.S. Supreme Court case database includes case information for every case from 1946 to 2015, including information on the case issue area, information on the petitioner and respondent, the outcome of the case, whether the legal precedent was altered, and more. I restricted the sample to those cases heard before 2000 and to those cases that came through clearly identified state courts in order to facilitate linking the Supreme Court data with the IPUMS data. I then used the Issue variable in order to identify which cases involved the regulation of economic activity. Table 3.4 lists the issue categories that are included in this study, such as the regulation of commercial speech, union organization, arbitration, commercial liability, mergers, and state and local taxes, including special taxes for businesses.

Table 3.4 Economic Activity Case Types in Study

Issue Code	Description
30020	Commercial speech, excluding attorneys
70010	Arbitration (in the context of labor-management or employer-employee relations)
70020	Union antitrust: legality of anticompetitive union activity
70030	Union or closed shop: includes agency shop litigation
70040	Fair Labor Standards Act
70050	Occupational Safety and Health Act
70060	Union-union member dispute (except as pertains to union or closed shop)
70070 -	Labor-management disputes
70200	
80010	Antitrust (except in the context of mergers and union antitrust)
80020	Mergers
80030	Bankruptcy (except in the context of priority of federal fiscal claims)
80040	Sufficiency of evidence: typically in the context of a jury's determination of compensation for injury or death
80050	Election of remedies: legal remedies available to injured persons or things
80060 -	Liability
80080	
80090	Employee Retirement Income Security Act
80100	State or local government tax
80110	State or local government regulation, especially of business
80120	Federal or state regulation of securities
80130	Natural resources – environmental protection
80140	Corruption, governmental or governmental regulation of other than as in campaign spending
80150	Zoning: constitutionality of such ordinances, or restrictions on owners' or lessors' use of real property
80160	Arbitration (other than as pertains to labor-management or employer-employee relations)
80170	Federal or state consumer protection: typically under the Truth in Lending Act; Food, Drug and Cosmetic Act; and Consumer Protection Credit Act
80180 -	Patents and copyrights
80210	
80220 -	Federal or state regulation of transportation regulation
80340	
80350	Miscellaneous economic regulation
100020	Federal pre-emption of state court jurisdiction
100030	Federal pre-emption of state legislation or regulation
100050	National supremacy: commodities

Note: Data come from the U.S. Supreme Court Database (Spaeth et al. 2016).

I then identified those Supreme Court cases that altered a legal precedent. I focus on precedent-altering cases as these cases change the legal landscape, whereas other cases would uphold existing laws that would have already been part of the legal landscape and

the treatment variable in this case is a binary indicator for whether a precedent-altering court case was associated with a state in the previous 10-year period. For example, the Census year of 1960 in the IPUMS data was matched with cases that occurred from 1950 to 1959, the Census year of 1970 was matched with cases that occurred from 1960 to 1969, and so forth. I do not link court case data using 10-year periods only because of the constraints of the IPUMS data. Employment trends will depend not just on the Supreme Court cases that occurred the year before but over the past several years. It would not be appropriate to link 1960 IPUMS employment data with court cases decided in 1960 as court cases decided in 1960 would not have time to influence reported employment for that Census year.

Finally, I include two national-level control variables in this study. I first include the national labor force participation rate for each Census year, such that the employment outcome variable for 1960 is matched with the labor force participation rate for 1960. The labor force participation rate comes from the U.S. Bureau of Labor Statistics. The study sample period is associated with significant growth in the labor force participation rate, particularly as women entered the labor force (Mincer 1962; Bruce 1999; J. P. Smith and Ward 1985b). I also include the logged 10-year average gross domestic product (GDP) for each 10-year period associated with the IPUMS Census data, chained in billions of 2009 dollars, in order to control for overall economic growth in the model. National GDP data come from the U.S. Bureau of Economic Analysis. This variable is linked in the same 10-year period manner as the Supreme Court case data. State-level GDP and labor force participation data are not available for the full sample period.

Results

Table 3.5 reports the results for the t-test comparisons between the treatment group of case-originating states and the control group of other states. I find significant differences between the treatment and control groups for the logged number of entrepreneurs but for no other variables. I report the OLS marginal estimates for all results. A positive sign on the interaction between case-originating states and the Supreme Court treatment indicates that the Supreme Court decision increases the labor supply of entrepreneurs. A negative sign indicates that the decision decreases the labor supply of entrepreneurs. I find no null effect, suggesting that the labor supply of entrepreneurs is sensitive to court decisions, even at the federal level.

Table 3.5 Mean Comparisons and T-Tests for Treatment and Control States

Variable	Mean	Treatment	Control	T-Test
Number of Entrepreneurs	9.84 (0.09)	10.07 (0.14)	9.68 (0.11)	-2.19
Percent of Entrepreneurs	0.07 (0.00)	0.07 (0.00)	0.07 (0.00)	0.74
Labor Force Participation	62.76 (0.19)	62.73 (0.28)	62.77 (0.025)	0.11
National GDP	8.42 (0.03)	8.41 (0.05)	8.42 (0.04)	0.015

Note: Standard errors are listed in parentheses.

I first establish that the treatment and control groups are comparable by separating the treatment group of case-originating states from the control group of other states (Table 3.6). Although I can only compare the control variables of the labor force participation

rate and logged 10-year average national GDP, I find similar results for the magnitude, direction, and sign of the coefficients between the two groups. I also conduct a linear trends test to determine whether the results are simply the result of changes in labor supply over time. I do not find evidence that the overall time trend is driving the results.

Table 3.6 Results for Separate Treatment and Control Samples

Employment	Treatment	Control
Labor Force Participation	0.530*** (0.005)	0.540*** (0.004)
National GDP (2009\$)	-0.962*** (0.079)	-1.027*** (0.042)
Observations	154	201
Groups	22	29
R-squared	0.979	0.986

Note: Model uses the outcome measure of the number of entrepreneurs by state and year Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

I report the results for the logged number of entrepreneurs in Table 3.7. I find that when the Supreme Court decided a precedent-altering case regarding the regulation of economic activity, employment associated with entrepreneurship increased about 9 percent in case-originating states compared with the control group. The labor force participation rate is also positively associated with employment. The logged 10-year average national GDP is negatively associated with entrepreneurship. This result suggests that as national GDP grew over time, relatively fewer people started their own businesses

or became employers, possibly instead shifting into industries that were driving the growth.

I also report the results for the share of entrepreneurs in Table 3.7. I find that when the Supreme Court decided a precedent-altering case regarding the regulation of economic activity, the share of employment associated with entrepreneurship fell about 0.002 percentage points in case-originating states compared with the control group. The labor force participation rate is also still positively associated with employment, and the logged 10-year average national GDP is also still negatively associated with entrepreneurship.

Table 3.7 Results for the Effect of Court Decisions on the Supply of Entrepreneurs

Employment	(1)	(2)
Treated State * SCOTUS	0.090*** (0.018)	-0.002*** (0.001)
Labor Force Participation	0.539*** (0.005)	0.002*** (0.000)
National GDP (2009\$)	-1.021*** (0.040)	-0.008*** (0.002)
Observations	355	355
Groups	51	51
R-squared	0.985	0.854

Note: Model 1 uses the outcome measure of the number of entrepreneurs by state and Census year. Model 2 uses the outcome measure of the percent of entrepreneurs by state and Census year. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Conclusion

The Supreme Court of the United States, as the highest judicial authority in the country, has the final say on whether a law is constitutional. Thus, there is a high opportunity cost when the Court makes a decision that limits individuals in that it cannot be reversed by any entity other than the Court. In particular, court decisions can change the incentives and costs faced by entrepreneurs who play an essential role in market-based economies. Using Census data and a database of Supreme Court cases I find that from 1950 to 2000, precedent-altering Supreme Court cases increased the labor supply of entrepreneurs but decreased the share of entrepreneurs in the labor force. This suggests that while precedent-altering court cases increase the supply of entrepreneurs, they do so at a decreasing rate.

It is interesting that while the total number of entrepreneurs increased over time, the share of entrepreneurs fell significantly with respect to precedent-altering interventions from the U.S. Supreme Court. This supports my hypothesis that the labor supply of entrepreneurs is sensitive to federal court interventions, despite the fact that the policy applies to the whole country and not just the states from which the case originated. It is also interesting that the effect on the number of entrepreneurs and the share of entrepreneurs have opposite signs, illustrating that court decisions can influence the labor supply of entrepreneurs in a subtle fashion.

Many other factors may influence the labor supply of entrepreneurs over time, including factors that I cannot explicitly control for in the study. The movement of women into the full-time labor force during the study period cannot be understated;

however, if these women were not the heads of households, they would not be included in the analysis. Conflict over monetary and fiscal policy would have generated uncertainty and risk for entrepreneurs during the study period. However, I find that despite these other factors, court decisions do directly impact the supply of entrepreneurs. It might be surprising that court decisions at the federal level would significantly affect the supply of entrepreneurs at the state level given the nature of the federal system in the United States, but as the federal government's policies have grown over the last half of the twentieth century, it is perhaps less surprising that court decisions would also matter. Given the importance of entrepreneurs to the modern market-based economy, the role and influence of the Supreme Court in market planning should be considered more fully.

APPENDIX
SUPPLEMENTAL TABLES

Table A1 Results for the Effect of Any Licensing on Quality – Barbers

Yelp Rating	(3a)	(3b)	(3c)	(3d)	(3e)	(3f)	(3g)
Fees	0.170						-0.704*** (0.026)
D*Fees	0.285*** (0.0487)						0.196*** (0.029)
Education		0.450*** (0.048)					-0.201*** (0.029)
Exams			0.450*** (0.0479)				0.785*** (0.076)
Grade				0.197*** (0.028)			0.419*** (0.033)
D*Grade				0.010 (0.071)			0.075 (0.054)
Age					0.638*** (0.079)		-0.396*** (0.065)
D*Age					-0.252*** (0.083)		-0.267*** (0.087)
Licensed						0.450*** (0.048)	
D*Licensed						0.450*** (0.048)	
Border	0.228*** (3.52e-08)	-0.053 (0.048)	-0.053 (0.048)	-0.050 (0.064)	-0.241*** (0.079)	-0.053 (0.048)	-0.040 (0.087)
SBO Firm Count	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)	0.000*** (0.000)	0.000*** (0.000)	-0.000*** (0.000)
Observations	4,723	4,723	4,723	4,723	4,723	4,723	4,723
Groups	44	44	44	44	44	44	44
R-Squared	0.031	0.031	0.031	0.031	0.033	0.031	0.033

Note: Variables dropped for collinearity are not reported. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A2 Results for the Effect of Any Licensing on Quality – Cosmetologists

Yelp Rating	(3d)	(3e)	(3g)
Grade	-0.221*** (0.038)		-0.258*** (0.056)
D*Grade	0.005 (0.054)		0.043 (0.067)
Age		0.040 (0.029)	0.288*** (0.058)
D*Age		-0.033 (0.053)	-0.060 (0.067)
Border	0.015 (0.035)	-0.006 (0.041)	-0.004 (0.042)
SBO Firm Count	0.000** (0.000)	0.000*** (0.000)	0.000** (0.000)
Observations	24,013	24,013	24,013
Groups	48	48	48
R-Squared	0.033	0.033	0.033

Note: Variables dropped for collinearity are not reported. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A3 Results for the Effect of Any Licensing on Quality – Manicurists

Yelp Rating	(3a)	(3b)	(3c)	(3d)	(3f)	(3g)
Fees	0.553*** (0.035)					-7.998*** (0.055)
D*Fees	0.062 (0.051)					0.047 (0.077)
Education		0.680*** (0.018)				2.064*** (0.034)
D*Education		-0.087** (0.035)				-0.148*** (0.036)
Exams			0.268*** (0.014)			-6.998*** (0.001)
D*Exams			-0.087** (0.035)			
Grade				35.240*** (0.031)		35.240*** (0.043)
D*Grade				0.072 (0.063)		0.061 (0.079)
Licensed					0.268*** (0.014)	
D*Licensed					-0.087** (0.035)	
Border	0.049 (0.031)	-0.086	-0.086	0.026 (0.049)	-0.086*** (0.000)	-0.086*** (0.000)
SBO Firm Count	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.013*** (0.000)	0.000*** (0.000)	0.013*** (0.000)
Observations	17,382	17,382	17,382	17,382	17,382	17,382
Groups	47	47	47	47	47	47
R-Squared	0.007	0.007	0.007	0.008	0.007	0.007

Note: Variables dropped for collinearity are not reported. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A4 Results for the Effect of Any Licensing on Quality -- Massage Therapists

Yelp Rating	(3a)	(3b)	(3c)	(3d)	(3e)	(3f)	(3g)
Fees	0.256*** (0.062)						0.847** (0.337)
D*Fees	0.092 (0.089)						0.478** (0.180)
Education		0.175** (0.082)					-1.156*** (0.271)
D*Education		0.191* (0.109)					0.214** (0.083)
Exams			0.272*** (0.038)				-0.617*** (0.174)
D*Exams			0.078 (0.069)				-0.249* (0.133)
Grade				-0.821*** (0.054)			-0.776*** (0.062)
D*Grade				-0.141 (0.095)			-0.327** (0.139)
Age					0.975*** (0.144)		1.067*** (0.159)
D*Age					0.012 (0.085)		-0.185* (0.105)
Licensed						0.139* (0.074)	
D*Licensed						0.237** (0.099)	
Border	0.043 (0.077)	0.146 (0.102)	0.021 (0.045)	-0.061 (0.043)	-0.021 (0.072)	0.190** (0.092)	0.178** (0.087)
SBO Firm Count	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Observations	8,484	8,484	8,484	8,484	8,484	8,484	8,484
Groups	46	46	46	46	46	46	46
R-Squared	0.028	0.028	0.028	0.028	0.028	0.028	0.029

Note: Variables dropped for collinearity are not reported. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A5 Results for the Effect of a Minimum Wage Change on the Number of Commercial Street Crimes – Chicago

	Commercial	
	(1a)	(2a)
Crime Indicator	1.88***	1.00***
	(0.02)	(0.16)
Minimum Wage Indicator	-0.35	
	.	
Logged Minimum Wage		-0.79***
		(0.30)
Crime * Minimum Wage	0.02	0.43***
	(0.03)	(0.08)
Months Since Minimum Wage Increase	0.01***	0.01***
	(0.00)	(0.00)
Police Officers	-7.58	-34.60***
	.	(8.20)
Labor Force Percent Change	40.14	22.00*
	.	(11.46)
Observations	8,250	8,250
Groups	4,125	4,125
R-Squared	0.954	0.954

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A6 Results for the Effect of a Minimum Wage Change on the Number of Crimes – Chicago

	Prostitution		Drug Sales		Theft		Other	
	(1b)	(2b)	(1c)	(2c)	(1d)	(2d)	(1e)	(1b)
Crime Indicator	-1.73*** (0.03)	1.89*** (0.24)	-1.11*** (0.02)	-0.07 (0.19)	1.77*** (0.02)	0.74*** (0.16)	-0.68*** (0.02)	-2.84*** (0.15)
Minimum Wage Indicator	-0.92 (0.00)		-0.69 .		-0.15 .		-0.00 .	
Logged Minimum Wage Level		-1.30 (0.85)		-0.87 (0.59)		-0.73** (0.307)		-0.57* (0.32)
Crime * Minimum Wage	0.22*** (0.04)	-1.73*** (0.12)	0.08*** (0.03)	-0.49*** (0.09)	0.00 (0.03)	0.50*** (0.08)	-0.15*** (0.02)	1.02*** (0.07)
Months Since Minimum Wage Increase	0.01 (0.01)	0.01 (0.01)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Police Officers	-13.88 .	-7.88 (22.64)	-11.16 .	-47.21*** (16.10)	-5.75 .	-34.23*** (8.24)	-2.36 .	-2.79 (8.77)
Labor Force Percent Change	111.90 (0.00)	15.11 (27.53)	72.00 .	32.11 (21.37)	35.63 .	22.18* (11.68)	16.14 .	13.75 (11.54)
Observations	8,250	8,250	8,250	8,250	8,250	8,250	8,250	8,250
Groups	4,125	4,125	4,125	4,125	4,125	4,125	4,125	4,125
R-Squared	0.803	0.809	0.869	0.870	0.953	0.953	0.906	0.909

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A7 Results for the Effect of a Minimum Wage Change on the Number of Commercial Street Crimes – New York City

	Commercial	
	(1a)	(2a)
Crime Indicator	3.78***	5.02***
	(0.02)	(0.23)
Minimum Wage Indicator	-1.50	
	(2.89)	
Logged Minimum Wage		17.43
		(53.52)
Crime * Minimum Wage	0.59***	-0.53***
	(0.03)	(0.11)
Months Since Minimum Wage Increase	-7.35	-0.08
	(15.31)	(0.08)
Police Officers	-0.08	-27.89
	(0.08)	(103.50)
Labor Force Percent Change	2.18	-7.35
	(9.86)	(15.31)
Observations	18,480	18,480
Groups	9,240	9,240
R-Squared	0.831	0.828

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

Table A8 Results for the Effect of a Minimum Wage Change on the Number of Crimes – New York City

	Prostitution		Drug Sales		Theft		Other	
	(1b)	(2b)	(1c)	(2c)	(1d)	(2d)	(1e)	(1b)
Crime Indicator	-0.12*** (0.01)	1.10*** (0.16)	1.24*** (0.02)	5.85*** (0.24)	3.67*** (0.02)	4.21*** (0.23)	-0.04*** (0.01)	0.05 (0.17)
Minimum Wage Indicator	0.86 (1.38)		1.39 (2.20)		-1.96 (2.91)		0.72 (1.25)	
Logged Minimum Wage Level		18.87 (25.72)		37.20 (40.61)		8.21 (53.70)		25.04 (23.41)
Crime * Minimum Wage	0.08*** (0.02)	-0.59*** (0.08)	0.59*** (0.03)	-2.19*** (0.12)	0.55*** (0.03)	-0.19* (0.11)	0.33*** (0.02)	0.01 (0.08)
Months Since Minimum Wage Increase	0.01 (0.04)	0.01 (0.04)	0.02 (0.06)	0.02 (0.06)	-0.10 (0.08)	-0.10 (0.08)	0.01 (0.04)	0.01 (0.04)
Police Officers	-3.79 (4.72)	-36.31 (49.70)	-7.45 (7.48)	-70.69 (78.52)	4.03 (9.89)	-10.19 (103.9)	-3.48 (4.28)	-47.34 (45.20)
Labor Force Percent Change	1.72 (7.44)	1.721 (7.44)	3.71 (12.00)	3.71 (12.00)	-7.46 (15.48)	-7.46 (15.48)	-1.39 (6.63)	-1.39 (6.63)
Observations	18,480	18,480	18,480	18,480	18,480	18,480	18,480	18,480
Groups	9,240	9,240	9,240	9,240	9,240	9,240	9,240	9,240
R-Squared	0.206	0.207	0.508	0.506	0.822	0.819	0.208	0.199

Note: Model 1 reports the results using a binary indicator for a minimum wage increase. Model 2 reports the results using the logged minimum wage level. Robust standard errors are in parentheses. * $P < .10$; ** $P < .05$; *** $P < .01$.

REFERENCES

- Abbot, Roger. 2013. "Is Economic Protectionism a Legitimate Governmental Interest Under Rational Basis Review?" *Catholic University Law Review* 62 (2). <http://scholarship.law.edu/lawreview/vol62/iss2/5>.
- Adams, III, A. Frank, Robert B. Ekelund, Jr., and John D. Jackson. 2003. "Occupational Licensing of a Credence Good: The Regulation of Midwifery." *Southern Economic Journal* 69 (3): 659. doi:10.2307/1061700.
- Adams, III, A. Frank, John D. Jackson, and Robert B. Ekelund, Jr. 2002. "Occupational Licensing in a 'Competitive' Labor Market: The Case of Cosmetology." *Journal of Labor Research* 23(2) 261-278.
- Ahn, Tom, Peter Arcidiacono, and Walter Wessels. 2011a. "The Distributional Impacts of Minimum Wage Increases When Both Labor Supply and Labor Demand Are Endogenous." *Journal of Business & Economic Statistics* 29 (1): 12–23. doi:10.1198/jbes.2010.07076.
- . 2011b. "The Distributional Impacts of Minimum Wage Increases When Both Labor Supply and Labor Demand Are Endogenous." *Journal of Business & Economic Statistics* 29 (1): 12–23. doi:10.1198/jbes.2010.07076.
- Akerlof, George A. 1970. "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism." *Quarterly Journal of Economics* 84 (3): 488–500. doi:10.2307/1879431.
- Altonji, Joseph G, Todd E Elder, and Christopher R Taber. 2005. "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools." *Journal of Political Economy* 113 (1): 151–84.
- Balko, Radley. 2014. "Federal Appeals Court: Stop Using SWAT-Style Raids for Regulatory Inspections." *Washington Post*, September 19. <https://www.washingtonpost.com/news/the-watch/wp/2014/09/19/federal-appeals-court-stop-using-swat-style-raids-for-regulatory-inspections>.
- Bardach, Naomi S, Renée Asteria-Peñaloza, W John Boscardin, and R Adams Dudley. 2013. "The Relationship between Commercial Website Ratings and Traditional Hospital Performance Measures in the USA." *BMJ Quality & Safety* 22 (3): 194–

202. doi:10.1136/bmjqs-2012-001360.
- Baumol, William J. 1990. "Entrepreneurship: Productive, Unproductive, and Destructive." *Journal of Political Economy* 98 (5, Part 1): 893–921. doi:10.1086/261712.
- Baumol, William J., and Robert D. Willig. 1981. "Fixed Costs, Sunk Costs, Entry Barriers, and Sustainability of Monopoly." *The Quarterly Journal of Economics* 96 (3): 405. doi:10.2307/1882680.
- Beauchamp, Andrew, and Stacey Chan. 2014. "The Minimum Wage and Crime." *The B.E. Journal of Economic Analysis & Policy* 14 (3). De Gruyter: 1–23.
- Becker, Gary S. 1974. "Crime and Punishment: An Economic Approach." National Bureau of Economic Research, Inc, 1–54.
- Bellows, John, and Edward Miguel. 2009. "War and Local Collective Action in Sierra Leone." *Journal of Public Economics* 93 (11): 1144–57. doi:10.1016/j.jpubeco.2009.07.012.
- Besanko, David, Shabtai Donnenfeld, and Lawrence J. White. 1987. "Monopoly and Quality Distortion: Effects and Remedies." *The Quarterly Journal of Economics* 102 (4): 743. doi:10.2307/1884279.
- Bloom, N., C. Propper, S. Seiler, and J. Van Reenen. 2015. "The Impact of Competition on Management Quality: Evidence from Public Hospitals." *The Review of Economic Studies* 82 (2): 457–89. doi:10.1093/restud/rdu045.
- Bolton, Alexander. 2013. "Democrats Gird for Minimum-Wage Battle." *The Hill*, November 7. <http://thehill.com/homenews/senate/189524-democrats-gird-for-minimum-wage-battle>.
- Booth, Darryl. 2014. "Yelp Partners with Health Departments to Improve Food Safety." *Journal of Environmental Health* 76 (8): 52–55.
- Bosma, Niels, Mirjam van Praag, Roy Thurik, and Gerrit de Wit. 2004. "The Value of Human and Social Capital Investments for the Business Performance of Startups." *Small Business Economics* 23 (3): 227–36. doi:10.1023/B:SBEJ.0000032032.21192.72.
- Brantingham, P, and P Brantingham. 1995. "Criminality of Place: Crime Generators and Crime Attractors." *European Journal on Criminal Policy and Research* 3 (3): 1–26.
- Bruce, Donald. 1999. "Do Husbands Matter? Married Women Entering Self-Employment." *Small Business Economics* 13 (4): 317–29.

doi:10.1023/A:1008179214572.

- Campbell, Colin. 2017. "NC Democrats File Bill to Raise Minimum Wage, Require Paid Sick Leave." *The News & Observer*. <http://www.newsobserver.com/news/politics-government/state-politics/article135983968.html>.
- Card, David E., Alan B. Krueger, David Card, and Alan Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4): 772–93.
- Carpenter, II, Dick M. 2012. "Testing the Utility of Licensing: Evidence from a Field Experiment on Occupational Regulation." *Journal of Applied Business and Economics* 13 (2): 28–41.
- Carpenter II, Dick M. 2011. "Blooming Nonsense: Do Claims about the Consumer Benefit of Licensure Withstand Empirical Scrutiny?" *Regulation* Spring: 44–47.
- Carpenter II, Dick M., Lisa Knepper, Angela C. Erickson, and John K. Ross. 2012. "License to Work: A National Study of Burdens From Occupational Licensing, 2012."
- Carroll, Glenn R., and Elaine Mosakowski. 1987. "The Career Dynamics of Self-Employment." *Administrative Science Quarterly* 32 (4): 570. doi:10.2307/2392884.
- Carroll, S.L., and R.J. Gaston. 1983. "Occupational Licensing and the Quality of Service: An Overview." *Law and Human Behavior* 7 (2): 139–146.
- City of Chicago. 2015. "Crimes Database." <https://data.cityofchicago.org/Public-Safety/Crimes-2001-to-present/ijzp-q8t2>.
- Crews, Clyde Wayne. 2013. "How Much Does Federal Paperwork And Tax Compliance Cost?" *Competitive Enterprise Institute*. <https://cei.org/blog/how-much-does-federal-paperwork-and-tax-compliance-cost>.
- Currie, Janet, and Bruce Fallick. 1996a. "The Minimum Wage and the Employment of Youth Evidence from the NLSY." *Journal of Human Resources* 31 (2): 404–28.
- . 1996b. "The Minimum Wage and the Employment of Youth Evidence from the NLSY." *Journal of Human Resources* 31 (2): 404–28.
- Dimov, Dimo P, and Dean A Shepherd. 2005. "Human Capital Theory and Venture Capital Firms: Exploring home Runs" and strike Outs"." *Journal of Business Venturing* 20 (1): 1–21. doi:10.1016/j.jbusvent.2003.12.007.
- Dorsey, Stuart. 1983. "Occupational Licensing and Minorities." *Law and Human Behavior* 7 (2–3): 171–81. doi:10.1007/BF01044521.

- Fairlie, Robert W. 2005. "Self-Employment, Entrepreneurship, and the NLSY79." *Monthly Labor Review* 128.
- Faith, Roger L, and Robert D. Tollison. 1983. "The Supply of Occupational Regulation." *Economic Inquiry* 21 (2): 232–40. doi:10.1111/j.1465-7295.1983.tb00628.x.
- Federman, Maya N, David E Harrington, and Kathy J Krynski. 2006. "The Impact of State Licensing Regulations on Low-Skilled Immigrants: The Case of Vietnamese Manicurists." *American Economic Review* 96 (2): 237–41. doi:10.1257/000282806777211630.
- Fernandez, Jose, Thomas Holman, and John V. Pepper. 2014a. "The Impact of Living-Wage Ordinances on Urban Crime." *Industrial Relations: A Journal of Economy and Society* 53 (3): 478–500. doi:10.1111/irel.12065.
- . 2014b. "The Impact of Living-Wage Ordinances on Urban Crime." *Industrial Relations: A Journal of Economy and Society* 53 (3): 478–500. doi:10.1111/irel.12065.
- Gardecki, Rosella, and David Neumark. 1998. "Order from Chaos? The Effects of Early Labor Market Experiences on Adult Labor Market Outcomes." *ILR Review* 51 (2): 299–322. doi:10.1177/001979399805100209.
- Gergaud, Olivier, Karl Storchmann, and Vincenzo Verardi. 2015. "Expert Opinion and Product Quality: Evidence From New York City Restaurants." *Economic Inquiry* 53 (2): 812–35. doi:10.1111/ecin.12178.
- Gilman, Felix. 2004. "The Famous Footnote Four: A History of the Carolene Products Footnote." *South Texas Law Review* 46.
- Gould, Eric D., Bruce A. Weinberg, and David B. Mustard. 2002. "Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997." *Review of Economics and Statistics* 84 (1): 45–61. doi:10.1162/003465302317331919.
- Hamilton, Barton H. 2000. "Does Entrepreneurship Pay? An Empirical Analysis of the Returns to Self-Employment." *Journal of Political Economy* 108 (3): 604–31. doi:10.1086/262131.
- Handy, Jim. 2012. "Think Yelp Is Unbiased? Think Again!!" *Forbes.com*, August 16. <http://www.forbes.com/sites/jimhandy/2012/08/16/think-yelp-is-unbiased-think-again/#2f0cb1cb1958>.
- Hashimoto, Masanori. 1987. "The Minimum Wage Law and Youth Crimes: Time-Series Evidence." *The Journal of Law and Economics* 30 (2): 443–64. doi:10.1086/467144.

- Heilman, Madeline E., and Julie J. Chen. 2003. "Entrepreneurship as a Solution: The Allure of Self-Employment for Women and Minorities." *Human Resource Management Review* 13 (2): 347–64. doi:10.1016/S1053-4822(03)00021-4.
- Hogan, Daniel B. 1983. "The Effectiveness of Licensing: History, Evidence, and Recommendations." *Law and Human Behavior* 7 (2–3): 117–38. doi:10.1007/BF01044517.
- Hotz, V. Joseph, and Mo Xiao. 2011. "The Impact of Regulations on the Supply and Quality of Care in Child Care Markets." *American Economic Review* 101 (5): 1775–1805. doi:10.1257/aer.101.5.1775.
- Jacob, Brian A, and Lars Lefgren. 2003. "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration, and Juvenile Crime." *American Economic Review* 93 (5): 1560–77. doi:10.1257/000282803322655446.
- Klein, Daniel B. 1998. "Quality-and-Safety Assurance Distinguishing Quality-and-Safety Restrictions from Paternalism." *The Independent Review*, no. 4: 1086–1653.
- Kleiner, Morris M. 2000. "Occupational Licensing." *Journal of Economic Perspectives* 14 (4): 189–202. doi:10.1257/jep.14.4.189.
- . 2006. "A License for Protection." *Regulation* 29 (3): 17–21.
- Kleiner, Morris M., and Alan B. Krueger. 2013. "Analyzing the Extent and Influence of Occupational Licensing on the Labor Market." *Journal of Labor Economics* 31 (2): S173-202. <http://www.jstor.org/stable/info/10.1086/669060>.
- Kleiner, Morris M, and Robert T Kudrle. 2000. "Does Regulation Affect Economic Outcomes? The Case of Dentistry." *Journal of Law and Economics* 43 (2): 547–82.
- Kolker, Robert. 2013. "The New Prostitutes." *The New York Times*, June 29. http://opinionator.blogs.nytimes.com/2013/06/29/the-new-prostitutes/?_r=0.
- Larsen, Bradley. 2015. "Occupational Licensing and Quality: Distributional and Heterogeneous Effects in the Teaching Profession." Working Paper.
- Luca, Michael. 2016. "Reviews, Reputation, and Revenue: The Case of Yelp.com." 12–016. Cambridge, Mass. [http://www.hbs.edu/faculty/Publication Files/12-016_a7e4a5a2-03f9-490d-b093-8f951238dba2.pdf](http://www.hbs.edu/faculty/Publication%20Files/12-016_a7e4a5a2-03f9-490d-b093-8f951238dba2.pdf).
- Mellor, Chip. 2012. "Armour Plating the Power of Government." *Forbes*, February 28. <https://www.forbes.com/sites/chipmellor/2012/02/28/armour-plating-the-power-of-government/#2123139c3d97>.
- Meyer, Ali. 2016. "State Hair Braiding Laws Kill Jobs, Punish Workers." *The*

- Washington Free Beacon*, August 2. <http://freebeacon.com/issues/state-hair-braiding-laws-kill-jobs-punish-workers/>.
- Mincer, Jacob. 1962. "Labor Force Participation of Married Women: A Study of Labor Supply." Princeton University Press, 63–105. <http://www.nber.org/chapters/c0603>.
- NBC 13*. 2015. "Iowa Democrats Hoping to Increase State's Minimum Wage," February 12. <http://whotv.com/2015/02/12/iowa-democrats-hoping-to-increase-states-minimum-wage/>.
- Neumark, David, and William L. Wascher. 2006. "Minimum Wages and Employment." *Foundations and Trends® in Microeconomics* 3 (1–2). Now Publishers, Inc.: 1–182. doi:10.1561/07000000015.
- New York City Police Department. 2015. "New York Police Department (NYPD) Stop, Question, and Frisk Database. ICPSR21660-v1." Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2008-06-18.
- Nunn, Nathan, and Leonard Wantchekon. 2011. "The Slave Trade and the Origins of Mistrust in Africa." *American Economic Review* 101 (7): 3221–52. doi:10.1257/aer.101.7.3221.
- O'Leary, M. 2011. "Modeling Criminal Distance Decay." *Cityscape* 13 (3): 161–98.
- Ranard, Benjamin L, Rachel M Werner, Tadas Antanavicius, H Andrew Schwartz, Robert J Smith, Zachary F Meisel, David A Asch, Lyle H Ungar, and Raina M Merchant. 2016. "Yelp Reviews Of Hospital Care Can Supplement And Inform Traditional Surveys Of The Patient Experience Of Care." 35 (4): 697–705. doi:10.1377/hlthaff.2015.1030.
- Raphael, Steven, and Rudolf Winter-Ebmer. 2001. "Identifying the Effect of Unemployment on Crime." *The Journal of Law and Economics* 44 (1): 259–83. doi:10.1086/320275.
- Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2015. "Integrated Public Use Microdata Series: Version 6.0 [Machine-Readable Database]." Minneapolis: University of Minnesota.
- Sabia, Joseph, Richard Burkhauser, and Benjamin Hansen. 2012. "Are the Effects of Minimum Wage Increases Always Small? New Evidence from a Case Study of New York State." *ILR Review* 65 (2). <http://digitalcommons.ilr.cornell.edu/ilrreview/vol65/iss2/7>.
- Savage, David G. 2014. "New Legal Battle Opens over Obamacare and Contraceptives." *Los Angeles Times*, May 8. <http://www.latimes.com/nation/la-na-court-obamacare->

20140509-story.html.

- Shapiro, Carl. 1982. "Consumer Information, Product Quality, and Seller Reputation." *The Bell Journal of Economics* 13 (1): 20. doi:10.2307/3003427.
- . 1983. "Premiums for High Quality Products as Returns to Reputations." *The Quarterly Journal of Economics* 98 (4): 659. doi:10.2307/1881782.
- . 1986. "Investment, Moral Hazard, and Occupational Licensing." *The Review of Economic Studies* 53 (5): 843. doi:10.2307/2297722.
- Sibilla, Nick. 2016. "Shampooing Hair Without A License Could Mean Jail Time In Tennessee." *Forbes.com*, May 5.
<http://www.forbes.com/sites/instituteforjustice/2016/05/05/shampooing-hair-without-a-license-could-mean-jail-time-in-tennessee/#753d5ee47f2b>.
- Smith, James P., and Michael P. Ward. 1985a. "Time-Series Growth in the Female Labor Force." *Journal of Labor Economics* 3 (1, Part 2): S59–90. doi:10.1086/298076.
- . 1985b. "Time-Series Growth in the Female Labor Force." *Journal of Labor Economics* 3 (1, Part 2) : S59–90. doi:10.1086/298076.
- Smith, Janet Kiholm. 1982. "Production of Licensing Legislation: An Economic Analysis of Interstate Differences." *The Journal of Legal Studies* 11 (1): 117–37. doi:10.1086/467695.
- Spaeth, Harold J., Lee Epstein, Andrew D. Martin, Jeffrey A. Segal, Theodore J. Ruger, and Sara C. Benesh. 2016. "2016 Supreme Court Database, Version 2016 Release 01."
- "The Notorious RBT (Rational Basis Test)." n.d. *Institute for Justice*. <http://ij.org/center-for-judicial-engagement/programs/the-notorious-rbt-rational-basis-test/>.
- The Washington Times*. 2016. "Democrat-Led Legislature Likely to Increase Minimum Wage," December 28.
<http://www.washingtontimes.com/news/2016/dec/28/democrat-led-legislature-likely-to-increase-minimu/>.
- The White House. 2015. "Occupational Licensing: A Framework for Policymakers." https://www.whitehouse.gov/sites/default/files/docs/licensing_report_final_nonembargo.pdf.
- Theiss, Evelyn. 2011. "When It Comes to Pedicures, State Agency Keeps Nail Salons on Their Toes." *Cleveland.com*, August 22.
http://www.cleveland.com/healthfit/index.ssf/2011/08/state_agency_keeps_nail_salo

ns.html.

- Thompson, Jeffrey P. 2009. "Using Local Labor Market Data to Re-Examine the Employment Effects of the Minimum Wage." *ILR Review* 62 (3): 343–66. doi:10.1177/001979390906200305.
- Thornton, Robert J., and Edward J. Timmons. 2013. "Licensing One of the World's Oldest Professions: Massage." *The Journal of Law and Economics* 56 (2): 371–88. doi:10.1086/667840.
- Timmons, Edward J., and Robert J. Thornton. 2010. "The Licensing of Barbers in the USA." *British Journal of Industrial Relations* 48 (4): 740–57. doi:10.1111/j.1467-8543.2010.00811.x.
- Tshuma, M.C, and B Jari. 2013. "The Informal Sector as a Source of Household Income: The Case of Alice Town in the Eastern Cape Province of South Africa." *Journal of African Studies and Development* 5 (8): 250–60. doi:10.5897/JASD12.042.
- U.S. District Court Eastern District of Missouri. 2015. "Niang v. Carroll, et Al. Wayne Kindle Deposition Excerpts."
- U.S. Small Business Administration. 2017. "Small Business Trends." <https://www.sba.gov/managing-business/running-business/energy-efficiency/sustainable-business-practices/small-business-trends>.
- Whaley, Kacie. 2016. "Two Black Women Win Lawsuit To Make African Hair-Braiding Legal In Iowa." *Financial Juneteenth*, July 29. <http://financialjuneteenth.com/two-black-women-win-lawsuit-make-african-hair-braiding-legal-iowa/>.
- Zavodny, Madeline. 2000. "The Effect of the Minimum Wage on Employment and Hours." *Labour Economics* 7 (6): 729–50. doi:10.1016/S0927-5371(00)00021-X.

BIOGRAPHY

Darwyn Deyo received her Bachelor of Science in Economics and Bachelor of Arts in International Area Studies from Saint Mary's College of California in 2008. She worked as a journalist before receiving her Master of Arts in Economics from George Mason University in 2014. She is a Research Fellow at the Harvey L. Neiman Health Policy Institute and is an affiliate scholar with the Center for Micro-Economic Policy Research at George Mason University. She also enjoys travelling and competitive highland dance.